

From: "Tatiana M. Dedkova" <tatm@insec.quorus.e-burg.su>
To: K.Briffa@uea.ac.uk
Subject: schijatov
Date: Thu, 7 Mar 96 09:41:07 +0500

Dear Keith, March 6, 1996

I and Eugene received your E-mail of 04.03.1996. This day I talked over the telephone with Eugene and he asked me to send an answer from both of us.

Thank you for the information concerning proposals to the INCO/COPERNICUS. We agree with your strategy used and we hope that this proposal will not be rejected.

The results of INTAS-RFBR proposal will be known at the beginning of May. We know that they received many proposals and a competition is high (only 1 in 10 proposals might get money). Of course, you included in as a participant. Fritz is a coordinator from the INTAS countries.

This year our laboratory received two small grants (approximately 8,000-10,000 USD per year) from the Russian Foundation of Basic Researches (RFBR) for the next three years: the first one for developing the Yamal supra-long chronology and the second one for developing tree-ring chronologies from living trees growing at the polar timberline in Siberia (together with Vaganov's laboratory). These money are very important for us as they will allow to maintain the staff of our laboratories.

I and Valery Mazepa were in Krasnoyarsk during one month and together with E.Vaganov wrote the manuscript of book "Dendroclimatic Studies in the Ural-Siberian Subarctic". The problem now is to find money for its publication. If we find enough money soon (20 million roubles), the book will be published this autumn. We analysed 61 mean ring-width and 6 cell chronologies which we intend to publish in form of tables in the Appendix. We can send to you all raw measurements which were used for developing these chronologies.

Of course, we are in need of additional money, especially for collecting wood samples at high latitudes and in remote regions. The cost of field works in these areas is increased many times during the last some years. That is why it is important for us to get money from additional sources, in particular from the ADVANCE and INTAS ones. Also, it is important for us if you can transfer the ADVANCE money on the personal accounts which we gave you earlier

and the sum for one occasion transfer (for example, during one day) will not be more than 10,000 USD. Only in this case we can avoid big taxes and use money for our work as much as possible. Please, inform us what kind of documents and financial reports we must represent you and your administration for these money.

I and Eugene have a possibility to participate in the Cambridge meeteng in July, but we need extra many and special invitations. If you do not have enough money to invite both of us, Eugene does not insist upon this visit.

The best wishes to you and Phil.

Yours sincerely

Stepan Shiyatov

From: "Tatiana M. Dedkova" <tatm@insec.quorus.e-burg.su>
To: k.briffa@uea.ac.uk
Subject: schiyatov
Date: Mon, 17 Jun 96 08:47:18 +0500

Dear Keith,

I have bought the tickets from Moscow to London and back. My arrival to London (Heathrow Airport) is by flight SU 245 (Aeroflot Company) on July 19. Departure from Moscow is at 20.10 (local time), arrival to London is approximately at the same local time. As I know, Evgeny Vaganov did not buy tickets until now, but he informed of my dates and can buy tickets the same flights. My departure from London to Moscow is on August 1 by the Aeroflot Company flight SU 244 at 09.00 of local time.

Please, inform me how can I arrive at Cambridge from London? Is there the program of this meeting? We must be ready to do some reports? For example, I can prepare a report about the progress in developing the Yamal supra-long chronology and together with Evgeny about dendroclimatic investigation in the Ural-Siberian subarctic.

Rashit Hantemirov and Alexander Surkov will go soon to the Yamal peninsula (June 24). This summer they want to collect subfossil material from areas which are much more remote and situated at higher latitudes. We hoped to use some money of the ADVANCE project. But we have not received this money until now and the program of collecting during this summer will be reduced.

Some days ago I received an information that the INTAS-RFBR project was rejected. The competition was very high.

Sincerely yours

Stepan Shiyatov

From: km_king@ccmail.pnl.gov
To: F028@uea.ac.uk
Subject: URGENT RESPONSE NEEDED - Early Detection Work
Date: Wed, 26 Jun 1996 16:13 -0700 (PDT)

Dr. Jones,

I am contacting you on behalf of Dave Bader and Tim Barnett regarding a couple action items in support of early detection on climate change. Based upon the anticipated award for NOAA support during fiscal year 1997 on climate change data and detection, DOE has authorized the Pacific Northwest National Laboratory (PNNL) to utilize existing funding through 9/30/96 to conduct a meeting of the experts, and to begin preliminary investigations.

PNNL would like to place a contract with you as soon as possible to provide support through 9/30/96. In order to place a contract with you, I need to submit a statement of work and signed cost proposal to our Contracts Department. If you could please fax this to me as soon as possible on (509) 375-2698, it would be greatly appreciated.

I thought your activity may look something like the following (feel free to change/edit):

Scope of Work

Dr. Phillip Jones shall begin initial work in support of the pilot project identified in the Early Detection of Climate Trends report. He shall prepare for and participate in a meeting on greenhouse signal detection, to be held in Washington, DC on September 17-18, 1996. In addition, Dr. Jones shall conduct a preliminary analysis ?????? (please provide input)

Deliverables

Prepare for and participate in 9/17-18, 1996 meetings on greenhouse signal.

Provide a summary report on the preliminary analysis of ?????? on or before September 30, 1996.

Also, for your information the current plan for the meeting is for September 17-18, 1996 at the Courtyard by Marriott - Greenbelt, 6301 Golden Triangle Drive,

Greenbelt, MD. (301) 441-3311, fax: (301) 441-4978. Government room rate is \$89/day.

When you provide your cost estimate, it would be appreciated if you could provide your hourly rate, in addition to travel estimates for the September meeting. To expedite the process, it is very helpful if can include documentation to support your hourly rate.

Please feel free to contact me with any questions. My phone number is 509-375-2861, fax is 509-375-2698.

Thank you,

Karen

From: Alan Robock <alan@atmos.umd.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: your mail
Date: Thu, 11 Jul 1996 10:07:13 -0400 (EDT)

Dear Phil,

It looks like you have found Baitoushan. Vol. 2 lists Kuwae as VEI 6 in 1452 +/- 10 AD. How accurate are your dates? By the way, Chris Newhall thinks 1600 is the Parker volcano on Mindanao in the Philippines. He hasn't published that so far, as I know.

Could you please define "utter prat" for me? Sometimes I think we speak the same language, and sometimes I'm not so sure.

I'm doing fine. We have a new building with nice new offices. I'm going to Australia next week with Sherri and Danny, and after the meeting, will visit Cairns, Adelaide, and New Zealand. I'm looking forward to skiing on a volcano, if it stops erupting.

Alan

Prof. Alan Robock
Department of Meteorology
University of Maryland
College Park, MD 20742

Phone: (301) 405-5377
Fax: (301) 314-9482
Email: alan@atmos.umd.edu
<http://www.meto.umd.edu/~alan>

On Thu, 11 Jul 1996, Phil Jones wrote:

> Alan,
> Thanks for the quick response. We'll expect something from Melissa
> in the next few weeks. I also hope our copy of the 2cnd edition arrives
> soon. In our maximum latewood density reconstruction from the polar Urals
> to AD 914, the most anomalous summer is AD 1032. A lot of other volcano
> years are there with summers of -3 to -4 sigma such as 1816,1601,1783 and
> 1453 (I think this later one is Kuwae that is being found in the Ice Cores
> in the Antarctic. However 1032 is 6 sigma and it may be the Baitoushan
> event which you say is 1010 +/- 50 years or the Billy Mitchell event.
>
> I hope all's well with you.
>

> Cheers

> Phil

>

> PS Britain seems to have found it's Pat Michaels/Fred Singer/Bob Balling/
> Dick Lindzen. Our population is only 25 % of yours so we only get 1 for
> every 4 you have. His name in case you should come across him is
> Piers Corbyn. He is nowhere near as good as a couple of yours and he's
> an utter prat but he's getting a lot of air time at the moment. For his
> day job he teaches physics and astronomy at a University and he predicts
> the weather from solar phenomena. He bets on his predictions months
> ahead for what will happen in Britain. He now believes he knows all
> there is to know about the global warming issue. He's not all bad as
> he doesn't have much confidence in nuclear-power safety. Always says
> that at the begining of his interviews to show he's not all bad !

>

> Cheers Again

>

> Phil

> Dr Phil Jones

> Climatic Research Unit

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> School of Environmental Sciences

Fax +44 (0) 1603 507784

> Norwich

Email p.jones@uea.ac.uk

> NR4 7TJ

> UK

> -----

>

>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Neil Loader
Subject: Cambridge details
Date: Fri Jul 12 14:56:40 1996

>Date: Fri, 12 Jul 1996 12:05:15 +0100
>To: "Tatiana M. Dedkova" <tatm@insec.quorus.e-burg.su>
>From: Keith Briffa <k.briffa@uea.ac.uk>
>Subject: Cambridge details
>Cc: Neil Loader

>At 08:47 17/06/96 +0500, you wrote:

>>Dear Keith,

>> I have bought the tickets from Moscow to London and back. My arrival
>>to London (Heathrow Airport) is by flight SU 245 (Aeroflot Company) on
>>July 19. Departure from Moscow is at 20.10 (local time), arrival to London
>>is approximately at the same local time. As I know, Evgeny Vaganov did not
>>buy tickets until now, but he informed of my dates and can buy tickets the
>>same flights. My departure from London to Moscow is on August 1 by the
>>Aeroflot Company flight SU 244 at 09.00 of local time.

>> Please, inform me how can I arrive at Cambridge from London? Is
>>there the program of this meeting? We must be ready to do some reports?
>>For example, I can prepare a report about the progress in developing the
>>Yamal supra-long chronology and together with Evgeny about dendroclimatic
>>investigation in the Ural-Siberian subarctic.

>> Rashit Hantemirov and Alexander Surkov will go soon to the Yamal
>>peninsula (June 24). This summer they want to collect subfossil material
>>from areas which are much more remote and situated at higher latitudes.
>>We hoped to use some money of the ADVANCE project. But we have not received
>>this money until now and the program of collecting during this summer will
>>be reduced.

>> Some days ago I received an information that the INTAS-RFBR project
>>was rejected. The competition was very high.

>> Sincerely yours Stepan Shiyatov

> Dear Stepan ,

> I have sent your message on to Neil Loader who is organising
>the logistics for the Cambridge meeting. By the time you arrive you could
>still get the underground to London and take a train to Cambridge. This will take about 3 to 4 hours and so you will not arrive until very late. You may
>wish to stay in a hotel near Heathrow - for the night and take a train in
>the morning. It will not be advisable to go into London and search for a
>reasonable hotel at that time . If you go to information at the airport they
>will arrange for a hotel and courier service to and from the hotel. It is
>best to ask when you arrive. You could also phone me and/or Niel to let us
>know your situation. My home phone number is (01953 851013). Niel will
>probably give you a contact number in Cambridge. You will need money only for
>your travel and hotel expenses until you get to Cambridge. I will refund this
>and give you additional funds when I arrive on Saturday evening. If you need
>to, you will be able to change money in Heathrow when you arrive.

> Please let me know if any of this is not feasible. Perhaps Neil or
>someone here can book you a hotel room if you decide whether or not to go
>to Cambridge the same night you arrive.

> I will send this message to Neil and he may contact you seperately.

>Let me know your thoughts on this .

> As for the meeting - if you wish to give a presentation on the Urals
>and Taimyr work that would be good. The main reason you are coming is to meet

>everyone and to discuss further work plans - so do not worry about a talk.
> It's up to you. After the meeting I thought you might like to come back
>to my house near Norwich for a day or two or have a holiday in and around
>Cambridge. We can discuss this later. Fritz Schweingruber will not now
>come to Cambridge.

> Thats all for now - I look forward to hearing from you

> best wishes

> Keith

>

From: John Daly <daly@vision.net.au>

To: n.nicholls@BoM.Gov.Au

Subject: Re: Climatic warming in Tasmania

Date: Fri, 09 Aug 1996 20:04:00 +1100

Cc: Ed Cook <drendro@ldgo.columbia.edu>, NNU-NB@palais.natmus.min.dk, k.briffa@uea.ac.uk, Mike Barbetti <mikeb@emu.su.oz.au>, zetterberg@joyl.joensuu.fi, rjf@dar.csiro.au

Dear Neville,

You mentioned to me some time ago that in your view, the 11-year solar cycle did not influence temperature. There have been numerous attempts by academics to establish a correlation, but each has been shot down on some ground or other. I remember Barrie Pittock was especially dismissive of attempts to correlate solar cycle with temperature.

Have you tried this approach?

Load "Mathematica" into your PC and run the following set of instructions -

```
data = ReadList[ "c:\sydney.txt", Number]
dataElements = Length[data]
X = ListPlot[ data, PlotJoined-> True];
fourierTrans = Fourier[data];
ListPlot[Abs[fourierTrans], PlotJoined -> True];

fitfun1 = Fit[data, {1,x,x^2,x^3,Sin[11 2 Pi x/dataElements],
    Cos[11 2 Pi x/dataElements]},x];
fittable = Table[N[fitfun1], {x, dataElements}];
Y = ListPlot[fittable, PlotJoined -> True];
Show[X, Y]
```

The reference to "c:\sydney.txt" is a suggested pathname for the following set of data - which is Sydney's annual mean temperature.

```
16.8 16.5 16.8 17 17 16.7 17.1 17.4 17.9 17.4 17.2 17.1 16.9 17 17.2 17.2 17.4
17.6 17.6 17.6 16.7 17.1 16.8 17.4 16.8 17.3 17.8 17.5 17.1 17.2 17.6 17.3 17.1
16.9 16.9 17.3 17.3 17.3 17.6 17.5 17.4 17.2 17.1 17.3 17.2 17.2 16.9 17.5 17.4
17.2 17 17.5 17.4 17.5 17.7 18.3 17.8 17.4 17.2 17.4 18.3 17.3 18 18.1 18 17.5
17.3 18 17 18.2 17.4 17.6 17.5 17.4 17.1 17.4 17.3 17.5 17.7 18 17.8 18 17.4
17.8 16.8 17.5 17.4 17.6 17.6 17.2 17.4 17.9 17.9 17.6 17.7 17.8 17.7 17.6 17.8
18.3 18 17.6 17.8 17.8 17.8 18.1 17.9 17.5 17.8 18.3 18 17.7 17.3 17.5 18.5 17.4
17.8 17.7 17.8 17.7 18 18.5 18.2 17.8 18.1 17.5 17.8 17.8 18 18.6 18.1 18.1
18.6
```

So Far so good.

"Mathematica" first plots out the data itself (see Attachment 1)

The first part of the instruction set lets "mathematica" do a Fourier Transform on the data, ie. searching out the periodicities, if there are any. The result is shown on Attachment 2.

The transform result shows a sharp spike at the 11 year point (I wonder what is significant about 11 years?). The second part of the instructions now acts upon this observed spike (the Cos 11 bit), to extract it's waveform from the rest of the noise. The result is shown as a waveform in attachment 3, the waves having an 11-year period, with the long-term Sydney warming easily evident.

Attachment 4 shows the original Sydney data overlaid against the 11-year periodicity.

It would appear that the solar cycle does indeed affect temperature.

(I tried the same run on the CRU global temperature set. Even though CRU must be highly smoothed by the time all the averages are worked out, the 11-year pulse is still there, albeit about half the size of Sydneys).

Stay cool.

John Daly <http://www.vision.net.au/~daly>

Attachment Converted: c:\eudora\attach\Sydney.gif

Attachment Converted: c:\eudora\attach\Fourier.gif

Attachment Converted: c:\eudora\attach\Solar1.gif

Attachment Converted: c:\eudora\attach\Solar2.gif

From: Tom Wigley <wigley@meeker.ucar.edu>
To: dgm@lamont.ldgo.columbia.edu
Subject: Re: Your help, please?
Date: Mon, 12 Aug 1996 10:07:42 -0600 (MDT)
Cc: trenbert@ucar.edu, boville@ucar.edu, branst@ucar.edu, kiehl@ucar.edu, francisb@ssec.wisc.edu, rjcicero@uci.edu, covey@triton.llnl.gov, tom@astra.tamu.edu, curry@cloud.colorado.edu, pdadd@nassgiss.giss.nasa.gov, gates5@llnl.gov, graumlich@ccit.arizona.edu, dennis@atmos.washington.edu, barafu@mace.wisc.edu, tkarl@ncdc.noaa.gov, lindzen@wind.mit.edu, liu@pacific.jpl.nasa.gov, sloman@wind.mit.edu, jm@gfdl.gov, rcm@lanl.gov, meehl@ucar.edu, berrien@global.sr.unh.edu, dickm@atmos.washington.edu, neelin@nino.atmos.ucla.edu, newell@newell1.mit.edu, north@csrp.tamu.edu, obrien@masig.fsu.edu, peltier@atmos.physics.utoronto.ca, rtpl@midwiu.uchicago.edu, ram@ucsd.edu, randall@redfish.atmos.colostate.edu, erasmu@atmos.umd.edu, cddhr@nasagiss.giss.nasa.gov, alan@atmos.umd.edu, njrosenberg@pnl.gov, sarachik@atmos.washington.edu, schlesin@uiatma.atmos.uiuc.edu, schneide@cola.iges.org, shukla@cola.iges.org, esmith@metsat.met.fsu.edu, rsomervi@icsd.edu, turco@yosemite.atmos.ucla.edu, waliser@terra.msfc.sunysb.edu, wallace@atmos.washington.edu, walsh@wx.atmos.uiuc.edu, wang@climate.asrc.albany.edu, "P.D. Jones" <p.jones@uea.ac.uk>, drdendro@lamont.ldgo.columbia.edu, k.briffa@uea.ac.uk, mhughes@vms.ccit.arizona.edu, rbradley@climate1.geo.umass.edu, Tim Barnett <tbarnett@ucsd.edu>, jfein@nsf.gov, Ben Santer <bsanter@rainbow.llnl.gov>, dgm@ldgo.columbia.edu

Dear Doug,

In response to Jay Fein's e-mail re den-cen, here are some points (which may merely echo where you are already).

(1) Why study den-cen? Reason is: improve understanding of climate system to aid in detection and prediction. You should read Ch. 8 (detection) of IPCC WGI SAR in this regard.

(2) How to study den-cen? Models and observed data are equally important. Models (coupled O/AGCMs) can only give the internal component of variability, instrumental and paleodata give internal-plus-external.

(3) How useful are paleodata? I support the continued collection of such data, but I am disturbed by how some people in the paleo community try to oversell their product. A specific example is the ice core isotope record, which correlates very poorly with temperature on the annual to decadal timescale (and possibly also on the century timescale)---question, how do we ever demonstrate the usefulness or otherwise of ice core isotopes on this timescale?

There are other well known proxy data issues that need careful thought...

(a) Sedimentary records---dating. Are ¹⁴C-dated records of any value at all (unless wiggle matched)?

(b) Seasonal specificity---how useful is a proxy record that tells us about a single season (or only part of the year)?

(c) Climate variance explained by the proxy variable--close to zero for ice core isotopes, up to 50% for tree rings, somewhere in between for most other indicators. How valuable are such partially explained records in helping explain the past?

(d) Signal-to-noise problems---a key issue is, what role has external forcing had on climate over the past 10,000 years. There is a tendency to interpret observed changes as evidence of external forcing---usually unjustifiably. Few workers in the area realize that paleo interpretation has a detection aspect, just like interpreting the past 100+ years---only much more difficult. More work is needed on this.

(e) Frequency dependence of explained variance---the classic example here is tree rings, where it is exceedingly difficult to get out a credible low frequency (50+ year time scale) message. Work in this area could reap useful rewards.

(f) Coverage---what about den-cen data from the oceans? We need much more of this, especially in regions that might provide insights into mechanisms (like NADW changes).

(4) Causes. Here, ice cores are more valuable (CO₂, CH₄ and volcanic aerosol changes). But the main external candidate is solar, and more work is required to improve the "paleo" solar forcing record and to understand how the climate system responds both globally and regionally to solar forcing.

I hope these very hasty ramblings are helpful

Cheers,
Tom

P.S. I've added Ben Santer, Tim Barnett, Ed Cook, Keith Briffa, Malcolm Hughes, Ray Bradley and Phil Jones to your mailing list.

On Thu, 8 Aug 1996, it was written:

> Dear Colleague:

>

> Doug Martinson is the Chair of the NAS, Climate Research
> Committee's Dec-Cen panel. He and his Panelists are drafting a
> Decadal-Century Climate Variability Science Plan (a US CLIVAR
> contribution). Doug and his Panel are trying to get the broadest
> possible scientific input for this Plan. Doug's approach is one

> that I strongly endorse. In this regard he asked me to solicit
> your comments on highest priority science questions and asks also
> for some help regarding examples of published work that would be
> useful for the Plan.
>
> I know you are busy, but urge you to think about this and comment.
> Doug's committee meet in mid-September, so to be of most use to
> him, your comments should be received by the end of August.
>
> Please email to Doug with a cc to me.
>
> Doug Martinson: dgm@lamont.ldeo.columbia.edu
> Jay Fein: jfein@nsf.gov
>
> Thanks very much. Jay
>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Jean-Claude.Duplessy@cfr.cnrs-gif.fr (Jean-Claude Duplessy)
Subject: Re:
Date: Fri Aug 30 11:27:05 1996

At 13:52 27/08/96 -0500, you wrote:

>Dear Keith,

>

> I have been asked to write a white paper on the possibility for the
>paleo community to interact with CLIVAR.

>

> Evidently part of the jow has been made during the Venice meeting,
>but I would like to know if you have somme recent recent work of yours that
>I could include in this paper.

>

> Any suggestion wouldmd be welcome.

>

>Best regards

>

>jean claude

>

Dear Jean-Claude

It is good news indeed that these initiatives are now meaningfully underway to join the palaeo , pure climate , and modelling communities. I will join the short CLIVAR/PAGES meeting (24/25 Oct.) and a colleague - Tim Osborn will attend the larger meeting from Oct.28-Nov.1. As for question about new results , Ed Cook and I have a paper in press describing an initial attempt to reconstruct a North Atlantic Oscillation index back into the 1700s using tree-ring chronologies in Europe and North America. I will have a copy sent to you. Otherwise we have a paper soon to come out in an American book describing our early analyses of the growing Russian data. This work, developing the density network is progressing well and we have some very good reconstructions of growing season degree days- excellent spatial maps over western siberia going back several hundred years. We recently published a paper in Nature describing a 1000-year summer temperature reconstruction in the northern Urals and a brief but interesting paper demonstrating a strong volcanic influence in the tree-ring density data when they show extreme low density over large areas. We have very interesting developments from these areas of work but they are only now being written up.

The usefull thing to stress is that these researches are in progress and the development of the tree-ring network is continuing well and is already providing patterns of past climate variability in northern Europe/Russia and at a number of special locations- nortern Sweden/Finland, Yamal, and Taimyr we have already got continuous 2000-year chronologies and have the potential (indeed we already are) to build 7-8000 year series at ech location.

I will send you some reprints/preprints and an overhead that shows the present state of the northern chronology network. Any stress on the importance of future collaboration btween us and the Russians would be wellcome. I have just heard that a proposal I submitted to Copernicus to do just this was to my amazement ruled not relevant to the programme!

I look forward to seeing you in October. Very best wishes. Needless to say, if I can offer any help with drafting the white paper or similar I am happy to oblige.

cheers

Keith

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Gary Funkhouser <gary@LTRR.Arizona.EDU>
Subject: Re: russian data
Date: Tue Sep 17 16:42:28 1996

Gary,
it's great to hear from you. The stuff you are doing is very interesting to say the least. From the details you give the precip. stuff looks the more relevant for the Holocene though I note that you don't have a manuscript yet. The other stuff is of course interesting but I would have to see it and the board would want the larger implications of the stats clearly phrased in general and widely understandable (by the ignorant masses) terms before they would consider it not too specialised. I suspect that this might not be straight forward. Are you not being (in the time honoured Don Graybill fashion) too demanding of the response function results when you say deriving a transfer function is not justified? We all strive for perfection but does it exist? Seriously , it would be easier as regards publication policy to get the Editor to accept a reconstruction/reconstruction based paper than one describing chronology inferences.

I don't know whether this is any use but I hope you'll send us something. I also hope life going O.K. for you these days. I can't see me getting to Tucson for some considerable time and I don't suppose you have any plans for cruising this way so I'll see you when I see you.

keep in touch and let me know what you you decide.

the best to you
Keith

At 16:44 11/09/96 -0700, you wrote:

>Keith,
>How's it going?
>
>I've been working on some of the data that Don collected with
>Shiyatov, Mazepa and Vaganov in the late 80's and I was wondering
>if you thought any of it might be appropriate for The Holocene - or
>if you have any ideas about where we could go with it.
>
>I already have a fair draft dealing with the Kyrgyzstan juniper
>chronologies. Although I wasn't able to get any climatic

>reconstructions out of it, the material has some interesting
>properties similar to some of our long-lived trees in the southwest
>US. For example, autocorrelation in the series increases as a direct
>function of stand elevation, there is a shift from high to low
>frequency variation with increasing elevation, and the
>intercorrelation among the highest elevation stands is greater
>than that for the lower stands.

>
>Maybe this means that the lower altitude sites are responding
>to more local conditions (precipitation), while the higher stands
>are responding to a more regional (temperature) signal. Response
>function analyses with the indices may suggest this, but again,
>it's not strong enough to justify developing a transfer function.

>
>The draft is about 2500 words plus figures and tables. Stepan hasn't
>seen it yet, but I can't imagine that he will change it very much -
>I know that Valeri didn't find any great climate responses either.

>
>There are also 12 chronologies from central and southern Siberia, some
>which are pretty close to Jacoby's Mongolian sites. I was able to
>build 3 precipitation reconstructions - one has about 50% explained
>variance for a May - June season. I haven't composed a draft yet and
>although Gordon's dealing with temperature, a couple of the
>chronologies are of comparable length and I want to look at our
>low frequency variation relative to his.

>
>Jeff Dean and I are headed to the White Mountains this Friday for
>a little 5-day collection trip. Thanks for your time, Keith.

>
>Cheers, Gary
>Gary Funkhouser
>Lab. of Tree-Ring Research
>The University of Arizona
>Tucson, Arizona 85721 USA
>phone: (520) 621-2946
>fax: (520) 621-8229
>e-mail: gary@ltrr.arizona.edu

>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Mike Salmon <m.salmon@uea.ac.uk>
Subject: Re: shiyatov
Date: Tue Sep 17 17:38:34 1996

Dear Stepan

I have received the receipts. Thankyou.
Unfortunately I have also heard that our request to COPERNICUS was not successful. I am very disappointed about this. The letter I recieved said that the proposal " was not considered relevant" so you can imagine that I am seriously exploring what this is all about. I have just returned from a PEP3 meeting in Paris . I tried to emphasise how important the Russian work is and , of course , our collaboration. I am relly angry that our proposal was not considered by referees - just rejected by the committee.

Thanks for the piece for the Web page - It is already on. It is now more important than ever that we publish some papers over the next few months on the different aspects of the network reconstructions and the long series. Have you considered my suggestion to think about a long,detailed paper on the Yamal work for submission to The Holocene? I am happy to help as much as possible with such an effort. I am glad you are safely home and I send my best wishes to you all.

Keith

From: Gary Funkhouser <gary@LTRR.Arizona.EDU>
To: k.briffa@uea.ac.uk
Subject: kyrgyzstan and siberian data
Date: Thu, 19 Sep 1996 15:37:09 -0700

Keith,

Thanks for your consideration. Once I get a draft of the central and southern siberian data and talk to Stepan and Eugene I'll send it to you.

I really wish I could be more positive about the Kyrgyzstan material, but I swear I pulled every trick out of my sleeve trying to milk something out of that. It was pretty funny though - I told Malcolm what you said about my possibly being too Graybill-like in evaluating the response functions - he laughed and said that's what he thought at first also. The data's tempting but there's too much variation even within stands. I don't think it'd be productive to try and juggle the chronology statistics any more than I already have - they just are what they are (that does sound Graybillian). I think I'll have to look for an option where I can let this little story go as it is.

Not having seen the sites I can only speculate, but I'd be optimistic if someone could get back there and spend more time collecting samples, particularly at the upper elevations.

Yeah, I doubt I'll be over your way anytime soon. Too bad, I'd like to get together with you and Ed for a beer or two. Probably someday though.

Cheers, Gary
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From: "Tatiana M. Dedkova" <tatm@insec.quorus.e-burg.su>
To: k.briffa@uea.ac.uk
Subject: Rashit
Date: Thu, 10 Oct 96 13:24:01 +0500

Dear Keith,
enclosed are data concerning Yamal chronology.
1 - list of samples: 139 subfossil samples (checked only),
covered time span from about 350 BC and 18 samples from living
trees (jah- from Yada river, m- and x- Hadyta river, por- from
Portsa river);
2 - general chronology (1248 BC - 1994 AD). I have some little
doubt about 360 BC - may be it is false. It was found that
in chronology I sent you before 155 BC was false ring;
3 - ring widths of living trees from Yada and Hadyta;
4 - ring widths of living trees from Portsa. Some of them didn't
include in chronology, because were not measured at that time;
5 - ring widths of subfossil trees. Zero means that ring didn't
find on sample.

I don't send description of collection sites, deposits and etc.
for the present. Some details you can find in our article
(Shiyatov,....., Loosli). By the way, do you know something about
its fate?

Please, inform me if you have any questions about these data.

Sincerely yours,
Rashit Hantemirov

begin 644 data.arj

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M04(N4%).`#GS`H`J'.ZU=D:<BXXD<_W%E3;;;?/\$*\T4NA"J2:+=A
M2"7OC?W_O_>O-SV0BJJ`#<TM17_?#<W9\$W/;<[?;W[;=Z_/U[;>P(OP>[
M_&WV=_AMZO7X=_N[_9W_;&^6_X?M[^_Y_FZOGT<OE_O]HN7T%W\?O[O5KVG
M_OJ[_?FB9'ZN[W>_PZ>T^H/GY;=R8J\-O\$(>_[/DB9'X?=[OM_/&"I0P>/
MQ]PR/Y_F_]E7_F:%^1[2.VWTIXZ-1[3E&UQYR.5^<75%;;HCL,\$0#U)GH*-Y
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M%FOI%PC3`>JEZP%&\;0JN\$6Q;)T7Q1P7VMB]97&%91"^]U^I##C(N\$8=[L[
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MA_38)LE)S4ZC7@=58]HP\YN]"1%#!8BQ=/W>ALY-@FRF0)`F<9,>JTW1=4;>
MUJ[T`))L=5=J(\$`WIC"SO2P)9"@*C2A+IB#HNJ-GJ[O2[KT!^DG;&(+GL!*>
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MJ^_M25W8)6%JR)27)LX4_^),\$K"R+R//9N'MVO^@^T=\.\$<'JWY/.N,6H1N?
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M;WR:K/TCP<6L\$&;]">M*ZU7U+SH=(MS=J/2\N,>8H?F` ;^CU=WH\D*W<WRQ
MY2G]*">(>F")->>>1T/.4&EG7J7SQ%46B.KTBC>UR[T"6T,\$II<(U[_C&#<W
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M`Y^Y,YN\$<\7^>*EAJ^W&D2(V"WJ4Y-PCB;_O)M)F[8BT.V,".L))X63<0\$
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M458Q.APHM5@S7D4E1=4>N_@]L=.]^;O+!=\0V\D=/+OX/:0D,B#UYN!U/7
MBGW=\M_`&%`=[5B\G02/-C"3!%7R=YMHNH"_O["!T7LE?(E8L:@Z,G1=4>
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M9X\WVWO/+:\M[YN>;>,\$B]^7B"Q5=G'Q@>(KOG(WV?[HZS,]+H:I.&S.,G53
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M8R\$+X/9'F1V_P%Z#A@A0%\$C98ST,;U;E!SGN)Q\Z&TX^+]]2M:#?X7)<CR
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M*,+-MJ?%5>-%_ZSD(VMD;\$%D=XA_::%2-"LO-AEHS^%#0\$B4<7W67G\$+&>6.
ML0<5\$)IIA*;JJ*GO8HOQ[<-"!+<J3?E5AA^5EP.VT?*9=MMB7A.<(&H?>(M
MY.#U4&X+802JA!Z2C?-)4R/^?>64>-9VCXN!`.\GMRC?;/SU,C\0=I'OLSE
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M_]0X\$EQY-QT\$4AKQFP.JLVC=PXQWX6RWZ)2G+;+(`RW_U_!4((FR+@'/2.#
M6KU>SX(\$,)99S[Y6=5)=;H5*MOW\$?_L'@7!(461;W3M!72@'C\$_OT'0AS@N
MFF;!LJ->N?=PA_(M)0..15'FULLVD#^U^O[\533<_9;PS)33YQ*-5:7\$4&(

M_XXDXI\$FQ;!;JA`8<MIZ&M;+UJIYU1% CJ*`4XXQ.U153-G^N=OZY+RWY0FC?
M:O*-%*F&M'A_6M*NR5.4T5A6<<`RO?];E:CW8J"L[`,@".5=3T=@MFJB6
M,Z9O]","JR:+5L"^M5GIRFUTN4^AS\$TA.W;V&[BD]UE5_742W(\$[,),88-7S[
M=L%4.(#;5:C,;O\UZ8<NG@ED\$Z\$?+<Q=(V^ZR:FY)%Q7]FJY/&:]DPG%FLP
M2&SF^HTC8&_`H+5EZ&^"+>G+F@FR1<X_)ZR_/7)S&0"9<0:J_]:N5K;JI(8
M`_CGBM9K7C.+^84(J)V:![=\$%QFI<6A.?NX@=(D>A"HCY-^PF_)5586MI)\
MMJ^S\BO_NE+:OXQ\$LKCS#U)3F@9TX+F[@MGI78\$-;9IO\L6K7GQ,Y(G,B9AT
MFC@.7.)UHKSW1T_KN?"G\3+?-&=MH(G<*R_-Q0X;T-(59:G;;;>N37+[3(Y
M@]OW]D/:T1A*#);?QQ%9MKS7K\<_)25ZU1E;4V'B98.M?M4']35]%<^4F!C
MX#TT\3K<0N_Q634#K*)H=M.`YQ50T-H`?52[@62X#)#+OGOVD+C,>YK7GDQ
MT=\VL;8.\J\$O5;6F\@WL5N"]LX(T!C:@>NC0[6:7;J]W]1AP'3?]&5\$[:;70
ML56U3Y-,AFL[PT3A'C?P%RLDB93^5HGZ9`8AES.9CK!'_@E[M'C]^T?,#MXQ
M?Z@2?MY15<\"=47Y3PNL]A76V/+27+IA^,;*UJKI-]%^V'312EWF47E01=
M`SZTSADZ49]1'X5'3&N=(C^VF97)6_?A\$^W+UY=,+SZ^]7NV%[0J:9V.&MZ
MK->_E,;G'ZK`?@^E9.`)*!",\$ZU6)>DL7/[[]EC]_L:Y]5[?F7_?@AOK1
MG#7=N62^:L-9R?A#%//%<&*URHC_52\G^X<WW=6,MAU\7RZ<C%.[&,=6+6:[
M,,_ZSIBRU7'A\$Q6B@H.O*1[U9^_A49(\$<>A6<TB Y&F"TXH%_`_]5^%//%J2%O
MJM]J6MNL^RR,,_M4!!/X4T9+&4YO&]D)_/'<^9:Z1_`ODC]?A'!(/Z.O-:\
M3CJUHY:B[KB9]=G%TX<V-'\$NZQC:9Q^4<6S'_E6*X4R,6&W&9J);+9PMJTY_
MIK@PPA2H;:_YJ4O?/+@&/(#@NKRXXVJNTO.*R=[9:#Q&B\$-F+9L,;VK[7[^
MM8AMM*^Y8V_W^-/?/\$]6^W<6693_YBY5Y2(+[[N-]_?A!)6Y?M-<8\$YW=X
M0'/G[]LPQ>K5%2+"9PH^G#;NEM;#1\$R,<!=TB]?@33O*,<4?"TEL&S_AR2@
M+2-B,Z8HU?@'QG8]N?4K\7);L<LA8LD+><>:_Z`1]0R5DG+.,=**X6\$FSE[8
MRWQQ+^%<>LT?J@1A1RTH+E]B=#^6[M/P`YV@?W];IPI+7"=FZ!K/3WK(GOX.
M1[_:L_&'SVY)K<5)E&V-B\$!;,R@<.`!&ZN#6<_3_`8.HJ!X&`0`0`0";
M9%-(0T1`"Q+@`XV5PAP``(```\$Q)5DE.1RY25TT``-)<:KD``NE<W*Q
MW:M3IN)#^`8`I-V22=85@,1`)48C`]@1,-N\YS=[]_1@8`Q#F^I7_#OM9&V
MG/6^=N?A_<?#&]M^];?V_E_3&<6[_OX-^YM_S?^_W_Z_;^RV7LV+-NV.
M6QBV,VPX^;[L_EFO9H[Z_[@^K)_-V;RSYLV,;]O/+9'VC3Q[+CWX\%.I9V-
M/CYVX/R-MC6!N\$?D>K(^\$:?G@TR/R-5&O>#3S[X](T)>^Y\.!H;X*`GYPCXV
M%Q\!_WRE,6]Q]KI'P@F#CLE"EJ5S]GMDJC&O<CPX#X_['DURVO+:V,K&`U
MB-/A5^>#P1[(00?)\&.\?&`H5N/B[VX<UN/5<;_W%NGU0RL?/F+WP4?UBV[C
M+A!(VGE,>+CPCG@\@;<4<=-.S+;Q_V%LW:UTM]DMY<D,7X,78Z`&DSE-EANY
MQ@?1<LP<ZWR7/8\4%\ (M&_:N7IPA5Y@F#@C>Y0U>%SFEX4YM(='M=,_&=<P
M:F(MRW&&OOB8*8U*/4%4+:63T"_)N8K(9OK@9_L9W983"WQO\$)0/F=D:H[5
MNSJ-LLS#,F`GW!C3W,!R3F8KKF2XN5P?;R8,M1O.1!P/PJ!PR!9[#3V4
MCB=KY,LT9?PP\A.G'DW@?80&8,/A\+3+,8L@DH6^S!J`Z/?B]1I%^.#LA@I
M\$!P!`OV@0.XD\$#;?F"!S(3*(JX`\$,<H8Z(?0XOO\$YD)(L,\$2+,FQ,;L+[R(
M_+QI0T^).XD.S;H\PRG#`4-AACM[;CV[Q&=C;>1J1E!X631@\K'\6YQI\2X1
M-!#DX@3Z9H(R*:(/'#\S-2]2Q0#;Y&I>8P^%ZAH@WD2`+(EJ0C]R8.4K4.E
M*K[+@HQ,;&EC`!V<1"&%5O;PP>\C3\1HWEP!6\$ER`*-D0D`TE]D;5#2-G;4+
MHY\$0?T*`,`8#L`<""ZFRY`=-,KB7`?,MW48HPR!8S,RBE3\$MR\$_B6[<@@;
MH:QZG8<LPP1AB\$GDPV\$14C6*AX+WR#;3CPF""AP\$60:.5`6@22/X-)(<WR?

M^A,R\$LAO`M]C*.O^L]!M9,>A5I% C9H,4)T=2\$*9+`RM2&`#W[S@G6+N7UD;+
M9@:3[LF" _'E!QD!%J4:-\$C`9+CWDR>0,4'2G#\$K% "^HJB0:N4;3SXQ`OT`2!
M*68X6W)'P#K\$Z\$ZV&#GU+,-,C!):[(K<O(O,>2`+S\Z\$:"XL-W#Q'*`RL:U
M;R/MW#:QR\?D1Z8OJ)ZL>:'YSL?;&-"0_NL;C'PR>/#U&#>=M&%Q<@+=&"OT
M3VQK8R-/#%O@&N:'GB:``O=(OFB<3H,JCPOV-/W>?KS0U('I:'X?#:-^C_MN
MD#V,,05<WDHK1\X\FK9[&SJ`5:D!<^G%]FA&QV80V[9M5`_JT+9;1E9<;4/^
MKU&B6Z/?B]&&++]&.\$R)S0P9R,?"%CC<&15L;AHHX4W;0)FU*&O2.8*EI40B
MVI^RI46/DLEXAS8_`Q*\\$?&P%<H<9##]Q_TI\$Y1S&I7X)CTQBG<V*:)@Z9C
M'PT-/"9]G=5[\$I>3]BHH<JY;HY?%LMI`JT067\$+-P\+`9^*6+H<T'C].GY+.
M4[N(;8A8B#MSHWA?N[_"\$R=P\$U6E!>Y\`GGO/S=0GJ=LL!2/IIR6Q:8'/%YR
M<V^E2;#P'>(_EM&BAN]>S(^>S(W4R+/]Z]C!`#F?3P\$KY(5Z4'Q45Y9,`UE
M^&D@QFJ1IYJ,<;FLX\(*UZ(S)=>C1[(Q71I0,(57)?E!^W`MWD&^FF+!&F?:
MNB#8V1M40M1.B1^G1@ZB/\$1Z=[#X0RS<T"(A@)MPT\53%EO&)FKA8.\$=H(#
ML_,E0QCG1G&YIZ<D@V1D2L+;5-18S129\OSE5:QK84/(?U'A&EO53&A?%6[B
M/%;8*I[8/^-I`MC19ZS2M\$_F\$HZ:->//YB:-1E9S3:,0RJ\$?%0]DT'I89C
ME49!H-/#9<])82KU438#&RE%MKU+3T<7&I1)3%+%X)L<7-\$.#O5'&:HS>#1
MG"FMFJVR1AH0CH\$AZ,I",XB,..!Q^^11TA\HIQ/#[8<J1;E;OLJJ2\NSBD-T7
M'ZFY8]+*L4\$5&*CH!\$<:G-&-XH[AJU44^CS6<&XOFZH>C&D<6F?\H]@V;08A
M>49&)HSYWW5:(W8FL_&[?^FJ_Y-87:)R%C3HP:4;A+\$(D).-F3LC7`Z7JE'D
MDAU6N757` ;N369!S]YG><XCTM!E--N878QD?]"\$VYI(&5&L;D7VI.W-0:JR
M;+#U*><-A/MT=#H&.(048D\$11E64KD>`*CTS0*:9*0:6H\$[F%>,-DU'CG9
M,U9K,;X@581JI#<RJG4PF*[D!\$)B;1UT^%YJ_81S""N,\$H`O\Q<68P).Y-`)
M*K]O3-N]F:*\&8;SAA7SQ:B>1,9\$<VS7VM#OE/Y`7\;>EI5OQH\!O'AZ9\$G
M,>,+WXTAKB0/@D>\$A[*I.##6Y,%Z#O(.L+>;^^J^>N\'F0EU(P@^VEE!P
MTURB:GTV/T3.*ZM!KV&\^K4+<1&+Q62?DIODVSVH&P&G@,YAYH^&C+0!XF:
M(TA0K[F9?)5>'W>5;B:K@!9<*WJ1C#;<8W\HSG7!#D)!RY*'E0H&&F4Q6C&
M3;=R6E4_G)H'U>=JQ.T:^\\$(.9C84!<.M)&J)S--W-%6Q\$?+WU,PG:F@E8A>9
MS/"-Y7&5%\$`FYX20O-^\$6/NJ`COH+?""WC]06Q10%1)%>+"-F5_7U66+B2TI
M&U5UWDL[,T4\W@!II12'<.=U,UFU5!FE_%+#\$7AKU8_ZF).8SN\$MH>H;M*/U
M3ZQ\-\$^FN1B(3JV3A:)SX.9J\J9M'.R&M*%>9%CEK!KX8ASPJC-U5Q'3-'
M')U7B=<5*.1B</S&SH(EK94@W5QXQ1\`>Z^5G;^UK(U@`=[1WT2<E;\G;2Q
MD<`J@<L/;4;\:INYG!/&0+U;5&2&<FDB>6++@X#7-39"558!LI]_@0J%1!N
MXEW+H5`9JEU7%3ZI]"XO\$MH%6.1=)LBB\\$(*)>C'DP'5.DO-TM;YQ>,BQ/
M-)+-?G5?@U9W"K937!3CZ8^H.(!;SJGOGB-/II#HOXW\$\$<1!MPM/TW5\$RIA
MK&+6VI5YILVI<3>;>1ZB+TJ)6Q]+`RXD'DH0F][FO-]XV+WGH]<\$X+7;])P
M80^C.<G=`_MC`.0V2\$./ATSCGSB]9\,>L<RU@&TQX1".#BC_D#%8AI\>8#
M;G0^&:_AG\$B0",6S2%"&'D^O>IA)ZQL1(\+*-%6[62S5DWF'+QO#&_!5[7P>
M%;"\$6.DQH:B3P<,8@=G=0Y=C"[3=N,-XT:!)RC4&>[5<;M-G"5&CSS=>&V]
MZ2DXQ<!/A<F!%<;:K9H)Z8'-9Q10&0KE;^,WI7_BD;&43C[R(ADH.\$Q*=:..
MO*5_`C+:9*IE7(@)0&S\$`O@!"XDM2?VI:;%QZX\$QC2_`RJ]<16-/TFUD"*Q
M9L(.S`=HPE0BR'?0&ME/B@J^4N\XMD2/L9H%E^J1ABW0H:Y6/2\]\$II-\-/
M'K!4M)G^&F_V'L+B5&&OM@H5(@.ISN:A0V7Q)P(H#^*1]0%H>@3+F,P:J=2U
M3@TW=*U(2Z8F@S&7!.*\$R,ZD_:@S@P=J=Q71\8Y44UU`31XF7J_-8<[+^+O^

MMA!R6%KE6/#-!%`M@L1Z(>#84M)F"B-FCTD@X2UZEP6^2!UD%R@`Y[CA(C 1
M@OKMV>[1QT'L?D&,1@;MT&*O&\[GAL31DO(7%A\M-T<=ZS:%//T_]V[&%
M2-#:!E(X8@<@N>&]U!S8!DC28X].#=#&`438)DGESR#]RM/^%Y@BY^^@4:1!%
M68;Y74J(S^I%6F9),_&./@W>A12U3B.RH%C9H<]':H\$BN12\$;IGBDS``35.9
M4!66F(=^2&`\$!,D@M)5ZCR/BI#;4!&:!]2=S2XD`+V&E^24_8RT6Y.#4K.:H
MS,&+%Y1_%2_M>2O%`ILHGI4MY^>6^4I8^9A00#JGB(H0CZ00^I"-7)7*1Z0P
M"\$A,5/OA(+PQAGN'_>2`\$@MD'ER0'V]D^CNV_RD^^UV_Q^DGT6V@,4CF/#3,
M[B,QQR'7'^4TNUS-G:Y\$@C]Q0D1DHLL,\&/T;0P<|(3'3P=HC-4\$/<9)O,5
M`.XJ=(E0B36/SF!+Y<WYIYLCI-\$(I1YP#H.1'+L>5U\$P41)"8B*HF),C%M&
M@0P=P=D>4:N>%6/.Z1YX2@5<S&*VB`36#0FQYT\$>E"=J@WW>J[4QD>R?!V[
ME_ "\$4)`+R7*N%87-\$LBNF7+QJ!!YIKD-1"Q^&[=AJ/E_:5.:F.]&\$&Q/1;BE
MAT=[GAP\$=-V:%M=?AR\C7&@%2WA2&JI+/+KCCOM5HQN5Q\$KV>=?4EZU76IH`
MCJ7LGTR(F0[I7M%!I8.WE1E9>YH^H5T?3SDZE\$JC1.:9Z`ZY/6*;1/K% @,!
M14,UL5Y;`NZ_,QR!4>_TR1I9)KDL'5;0/.NAI[*ZK GK\;GO=+=70_>R\OFNH
M8VXZAC[#/_>7E,<71F3T?%-;5"/M6\#5<J*GCAH#2(I[TLAN+5PP@.I>:>4S
MGSSRJNQ^F:/[\$PG=(;TE1#6[/3D3&55#RS2G3--A]Z:\O)/3;B*1<[M
MCZJLE\$9GV\$Y5&:D^8[GJ7@6[*4+IX`Z6NH8[0E"=_>9=K+I;#I,WQU4<*?Q-
M87TUNS*M;T<UGM<\$OT>%,GFS#O/6C*H/72F:[/_>5@Q0TFO[7#S:JY33K-5P
M-BO;7FO,D"B]\$G5U@4@ZW1-\$QIECSE!,4R3'H/WK>EY*>S-1(N`HZH+TW%6T
MZE V732: +?<KX4R?V[*5TD%L-`_7?_ -RNOR[/5':>8%)=>2]E"/;M>KLUHC4
M?P8?\7F"-#!/57M[W(XD9>6`SV+6IA'&5JZG"/G@75P.)X:[/L"E./D.(R;I
ME3&@3SJK6A",*06NL(28?06WLD0<'77AH^QLR/RX`11<#SG^%`5_`TXU[_
M&R>D&G/^?N#2PJ2KM%35%1?TF/P?,+J4F]Z/ANJS\$Q'Q@XX1R,!MH[F]P=<
MQK)>.2\S=TPU?LW,Z[/48M4NZ!^7*4Z#R]JHN<C;&NB([HKJ!S*;<;V90-]K@
MF5>WM/&7:.)#W#^95[JHC#VH2*XF[MTFDJ1%`<O0"S5V:B.9]W]J"?>Y'L24
M"0?+F>U,M6WR#K2`XO7;I^G\;GYO6MB"B>WB9ZM&V^3>V74\W^21B\$+LE(^T
M0\$@+2JH1_LD>LN6#H0Q2JH3610>JS9FWK9%:F^0"1<5>\$]4+ZXIU7>LA;5V
M_5O5@!E==TT\UJ2;`>?C9=U[V5XQ5#+ZL#=#^!6C`)MA\T."FM//KE*B-3V
M:1=[KJ\OH7#,R?3SYIBBO5D8]3DU! ?Z]-0MK**AW2TYJ@M:\]3`DZMZ&1*W
MKO,/8,IOU-?/X#[[4HRZUZA\KYS80]F,^>\$0-@!WW^-2^:(#F__V`"JO53U+
MF*)\$OGDW147<&%@X`-V`2P`X>(=-;J?XM(:TR/L*![<60IR3<8FM:PC'-ENO
MFI XQD:Q[SPC3"NLKV>A`7&8(\A?P)MO<T"H`AJYG]6!Z.-(#X\X4GINB&7\$!
M.>JNV;_Q<AKNCX.5T7ZT">;/O-MV;";>*2C.J_QRN\$`KYS1#5_"T?])4+K
MZ[S1?G9N.S_&I9_AA-48EO:`G5?ATQ:VB5E<^!`NS(EA1/3=WS^+"UPS0Q:E
M;KGMET%EKSZMUS/ZB_,6T\PI1@[=K^7^TN%1@K^-S/)>49GG0*YMXL_0K*
MJ3G5BL9?^%]?+S_#Q%3\$-1."LUAR>SSPV(@X96>"<ZOYHGBJO1<T5T\ (NUD
M5!4]41DB6H`B0_ID?JBUG@>6E;)YY).`[7\9DY55@FETUT,-)GB7<&#J]P`>
M!@\$`\$`\$G*U,2B\$`\$@``_2T``.QI`]``````!03U(N4E=-``"%IGC\```,
M)'MVD>&M:G3>I5_`T!@53=DDDC"M1(8@(X%SAMS,S9_WJ'0Z'0>\A_A[M;(V
MF[?Z:;_O_J?A&M]_O_I]_O_M_Q:(^`_O\I_PG7^=_[6_Q_Y_K_3[%_N6B<
M_=Y]N_W1_N?=Q%NO@QF?M]^T/];./MC\]^QC[%OH'PO]F_V%S<7&'SOX89N
M/N]&!Q;`VM?8T-:C7/U^_9M]>_TPOAO[_V:^TK[#B3C[._HR,/O;[!QT9Q
MIYP,LNNH@]1]SX:BSH>\GH#BMSJ7?%\\`8`QO>AA^L_<6DQI\:_Z,IX<[&`

M#2PY5UA,2<AE:B&M-K<?,G\$A^1G^<CN!A_VETHR"YR.+_?!#&2AN7I"3]CNP
M8Q8ZQN/M<[\\$[#TM\#H&5/=YPGAT",`7%;8^80/'T67'N<)&XP@&!G^3KJ!
MV0UN\$(;\$&E_CAYX`^73_CC<'@GX+=YP/M*/F5NP?E<J[6^NR.M#T9T^TZ).
MXR8X]>M]CMWBIS#)UB>W'!PMTR>:-.FC:H2[[-ZUF.E&W1LN4C`Z"\$N%@@
M:!"A^1AATGI,#269<I;@2%HH'K3^3]3FNEN#:7<5Z1>8"6@R.>TN5FY2>BG
MU(-K'AV0<%+%_1W5ZZL\G5(2A-\$!90_VH^U/W0P[M,K3%H^WE,1P>;CAY7!A
M\63HH[K+U^L#/'&[PW"/AAMG[.I0AP=3O7W.0\$)C\$N"/;?=L/R-^7/#7NRD
MU`+6\`_SG@'Y_@!_DP-U:``X<NF-XB;S;Z,Y>^HG92EC,CG=V=[YF;:CFHU:
M<#6@SH(:'4VBP<OGF8"A^=D`.7S&]7WK\$8QN]^YC.8P.P`_Q`W"\$Y[2+#O98
M,.RX-`XG.HW(2?%IML?Z+\&^#TNMQH=&\$1'%XMD+RB^QHL?8*?^Z%)X+]0S`
M:>C1S;@"KA@/A/!3!<YWP)L[`.2.,!`Z6Z.:5TV6_KP`\^(&_6RM(<T,G!8&
M<#Q\M4IF\3X;GEZ4S?_]3-TS4\$5AUDA)C<9:!6Q,%LLMP=+#LH""N6W.HPS
M4=X!GD8(^TNC#SW6?-TRA,+%HJ\$TYI/B80^(V\..C#JA(YX4@\$CDID#FT8&E1
MOD::"G"2('VDD8V'FQV@1B.Q+C?N;:*"8<D^MWD(@,6EFD;BV.O*F0]EQ
MJ9R5M#PMT;^M<[[&JPZ0.ZXFE82^"RR.HM'R&Z`U#/#88EX</*LT3/T=H&7+
MG0@X@?*&JV]KA/-E[#"\$HBMAE#^5_NH`PO\$C:@;J`BWR#)!`P\$98=)SH'Y@_
MFZ\$K/^0ET(IHD801]2/?R@R>(D%`(DC)KBE",GHBE<DK8HB<B2HA(DIKB'FV
M6<E;(\1/YK'N@[T;J`SS^=J[IVU%%K2S'@1(4MD1;;NMWH@D8[\A^FE]D/!
M;-T<G=%R2CP)9RSVKU;L'^&U2/D6?V)5D[CXW?,I4*-[/T?S=#HO)Y>ND[;
MM#ZG,Q@Y\$+&30]9W11\$HQ]+\$.!LY^Z@GLHT]\$8:34PX.TY6,^2-/Y2&`OP
MZ0D[=*8DW"1R,'8&TVKIDB:6DY3J1%%(9^VH\7>WAJ]9H!"\` (DH>Y4OI
MM;#3V3L1G/'90;C`T2),(J64L%-;CM1M\$P\VD6E+2"\$%W2J#NX<\8`0%\$\$8!
M4?4G8/*@1>R\$%&- "RNL;*IL)<(6(6T00O2A_<*)9UN\D/<#YC0(Y0?<+`4
M+2;TYA?G-0>;4B,!R6T2M*4U#WT!VG1,)L@\$?C3XTA%Z;4PMC%#AAR!QR1+I
M:.PM/H\$;L`IFQ`"G72AEB\2NTXF[%\$#!BL@R2"BKD/X8?W/A^9\G%J7#G
M_Y<(QD&+2,*"<-`0`&V#=#&(SB0)>\$CA"04<"I0TV#+H)S"\$5V4=W<:2M<F,-
M!;!&T`R@5#!G64;TH.G*83)J8(+IJ"_"#R"8X))W(,=V#&P(2(3U1\$;BM
M%"LGFII^HI'2^;Z_*_V%%O*+M@NYD4%".F(W[0KY\$@E1M.3_J[J?U^7&`PNI
M/PLL83N>UJK:&@?2Y72ROMQ8\]764;EFHWZF:EK=]\M/OL?E%.5C#0"TD@\
M6\DK3A"V4=(PDY;D*&RN7<1V^%'H86]7S&8KKW'R@?KJ4+RM(%O*\$TRX)*
MAQ1(9Y*D,G'Z'5"0W+T"9IS^E/+?Q4\MI..M)9B@Q;>_W*Z(J^BECKI5_K?
MLP\$=3+DCO/50LP*CS(YXJ+U+^U,E79CR,S/-NF@G+.(%@R+B42.C^XRPR^"
MVHEM&<4VEC-07I:NOVLVT]%WYJ/.*<LT0UDMGC.W&P.\$\E1T,K\$I;@*6(1,
MTGJ;U):19AG,E`9*)#,5;>.=IZ&\$\KQ`_2D]2Y=%SDTGNAG(B:<J9=\$5L6&
MU#,IK(Z?BH\$EG@Y1`S@26NF&3BH,``%PHK"%.:?08SF@RR^=!,D\$M!<,(O
M-1#DX3#BI/0QC2`5TD`OK".CK_L/+7\Q<PUJ)W)A]B33PQ6V;(;T1W(V@B
M%B4:HY8)!M;QRCZDK(LI%1R4\$H?F]0G!FM%0)H:<Y0^:!2=#AE]!I,7<]/..
M'-J\$&IN)N'L)9AJ=]S6Z(IQ8ADPOAKSHO4[O4]@>H0>OV/?-NO]7\$\$[O?3LU
M8PIY!SKI=ZZ_R</BLCIY>?9QLX&>EA/-?@RM_/E=\^;^9"ADGG=EO+\US_S/
MM+/%>+X,6Q]3VB#ZH4:T^DQ\$1)@Y:\CP)HF@A"DA)T;W%8KQ(V:C&_N@4M51
M')S41%-J1Y11:T(<&8Q+0_FAR*4P]PQ_T'E?PCS1@RSXI-QU1^(*[X._Q#`V
M3C3,BP'%#--J>VJ4!,G9;-1\$IF;!S1V-RVV0.K5#*XO4&"H%O3*S85!YZB#
MC,V2J%T9UPR;&6/+*A14PTR"2/\$I/[BN\E`"<+,IR1`(),F`KE11M.2"0

M&XF^M*<8&\$D5I.S90@AH3JZ/9TH7;Q/VC\(\$\?:<)F]`NF31WT4%VJ2*RKPH
M\F77\YH5"%;[/TM0_DYJ0.:&R,DZ9PZ1L-#](##J#BE6O0"M.WRGLX<1W'I8
M!TU7%2]U[5B1)\E?G#B#WJ#R\$,/0>6]^CQ]_S5DT=,Z>MB:FA],)-2+6K\$/L4
MF0\$)FAB.144V>X""HXJ"+(46?0-*<;A)5J#[CQRFJ;.^MX\U\?%1PT-P'M!Y
M:_^H#?Z/0YK?MU3DY5&X\3B1DCOI<1\C&[U'CEA"U4%LF^.J,NH:\2?U.UP
MI8/8WDFEMY]%(K;RD7V=JYLNH')3R559]!HX4&`6]W#*F%U-"O)!BK#A(,=8
M\,*+':@G+K/J@6,5_M)OHT)W%/FW['-F@^#,!J7J/\$9EL&L9U/6+:C?5\$>6)
M#H=T9#KBJ4;J?#\$`ZM0BD70ZA31;*D\;.[R<BJ*06`E^0PI<F;J"\$5>K,GK
MC]UR!.Q%X]2#&>U"XVT?ZY5!Y.-K;NE#-F#.U^IK^^N^/JG5-[_AO<+7C\
MED'19[50Q!,,\&\.(354CV_K!>8;I5V:ZY12SIPCIHMO*H@1AU%`(.,J,Q^
MF_:D9[40^7[%VM\$,T7T>HBF5*CZG7'K>=0U^P14833.N:-9J`W(Q98'6PJ;;
M]-?KF)K:WGD42Z<2ZAR;U-V""9K4L>CG;^,=WXS@:TFMWU`!>I2%%-N..W32
MS*M4<Z4>)0QTFY7>U`F*6@J*Y'3&OW<J(K;4GR9OJN"!FDM[[0*(AL76RPTP
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MNE'91Z2*J&JTR6G,U5,7*GPE.+\$MBAC*IOMZ(?H@I1)7T4&`7)SI\YE?TQCY
M+_W:LB>549.:H+_2;2P@3V-5>_EG47U^A7TU+3\$&O4XF*/G^2T/E(+D8GGK+
M?%. \$&%RPXFIRQ=VS_VJ>AB]4COBI9V5,B:5'KPXEOR\7W9D82..;%1)`HJL7
MHVJ^Z9=O@ND/3Z.A%\$Y-6_0MR!809/:@V@,IVI&_"N39P-&&PH.\RU[#O+,
M-9(:\1E8JF?U1T1"A&SEFLI%D8V*WCNL>DK=Y)6]_FC0"EK.6VY!J`UA9'
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M1B'MK>`=*(DD&Q7CX2G977S1_C:+A9CS2\$!#<EX+:K=&\$)\$YO;:*51C3MX/V
M*J#CG.S9-U9EBG`Z?=U?O-5H]T9IRNQC@T4%:3O[,X#>@M9=H5LA55\VYRG+
MLDV%*VEG\$:9=.K=LP=QA=V/4E.JKG`728Q\ (E47H*_:+!F"@CQ`&%!J+/*"&
M:.3BJ+** -D1==QNQ@=3:#Q#<;T<#"#?"*R8%MHJ/TK0\,65XF\$2R[B`5),
MR#JGL9.YBIH8A@VQ\$K3*@Q`S(L,I.8\$6D?,3MX54^V;46%)]\$/#N1KIW=U
M>>7<U2Q9)W8.LU,A@PC9*X]X-C+`80>]&A!T1JU^\$+*;4[B'M*&,<TV+U
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M^X7YL`XJ40P[[\$K7693VU,;NL[NR5J6_O;]3_>%1FS/ZVJ9'OYK]EQ;=)T50
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M,Z7Z?'C0H`>U5/W]30&:@>%5.W<PY5&G"YMJ^B-C9G1.+YR]2HO\DUL`+X
MVS@E1:+U3I0H/=:SNS%T/[FCS*TI^TIW<L/RMY'Y[JNFYBH/=HSMZKF34OH
MRW*HI7?>"NWJ6V^1B\^*V4D.T?U,):I#\$Q51"A/P_`AT58<,)6">,TS*C?
M*1L<PD>E55VOJJV<6%&#PH[K2MU=W0>K5;=C5D5."4LNLAN*AVCE"*_7I<
M10/JKJ^0A\$W@-8Z7'<_-=Q(<5"T;KP8/;,(5K\Z_`8/4%\$G;F.Z5,85.HRM:
M:=MPFK8K*A`Y)V]1G;E<\$GW-Z\&LB]?FIR;:L"FG*9%\$6ORK(OP9N71@B=HG
MAFVW=;+`#0)%>%ZRF&B&HZJY>IK_+D\3`.8?>1X6HC4T_20`L?5`\9YX'C
M^?%#,N"0!IFD*`/F9'=%#:7BCR`3TFWS5PD*2:H:[B"FGXFCOCOJ3\4L=A6
MRB"!*X6-3L>V<0P3%\$FN[S%6.;;(,6MUM,&)GY3[W=5?GWX9E5/ZO3%5XJK
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M@]73*)]=.%4JCN^WJN7O]U,>?UXPC\Z;Q/A;L5,/_-^O6W>*#(0E+(]:M:J

MLD_JQ\U8G5G-?/E>+<7UZK72_`0BLU"9.&49\$KG%7*JTZINLD<TVZ[\$TTPW#
MN/=\X]XY^XD9/<P[E]VEK9\4:K&@?"#7?[':;Y4S8O'MB(PX"-0Z1\"/[7D,
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M&L8CT0O5\>I,U>(J\$:]3(M[5\$,F/*22\MJO2B6,F*W.QEXSF8[Y'2&%2E>!9
M]]*&;_0R?"G?SXQW7@ORJ(F/7\5@X.K&-A!VTRJE?VIY!S8/OU'0*G#^HC1
M;E_*BRR5^4H5U#E^/[R-?'K<R43^LY.+55%V\=-'YXOX/5-N_;\VC^44V_C
MVUX20Q-[U5%Z.\'YZ"D[Z=U<.#G]7P/]67"WKIK;]_[=66K'[M'_?6DC\A),
MU5:OSR6YWIBO)XHJ]%%MYB*J.[@=>5?M+M._*\00SZHW.A`\$4M7"\$L=15QC;X
M^..X^UY@BO+#W,;TB2CO?1:2"WO:KKX=.,-Y!7*ZX.X^NB+X49YE"-N?[S[R
MD;";<QCK-<KP><V5/BF53.#EVXP?K\F2S<NJS+PJT^/Z<C+LQ=E)S9]Y0
M0I\$=GY&Z:R7>BY":\$8IA-0`RLTII3""\Z/)"SX2C'N5)H7D+R+CBAF^YJJK8
M<<RF+\U7V,*6",E[[S%M4*&J8\@QOZ_T8.HI`!X&`0`0`0"<7%%*(=1U``W
M2`\$`W8V8>P``(```%,M1D]3+E)730`ZR[-#0`&="#=I7!1+%.V_TOO[:`
MP_JDDDDDD8:U\$A(1P-G#;VJJNW?LT0<`8[S/9I#P]VMLD;3G@_J\W_O_Y'P_U
M76_]_^_G_BE*YKY_^_O]6_Z6U_VO_M3_K_Z_S_BM>UC^IOS>O.4\UKSO?-8/
M![YSOF=^:KYC?G,1Q_+5\KWRM/+4\N>;F_F(^W8Z/_/^\,='R\OYY'YV/S4_
M.T=+KS-!^8_U\>5V/!UP/S'-KH^;U%8!_CX2'/U/5Q[Z]_,]\W@^I/AK8'PO
M'C\>;CST4ECPU3[R/S@>_ML<0=]B*71X^/4QXF<"I]]H5.5`1V^!5JIH/P
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MO-<<K>]:<K2_G8I?SS&1^8PW11V^8Z:MLUK>HZ^]<9KN.RX.RC\Z'=QU,>/Y
MGSIZ*^:YY6/-YR.NCI;<|U'@Z1]A8UJ.II6F1U,86HZFW!Z&F;4O:+]><C)D
MJ)ZIIYV.9'YO6,O\=[`=1=+%XRC\-*QAZCX1UD>_CU7!4F/2'S9Y0H_%QML^
M><\\SY7)PN'@[-U'Z@5UCT)H;F*"#NHYJ\W%?HX(XCK?LV6QZR*"<N/])@T*T
M:P9R)#CT/!NNW'C([+1Y2] '\$UD>/XZJ+J[K/?0^5H?G%-#T<7*W-I%C[9
M4Z&JM'5ZPH_'RJ>'7@[H8C^?=B*Y<!'%">:=C_OE39%<YA;"CZ^HQIKHATL
MXF,U>)BO+K\$QNG]8F`CL.3G<)-HOUT+SN:J,B\$<(Y)(25V*+*\$/ (RZ]&:V,V
MVC>9%ED98XRV9%(8RLZ52>M!!4HEQU!S## 5T\ \$Y@XRI#P"N9CP>QC7CX;BB
M.H\>/E&<';39]A-G&,T_..]C.R,0VA]@FY*559XHSUQG]CV<?[AQA8J*WZ?G
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M^1A"A@N2G`LWR2DX*%.*`A%Z.E.&XM.L4-CF]7C,"P\$(OM%&S<4ZJF?DU8+3
MJT?)2GEC3&,2&K#!9&MFM.JRDZT>]Z?=%HHX_1UA:Z11\X;LRI8CU-2G(2\$
M08/"R7I&]@_X<'K9E`CHN_2*C\=\$4M^3=W@U7BQO>FH2N;]PV0J^ZC3+8GQ-
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M=G/YM,;V%:"F!W!=IIE;*)[9RL"O9PW8/!RF*:/GL^;J[]14,X;#=#,*!_WG
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M>\$;7"Q*;%'^7;T9?;M]RK`3F1;>/>;ZSD[N<AB7]_9V*K'*T&>G5UYO:MH2>
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MO#_ =C_ !</CHXX[BB"]H=RJXACN&>/H[\$L>%V%>IICZ<0(.;CV%"D:X?S"2D
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M&P678E!/X5"=<-9<PO:B13RKMM+6'F`<`6\BP+^5=HH'OYUO'NJ/[+V^L2[(
M5:O-,0F4OJZ])@B+PE^>BLZXGN>7BT"!U6EC0@0FP1"\$V36B,^N:6MVH\$MPI
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M2P?BD(^\$1_*"VT*:.<M"! [B4)1T<***@E^&8"O"PMCQ\<"\$N_2EQQQB@GA2BP
MFD\E0!DX>\$^-(/BU+EZ1"-`3&BJN0<6DYMRK&2GP`**[>[?>4HXOQV<6/<3:
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M[/AM.E?D^J0Q:K/],8LJGU>C!\7U_ ^;OR>T]+E"/.4?>0@B^D!9I&H+6301
MLN6&S2MXC*SY+NL)WN*SA^V3UY+*2.S"_S)2]91CKAL6T4'T<8P=@,`&@\
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M7\._)@TN\$.+Q<PC9_E\!-[48F+PW[QBPO>T?.H?4-^.;1B2[*R,3\$8XX8
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M]&\;C:CML23^Q9987&@OT!NA\$T:T>+=L\5&5;.,:R4NN6&54+31HR,'9!WB

From: Keith Briffa <k.briffa@uea.ac.uk>
To: tatm@insec.quorus.e-burg.su
Subject: the Yamal data
Date: Wed Oct 30 17:45:53 1996

Dear Rashit,

As always I seem to have been away bullshiting and politiking in various meetings for weeks! I try to convince myself that this is of use to us as a dendrochronological community but I am not so sure how much that is really true these days. I have the data you sent and I had to get someone here to decode it for me . That is fine now so I would like to try and reformat and RCS it . I will be back in touch soon. Your paper is in review for Denrochronologia. I am very keen to get a much more detailed paper in The Holocene dealing with this stuff and I hope you and Stepan will consider this - perhaps for some time in spring next year. Sorry I wasn't in touch sooner. Please give my regards to Stepan and Valerie.

very best wishes

Keith

From: Keith Briffa <k.briffa@uea.ac.uk>
To: tatm@insec.quorus.e-burg.su
Subject: the Yamal data
Date: Thu Oct 31 12:01:04 1996

Dear Rashit,

In looking at the data I now see that you have only sent data from abot 350bc onwards. What is the situation with the earlier data. I am very interested in the details of the 1st millennium B.C. and especially this period from about 500 to 100 B.C. We still have a gap in the Tornetrask data at about 350 B.C.

I was of the opinion that this period was very low growth in the chronology of yours shown by Stepan in Cambridge - but it does not seem so low in the chronology he gave me. What are your thoughts on this and is it possible to get the earlier data when you are happy with them?Thanks very best wishes

Keith

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eugene Vaganov <evag@ifor.krasnoyarsk.su>
Subject: Re: message from Vaganov
Date: Tue Nov 12 17:36:40 1996
Cc: tatm@insec.quorus.e-burg.su

Eugene

I have not received my copy of the book. A message to Malcolm is the best idea. I have been experimenting with the Yamal data mostly trying to fit RCS curves - and am finding problems with recent local chronologies behaving oddly - i.e. too much growth in recent years makes it difficult to derive a valid age/growth curve. I have produced a rcs standardised curve for taimyr and will fax a copy to you. I will send comments to you and stepan on the two papers reviewed for dendrocronologia on the development of the yamal and taimyr chronologies. I have made major changes to the tracheid paper and need to type and send the new version to you - also there are problems understanding some bits - I will ask specific questions. How goes the organisation of the Krasnoyarsk meeting? Stepan /Rashit I have had some comments on the Yamal paper that I will try to email tommorow.

best wishes
Keith

At 13:41 12/11/96 +0000, you wrote:

>Dear Keith
> How are you? Did you receive the material
>(chronologies on Siberian subarctic) from
>Stepan? Several days later I'll send to you
>some additional data (several samples) on
>Taymir supra-long chronology, which make
>more deep in sampling the interval around
>500-1000 year.
> There are a few questions to you.
>1. The volume of "Radiocarbon" with proceedings
> reach Krasnoyarsk with some months delay, so
> can you send me by fax (007)(3912)43-36-86
> the content of volume (only for references)?
>2. What about the draft of paper which I gave
> you in Germany (paper concerning the compa-
> rison of tracheid dimension, cell wall thickness
> and density)?
>Best wishes, Gene.
>

From: gjenkins@meto.gov.uk
To: p.jones@uea.ac.uk, deparker@meadow.meto.govt.uk
Subject: 1996 global temperatures
Date: Fri, 22 Nov 1996 11:23 +0000 (GMT)
Cc: llivingston@meadow.meto.govt.uk, djcarson@meadow.meto.govt.uk, ckfolland@meadow.meto.govt.uk

Phil

Remember all the fun we had last year over 1995 global temperatures, with early release of information (via Oz), "inventing" the December monthly value, letters to Nature etc etc?

I think we should have a cunning plan about what to do this year, simply to avoid a lot of wasted time.

I have been discussing with David P and suggest the following:

1. By 20 Dec we will have land and sea data up to Nov
2. David (?) computes the December land anomaly based on 500hPa heights up to 20 Dec.
3. We assume that Dec SST anomaly is the same as Nov
4. We can therefore give a good estimate of 1996 global temps by 20 Dec
5. We feed this selectively to Nick Nuttall (who has had this in the past and seems now to expect special treatment) so that he can write an article for the silly season. We could also give this to Neville Nicholls??
6. We explain that data is provisional and how the data has been created so early (ie the estimate for Dec) and also
7. We explain why the globe is 0.23k (or whatever the final figure is) cooler than 95 (NAO reversal, slight La Nina). Also that global annual avg is only accurate to a few hundredths of a degree (we said this last year - can we be more exact, eg PS/MS 0.05K or is this to big??)
8. FROM NOW ON WE ANSWER NO MORE ENQUIRIES ABOUT 1996 GLOBAL TEMPS BUT EXPLAIN THAT IT WILL BE RELEASED IN JANUARY.

9. We release the final estimate on 20 Jan, with a joint UEA/MetO press release. It may not evoke any interest by then.

10. For questions after the release to Nuttall, (I late Dec, early Jan) we give the same answer as we gave him.

Are you happy with this, or can you suggest something better (ie simpler)? I know it sound a bit cloak-and-dagger but its just meant to save time in the long run.

Im copying this to DEP and CKF also for comments.

Cheers

Geoff

From: Wolfgang Cramer <cramer@nis.pik-potsdam.de>
To: Mike Hulme <m.hulme@uea.ac.uk>, VXT_COPR@luecology.ecol.lu.se (I. Colin Prentice)
Subject: Re: EU proposals
Date: Fri, 22 Nov 1996 15:51:36 +0100

Hm, clearly coordination between the two (if it really ends up as two) groups is absolutely essential, otherwise we would look entirely stupid. The first thing that comes to my mind is that nitrogen would be emphasizing a component of our overall idea which otherwise would not receive great attention - hence it could be, perhaps, amalgamated. They probably see it the other way around: In their problem, climatic variability comes second in importance. My view on this is that all of our model intercomparisons have shown that models essentially do crazy things with interannual variability, simply because nobody ever has tested them for that in any detail. Esser's model would probably be the last candidate to use here, since it is "less mechanistic" than any of the others - in fact, Colin and I seemed to agree to "not necessarily" include it into this proposal. These are just some thoughts for the moment.

I just finished a very first, rough draft of our outline, and I attach it to the end of this message. I have just sent it to Martin Heimann, but I have still not yet talked to him. I also send this whole thing to Colin, hoping that he will catch the thread through it without problems. Gerard Dedieu is the one I want to approach next - Alberte is already talking to him about this in the context with other things.

Cheers,

Wolfgang

On Nov 22, 14:12, Mike Hulme wrote:

> Subject: EU proposals

> Wolfgang,

>

> This email (see below) has just arrived from Andrew Friend. I wonder if we

> are in danger of competing amongst ourselves here, or is the role of N

> sufficiently far away to avoid problems? Do you want me to talk with Andrew

> again or shall I wait for you to get back to me next week after

contacting

> Martin? Would Gerd Esser be one of 'our' C modellers?

>

> Looking at the call for proposals it seems that 'Theme 1.1.1 Basic processes

> in the climate system' fits best for us since there is a specific item (5)

> which states: 'studies of global budgets of greenhouse gases with

> particular emphasis on fluxes, transformations and storage in the biosphere,

> lithosphere and oceans.'

>

> If not here, then maybe under '1.1.3 Climate variability, simulation of

> climate and prediction of climate change' since there is an item (4)

> 'Development, validation and application of models for important

> climate-related quantities such as mean sea-levels, storm and surge climates

> and carbon cycling.' But here there is an emphasis on European approaches.

>

> About EU politics, Balabanis is the guy for ESCOBA, but that doesn't mean he

> is necessarily the one for us. Troen handles a lot of the climate projects

> in 1.1.1, 1.1.2 and 1.1.3. We have quite a bit to do with him. But it

> depends if there is someone else on carbon etc. Maybe Balabanis is the

> place to start.

>

> Regards,

>

> Mike

>

> *****

>

> Dear Mike

>

> Thank you very much for your hospitality the other day. I enjoyed my visit and

> look forward to continued collaboration. With regard to ESCOBA, this project is

> in domain 1.1 of the Environment and Climate Programme, and is thus
the
> responsibility of Balabanis.
>
> Has there been any progress with regard to a new proposal? I have
contacted
> Gerard Dedieu, and he says that he will have to think about the idea
some more.
> Meanwhile, I have received an invitation from Gerd Esser (another
ESCOBA
> partner) to put together a new proposal to look at 'The role of
nitrogen in the
> carbon balance of the terrestrial biosphere' for submission in
January. A
> couple of the other ESCOBA partners have expressed interest in this
proposal.
> Part of the new project will be to use global process-based carbon
models, such
> as our Hybrid model, to assess the biospheric sink for C (and its
geographical
> distribution) over the period 1750 to 1990. I guess there could be a
role for
> an improved climatology here.
>
> I could investigate further the current intention with regard to
climatology in
> this project if you wish.
>
> Andrew
>
>--- End of excerpt from Mike Hulme

2

Global, spatially explicit assessment of the interannual
variability in terrestrial carbon storage

VERY FIRST, INCOMPLETE draft for a new research proposal
to be submitted to the European Union
for the second phase of the Third Framework "Environment and
Climate"

Goal

A critical uncertainty in assessments of global change impacts and feedbacks is the source/sink relationship for carbon between atmosphere and the terrestrial biosphere, and particularly its interannual variability. Recent advances in modelling of atmospheric and biospheric processes, combined with significant progress in data gathering for climate, CO₂ and O₂, now allow for a dedicated experiment that is likely to reduce this uncertainty. Equilibrium approaches to the simulation of global carbon fluxes are no longer adequate for this, since empirical studies are showing both a long-term trend and a significant interannual variability of CO₂ fluxes, which appear to be most strongly driven by climatic impacts on terrestrial vegetation.

Experimental design

For a time period of several decades, we propose to perform a simulation of biospheric carbon fluxes using:

- ú a range of currently available biospheric models (ongoing intercomparisons indicate that there is no clear 'best approach' - therefore this project will use several approaches <<<and we would like to include the CESBIO people for the testing of all model outputs against global seasonal fPAR observations - or does this overload the project?>>>),
- ú a realistic, historical high-resolution climatology (which so far does not exist - a recent IGBP-workshop has however clearly identified the need for it and what would be necessary to achieve it within a short time-frame),
- ú a land use map from currently available observations <<<or from satellite?>>>),
- ú a 3D atmospheric transport model for the calculation of net CO₂ concentrations at the stations where these are observed <<<and of course those measurements themselves>>>.

Land use and different climatic elements will be combined in factorial combinations to investigate the role of each element in the full system response.

<to be continued... A critical question to me at this time is whether the project should go for two timeframes: if there is, in addition to the timeframe of available CO₂ measurements, also a 10 year timeframe, then we could compare all models against available seasonal fPAR profiles from satellites and hereby assess their capacity to recover other aspects of

biospheric dynamics. Another question is whether we should also throw in a GCM experiment to allow for future scenarios.>

Expected results

- ú Improved understanding of the global carbon cycle - realistic seasonal and interannual simulations are essential for identifying regional responses of the terrestrial biosphere

- ú From that: Improvements of mitigation assessments such as those required by the IPCC

- ú Global, historical, high-resolution climatology which is required by other assessments of impacts of global change

Consortium participants

Contractors

- ú Potsdam Institute for Climate Impact Research (PIK), Potsdam, Germany (Wolfgang Cramer): Project coordination, experimental design and analysis

- ú Climatic Research Unit, University of East Anglia (CRU-UEA), Norwich, UK (Michael Hulme): Development of a global high-resolution historical climatology

- ú Max Planck Institute for Meteorology (MPIM), Hamburg, Germany (Martin Heimann): Atmospheric transport model, ocean component, analysis of results against measurements, TBM simulations using SILVAN

- ú possibly a fourth one (CESBIO, Toulouse?) if we decide to go for a significant remote sensing component

Subcontractors

- ú Department of Ecology, Lund University, Lund, Sweden (I. Colin Prentice): TBM simulations using BIOME3

- ú Institute of Terrestrial Ecology, Edinburgh, UK (Andrew Friend): TBM simulations using HYBRID

- ú Department of Chemistry, Frankfurt University, Frankfurt, Germany (Gundolf H. Kohlmaier): TBM simulations using FBM

- ú Sheffield University, Sheffield, UK (F. Ian Woodward): TBM simulations using Sheffield-DGVM or DOLY

- ú (if politically possible:) Center for Resources and Environmental Studies, Australian National University (ANU-CRES), Canberra, Australia (Michael F. Hutchinson): Development of suitable scaling algorithms for climatic data assimilation

From: "Tatiana M. Dedkova" <tatm@insec.quorus.e-burg.su>
 To: k.briffa@uea.ac.uk
 Subject: from Rashit
 Date: Mon, 9 Dec 96 14:19:37 +0500

Dear Keith,
 we received your letters concerning our paper for Dendrochronologia and three long chronologies.

1. As regards individual ring width data of living trees from Yamal we would remind you that you have them. Stepan gave to you in England one diskette. There are data for *Larix sibirica* from three sites (KHA - from Khadyta river, 67812'N 69850'E; JAH - from Yahody river 67807'N 69854'E and POR - from Portsya river 67827'N 71800'E) and for *Picea obovata* from two points (SCH - Shtshutshya river 66849'N 69850'E and KHD - from Khadyta river 67807'N 69854'E).

2. We would be very grateful if you can do some corrections and additions in the paper for Dendrochronologia. We did not quite understand what we have to do on missing rings? Just enumerate years when missing rings occur? If so, these are following years:

Year absent	%	ind	%	Year absent	%	ind	%
-1172	1 of 4	25%	51	700	2 of 8	25%	31
-1171	1 of 4	25%	12	707	1 of 9	11%	31
-1168	1 of 4	25%	13	718	1 of 8	13%	33
-1142	1 of 5	20%	50	773	1 of 8	13%	38
-1127	1 of 5	20%	15	777	1 of 9	11%	67
-1126	1 of 5	20%	10	814	3 of 9	33%	12
-1029	1 of 10	10%	57	816	3 of 9	33%	10
-1021	1 of 10	10%	55	818	3 of 10	30%	14
-988	1 of 10	10%	17	867	1 of 11	9%	34
-987	1 of 10	10%	12	903	1 of 11	9%	12
-986	2 of 10	20%	17	904	1 of 10	10%	30
-971	1 of 12	8%	44	914	1 of 9	11%	25
-969	1 of 12	8%	67	915	1 of 9	11%	61
-964	1 of 12	8%	14	959	1 of 10	10%	59
-899	1 of 10	10%	29	1006	1 of 12	8%	28
-886	1 of 9	11%	42	1007	1 of 12	8%	28
-882	4 of 9	44%	5	1170	2 of 12	17%	8
-860	1 of 11	9%	20	1259	1 of 10	10%	28
-823	2 of 8	25%	18	1270	1 of 11	9%	36
-792	1 of 6	17%	15	1278	3 of 11	27%	15

-547 2 of 5 40% 61 1290 1 of 10 10% 44
 -543 1 of 6 17% 91 1300 1 of 9 11% 18
 -318 1 of 5 20% 29 1302 1 of 9 11% 58
 -294 1 of 5 20% 66 1323 1 of 7 14% 18
 -292 1 of 6 17% 24 1334 1 of 8 13% 53
 -288 1 of 6 17% 61 1342 1 of 9 11% 8
 -287 2 of 6 33% 25 1347 1 of 9 11% 14
 -261 1 of 5 20% 30 1380 1 of 12 8% 38
 -248 1 of 5 20% 13 1453 5 of 13 38% 9
 -246 1 of 5 20% 25 1456 1 of 13 8% 20
 -241 1 of 5 20% 12 1460 1 of 13 8% 24
 -239 1 of 5 20% 25 1466 1 of 12 8% 30
 -139 2 of 7 29% 9 1529 2 of 7 29% 10
 -119 1 of 7 14% 14 1560 1 of 7 14% 6 living
 -118 1 of 7 14% 11 1714 1 of 11 9% 49 1 of 16 6%
 16 1 of 8 13% 26 1718 73 1 of 16 6%
 49 1 of 9 11% 11 1730 45 1 of 20 5%
 134 1 of 22 5% 33 1732 28 2 of 20 10%
 143 4 of 21 19% 7 1739 3 of 9 33% 50 1 of 20 5%
 155 1 of 21 5% 54 1742 23 3 of 20 15%
 207 1 of 16 6% 54 1749 57 1 of 20 5%
 426 1 of 6 17% 19 1752 67 1 of 21 5%
 492 1 of 9 11% 19 1755 72 1 of 21 5%
 493 1 of 9 11% 16 1783 39 1 of 22 5%
 495 1 of 9 11% 16 1788 83 1 of 22 5%
 536 1 of 12 8% 38 1789 92 1 of 22 5%
 546 1 of 12 8% 12 1795 102 1 of 22 5%
 579 1 of 16 6% 41 1806 68 1 of 22 5%
 589 1 of 19 5% 31 1808 97 1 of 22 5%
 596 1 of 18 6% 22 1812 35 1 of 22 5%
 598 1 of 18 6% 51 1814 54 1 of 22 5%
 623 3 of 17 18% 6 1815 30 1 of 22 5%
 636 2 of 17 12% 32 1816 2 of 3 67% 2 16 of 22 73%
 637 4 of 17 24% 9 1817 33 1 of 22 5%
 639 3 of 17 18% 9 1818 3 of 3 100% 4 14 of 22 64%
 640 7 of 17 41% 7 1819 22 6 of 22 27%
 644 1 of 18 6% 22 1820 1 of 3 33% 9 12 of 22 55%
 646 2 of 18 11% 26 1824 1 of 3 33% 66
 living
 1825 2 of 22 9% 38
 1828 1 of 22 5% 47
 1831 5 of 22 23% 28
 1833 4 of 22 18% 31

1837	1 of 22	5%	49
1867	3 of 23	13%	21
1882	1 of 23	4%	39
1883	1 of 23	4%	50
1884	1 of 23	4%	29
1885	1 of 23	4%	28
1889	1 of 24	4%	20
1891	1 of 24	4%	32
1903	2 of 24	8%	46
1934	1 of 24	4%	45
1946	1 of 24	4%	46
1947	1 of 24	4%	40
1967	1 of 20	5%	102
1971	1 of 20	5%	50
1975	1 of 20	5%	40

We have to note that frequency of missing rings on increment cores of living trees higher, because on samples of subfossil trees we try to find this kind of rings on whole disc.

Some periods are notable for missing rings: 988-964 BC, 882 BC, 143 AD, 623-646 AD (especially 640 AD), 814-816-818 AD, 1453 AD and beginning of 1800th AD.

3. Stepan ask what about book by Bailey?

Best wishes,

Rashit

From: Keith Briffa <k.briffa@uea.ac.uk>
To: tatm@insec.quorus.e-burg.su
Subject: the paper
Date: Mon Dec 9 15:17:42 1996

Dear Rashit and Stepan

Thanks for the message and the missing data info. I will make some additions and include a plot/list of these missing years. I assume you don't mind me including your plot of the recent Yamal curve and statistics about crossdating with Polar Urals. I'll send ammended paper as soon as possible. Thanks for the quick reply. Do you have a working fax? best wishes to you all

Keith

From: Tim Carter <tim.carter@fmi.fi>
To: d.viner@uea.ac.uk (David Viner - Climate Impacts LINK Project)
Subject: ECLAT 2
Date: Wed, 11 Dec 1996 11:11:18 +0200
Cc: m.hulme@uea.ac.uk

Dear David/Mike,

Thanks for sending me the ECLAT 2 proposal. First, let me say that I support the idea of a continued role for activities co-ordinating and facilitating the provision of climate change information for EC impacts research and other related research and policy. ECLAT 2 is one way of achieving this, but the fact that it is a Concerted Action Initiative imposes some limitations.

The major limitation is that CAIs are not supposed to involve original research. They are networking activities, with a view on forging research links and developing new research projects. In my view, there is a need for a number of targetted research activities on scenario development, that might be covered by the themes of the workshops you are suggesting in ECLAT 2, but which would be best served by some dedicated research projects. It really isn't satisfactory to wait until the end of ECLAT 2 before embarking on research. Many of the key topics are already known, and although research may be proceeding in some of these areas (especially in downscaling techniques, scenario development techniques, etc.), what is still lacking is co-ordination across Europe in the selection and application of climate change scenarios in impact assessment. In my view, there are two areas in sore need of targetted research:

(1) A project to analyse all available information from GCMs and historical data, which will provide some uncertainty bounds on the anticipated future climate in Europe (by region) for use in policy as well as in impacts assessment. Such a project should involve GCM groups (interpreting the GCM outputs), scenario developers (who can apply methods of generalising across a lot of GCM predictions and emissions scenarios, etc.), and a few impact analysts, who can advise on suitable scenarios for use in a variety of applications (entry level or basic scenarios).

(2) A project to develop guidelines for impact analysts on the application of climate change (and related) scenarios in European impact assessments. This work would need to be linked closely to any co-ordinated, entry-level scenarios selected for use in EC projects.

However, unless you have a project proposal in the pipeline at CRU (?) I don't think there is now time to develop a new proposal to meet the 15 January deadline.

Comments on the draft document:

1. It is unclear to me how Figure 1 relates to the text. The arrows are not well differentiated in the fax version I have, and the boxes are not explained.
2. Similarly, Figure 2 is also misleading. It implies that there is a large transfer of information from the CC modelling community to the CC impacts community, but surely the whole function of the ECLAT SE would be to act as a filter in this transfer. Note that the title of the figure should be revised.
3. PLEASE REMOVE the reference to ECLAIR - there is no such name! This was a light-hearted emailed suggestion for ECLAT 2, not for Martin's CA which doesn't have a name to my knowledge.
4. In the suggested steering committee, I would strengthen the representation of the impacts community. This could be done by time horizon: e.g. one hydrologist to cover a range of time periods from sub-daily to century scale; one forester or soils expert for the long term, one agriculture person for the medium term (maybe I could represent this community), desertification/erosion/fire risk person for short to medium term and/or an integrated assessment person (perhaps three or four persons). You should try to avoid the group being dominated by GCM'ers (do all GCM groups have to be represented?)

You might ask Ib Troen if there would be any opportunity to obtain EC funding BEFORE THE FIFTH FRAMEWORK CALL FOR PROPOSALS for a targeted research topic, if this was strongly and urgently recommended by a task group workshop. Might there be special funding from DG XI, ENRICH or the Environment Agency?

Best wishes,

Tim

Dr. Timothy Carter
Affiliation: Agricultural Research Centre of Finland

Postal address: c/o Finnish Meteorological Institute
Box 503, FIN-00101 Helsinki, FINLAND

Tel: +358-9-1929-4125

Fax: +358-9-1929-4129

Email: tim.carter@fmi.fi

From: Richard Warrick <cearsr@waikato.ac.nz>
To: 'Mike Hulme' <m.hulme@uea.ac.uk>
Subject: RE: Scengen and CC:Train
Date: Thu, 16 Jan 1997 10:00:48 +-1300

Dear Mike,

Thanks for your detailed reply concerning Scengen and CC:Train. I was not proposing to incorporate Scengen in a major way into the training package, and I am quite aware of the problems of consistency regarding aerosol effects, natural variability, etc. Rather, I thought that the training package would be an excellent way to introduce the existence of Scengen (and MAGICC) to the Country Teams which are responsible for coordinating national assessments. (the intention was NOT to provide intensive technical training in its use -- the country team members are largely coordinators, not technical climate experts). In this way, when it comes time to actually carry out the national assessments, Scengen would be recognised as a major tool for scenario generation and, if appropriate, CRU could be contacted regarding its application, technical training or collaboration. You had mentioned to me at the IPCC meeting in London that one of your major aims was to get Scengen recognised as the "standard" for scenario generation for impact assessments, and I simply thought I saw a way of furthering that aim through the CC:Train mechanism.

Given the training programmes that you are currently proposing through ENRICH and others, I can understand your fears that we might "muddy the waters". Let me pose the following options; that we

- (1) use some hard-copy examples from Scengen;
- (2) incorporate a demonstration diskette (do you have one?);
- (3) just mention the existence of Scengen;
- (4) not mention Scengen at all.

Frankly, I am quite happy with any of these. The part on climate change scenarios is really only a small bit of the overall V&A training package in any case.

Good luck with your proposals.

Cheers,
Dick

From: Mike Hulme[SMTP:m.hulme@uea.ac.uk]
Sent: Thursday, 16 January 1997 00:45
To: Richard Warrick
Cc: m.kelly@uea.ac.uk; tim.carter@fmi.fi; wigley@ncar.ucar.edu
Subject: Re: Scengen and CC:Train

Dick,

And Happy New Year to you also.

You've posed me a tricky one re. SCENGEN and my answer about it being incorporated into the CC:Train package as a component tool is going to have to be 'no'. Let me explain.

We too here have plans to exploit SCENGEN (and MAGICC) in a training/educational context. I ran a pilot seminar here for UNEP before Christmas on scenario construction, although this was using the new WINDOWS/Unix versions of both MAGICC and SCENGEN (MAGICC 2 and SCENGEN 2; IPCC 1995 compatible) we have re-written. Also, I have just submitted a proposal (called SPARCCS) to ENRICH in DGXII for a support package for regional climate change scenarios. This would be a 2-year project with emissions people, as well as MAGICC, SCENGEN and our new global historic climatology. I think we have a good chance of funding.

With this background I do not want SCENGEN (and especially the old DOS version) 'leaking' out into the climate training community at this stage.

I am confirmed in this view by thinking that the complex issues surrounding scenario creation (and the new IPCC Taskgroup on scenarios for the 3rd assessment is grappling with these - ask Tim Carter about it) should not be an essential part of a vulnerability/adaptation package. And even if you think differently then let me suggest the following: if you think it should be a minor part then I do not think that you need SCENGEN formally incorporated; if you think it should be a major part then not only do I think you are wrong in thinking so, but there is more to the scenario issue than can be supplied by SCENGEN - for example, you need MAGICC, you need to consider how you handle aerosols, and you need to think about natural variability and signal/noise issues.

My feeling is that by all means use SCENGEN within CEARS in thinking about the training package and coming up with some off-line examples (either

sample scenarios or guided sensitivity), but do not incorporate it in the package. [By the way SCENGEN does not have imaginary countries!]. If people want more detailed thinking on scenarios then you could always refer them to CRU (which is what our speciality is).

I hope you understand my feelings on this - I am not trying to be negative, but am thinking ahead and about the complexity of the scenario issue. I have talked with Tim Carter recently at some length about some of these things so I will copy this correspondence to him.

Good luck with CC:Train anyway and I'm sure you'll come up with something good.

Regards,

Mike

At 14:41 10/01/97 +-1300, you wrote:

>Dear Mike,

>

>Happy New Year's Greetings from Downunder!

>

>I have a question for you regarding Scengen that relates to a "training package" which CEARS have agreed to develop for CC:Train (under UNITAR).
> CC:Train is currently developing about four such training packages
>pertaining to climate change, of which CEARS has agreed to undertake one,
>on Vulnerability and Adaptation assessment. The V&A and other packages are

>supposed to be flexible enough to be used under a variety of regional and
>country contexts. These packages build upon existing guidelines and
>>manuals (e.g. Carter et al's IPCC Guidelines...) and are designed for
>trainers who will be conducting training workshops for the coordinators of
>national assessments (the CC:Train "Country Teams"). Beginning on 21
>January, Tim Carter will be here for 3 weeks, as will Stephanie Lenhart
>(U.S. Country Studies Program), in order to help with this task. The V&A
>training modules will closely follow the IPCC Guidelines. I have proposed
>developing the package as a kind of role-playing simulation exercise in
>which the participants carry out a mini-assessment for a hypothetical
>country.

>

>One of the major steps in the assessment, of course, is the development of
>climate change scenarios. I thought it would be very effective to use

From: druid@ldgo.columbia.edu (Gordon Jacoby)

To: k.briffa@uea.ac.uk

Subject: Russia

Date: Sun, 26 Jan 1997 14:16:32 -0500

Hi Keith:

As you are aware, the situation in Russia is very uncertain with their unfortunate economic condition, especially science support. There is interest, hope, and dots on maps showing intent but actual activity is difficult to judge. In the particular area I am interested in, the Taymyr, there is no current active tree-ring research going on although it has been previously sampled and some reports are in preparation. Ed probably told you that I have submitted a proposal to do work there. My understanding is that unless there is some external funding support, such as my project, tree-ring sampling there is in abeyance. Several people, including yourself, recognize the great potential in the region. From my perspective it seems that the Polar Urals are being studied, Yakutia to the far east is being studied, some work has been done by Szeicz and Macdonald at the Lena but there is need for more intensive effort in Taymyr. I would like to hear your perspective on the situation.

In a related topic, I am thinking of using the option in Ed's new ARSTAN to use the regional standardization method. In Russia and other locales the establishment of trees is episodic. In particular, in Alaska Glenn Juday has data showing cohort groups being established in favorable times. In Taymyr also, the establishment of trees is not evenly distributed through time. There are times of growth and times of demise. This concerns me as it could affect the development of a regional curve. do you see problems arising from this?

I am also curious to hear any comments you care to make about my recent letter to Fritz Schweingruber. He obviously will pursue any style of sampling and analyses he chooses to. My only contention is that he should not represent his data as the definitive tree-ring information, particularly ring-width data. His opinions are influential but there is an accumulating body of ring-width data that clearly shows him to be missing much important information with his style of sampling. Scientists and others should be aware of this fact.

Cheers, Gordon

From: Eugene Vaganov <evag@ifor.krasnoyarsk.su>
To: k.briffa@uea.ac.uk
Subject: from Vaganov
Date: Thu, 6 Mar 97 14:40:15 +0000 (KRS)

06.03.97
fAJL partid.txt

2.1 CO
2.2 Professor
2.3 Head of Group
2.4 M
2.5 Fritz
2.6
2.7 Schweingruber
2.8.1 Swiss Federal Institute for Forest, Snow and Landscape
Research
2.8.2 Department of Ecology
2.8.3 Forest and Climate Research Unit
2.9
2.10 Zuercherstrasse 111
2.11
2.12 8903
2.13 Birmensdorf
2.14 CH
2.15 41 1 7392281
2.16 41 1 7392215
2.17 fritz.schweingruber@wsl.ch
2.18 1
2.19 6000
2.20 0
2.21 2000
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2.23 0
2.24 1000
2.26 0

2.1 CR
2.2 Doctor of Philosophy
2.3 Senior Research Associate
2.4 M
2.5 Keith

2.6
2.7 Briffa
2.8.1 University of East Anglia
2.8.2 School of Environmental Sciences
2.8.3 Climatic Research Unit
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2.16 44 1603 507784
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2.1 CR
2.2 Doctor of Biological Sciences
2.3 Head of the Laboratory of Dendrochronology
2.4 M
2.5 Stepan
2.6 Grigor'evich
2.7 Shiyatov
2.8.1 Institute of Plant and Animal Ecology
2.8.2
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2.9 Ural Branch RAS
2.10 8 Marta Street 202
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2.15 7 3432 294080
2.16 7 3432 294161
2.17 plant@insec.quorus.e-burg.su
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2.25 5000

2.1 CR
2.2 Doctor of Biological Sciences
2.3 Director of Forest Institute
2.4 M
2.5 Evgeny
2.6 Alexandrovich
2.7 Vaganov
2.8.1 Institute of Forest
2.8.2
2.8.3 Laboratory of Dendrochronology
2.9 Siberian Branch RAS
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"MULTI-MILLENNIAL-LENGTH DENDROCLIMATIC RECONSTRUCTIONS AT HIGH-LATITUDE REGIONS OF SIBERIA".

By signing this declaration, I certify that the information given in this proposal relating to me and the team I represent is to the best of my knowledge true and complete. I have been involved in the preparation of the full proposal and I agree with its contents. I am fully authorised to commit myself and the team I represent to be ready to set up and execute all tasks, duties and obligations assigned to us in this research proposal, if selected.

I hereby authorise the co-ordinator as lawful attorney and administrator and empower him to act all of the necessary actions to administrate validly the herein said rights on behalf of me in case the proposal should be selected by INTAS, inter alia, to negotiate and to conclude the co-operation agreement, as well as any amendments, variations or additions to the co-operation agreement on my behalf.

Laboratory of Dendrochronology
Institute of Forest SB RAS
Krasnoyarsk

Dr.Eugene A.Vaganov

5 March, 1997

fAJL projid.txt

1.1 Multi-millennial-length dendroclimatic reconstructions
at high-latitude regions of Siberia.

1.2 5

1.3 600

1.4 36

1.5 Oct-97

1.6 4

1.7 60000

By signing this proposal, I certify that the information given in this proposal is the best of my knowledge, true and complete as received from all project participants; that all participants were involved in the preparation, agree with this project proposal and have declared themselves ready to perform the project as proposed in case of selection.

I am fully authorised to commit myself and the team I represent to be ready to set up and execute all tasks, duties and obligations assigned to us in this research proposal and I am ready to act as the co-ordinator of the project.

The proposal contains pages.

PROJECT CO-ORDINATOR First name and family name:
Fritz Schweingruber

Date: March,1997 Original signature:

fAJL sum.txt

4.1. TITLE OF THE PROJECT

Multi-millennial-length dendroclimaticreconstructions at high-latitude regions of Siberia

4.2. SUMMARY

This research will make a major contribution to our knowledge of high-resolution climate variability at high latitudes of Western and Middle Siberia throughout the Holocene using the unique potential of tree-ring data.

The specific objectives of this proposal are the development of two supra-long (each spanning 6-9000 years up to present) continuous larch ring-width chronologies at two distant each other high-latitude locations of Siberia (Yamal and Taimyr peninsulas). Ring-width chronologies developed from coniferous trees growing at the polar timberline in Siberia contain a very strong climatic signal, mainly summer air temperatures. With these chronologies high-resolution continuous and quantitative

reconstruction of summer temperatures will be made.

As in the areas of the past and present polar and upper timberlines trees megafossils have been preserved properly in large quantities in the Holocene deposits (alluvial, lacustrine and peat), there is a good possibility to develop continuous, multi-millennial tree-ring chronologies.

Now the material already collected and measured (1800 subfossil wood samples from Yamal and 280 samples from Taimyr) has yielded the ring-width chronologies continuously spanning the last 3200 years (Yamal) and 950 years (Taimyr).

However, there are also many more samples that have been measured and have provided data, now assembled in a number of provisionally "floating" chronologies covering much of the period from 7000 to 1700 B.C. (based on some 70 radiocarbon dates of samples of this wood). There is a fair chance that a 6-9000-year continuous chronologies will be constructed within the span of the proposed project.

These chronologies and temperature reconstructions will be the first to be so long, reliable, annually-resolved and precisely-dated with known reliability across the whole of northern Hemisphere. These reconstructions will allow to compare and contrast the details of temperature changes at the moderate-continental region of Yamal Peninsula with the continental region of Taimyr Peninsula and allow modern and predicted temperature patterns to be compared with variability patterns of pre-industrial era. Participants of the proposed project are the well-known institutions which are engaged in the field of dendrochronology and dendroclimatology and have collaborated with each other during the last 6 years.

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3.1 TITLE

Multi-millennial-length dendroclimatic reconstructions at high-latitude regions of Siberia

3.2 OBJECTIVES

This research will make a major contribution to our knowledge of high-resolution climate variability at high latitudes of Western and Middle Siberia throughout the Holocene using the unique potential of tree-ring data.

The specific objectives of this proposal are as follows:

- to develop two supra-long (each spanning 6-9000 years up to present) continuous ring-width larch chronologies at two high-latitude locations of Siberia;
- using these tree-ring chronologies, to make a multi-millennial high-resolution continuous and quantitative reconstruction of summer temperatures;
- to analyse spatio-temporal patterns of temperature variability at these locations over a range of timescales (annual, decadal, multi-decadal and centennial) and their connections with various forcing factors and other annual resolution records being developed elsewhere in the Arctic and Subarctic.

3.3. BACKGROUND

Reconstruction and analysis of natural climatic changes through the whole Holocene at high latitudes are of great importance as climatic conditions, especially air temperature, are most variable and sensitive to various forcing functions (Budyko, 1980; Jones and Kelly, 1983; Intergovernmental Panel on Climate Change, 1990). However, there are a minute quantity of long, precisely-dated and high-resolution proxy climatic series for these regions.

The territory of Yamal Peninsula located on the eastern boundary of influence of the Atlantic air masses and the territory of the eastern part of Taimyr Peninsula located between the Arctic High and Siberian High are of major importance for monitoring regional and global-mean air temperatures and assessing theories and models concerned with past, current and future climate changes (Lamb, 1977; Briffa and Jones, 1993; Moses et al., 1987).

Tree rings as a proxy indicator of the past climatic conditions are of special interest as they allow to reconstruct climatic parameters with seasonal and annual resolution for many hundred and thousand years, to provide an exact absolute and relative dating of the tree-ring data, to establish high-frequency climate changes (from interannual to centennial timescales) with high confidence, to obtain dendroclimatic information practically for

every site where trees grow at present or grew in the past.

Intensive dendroclimatic investigations are carrying out in many countries and regions, mainly in temperate and subtropic zones (Fritts, 1976, 1991). At high latitudes such works began later (during the last two decades) and living trees were used primarily for developing tree-ring chronologies of 200-500 years long (Aniol and Eckstein, 1984; Shiyatov, 1984, 1986; Jacoby and D'Arrigo, 1989; Schweingruber, Briffa and Nogler, 1993; Briffa, Jones, Schweingruber, Shiyatov and Vaganov, 1996; Jacoby, Wiles, D'Arrigo, 1996; Vaganov, Shiyatov and Mazepa, 1996). As in the areas of the past and present polar and upper timberlines trees megafossils have been preserved properly in large quantities on the surface and in the Holocene deposits (alluvial, lacustrine and peat), there is a possibility to develop continuous, multi-millennium and sensitive to climate tree-ring chronologies. Such works began in the Polar Ural Mountains (Shiyatov, 1986; Graybill and Shiyatov, 1992; Briffa, Jones, Schweingruber, Shiyatov and Cook, 1995), in the southern part of Yamal Peninsula (Shiyatov, Surkov, 1980; Hantemirov, 1995), in Finnish Lapland and Northern Sweden (Zetterberg, Eronen and Briffa, 1995), in the eastern part of Taimyr Peninsula (Vaganov, Naurazbaev, Schweingruber and Briffa, in press) and in the Lower Indigirka River at present. Now the longest, continuous and absolute-dated ring-width chronologies developed for the Yamal Peninsula (spanning 3200 years) and for the Northern Scandinavia (spanning 2160 years) and the "floating" chronologies dated by the radiocarbon method extended back 9500 and over 7000 years respectively.

Ring-width chronologies developed from coniferous trees growing at the polar timberline in moderate-continental and continental regions of Siberia contain a very strong climatic signal, mainly summer air temperatures of tree growth year (Graybill and Shiyatov, 1992; Briffa, Jones, Schweingruber, Shiyatov and Cook, 1995; Hantemirov, 1995; Vaganov, Shiyatov and Mazepa, 1996). The explained variance over the calibration and verification periods is highest reported in the literature to date (65-70%) and it allows to make a quantitative reconstructions of summer temperatures. These chronologies and temperature reconstructions will be the first to be so long, reliable, annually-resolved and precisely-dated with known reliability across the whole of northern Hemisphere. These reconstructions will allow to compare

and contrast the details of temperature changes at the moderate-continental region of Yamal Peninsula with the continental region of Taimyr Peninsula and allow modern and predicted temperature patterns to be compared with variability patterns of pre-industrial era.

Participants of the proposed project are the well-known institutions which are engaged in the field of dendrochronology and dendroclimatology and have collaborated with each other during the last 6 years.

- The Group of Tree-Ring and Site of the Swiss Federal Institute for Forest, Snow and Landscape Research (Birmensdorf, Switzerland). The Group is currently engaged on a major programme of densitometric and ring-width chronology development involving many sites across the whole of the Northern Hemisphere including sites with living trees in the polar timberline area of Russia. This work is specifically designed to provide climatically-sensitive data for use in large spatial climate reconstruction work. Dr. F.H.Schweingruber, Head of the Group, is known throughout the world for his work in wood anatomy and dendrochronology and the development of tree-ring densitometry. He has published extensively in different areas of wood anatomy and tree-growth research and has authored several classic books.

- The Laboratory of Dendrochronology of the Institute of Plant and Animal Ecology of the Russian Academy of Sciences, Ekaterinburg, Russia is one of the leading laboratory in the field of dendrochronology in Russia. The Laboratory has an international reputation for its work on the developing ring-width chronologies at high latitudes and altitudes, reconstruction of climatic conditions, developing long-term chronologies, studying cycles in tree-ring series, using tree-ring data for studies of the upper and polar timberlines dynamics and forest succession. Dr. S.G.Shiyatov, Head of the Laboratory, is one of the pioneers of dendrochronology in Russia and has worked for more than 30 years in the Far North and mountains of the Urals, Siberia, Far East and Middle Asia. He has published more than 130 articles and three monographs. Dr. Shiyatov was the first who began to collect subfossil wood in Russia for developing long-term chronologies.

- The Laboratory of Dendrochronology of the Institute of Forest

of the Russian Academy of Sciences, Krasnoyarsk, Russia is another leading laboratory in the field of dendrochronology in Russia. Dr. E.A.Vaganov, Director of the Institute of Forest and Head of the Laboratory of Dendrochronology, has an international reputation for his work on the cell structure of wood layers of coniferous trees, seasonal growth variations and cambium activity, developing simulation models of seasonal tree growth, developing ring-width and cell chronologies, reconstructing climatic conditions of the past using tree-ring chronologies. He has published more than 100 articles and 5 monographs.

- The Climatic Research Unit of the University of East Anglia, Norwich, Great Britain is one of the world's leading research organisation specialising in the study of climate change: climate history, current climates, projected changes and impacts. Dr. K.R.Briffa, Senior Research Associate at the Climatic Research Unit, has considerable experience in climatology and with the use of statistical methods of climate analyses and dendroclimatic reconstruction, especially with regard to large-spatial-scale reconstructions of climate patterns and published many articles on the theoretical and practical aspects of dendrochronology and dendroclimatology, and on use of paleoclimate data for understanding current and possible future climates.

3.4 SCIENTIFIC AND TECHNICAL DESCRIPTION

3.4.1. RESEARCH ACTIVITIES

Tree-ring data will be obtained from living trees and subfossil wood of Siberian larch (*Larix sibirica* Ledeb.) in western Siberia and Gmelini larch (*Larix Gmelini* Pilger) in central Siberia. The first location is situated in the southern part of Yamal Peninsula (67-688N, 69-718E), the second location in the eastern part of Taimyr Peninsula (71-738N, 98-1058E). There is a great many properly preserved subfossil wood in the Holocene deposits at both locations, mainly in the alluvial and peat deposits.

The main variable measured will be ring width. This variable reflects properly climate influences on tree growth at the polar timberline areas of Siberia having a continental climate.

Ring-width chronologies for the last 400-500 years will be developed from the oldest living trees. Extensions to these chronologies back further in time will be made by using subfossil

material, joined with the living material by standard crossdating procedures. High-precision radiocarbon dates will be used for rough dating of "floating" tree-ring chronologies.

The sampling subfossil wood and development of the Yamal's supra-long chronology began since 1982 by the workers of the Laboratory of Dendrochronology (Ekaterinburg). Most intensively this work was carried out during the last five years. Now the material already collected and measured (1800 subfossil wood cuts) has yielded the ring-width chronology continuously spanning the last 3200 years. However, there are also many more samples that have been measured and have provided data, now assembled in a number of provisionally "floating" chronologies covering much of the period from 7000 to 1700 B.C. (based on some 45 radiocarbon dates of samples of this wood). These chronologies separated by 50 to 500 year length gaps. There is a fair chance that a 9000-year continuous chronology will be constructed for this location within the span of the proposed project.

Similarly, work with a shorter history than the Yamal's research has clearly established potential to build a chronology at least as long in the Taimyr Peninsula where the modern polar timberline extends to about 72°30'N, most northern over the world. This work is not so advanced as in Yamal, but the work to date suggests that very rapid progress is likely. Samples from living and dead trees have already been assembled at the Laboratory of Dendrochronology (Krasnoyarsk) into the 950-year continuous chronology. The collections from this location are not so extensive as those made to date at Yamal (280 subfossil wood samples), but there is an abundant supply of subfossil trees, many with over 300 annual rings. 25 radiocarbon dates of samples of this material suggest major phases of tree growth around 8500 B.P. and 5000 B.P. The general distribution of the radiocarbon dates suggests that, eventually, sufficient trees can probably be located to span the whole of the last 10000 years. It is not expected that a continuous 10000-year ring-width chronology will be produced within timeframe of this project. However, there are good prospects of producing a 5-6000- year chronology to the present.

3.4.2 RESEARCH RESULTS

During three years we expect to develop the continuous and good-replicated tree-ring 9000-year larch chronology for the

Yamal Peninsula and the 5-6000-year larch chronology for the Taimyr Peninsula. Using these chronologies we intend to reconstruct and analyse a summer temperature variation at several time scales (annual, decadal, multi-decadal and centennial) and compare the data obtained with other high-resolution Holocene-length proxy data (ice cores, laminated sediments, historical documents).

The results of this project will be published primarily in the scientific literature in Russian and English and presented at different national and international conferences. Because of the fundamental interdisciplinarity and collaborative interaction within the subgroups, a number of multi-authored papers will be produced. The individual and mean ring-width chronologies and the reconstructions produced will be distributed to the international scientific community through submission to the International Tree-Ring Data Bank (Boulder, Colorado, USA) and to other national and international institutions and data centres.

3.5 MANAGEMENT INFORMATION

3.5.1 TASK DIVISION

Dr F.H.Schweingruber (Swiss Federal Institute for Forest, Snow and Landscape Research) will be the project co-ordinator on the proposed project from the INTAS countries.

Dr S.G.Shiyatov (Institute of Plant and Animal Ecology) will be the responsible scientist on the proposed project and he will take part in collecting, dating, developing and analysing the multi-millennial ring-width chronology at the area of Yamal Peninsula. The next young scientists of the Institute will be involved in the project:

Rashit M. Hantemirov, Candidate of Biological Sciences, 34 years old. He will take part in collecting, cross-dating and analysing the material.

Alexander Yu. Surkov, technician, 30 years old. He will take part in collecting, preparing and measuring the subfossil wood samples.

Dr E.A.Vaganov (Institute of Forest) will be the responsible scientist on the proposed project and he will take part in collecting, dating, developing and analysing the multi-millennial ring-width chronology at the area of Taimyr Peninsula. The next young scientists will be involved in the project:

Mukhtar M. Naurazbaev, junior research fellow, 35 years old.
He will take part in collecting, preparing, measuring, cross-dating and analysing the material.

Alexander V. Kirdyanov, post-graduate, 25 years old.
He will take part in data processing, density measurements, chronology analysis.

Dmitry V. Ovchinnikov, post-graduate, 26 years old.
He will take part in cross-dating, data processing, chronology analysis.

Dr K.R. Briffa (Climatic Research Unit) will be the responsible scientist on the proposed project and he will take part in analysing growth-climate relationships, developing statistical models of tree growth, extracting climatic signal, reconstructing and analysing climatic conditions of the remote past.

3.5.2 PLANNING

To carry-out the objectives of this proposal the workers of the Russian laboratories will carry out an intensive collecting subfossil wood during summers of 1997-1998 at two high-latitude locations (Yamal and Taimyr peninsulas) using helicopters, boats and ships. To finish the development of the Yamal chronology it is necessary to collect additionally no less than 300-400 cuts of subfossil wood. Much more intensive collecting (600-800 cuts for two field seasons) is needed to develop the Taimyr chronology. All samples collected during these two years and earlier will be measured and cross-dated at Ekaterinburg and Krasnoyarsk laboratories until the middle of 1999.

The Russian laboratories together with the Climatic Research Unit of the University of East Anglia during 1997-1999 will be analysing the material obtained (standardization of individual series, development of mean chronologies, studying growth-climate relationships, developing statistical models of tree growth, extracting climatic signal, reconstructing and analysing climatic conditions of the remote past). This work will be finished at the end of 1999.

3.5.3 EQUIPMENT

Participants of the proposed project have the necessary equipment for fieldwork, measuring equipment and compatible software.

3.5.4 SCIENTIFIC REFERENCES

Briffa, K.R., Jones, P.D., Schweingruber, F.H., Shiyatov, S.G. and Cook, E.R. Unusual twentieth-century summer warmth in a 1,000-year temperature record from Siberia. *Nature*, 1995, Vol. 376, 13 July, 156-159.

Briffa, K.R., Jones, P.D., Schweingruber, F.H., Shiyatov, S.G., Vaganov, E.A. Development of a North Eurasian chronology network: Rationale and preliminary results of comparative ring-width and densitometric analyses in Northern Russia. *Radiocarbon*, 1996, 25-41.

Hantemirov, R.M. A 2,305 year tree-ring reconstruction of mean June-July temperature deviations in the Yamal Peninsula. *Publication of the Academy of Finland*, 1995, 6, 124-127.

Shiyatov, S.G., Mazepa, V.S., Vaganov, E.A., Schweingruber, F.H. Summer temperature variations reconstructed by tree-ring Data at the polar timberline in Siberia. *Radiocarbon*, 1996, 61-70.

Vaganov, E.A., Shiyatov, S.G., Mazepa, V.S. *Dendroclimatic Study in Ural-Siberian Subarctic*. Novosibirsk: "Nauka", Siberian Publishing Firm RAS, 1996, 246 pp. (in Russian).

From: Keith Briffa <k.briffa@uea.ac.uk>

To: m.salmon@uea

Subject: from Rashit

Date: Fri Apr 4 14:26:42 1997

>To: k.briffa@uea.ac.uk

>Organization: ECOLOGY INSTITUTE

>From: "Tatiana M. Dedkova" <tatm@insec.quorus.e-burg.su>

>Date: Mon, 9 Dec 96 14:19:37 +0500

>Return-Receipt-To: tatm@insec.quorus.e-burg.su

>Subject: from Rashit

>Return-Receipt-To: tatm@insec.quorus.e-burg.su

>Lines: 106

>

>Dear Keith,

>we received your letters concerning our paper for Dendrochronologia
>and three long chronologies.

>1. As regards individual ring width data of living trees from
>Yamal we would remind you that you have them. Stepan gave to you
>in England one diskette. There are data for Larix sibirica from
>three sites (KHA - from Khadyta river, 67812'N 69850'E; JAH -
>from Yahody river 67807'N 69854'E and POR - from Portsa river
>67827'N 71800'E) and for Picea obovata from two points (SCH -
>Shtshutshya river 66849'N 69850'E and KHD - from Khadyta river
>67807'N 69854'E).

>2. We would be very gratefull if you can do some corrections and
>additions in the paper for Dendrochronologia. We did not quite
>understand what we have to do on missing rings? Just enumerate
>years when missing rings occur? If so, these are following years:

>

> Year absent % ind % Year absent % ind %

>-1172 1 of 4 25% 51 700 2 of 8 25% 31

>-1171 1 of 4 25% 12 707 1 of 9 11% 31

>-1168 1 of 4 25% 13 718 1 of 8 13% 33

>-1142 1 of 5 20% 50 773 1 of 8 13% 38

>-1127 1 of 5 20% 15 777 1 of 9 11% 67

>-1126 1 of 5 20% 10 814 3 of 9 33% 12

>-1029 1 of 10 10% 57 816 3 of 9 33% 10

>-1021 1 of 10 10% 55 818 3 of 10 30% 14

> -988 1 of 10 10% 17 867 1 of 11 9% 34

> -987 1 of 10 10% 12 903 1 of 11 9% 12

> -986 2 of 10 20% 17 904 1 of 10 10% 30

> -971 1 of 12 8% 44 914 1 of 9 11% 25
 > -969 1 of 12 8% 67 915 1 of 9 11% 61
 > -964 1 of 12 8% 14 959 1 of 10 10% 59
 > -899 1 of 10 10% 29 1006 1 of 12 8% 28
 > -886 1 of 9 11% 42 1007 1 of 12 8% 28
 > -882 4 of 9 44% 5 1170 2 of 12 17% 8
 > -860 1 of 11 9% 20 1259 1 of 10 10% 28
 > -823 2 of 8 25% 18 1270 1 of 11 9% 36
 > -792 1 of 6 17% 15 1278 3 of 11 27% 15
 > -547 2 of 5 40% 61 1290 1 of 10 10% 44
 > -543 1 of 6 17% 91 1300 1 of 9 11% 18
 > -318 1 of 5 20% 29 1302 1 of 9 11% 58
 > -294 1 of 5 20% 66 1323 1 of 7 14% 18
 > -292 1 of 6 17% 24 1334 1 of 8 13% 53
 > -288 1 of 6 17% 61 1342 1 of 9 11% 8
 > -287 2 of 6 33% 25 1347 1 of 9 11% 14
 > -261 1 of 5 20% 30 1380 1 of 12 8% 38
 > -248 1 of 5 20% 13 1453 5 of 13 38% 9
 > -246 1 of 5 20% 25 1456 1 of 13 8% 20
 > -241 1 of 5 20% 12 1460 1 of 13 8% 24
 > -239 1 of 5 20% 25 1466 1 of 12 8% 30
 > -139 2 of 7 29% 9 1529 2 of 7 29% 10
 > -119 1 of 7 14% 14 1560 1 of 7 14% 6 living
 > -118 1 of 7 14% 11 1714 1 of 11 9% 49 1 of 16 6%
 > 16 1 of 8 13% 26 1718 73 1 of 16 6%
 > 49 1 of 9 11% 11 1730 45 1 of 20 5%
 > 134 1 of 22 5% 33 1732 28 2 of 20 10%
 > 143 4 of 21 19% 7 1739 3 of 9 33% 50 1 of 20 5%
 > 155 1 of 21 5% 54 1742 23 3 of 20 15%
 > 207 1 of 16 6% 54 1749 57 1 of 20 5%
 > 426 1 of 6 17% 19 1752 67 1 of 21 5%
 > 492 1 of 9 11% 19 1755 72 1 of 21 5%
 > 493 1 of 9 11% 16 1783 39 1 of 22 5%
 > 495 1 of 9 11% 16 1788 83 1 of 22 5%
 > 536 1 of 12 8% 38 1789 92 1 of 22 5%
 > 546 1 of 12 8% 12 1795 102 1 of 22 5%
 > 579 1 of 16 6% 41 1806 68 1 of 22 5%
 > 589 1 of 19 5% 31 1808 97 1 of 22 5%
 > 596 1 of 18 6% 22 1812 35 1 of 22 5%
 > 598 1 of 18 6% 51 1814 54 1 of 22 5%
 > 623 3 of 17 18% 6 1815 30 1 of 22 5%
 > 636 2 of 17 12% 32 1816 2 of 3 67% 2 16 of 22 73%
 > 637 4 of 17 24% 9 1817 33 1 of 22 5%

> 639 3 of 17 18% 9 1818 3 of 3 100% 4 14 of 22 64%
> 640 7 of 17 41% 7 1819 22 6 of 22 27%
> 644 1 of 18 6% 22 1820 1 of 3 33% 9 12 of 22 55%
> 646 2 of 18 11% 26 1824 1 of 3 33% 66

l i v i n g

> 1825 2 of 22 9% 38
> 1828 1 of 22 5% 47
> 1831 5 of 22 23% 28
> 1833 4 of 22 18% 31
> 1837 1 of 22 5% 49
> 1867 3 of 23 13% 21
> 1882 1 of 23 4% 39
> 1883 1 of 23 4% 50
> 1884 1 of 23 4% 29
> 1885 1 of 23 4% 28
> 1889 1 of 24 4% 20
> 1891 1 of 24 4% 32
> 1903 2 of 24 8% 46
> 1934 1 of 24 4% 45
> 1946 1 of 24 4% 46
> 1947 1 of 24 4% 40
> 1967 1 of 20 5% 102
> 1971 1 of 20 5% 50
> 1975 1 of 20 5% 40

>We have to note that frequency of missing rings on increment
>cores of living trees higher, because on samples of subfossil
>trees we try to find this kind of rings on whole disc.
>Some periods are notable for missing rings: 988-964 BC, 882 BC,
>143 AD, 623-646 AD (especially 640 AD), 814-816-818 AD, 1453 AD
>and beginning of 1800th AD.
>3. Stepan ask what about book by Bailey?
>Best wishes,
>Rashit

From: "Tatiana M. Dedkova" <tatm@insec.quorus.e-burg.su>
To: k.briffa@uea.ac.uk
Subject: from Shiyatov
Date: Mon, 5 May 97 09:44:43 +0500

Dear Keith,

After our long silence we would like inform you about our successes, problems and plans.

1. The main success to our mind is the next. We have filled up the gap (1350-1250 BC) between the absolutely dated 3250-year Yamal chronology and the nearest floating chronology. It was happened few weeks ago using samples collected in 1996. Now there are no obstacles to develop in the nearest future the 7000-7500-year length continuous chronology. Now we are working with ancient samples: searching the places of missing and false rings, making more precise datings of individual chronologies and so on. During this time interval we have some problems. For example, no more samples were found up to now to confirm the absence of false ring near 360 BC.

2. This summer we intend to hold an expedition from the end of June to the middle of August in the southern part of Yamal peninsula to collect more samples of subfossil wood which have a great many of rings, are sensitive and cover the intervals represented by insufficient quantity of samples at present. We think that during this field season we must collect a necessary quantity of samples to develop a well represented 7000-7500 years chronology. Next year we intend to collect subfossil samples of wood from the middle part of Yamal peninsula to reconstruct the dynamics of polar timberline during the Holocene in detail using a large number of tree remnants absolutely dated by dendrochronological method.

2. This year we have a small grant the from the Russian Science Foundation for developing the Yamal supra-long chronology (approximately 4000 USD). But we are not sure that all this sum we will receive. For example, last year we have received 37% from the promised sum of money. As cost of helicopter's rent is increased again this year (about \$ 2.000 for one hour), we have the problem how to reach our research

area in the Yamal peninsula. E. Vaganov have the same problem with organisation of field works over the territory of Taimyr peninsula. That is why we and E.Vaganov ask you to transfer each of us 7-8.000 USD until the end of June from the ADVANCE project, if it is possible. Last summer, when I was in England, you promised to help us with money to organise field works this year.

3. I am finishing a measurements of rings of subfossil wood samples collected last year on the surface and in one lake and some bogs in the Polar Ural Mountains. I found a little more ancient wood (not all samples are dated until now) and can prolong this chronology at least up to one hundred years. This summer I will be in the mountains and try to collect wood from other lakes. I want to develop the Polar Urals chronology for the last 2.000 years.

4. Now we are preparing the paper concerning Yamal project in Russian and we need to cite the paper prepared for Dendrochronologia in English. Could you send to us the last version of this articles by e-mail or by post?

We wish you and your family the best. We wish the same to Phil Jones and his family.

Sincerely yours

Stepan Shiyatov and Rashit Hantemirov

From: "Isaak M. Khalatnikov" <khalat@itp.ac.ru>
To: k.briffa@uea.ac.uk
Subject: Keith Briffa
Date: Tue, 10 Jun 97 07:18:26 +0400 (MSD)

Dear Keith,

Thank you for the message of 5 June, 1997.

I am understanding your difficulties with transferring money and I think the best way for us if you will bring money to Krasnoyarsk and I give you a receipt.

Rashit will go to Yamal at the end of June and I go to the Polar Urals at the beginning of July. We can find money temporary at our Institute and other sources for three months to fulfill our fieldworks. Now I am at two weeks holiday with my wife and granddother near Moscow after the meeting of Russian Academy of Sciences where E.Vaganov was elected as the Academician. It is important for dendrochronological studies at our country and international collaboration.

Sincerely yours Stepan Shiyatov

From: "Tatiana M. Dedkova" <tatm@insec.quorus.e-burg.su>
To: k.briffa@uea.ac.uk
Subject: from Shiyatov
Date: Tue, 17 Jun 97 14:36:06 +0500

Dear Keith,

I am not sure you received my message sent the last week from Moscow. Therefore I decided to repeat it.

Thank you for the message of 5 June,1997.

I am understanding your difficulties with transferring money and I think the best way for us if you will bring money to Krasnoyarsk and I give you a receipt.

Rashit will go to Yamal at the end of June and I go to the Polar Urals at the beginning of July. We can find money temporary at our Institute and other sources for three months to fulfill our fieldworks. Now I am at two weeks holiday with my wife and grand-daughter near Moscow after the meeting of Russian Academy of Sciences where E.Vaganov was elected as the Academician of RAS. It is important for dendrochronological studies at our country and international collaboration.

Sincerely yours Stepan Shiyatov

From: Arnulf Gruebler <gruebler@iiasa.ac.at>
To: alcamo@usf.uni-kassel.de, knut.alfsen@cicero.uio.no,
dennis.anderson@ic.ac.uk, becon@public3.bta.net.cn,
g.r.davis@pxg.silon.simis.com, fisher@iiasa.ac.at, gruebler@iiasa.ac.at,
ja_edmonds@pnl.gov, j.fennhann@risoe.dk, stuart@edf.org,
Fewewar@tarnet.pl, kennethgregory@msn.com, ehaites@hookup.net,
bhare@ams.greenpeace.org, m.hulme@uea.ac.uk, jefferson@wec.co.uk,
tyjung@ccmail.keei.re.kr, emilio@ppe.ufrj.br, brahman@ktmp.kaist.ac.kr,
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naki@iiasa.ac.at, t-morita@nies.go.jp, rmoss@usgcrip.gov,
naki@iiasa.ac.at, ynassef@sl.minfor.gov.eg, wpepper@icfkaiser.com,
hm_pitcher@pnl.gov, lkprice@lbl.gov, crosenzweig@giss.nasa.gov,
shs@leland.stanford.edu, shukla@iimahd.ernet.in, J.F.Skea@sussex.ac.uk,
leena@teri.ernet.in, ipcc_sec@gateway.wmo.ch, rob.swart@rivm.nl,
rwatson@worldbank.org, weyant@leland.stanford.edu,
e.worrell@nwsmail.chem.ruu.nl, rogner@iiasa.ac.at
Subject: No Subject
Date: Fri, 01 Aug 1997 15:51:38 +0200

<x-rich>

Dear Participants,

Please find attached the Minutes of the SRES Meeting in Laxenburg, June 14-16. 1997.

Please note that the list of participants will be sent additionally Monday, 4th of August.

Best regards,

Arnulf

</x-rich>

Attachment Converted: "c:\eudora\attach\finalmin.doc"

<x-rich>

<center>Dr. Arnulf Gruebler

Environmentally Compatible Energy Strategies

International Institute for | Email: gruebler@iiasa.ac.at

Applied Systems Analysis | Phone: +43 2236 807 470

A-2361 Laxenburg, Austria | Fax: +43 2236 71313</center></x-rich>

From: "Wallace, Helen" <helen.wallace@uk.greenpeace.org>
To: "'t.mcmichael@lshtm.ac.uk'" <t.mcmichael@lshtm.ac.uk>, "'m.hulme@uea.ac.uk'" <m.hulme@uea.ac.uk>
Subject: Letter
Date: Thu, 21 Aug 1997 18:21:04 +0100

Dear Tony and Michael,

The final draft of the letter to the Times is attached, incorporating your changes (I hope I have combined them in a way that you are both happy with).

Brian Hoskins and Adrian Jenkins have both decided that they prefer not to sign the letter, although agreeing with its message. I haven't been able to contact anyone else in the short time available, so I leave it up to you to decide whether you are still both happy to go ahead.

If so, Mike could you please reply to both Tony and myself and let us know, and Tony could you then send it as agreed?

Thank you both very much for your time and trouble.

Best regards,
Helen

Dr Helen Wallace
Senior Scientist
Greenpeace UK

Greenpeace, Canonbury Villas, London, N1 2PN

Tel: +44-171-865-8241
Fax: +44-171-865-8202

FINAL DRAFT

Letters Editor
The Times

Fax: 0171-782-5046
Email: letters@the-times.co.uk

21 June 1997
Dear Sir,

Without wishing to comment on the dispute between BP and Greenpeace (Editorial, 20 August), we would like to remind your readers of the seriousness of the potential threat caused by our continued use of fossil fuels. This damage occurs both locally - as evidenced by the deterioration of air quality in UK cities in the past few weeks - and also globally.

As scientists studying the impacts of climate change, we consider the global threat from greenhouse gases to be serious and to need addressing. Adverse effects on human populations are likely to result from changes in weather patterns, shifts in storm frequencies, rises in sea level and the spread of certain pests and infectious diseases. A wide variety of ecosystems throughout the world will be at increasing risk.

We have little idea whether or not we can manage such adverse effects and therefore the prudent course of action is to limit the cause of the threat.

Major shifts in investment away from fossil fuels will therefore be required to make the necessary reductions in emissions of carbon dioxide to the atmosphere. Large companies like British Petroleum seem to us to be well placed to take an active part in investing in these changes. There is no doubt the need for precautionary, preventative action is urgent.

Yours sincerely,

Prof. A.J. McMichael
London School of Hygiene and Tropical Medicine
University of London
Keppel Street
London
WC1E 7HT

Dr. M. Hulme
Climatic Research Unit
University of East Anglia
Norwich
NR4 7TJ

From: Nebojsa Nakicenovic <naki@iiasa.ac.at>
To: alcamo@usf.uni-kassel.de, knut.alfsen@cicero.uio.no,
dennis.anderson@ic.ac.uk, becon@public3.bta.net.cn,
Ged.R.Davis@si.simis.com, ja_edmonds@pnl.gov, j.fenhann@risoe.dk,
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kennethgregory@msn.com, gruebler@iiasa.ac.at, ehaites@hookup.net,
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brahman@ktmp.kaist.ac.kr, Rik.Leemans@rivm.nl, vc@vc.udsm.ac.tz,
dpid@[169.158.128.138], Doug.D.Mckay@si.simis.com,
laurie.michaelis@oecd.org, mori@shun-sea.ia.noda.sut.ac.jp, t-
morita@nies.go.jp, rmoss@usgcrp.gov, nassef@hotmail.com,
wpepper@icfkaiser.com, hm_pitcher@pnl.gov, lkprice@lbl.gov,
rogner@iiasa.ac.at, crosenzweig@giss.nasa.gov, shs@leland.stanford.edu,
leo@iiasa.ac.at, shukla@iimahd.ernet.in, J.F.Skea@sussex.ac.uk,
leena@teri.ernet.in, rob.swart@rivm.nl, Bert.de.Vries@rivm.nl,
weyant@leland.stanford.edu, e.worrell@nwsmail.chem.ruu.nl,
dgvictor@iiasa.ac.at
Subject: IPCC - a) Meeting, 17-19. Sept. 97; b) New Bureau
Date: Mon, 06 Oct 1997 16:54:08 +0200
Cc: macdon@uea.ac.uk, jaeger@uea.ac.uk, leo@uea.ac.uk, johnson@uea.ac.uk,
mcdonald@uea.ac.uk

<x-rich>Dear Colleagues,

I would like to take this opportunity to thank all of you who have attended the

SRES Lead Authors' meeting (17-19 September 1997) and Rob Swart and his colleagues from RIVM for organizing and hosting the meeting.

We have achieved a lot in the three short days as you will soon also see from the minutes. The minutes of the meeting will be forwarded to you later this week

together with the revised SRES work plan that we have discussed during the meeting.

Sorry that it took a while longer this time for the completion of the minutes,

but I hope that they will refresh you memory about the outcome of the meeting.

Erik Haites just e-mailed that he returned from the IPCC plenary meeting in

Maldives and that the new IPCC Bureau has been appointed. It consists of

30 members: the Chair (Bob Watson), 5 Vice-Chairs (R. Pachuari (India), R.

Odingo (Kenya), G. Meira Filho (Brazil), Y. Izrael (Russia), K. Seiki (Japan), and 8 Bureau members for each of the three Working Groups. The Bureau for Working Group III (responsible for SRES) is B. Metz (Netherlands), O. Davidson (Sierra Leone), E. Jochem (Germany), M. Munasinghe (Sri Lanka), E. Calvo (Peru), R. Madruga (Cuba), R.T.M. Sutamihardja (Indonesia), and L. Lorentsen (Norway).

Best regards,

Naki

<center>Nebojsa Nakicenovic

Project Leader

Environmentally Compatible Energy Strategies

International Institute for | Email: naki@iiasa.ac.at

Applied Systems Analysis | Phone: +43 2236 807 411

A-2361 Laxenburg, Austria | Fax: +43 2236 71313</center>
</x-rich>

From: Angela.LIBERATORE@DG12.cec.be

To: "m.hulme" <m.hulme@uea.ac.uk>, "Martin.OConnor" <Martin.OConnor@c3ed.uvsq.fr>, alcamo <alcamo@usf.uni-kassel.de>, jaeger <jaeger@eawag.ch>, dvm <dvm@xs4all.nl>, eepriia <eepriia@gn.apc.org>, hourcade <hourcade@alize.msh-paris.fr>, "t.jackson" <t.jackson@surrey.ac.uk>, jaeger <jaeger@iiasa.ac.at>, vertic <vertic@gn.apc.org>, "pier.vellinga" <pier.vellinga@ivm.vu.nl>, pweingart <pweingart@bird.zif.uni-bielefeld.de>, fy1 <fy1@soas.ac.uk>

Subject: Copy of: climate: Japanese proposal

Date: Tue, 7 Oct 1997 14:55:31 +0200

From: Andrew Kerr <101322.3724@compuserve.com>

Sender: Andrew Kerr <101322.3724@compuserve.com>

To: Peter DEBRINE <Peter.Debrine@wwfus.org>,

Patricia DESMARES <patricia.desmares@wwf.be>,

Cherry FARROW <cfarrow@wwfnet.org>, Elizabeth FOLEY <EFOLEY@wwfnet.org>,

Karen GILL <kgill@wwfnet.org>,

"Merylyn HEDGER (wwfnet)" <mmhedger@wwfnet.org>,

Martin HILLER <mhiller@wwfnet.org>, Aldo IACOMELLI <aldo.jacomelli@wwf.it>,

Lars Georg JENSEN <wwf2@post4.tele.dk>, Steve JUDD <smjudd@sun.ihep.ac.cn>,

Paolo LOMBARDI <mc2236@mclink.it>, Tony LONG <tlong@wwfnet.org>,

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"Ad van WIJK (ecofys)" <a.vanwijk@ecofys.nl>,

"Ad van WIJK (uu)" <vwijk@chem.ruu.nl>

Subject: climate: Japanese proposal

Message-ID: <199710051347_MC2-22DC-A5E4@compuserve.com>

MIME-Version: 1.0

Content-type: text/plain; charset="iso-8859-1"

From: Andrew Kerr, WWF Climate Change Campaign

re.: "scandalous" Japanese climate change proposal

Dear All

I am in Japan for the next week. If you need to, you can contact me by phone at the following numbers:

- * Monday - +81 10 760 5022 (Yurika's mobile)
- * Tuesday-Thursday - via WWF Japan. Tel: +81 3 3769 1711; fax: 3326 1717.
- * Friday - Tokyo Grand Hotel. Tel: +81 3 3456 2222

Tomorrow the Japanese government is due to formally announce its emission reduction proposal for the industrialised world for the Kyoto climate summit: a maximum of a 5% reduction from 1990 levels for a basket of three greenhouse gases over the period 2008-2012. In a second period up to 2017, industrialised countries would not be obliged to make further reductions.

See below for fuller details and an analysis of the emission reduction implications for various industrialised nations.

The information has been well-leaked. In a talk to the Foreign Correspondents Club of Japan last Friday I described the proposal as a "joke". This was well picked up by the written press here.

Now more details have emerged, the proposal is even weaker than first thought. We are faxing a press release out this afternoon to Japan-based

agencies and press with WWF's reaction (see below). You might like to join in the condemnation of what Japan is proposing and ensure that your country flatly rejects the proposal.

Japan's Special Ambassador, Toshiaki Tanabe, is on a world tour canvassing for the support of other industrialised nations. After visiting Washington DC he moved on to Hawaii a few days ago for an informal conference including Australia, New Zealand, Canada and the US. Today's Yomiuri Shimbun gave front-page coverage to Australia's outrage over the stringency of the Japanese proposal!

Tanabe is moving to Europe for talks in the next few days. It is vital that European governments reject the proposal in no uncertain terms and urge Japan to at least support the EU standpoint. (Note: the WWF policies and measures study for Japan identifies how to cut CO2 emissions 8.8% below 1990 levels by 2005 and 14.8% by 2010 - very similar to the EU position). It would also be very useful if progressive business groups would express their horror at the new economic opportunities which will be foregone if Kyoto is a flop.

Best wishes, Andrew

CLIMATE CHANGE: JAPANESE PROPOSAL FOR KYOTO

To be formally announced by the Japanese government, Monday 6 October 1997

Following information is from the Nikkei Journal, 4 October 1997

A. Content of the proposal

1. First period: the five years from 2008 to 2012

Reduction of 5%; Base year: 1990

- 1) Gases: CO₂, methane, Nitrous oxide
- 2) Target figures will be flexible according to the future energy situation, changes in industrial structures, etc. But in any case, the total emission should not exceed 1990 level.
- 3) Each country's target would be based on emission per GDP, emission per capita, and population growth rate.

If emission per GDP of 1990 (A) is smaller than emission per GDP of all countries (B), the reduction rate should be $5\% \times (A/B)$

If per capita emission of 1990(C) is smaller than per capita emission of all countries (D), the reduction rate should be $5\% \times (C/D)$.

If population growth rate from 1990 to 1995 is more than the population growth rate of all other countries, the reduction target of that country should put into consideration their high population growth rate.

Banking, Borrowing, Joint Implementaion and Emission Trading schemes should be introduced with certain conditions.

2. Second period: 2013-2017

Emission should not exceed the level of the first period.

More sophisticated differentiation scheme should be adopted for the second period.

B. Implications of the proposal

Resulting emission reduction targets for the five years 2008-2012, relative to 1990:

	%
Australia	1.8

Czech Republic	5.0
Denmark	2.5
Germany	3.1
Italy	2.5
Japan	2.5
Portugal	1.6
Russia	5.0
Spain	2.2
Switzerland	1.3
UK	3.7
US	2.6

Overall reduction for all industrialised countries: 3.2 %

WWF PRESS RELEASE

JAPAN PROPOSAL FOR KYOTO SUMMIT SCANDALOUS, WWF SAYS

KYOTO, JAPAN, 5 October 1997 ? The World Wide Fund for Nature condemned as "scandalous" the Japanese government's proposal for reducing greenhouse gases responsible for climate change, Sunday, and called on industrialised nations to flatly reject it.

As full details of the proposal emerged over the weekend, it was revealed that Japan suggests allowing industrialised countries to make extremely marginal reductions in their emissions by as late as 2008-2012. In a second five-year period up to 2017, countries would only be required to ensure their emissions were lower than in 1990.

"The Japanese plan presents a bleak future for the environment, already

suffering from the serious impacts of global warming including rising sea-levels, rising sea temperatures, and increased extreme weather patterns ? to name just a few," said Andrew Kerr of WWF's international Climate Change Campaign. "The plan is laughable when you consider that some European nations already have cut their greenhouse gas emissions by several times more than the amount Japan proposes for emission reductions more than a decade from now."

According to the just released "WWF State of the Climate" report that evaluates the global impacts of climate change, a long list of impacts already are visible today including the destruction of several land and marine ecosystems in Asia and around the world because they cannot keep up with the pace of global warming.

The Japanese proposal also proves the government is back-tracking on a Ministerial Declaration concluded at the 1996 climate summit in Geneva. At that conference, 130 countries, including Japan, agreed that the Kyoto Summit should agree on "legally-binding objectives for emission limitations and significant overall reductions" of greenhouse gases. At the Geneva meeting, the Ministers recognised that climate change science showed human activities, primarily the burning of coal, oil and gasoline, are already affecting the planet's climate and the impacts would be wide-ranging and irreversible, posing threats to food supplies, public health and the survival of many species. Nations also agreed that "significant reductions in net greenhouse gas emissions are technically possible and economically feasible".

WWF is calling on industrial nations to cut their carbon dioxide emissions

20 percent below 1990 levels by 2005. A WWF report written by Dr. Haruki Tsuchiya of the Research Institute for Systems Technology, in Tokyo, (to be released by WWF later this month) shows that Japan can reduce its carbon dioxide emissions by nearly nine percent by 2005 and by almost 15 percent by 2010 without damaging the economy. Policies and measures suggested by the WWF report would stimulate the economy and help position Japan as a world leader in the development of new, energy efficient technologies.

"Environmentally, Japan's plan is worse than no plan whatsoever because it pretends to legitimise an emissions cut that is so low it will produce no tangible result in the effort to combat climate change," said Kerr. "Even more alarming, it encourages many nations also to cut their emissions by much less than they now plan. This proposal is an embarrassment for Japan because it spells disaster for the Kyoto Summit in December which will be seen as an absolute failure by several European nations and the entire environmental community if such meagre greenhouse gas emission cuts are adopted."

The complicated emission-reduction formulae that Japan proposes would require Japan to make only a 2.5 percent cut in emissions. The United States, responsible for over one-fifth of world releases of carbon dioxide, would only need to make a 2.6 percent reduction. Highlighting the political irrelevance of the Japanese formula, Germany, Denmark and the UK would have to make reductions of 3.1 percent, 2.5 percent and 3.7 percent respectively. But Germany already has achieved around half of its national target of cutting carbon dioxide emissions by 25 percent by 2005. Denmark is aiming for a 20 percent reduction by the same date and the UK's target

is a 20 percent cut by 2010.

Contact: Andrew Kerr or Yurika Ayukawa. Mobile tel: 010-760 5022 and

Hearton Hotel, 075-222 1300.

From: Joseph Alcamo <alcamo@usf.uni-kassel.de>
To: m.hulme@uea.ac.uk, Rob.Swart@rivm.nl
Subject: Timing, Distribution of the Statement
Date: Thu, 9 Oct 1997 18:52:33 0100
Reply-to: alcamo@usf.uni-kassel.de

Mike, Rob,

Sounds like you guys have been busy doing good things for the cause.

I would like to weigh in on two important questions --

Distribution for Endorsements --

I am very strongly in favor of as wide and rapid a distribution as possible for endorsements. I think the only thing that counts is numbers. The media is going to say "1000 scientists signed" or "1500 signed". No one is going to check if it is 600 with PhDs versus 2000 without. They will mention the prominent ones, but that is a different story.

Conclusion -- Forget the screening, forget asking them about their last publication (most will ignore you.) Get those names!

Timing -- I feel strongly that the week of 24 November is too late.

1. We wanted to announce the Statement in the period when there was a sag in related news, but in the week before Kyoto we should expect that we will have to crowd out many other articles about climate.
2. If the Statement comes out just a few days before Kyoto I am afraid that the delegates who we want to influence will not have any time to pay attention to it. We should give them a few weeks to hear about it.
3. If Greenpeace is having an event the week before, we should have it a week before them so that they and other NGOs can further spread the word about the Statement. On the other hand, it wouldn't be so bad to release the Statement in the same week, but on a different day. The media might enjoy hearing the message from two very different directions.

Conclusion -- I suggest the week of 10 November, or the week of 17 November at the latest.

Mike -- I have no organized email list that could begin to compete with the list you can get from the Dutch. But I am still willing to send you what I have, if you wish.

Best wishes,

Joe Alcamo

Prof. Dr. Joseph Alcamo, Director
Center for Environmental Systems Research
University of Kassel
Kurt Wolters Strasse 3
D-34109 Kassel
Germany

Phone: +49 561 804 3898

Fax: +49 561 804 3176

From: Ben Santer <bsanter@pcmdi.llnl.gov>

To: ritson@slac.stanford.edu, p.jones@uea.ac.uk, covey@cirrus.llnl.gov, tbarnett-ul@ucsd.edu, k.briffa@uea.ac.uk

Subject: (Fwd) Re: Your Holocene paper with Barnett et al 6.3 1996 page 255

Date: Tue, 14 Oct 1997 16:17:44 -0700

Dear Dr. Ritson,

Your email to Phil Jones suggests that there are serious discrepancies between the ECHAM1/LSG power spectrum that I computed for the 1995 Barnett et al. Holocene paper and the ECHAM1/LSG power spectrum that Curt Covey posted on the WWW. This is not the case. At the time that Tim Barnett, Phil Jones, Keith Briffa and I performed the research that is the subject of the Holocene paper, only 600 years of control run data were available from ECHAM1/LSG. This is stated on page 256 of the Holocene paper. The first ca. 200-250 years of this control integration incorporated a large, non-linear climate drift component. This was manifested both in globally-averaged temperature and in other climate variables (see Santer et al., 1995, JGR 100, 10,693-10,725).

Prior to computing the spectrum I removed the overall (i.e., 600-year) least-squares linear trend. There is still considerable low-frequency variance in the residuals, in part (but not wholly) due to the non-linearity of the drift component in the first few centuries. This residual drift explains some portion of the GFDL-versus-ECHAM1 power discrepancies at timescales of >100 years.

The CMIP project received data from MPI well after the completion of the research described in the Barnett et al. paper. At that time, I believe that 1,250 years of ECHAM1/LSG control run data were made available. My understanding is that Curt did not use the first (drift-contaminated) 250 years of the ECHAM1/LSG control run when he computed the ECHAM1 spectrum displayed on the CMIP WWW page. His analysis relied on the last 1,000 years of the data.

Not surprisingly, neglecting the first 250 years makes a big difference to the computed spectrum. This is particularly apparent at low frequencies, and also in the variance ratio (between periods of 300 and 2 years) that you compute.

I hope this clarifies things. Should you still have residual concerns about our method of spectral analysis (which is standard and follows Jenkins and Watts), I'd be happy to provide you with a copy of the program that was used to generate the spectra.

Sincerely,

Ben Santer

--- Forwarded mail from Phil Jones <p.jones@uea.ac.uk>

Date: Tue, 14 Oct 1997 10:42:29 +0100

To: ritson@slac.stanford.edu

From: Phil Jones <p.jones@uea.ac.uk>

Subject: Re: Your Holocene paper with Barnett et al 6.3 1996 page 255

Cc: bsanter@rainbow.llnl.gov

David,

I can only suggest you contact Ben Santer who did the analysis for Table 1. Ben is generally very busy - his email is bsanter@rainbow.llnl.gov .

Cheers

Phil

At 01:10 PM 10/13/97 -0700, you wrote:

> Two quick questions about your Fig 1, power spectrum of global mean
> averaged temperature.

>

>1) You don't provide units. I would have expected that

>

> $\langle DT^{**2} \rangle \text{Integral}(G(f).df)$

>

> would be the normalization with G(f) being the power spectrum and DT the
> RMS variance. Obviously this is not what you used. What are your units?

>

>2) I checked your ECHAM1 results for the ratio of the power spectrum at
> a period of 300 years to the value at 2 years against the posted CMIP
> LLNL power spectrum on the WWW. Aside from units the ratios of CMIP
> and yours appear to differ by a factor of the order of 6. As you are both
> using the same data base(?) and Curtis Covey of LLNL said he used Ben Santer's
> program for power spectra this discrepancy seems a little strange. Who is
> right

> or are you both right?

>

> I would check it myself in a matter of day(s) but getting model data bases
> is a bureaucratic nightmare.

>

> Dave

>

Dr Phil Jones

Climatic Research Unit Telephone +44 (0) 1603
592090 School of Environmental Sciences Fax +44 (0)
1603 507784 University of East Anglia
Norwich
Email p.jones@uea.ac.uk NR4 7TJ
UK

---End of forwarded mail from Phil Jones <p.jones@uea.ac.uk>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tom Wigley <>wigley@meeke.ucar.edu>
Subject: Re:
Date: Tue Nov 4 09:42:07 1997

Tom

please do. Actually I would be interested to know whether Malcolm mentioned these results to Dave as he was in Krasnoyarsk a few months ago when I showed this stuff. I will be over in New York in a few weeks to discuss with Ed the possibility of putting in an NSF/NERC proposal to look at the tree biomass change question. Also, the initial impetus to redo this stuff was as part of a NERC project we have running in collaboration with Ian Woodward - i which we are inputting high resolution climate data to Dolly to assess the roll of such variability on carbon uptake

cheers

Keith

At 02:54 PM 11/3/97 -0700, you wrote:

>Keith,

>

>Malcolm Hughes was here on Friday to see Dave Schimel about precisely the

>issue you raise. Dave wants to see if he can validate his ecosystem model

>using tree ring data. Sounds as if you already have the data to do this.

>Can I show your e-mail to Dave?

>

>Tom

>

>On Mon, 3 Nov 1997, Keith Briffa wrote:

>

>>

>> Tom

>> thanks for the info. Actually this is a chance for me to mention that

>> we have for the last few months at least, been reworking the idea of looking in the Schweingruber network data for evidence of increasing tree

>> growth and hence, potentially at least, evidence of changing tree (read biomass) uptake of carbon.

>> The results are dramatic - not to say earth shattering because they demonstrate major time-dependent changes - but changes that are consistent

>> in different areas of the network. We have regionalised over 350 site collections, each with ring width and density data, age-banded the data

>> so that we look only at relative growth in similar ages of trees through

>> time and recombined the standardised curves to produce growth changes in

>> each region. Basically growth is roughly constant (except for relatively

>> small climate variability forcing) from 1700 to about 1850. It then
>> increases linearly by about up until about 1950 after which time young
(up
>> to 50 year old) basal area explodes but older trees remain constant .
The
>> implication is a major increase in carbon uptake before the mid 20th
>> century - temperature no doubt partly to blame but much more likely to
be
>> nitrate/CO₂ . Equally important though is the levelling off of carbon
>> uptake in the later 20th century. This levelling is coincident with
the
>> start of a density decline - we have a paper coming out in Nature
>> documenting the decline . In relative terms (i.e. by comparison with
>> increasing summer temperatures) the decline is represented in the ring
>> width and basal area data as a levelling off in the long-timescale
increase
>> (which you only see when you process the data as we have). The
density
>> data do not show the increase over and above what you expect from
>> temperature forcing.
>> I have been agonising for months that these results are not some
>> statistical artifact of the analysis method but we can't see how. For
just
>> two species (spruce in the western U.S. Great Basin area and larch in
>> eastern Siberia) we can push the method far enough to get an
indication of
>> much longer term growth changes (from about 1400) and the results
confirm
>> a late 20th century apparent fertilization! The method requires
>> standardizing (localized mean subtraction and standard deviation
division)
>> by species/age band so we reconstruct relative (e.g. per cent change)
only .
>> We have experimented with integrating the different signals in basal
area
>> and density(after extracting intra ring ring width and density data
where
>> available) within a 'flat mass' measure which shows a general late
20th
>> century increase - but whether this incorporates a defensible
relative
>> waiting on the different components (and what the relative carbon
>> components are) is debatable. We now need to make some horrible
simplistic
>> assumptions about absolute carbon in these (relatively small)
components of
>> the total biomass carbon pool and implications for terrestrial and
total
>> carbon fluxes over the last few hundred years - and beyond! Without
these
>> implications we will have difficulty convincing Nature that this work
is
>> mega important.

>> There are problems with explaining and interpreting these data but they are
>> by far the best produced for assessing large scale carbon-cycle-relevant
>> vegetation changes - at least as regards well-dated continuous trends.
I
>> will send you a couple of Figures (a tiny sample of the literally hundreds
>> we have) which illustrate some of this. I would appreciate your reaction.
>> Obviously this stuff is very hush hush till I get a couple of papers
>> written up on this. We are looking at a moisture sensitive network of data at
>> the moment to see if any similar results are produced when
>> non-temperature-sensitive data are used. You would expect perhaps a greater
>> effect in such data if Co2 acts on the water use efficiency .
>> At 09:30 AM 11/3/97 -0700, you wrote:
>> >Dear Keith,
>> >
>> >Look at Tremblay et al. GRL 24, 2027-30 (1997) and Dyke et al. Arctic 50,
>> >1-16 (1997). These papers deal with driftwood in the Arctic over the past
>> >9000 years. They note that genera can be distinguished, but not species
>> >Hence, they can't say where the wood comes from, North America versus
>> >Europe. Surely cross-dating could do this? May be worth getting in touch
>> >with Dyke et al.
>> >
>> >Tom
>> >
>> --
>> Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
>> Norwich, NR4 7TJ, United Kingdom
>> Phone: +44-1603-592090 Fax: +44-1603-507784
>>

>
>
> *****
> *Tom M.L. Wigley *
> *Senior Scientist *
> *National Center for Atmospheric Research *
> *P.O. Box 3000 *
> *Boulder, CO 80307-3000 *
> *USA *
> *Phone: 303-497-2690 *
> *Fax: 303-497-2699 *
> *E-mail: wigley@ucar.edu *
> *****
>
>

From: richard.tol@ivm.vu.nl
To: "m.hulme" <m.hulme@uea.ac.uk>
Subject: re: positives and negatives
Date: Wed, 12 Nov 97 15:09:29 CET
Cc: "timothy.mitchell" <timothy.mitchell@christ-church.oxford.ac.uk>

>It would indeed be interesting to poll all of our invitees using a more
>sophisticated
>questionnaire, but this is not what we are about. For example, if you
>disagree
>with the Statement I would be interested to know the grounds of your
>disagreement.

Mike,

Thanks.

I am always worried about this sort of things. Even if you have 1000
signitures, and appear to have a strong backup, how many of those asked did
not sign?

Also, I happen to be of the opinion that the US proposal for Kyoto is too
ambitious. But of course I am thinking of real policies, not of
negotiation-rhetoric.

Finally, I think that the text conveys the message that it is a scientific
defense for the EU position. There is not any. Even DG11 finds a hard to
defend (at least, in the draft version of their attempt -- I don't think the
final version has appeared yet). Whatever you think about long-term goals,
2010 is pretty soon. At the moment, no country has any experience with
serious emission reduction POLICY. Minus 15% is serious, particularly because
of the effort that will be spend on the monetary union and because the UK and
Germany are too optimistic on their baseline emissions. Rash action instead
careful thinking may well run serious, international climate policy deep into
the ground.

Cheers

Richard

From: Richard Baker <r.baker@csl.gov.uk>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: Finalising PRAPROC! 21st November 1997
Date: Mon, 17 Nov 1997 16:59:56 -0800
Reply-to: r.baker@csl.gov.uk

Mike

> I hope you had my comments from a few weeks ago.

Yes, sorry I've taken so long to reply.

> 1. Overheads: we charge EU projects 20% overheads and these are totally
> acceptable

Yes, you are quite right.

> 2. Budget: I will need to redraft our budget. Please tell me estimated
> start data and for how long the project will run. I envisage our budget
> remaining in the bracket 60-70k ECU

I guess we are looking to April 1998 at the very earliest. I heard that some SMT projects take up to 2 years to get going even after they've been approved due to wrangles over the budget. We have 1 million ECU for 3 years....so some project budgets will have to be cut. Yours looks fine.

> 3. Workplan: I am assuming the basic climate tasks remain pretty much as
> before, namely:
>
> a) 10' gridded monthly climate data for Europe for 1961-90 linked to a weather
> generator that will yield daily data. Key variables: precip., tmin, tmax,
> vapour pressure, sunshine/radiation, wind, wet days, frost days.

Yes, that'll do nicely!

> b) for the world a 0.5deg gridded dataset for 1961-90 at monthly timesteps

Excellent!

> c) what was decided about very high resolution climate surfaces for 1-2

- > regions?
- > This was in the original proposal but got dropped I think. Adding this back
- > to our work plan would involve extra time and hence resources. How
- > important are
- > these test 1km (?) resolution datasets?

We've had a problem contacting the Spaniards which is a bit of a blow because they gave a nice geospatial feel to the project. The Norwegians are proposing to conduct a high resolution study near Oslo..I think they'll be interpolating locally collected data. I'll send you their proposal as soon as I can get it into a little better shape but, in principle, I think it would be best if you could, at this stage, just stick to the low resolution work.

- > 4. Other EU projects: I suggest you mention my involvement in CLIVARA
- > which is
- > funded through the Environment/CLimate programme of DGXII. This is running
- > from
- > 1996-1999 and is concerned with mapping and modelling agriculture across the
- > EU under 1961-90 conditions and also under future climate change.
- > Co-ordinated
- > by Environmental Change Unit at University of Oxford. let me know if you want
- > more info. on this.

A brief update to your "partner information" would be great.

- > Can you confirm for me which forms I need to get completed? Do you
- > have copies to send me or should I get them from here.

I'm putting some in the post for you.

- > I shall not be able to be with you in York on Friday, but I am here
- > all this week if there are questions.

many thanks..there are sure to be some.

All the very best

Richard

From: Tom Wigley <wigley@meeker.ucar.edu>
To: jan.goudriaan@staff.tpe.wau.nl, grassl_h@gateway.wmo.ch, Klaus Hasselmann <klaus.hasselmann@dkrz.de>, Jill Jaeger <jaeger@iiasa.ac.at>, rector@iss.nl, oriordan@enviro.uct.ac.za, uctpa84@ucl.ac.uk, john@pik-potsdam.de, mparry@geog.ucl.ac.uk, pier.vellinga@ivm.vu.nlam.de
Subject: Re: ATTENTION. Invitation to influence Kyoto.
Date: Tue, 25 Nov 1997 11:52:09 -0700 (MST)
Reply-to: Tom Wigley <wigley@meeker.ucar.edu>
Cc: Mike Hulme <m.hulme@uea.ac.uk>, t.mitchell@uea.ac.uk

Dear Eleven,

I was very disturbed by your recent letter, and your attempt to get others to endorse it. Not only do I disagree with the content of this letter, but I also believe that you have severely distorted the IPCC "view" when you say that "the latest IPCC assessment makes a convincing economic case for immediate control of emissions." In contrast to the one-sided opinion expressed in your letter, IPCC WGIII SAR and TP3 review the literature and the issues in a balanced way presenting arguments in support of both "immediate control" and the spectrum of more cost-effective options. It is not IPCC's role to make "convincing cases" for any particular policy option; nor does it. However, most IPCC readers would draw the conclusion that the balance of economic evidence favors the emissions trajectories given in the WRE paper. This is contrary to your statement.

This is a complex issue, and your misrepresentation of it does you a dis-service. To someone like me, who knows the science, it is apparent that you are presenting a personal view, not an informed, balanced scientific assessment. What is unfortunate is that this will not be apparent to the vast majority of scientists you have contacted. In issues like this, scientists have an added responsibility to keep their personal views separate from the science, and to make it clear to others when they diverge from the objectivity they (hopefully) adhere to in their scientific research. I think you have failed to do this.

Your approach of trying to gain scientific credibility for your personal views by asking people to endorse your letter is reprehensible. No scientist who wishes to maintain respect in the community should ever endorse any statement unless they have examined the issue fully themselves. You are asking people to prostitute themselves by doing just this! I fear that some will endorse your letter, in the mistaken belief that you are making a balanced and knowledgeable assessment of the science

-- when, in fact, you are presenting a flawed view that neither accords with IPCC nor with the bulk of the scientific and economic literature on the subject.

Let me remind you of the science. The issue you address is one of the

timing of emissions reductions below BAU. Note that this is not the same as the timing of action -- and note that your letter categorically addresses the former rather than the latter issue. Emissions reduction timing is epitomized by the differences between the Sxxx and WRExxx pathways towards CO2 concentration stabilization. It has been clearly demonstrated in the literature that the mitigation costs of following an Sxxx pathway are up to five times the cost of following an equivalent WRExxx pathway. It has also been shown that there is likely to be an equal or greater cost differential for non-Annex I countries, and that the economic burden in Annex I countries would fall disproportionately on poorer people.

Furthermore, since there has been no credible analysis of the benefits (averted impacts) side of the equation, it is impossible to assess fully the benefits differential between the Sxxx and WRExxx stabilization profiles. Indeed, uncertainties in predicting the regional details of future climate change that would arise from following these pathways, and the even greater uncertainties that attend any assessment of the impacts of such climate changes, preclude any credible assessment of the relative benefits. As shown in the WRE paper (Nature v. 379, pp. 240-243), the differentials at the global-mean level are so small, at most a few tenths of a degree Celsius and a few cm in sea level rise and declining to minuscule amounts as the pathways approach the SAME target, that it is unlikely that an analysis of future climate data could even distinguish between the pathways. Certainly, given the much larger noise at the regional level, and noting that even the absolute changes in many variables at the regional level remain within the noise out to 2030 or later, the two pathways would certainly be indistinguishable at the regional level until well into the 21st century.

The crux of this issue is developing policies for controlling greenhouse gas emissions where the reductions relative to BAU are neither too much, too soon (which could cause serious economic hardship to those who are most vulnerable, poor people and poor countries) nor too little, too late (which could lead to future impacts that would be bad for future generations of the same groups). Our ability to quantify the economic consequences of "too much, too soon" is far better than our ability to quantify the impacts that might arise from "too little, too late" -- to the extent that we cannot even define what this means! You appear to be putting too much weight on the highly uncertain impacts side of the equation. Worse than this, you have not even explained what the issues are. In my judgment, you are behaving in an irresponsible way that does you little credit. Furthermore, you have compounded your sin by actually putting a lie into the mouths of innocents ("after carefully examining the question of timing of emissions reductions, we find the arguments against postponement to be more compelling"). People who endorse your letter will NOT have "carefully examined" the issue.

When scientists color the science with their own PERSONAL views or make categorical statements without presenting the evidence for such statements, they have a clear responsibility to state that that is what

they are doing. You have failed to do so. Indeed, what you are doing is, in my view, a form of dishonesty more subtle but no less egregious than the statements made by the greenhouse skeptics, Michaels, Singer et al. I find this extremely disturbing.

Tom Wigley

On Tue, 11 Nov 1997, Tim Mitchell wrote:

> Reference: Statement of European Climate Scientists on Actions to
> Protect
> Global Climate
>
> Dear Colleague,
>
> Attached at the end of this email is a Statement, the purpose of which
> is
> to bolster or increase governmental and public support for controls of
> emissions of greenhouse gases in European and other industrialised
> countries in the negotiations during the Kyoto Climate Conference in
> December 1997. The Statement was drafted by a number of prominent
> European
> scientists concerned with the climate issue, 11 of whom are listed
> after
> the Statement and who are acting as formal sponsors of the Statement.
>
> ***** The 11 formal sponsors are: *****
>
> Jan Goudriaan Hartmut Grassl Klaus Hasselmann Jill Jäger
> Hans Opschoor Tim O'Riordan Martin Parry
> David Pearce
> Hans-Joachim Schellnhuber Wolfgang Seiler Pier Vellinga
>
> After endorsements from many hundreds of other European climate-related
> scientists are collected (and we hope that you agree to be one of
> these), the
> Statement will be brought to the attention of key decision-makers (e.g.
> EU
> Kyoto negotiators and Environment Ministers) and other opinion-makers
> in
> Europe (e.g. editorial boards of newspapers) during the week beginning
> 24th
> November. The UK and other European WWF offices have agreed to assist
> in
> this activity, although the preparation of the Statement itself has in
> no
> way been initiated or influenced by WWF or any other body. This is an
> initiative taken by us alone and supported by our 11 Statement
> sponsors.
>
> WHAT WE ASK FROM YOU

>
> We would very much like you to endorse this Statement. Unfortunately,
> at
> this time we can no longer take into account any suggested
> modifications.
> Nevertheless, we hope that it reflects your views closely enough so
> that
> you can support it. If you agree with the Statement, then:
>
> 1. PLEASE IMMEDIATELY FILL OUT the form below and either reply via
> email
> (preferably) or telefax (only if necessary) to the indicated fax
> number.
> Replies received after Wednesday 19th November will not be included.
> If
> replying by email please do not use the 'reply all' option. If this
> invitation has been forwarded from a colleague, please make sure your
> reply
> is directed to the originators of this invitation, namely:
> t.mitchell@uea.ac.uk (on behalf of Mike Hulme and Joe Alcamo).
>
> 2. We have identified about 700 climate-related scientists in Europe
> who
> are receiving this email directly from us. If you feel it is
> appropriate,
> PLEASE FORWARD THIS MESSAGE to up to three colleagues in your country
> who
> are working in climate-related fields, who you think may support the
> Statement and whom we have not targeted. To identify colleagues whom
> we
> have already invited you can examine the email address list we have
> used
> for your country in the email header (or else appended to the end of
> this
> email).
>
> We realize that you are very busy, but this action may have a very
> positive
> influence on public discussions during the critical period leading up
> to
> Kyoto and during the Conference itself.
>
> With best wishes,
>
> Michael Hulme, Climatic Research Unit, UEA, Norwich
> Joseph Alcamo, University of Kassel, Germany
>
> (On behalf of the other signatories of the Statement)
>
>
>

>

> I agree to have my name placed on the list of scientists that endorse
the
> Statement of European Climate Scientists on Actions to Protect Global
> Climate.
>
> Full Title and Name
>
> Affiliation Country
>
> Signature (for fax replies only)
>
> Date
>
> Other comments:
>
>

>
> We would prefer you to return this email message to us by email, having
> duly completed the form above. You should be sending the form to:
>
> *****
> ** t.mitchell@uea.ac.uk **
> ** **
> *****
>
> If you would rather not use the email reply function, then please print
out
> the form above and fax it (filled in) to:
>
> "Attention: European Climate Statement"
> Climatic Research Unit, University of East Anglia
> Telefax: +44 1603 507784
>
>

>
>
> Statement of European Climate Scientists on Actions to Protect Global
Climate
>
=====

>
> In 1992, the nations of the world took a significant step to protect
global
> climate by signing the Framework Convention on Climate Change. This
year,
> at the coming Climate Summit in Kyoto*, they have the chance to take
> another important step. It is our belief that the nations of the world

> should agree to substantive action for controlling the growth of
greenhouse
> gas emissions.
>
> Our opinion is bolstered by the latest assessment of scientific
knowledge
> carried out by the Inter-governmental Panel on Climate Change (IPCC).
The
> IPCC reported that "the balance of evidence suggests a discernible
human
> influence on global climate". They also gave examples of observed
climate
> change up to now, including:
>
> ¶ Global mean surface air temperature has increased by between 0.3 to
0.6
> degrees Celsius since the late 19th century, and recent years have been
the
> warmest since 1860.
> ¶ Global sea level has risen between 10 and 25 centimeters over the
past
> 100 years.
>
> Based on estimates from computer models, the IPCC also maintained that
> humanity will have a continuing and cumulative effect on climate in the
> future. Future society may find that some climate impacts are positive,
as
> in the possible increase in rainfall and crop yield in some dry
regions;
> and society may be able to adapt to some impacts, such as by building
dikes
> against rising sea level. But many, if not most, climate impacts will
> increase risks to society and nature, and will be irreversible on the
human
> time scale. Among the possible changes are further increases in sea
level,
> the transformation of forest and other ecosystems, modifications of
crop
> yield, and shifts in the geographic range of pests and pathogens. It is
> also possible that infrequent but disastrous events, such as droughts
and
> floods, could occur more often in some regions. At particular risk are
> people living on arid or semi-arid land, in low-lying coastal areas and
> islands, in water-limited or flood-prone regions, or in mountainous
> regions. The risk to nature will be significant in the many areas where
> ecosystems cannot quickly adapt to changing climate, or where they are
> already under stress from environmental pollution or other factors.
>
> Because of these risks, we consider it important for nations to set
limits
> on the increase of global temperature due to human interference with
the
> climate system. We recommend that European and other industrialized
nations

> use such long-term climate protection goals as a guide to determining
> short-term emission targets. This approach has been adopted, for
example,
> by the European Union and the Alliance of Small Island States.
>
> Some may say that action to control emissions should be postponed
because
> of the scientific uncertainties of climate change and its impact. Our
view
> is that the risks and irreversibility of many climate impacts require
> "precautionary measures to anticipate, prevent, or minimize the causes
of
> climate change", as stated in the Framework Convention on Climate
Change.
>
> We also acknowledge that economic arguments have been put forward for
> postponing the control of emissions in Europe and elsewhere. However,
after
> carefully examining the question of timing of emission reductions, we
find
> the arguments against postponement to be more compelling. First,
postponing
> action could shift an unfair burden for more severe reductions of
emissions
> onto future generations. Second, it will lead to a greater accumulation
of
> greenhouse gases in the atmosphere and hence make it more difficult to
> prevent future climate change when action is finally taken. Third, the
> latest IPCC assessment makes a convincing economic case for immediate
> control of emissions.
>
> Rather than delay, we strongly urge governments in Europe and other
> industrialized countries to agree to control greenhouse emissions as
part
> of a Kyoto agreement. Some controls can be achieved by reducing fossil
fuel
> use at little or no net cost through accelerated improvements in the
> efficiency of energy systems, the faster introduction of renewable
energy
> sources, and the reduction of subsidies for fossil fuel use. Moreover,
> reducing the use of fossil fuels will also reduce local and regional
air
> pollution, and their related impacts on human health and ecosystems.
>
> We believe that the European Union (EU) proposal is consistent with
long
> term climate protection. This proposal would reduce key greenhouse gas
> emissions by 15% from industrialized countries (so-called Annex I
> countries) by the year 2010 (relative to year 1990). Although stronger
> emission reductions will be needed in the future, we see the EU, or
> similar, goal as a positive first step "to prevent dangerous
anthropogenic
> interference with the climate system" and to lessen risks to society
and

> nature. Such substantive action is needed now.
>
> *Third Conference of the Parties to the Framework Convention on Climate
> Change, Kyoto, Japan, December, 1997.
>
> Signed:
>
> Jan Goudriaan Hartmut Grassl Klaus Hasselmann
> Jill Jøger Hans Opschoor Tim O'Riordan
> Martin Parry David Pearce Hans-Joachim
Schellnhuber
> Wolfgang Seiler Pier Vellinga
>

>
>
>

> ** This message originated from the
> ** Climatic Research Unit, University of East Anglia, Norwich, UK.
> ** It was sent out by
> ** Mike Hulme and Tim Mitchell on behalf of the 11 key signatories.
> ** If you object to being on this email address list,
> ** please accept our apologies and inform us;
> ** we will then remove your address from the list.
> ** Please direct any comments to:
> ** t.mitchell@uea.ac.uk
>

>
> The list below consists of the people with UK email addresses to whom
this
> message has been sent:
>
> all CRU staff
> Adger, N
> Alcock, Graeme
> Allan, P
> Allan, Richard P
> Anderson, Dennis
> Armstrong, Adrian
> Arnell, N W
> Audsley, Eric
> Baker, Richard
> Baran, A J
> Barker, Terry
> Benestad, R E
> Bentham, G
> Bigg, G
> Boucher, Keith R
> Bouma, D
> Bramwell, Penny
> Brooks, Roger

> Brown, Philip RA
> Brugge, Roger
> Bullock, P
> Burkhardt, Ulrike
> Butterfield, Ruth
> Cai, Xiaoming
> Cannell, Melvyn
> Carling, Bob
> Castleford, John
> Chan, Angela H Y
> Clark, Douglas B
> Cluckie, I D
> Collins, Matthew
> Colman, Andrew
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> Cornford, Dan
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> Cox, Peter M
> Cui, Zhiqiang
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> Warrilow, David
> Washington, Richard
> Webb, Mark
> Wheeler, Tim
> Wigley, Tom
> Wilby, Rob
> Wild, Richard
> Williamson, P
> Woodward, Stephanie
> Wright, Peter
> Wynne, Brian
> Yamin, Farhana
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>

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*****  
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*P.O. Box 3000 *  
*Boulder, CO 80307-3000 *  
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*****
```

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Stepan, Eugene
Subject: papers/Holocene/etc.
Date: Fri Dec 5 16:12:59 1997
Cc: fritz.schweingruber@wsl.ch

Dear Stepan and Eugene

I don't know whether you have received your copies of the 1996 issue of *Dendrochronologia* yet but in case not I have seen the issue and it looks very good. Your two papers on Yamal and Taimyr are there and they both look excellent. Stepan I received receipt for money and the data and photographs your sent . I am very grateful for all . Thankyou. Again I can only say sorry about the problems of money transfer.

The first thing I wish to say is that I know we have been unsuccessful with our recent applications to INTAS and COPERNICUS . However , if you agree , I would like to resubmit a new proposal to INTAS in March to continue the development of the long chronologies. I will write it and stress the success todate and the need to carry on the formal collaboration. What is your joint opinion on this?

The Nature paper on the decline story is now officially accepted and I still hope it may come out before Christmas or at least shortly afterwards. I will be writing a story about increasing basal area on the long term as I showed in Krasnoyarsk and I also intend to submit this to Science or Nature and you will be coauthors on that. We also have done a lot of work on the growing season degree day reconstructions and will write up another joint paper on this soon - but I am trying to get the ringwidth data produced by you two incorporated with the ringwidth data produced from the density measurements - because Stepan told me these may be longer and anyway they will help the quality of the ringwidth data anyway. You may therefore get some messages or questions from Harry (Ian Harris) who works for me asking about the locations. Please be patient and try to help him with this if necessary.

Unfortunately, next year I have several major meetings to attend and present our joint results. Each of these meetings is very important. In March, I must give a major review paper at the PAGES open Science meeting in London. This must cover all dendro - or at least the best of it - which of course includes our own work! Early next year I will ask for the full data sets as they then stand, for Yamal and Taimyr so that I can try restandardising and calibrating against regional mean climate data. If there are not likely to be more data than I already have , can you let me know. Also in March, I will go to Copenhagen for an European Community meeting of project leaders of projects dealing with Arctic climates. This is the sort of meeting I must attend and put on a good show if we hope to get further funding in 1999 onwards. Later in the year there is a big climate conference here at which I must give a review of dendroclimatic research.

By January , we are supposed to exchange data within the project for possible research - but with the proviso that nothing can be written about work using others data without full collaboration and coauthorship. Are you both willing to let your chronologies as published be released to the rest of the group at that time?

Finally, I have got permission (provided I can find the money to pay for it) to have a special issue of *The Holocene* dedicated to the results (todate) of the ADVANCE-10K project. It will contain a series of

major articles describing each piece of the work and I wish these to include large ,detailed papers on the Yamal and Taimyr chronologies , and perhaps a separate paper on the Northern Urals work. I hope to get a firm committment now from Both of you that you will be prepared to do this. I would be happy to help with specific ideas and some analysis and plotting of all Figures and retyping if you wish. The provisional deadline for the production of the papers would be late summer or autumn at the earliest.

I am of course very keen to continue our collaboration and next year as soon as I know more about the details of the European Community Framework 5 plan (which , incidently now contains a heading 'Global Change') I will be putting together another application. I will try my best to include you both as full partners in this if it is at all possible.

After the Krasnoyarsk meeting I heard nothing about the final decision regarding an application for a Transect Office in Krasnoyarsk (at some time someone had asked me would I coauthor an application) . Has this idea died? Also will there be a proceedings book arising out of the meeting ? Do I have to prepare something?

Eugene, I have a revised version of the paper you gave me to read some time ago about the cell growth model work. Do you intend me to send this to Dendrochronologia or just send the annotated manuscript back to you? I have a question about meaning that held me up and needs your answer - can I fax you something?

Finally , - I wish you each and everyone in your laboratories and all your families the very best christmas and new year .

Keith

From: Nebojsa Nakicenovic <naki@iiasa.ac.at>
To: "Joseph M. Alcamo" <alcamo@usf.uni-kassel.de>, "Knut H. Alfsen" <knut.alfsen@cicero.uio.no>, Dennis Anderson <dennis.anderson@ic.ac.uk>, Zhou Dadi <becon@public3.bta.net.cn>, "Gerald R. Davis" <Ged.R.Davis@si.simis.com>, Benjamin Dessus <benjamin.dessus@cnrs-dir.fr>, Jae Edmonds <ja_edmonds@pnl.gov>, (although he cancelled) Joergen Fenhann <j.fenhann@risoe.dk>, "Stuart R. Gaffin" <stuart@edf.org>, Henryk Gaj <Fewewar@tarnet.pl>, Ken Gregory <kennethgregory@msn.com>, "A. Gruebler" <gruebler@iiasa.ac.at>, Erik Haites <EHaites@netcom.ca>, William Hare <bhare@ams.greenpeace.org>, Michael Hulme <m.hulme@uea.ac.uk>, Michael Jefferson <jefferson@wec.co.uk>, Tae-Yong Jung <tyjung@his.keei.re.kr>, Tom Kram <kram@ecm.nl>, Emilio Lebre La Rovere <emilio@ppe.ufrj.br>, Mathew Luhanga <vc@udsmucc.gn.apc.org>, Douglas McKay <Doug.D.Mckay@si.simis.com>, Julio Torres Martinez <dpid@[169.158.128.138]>, Laurie Michaelis <laurie.michaelis@oecd.org>, Shunsuke Mori <mori@shun-sea.ia.noda.sut.ac.jp>, Tsuneyuki Morita <tmorita@nies.go.jp>, Richard Moss <rmoss@usgcrp.gov>, "Youssef H. Nassef" <nassef@hotmail.com>, William Pepper <wpepper@icfkaiser.com>, "Hugh M. Pitcher" <hm_pitcher@pnl.gov>, Lynn Price <lkprice@lbl.gov>, Hans-Holger Rogner <h.h.rogner@iaea.org>, Cynthia Rosenzweig <crosenzweig@giss.nasa.gov>, "Jim F. Skea" <J.F.Skea@sussex.ac.uk>, Priyadarshi Shukla <shukla@iimahd.ernet.in>, Leena Srivastava <leena@teri.ernet.in>, Rob Swart <rob.swart@rivm.nl>, "H.J.M. de Vries" <Bert.de.Vries@rivm.nl>, "John P. Weyant" <weyant@leland.stanford.edu>, Ernst Worrell <e.worrell@nwsmail.chem.ruu.nl>
Subject: Invitation to the SRES meeting in Berkeley
Date: Tue, 13 Jan 1998 17:50:47 +0100

<x-rich>Dear Colleagues,

I would like to confirm that we will hold the next SRES meeting on 7-8 February

at Lawrence Berkeley National Laboratory in Berkeley, California. Lynn Price is the organizer of the meeting. Below is her contact information.

Ms. Lynn Price

Energy Analysis Program

Lawrence Berkeley National Laboratory

MS 90-4000, 1 Cyclotron Road

Berkeley, CA 94720

U.S.A.

(001-510) 486-6519

(001-510) 486-6996

e-mail: lkprice@lbl.gov

The main purpose of the meeting is to review the work progress of the four

modeling groups that have been involved in first quantifications of the four storylines. My expectation is that we can harmonize various model runs into four initial scenarios. Thus, this will be primarily a modelers'

meeting focusing on technical issues, storyline interpretation and consistency of first quantifications. It will not have the character of a

Lead Authors meeting in the strict sense. It is nevertheless an important

meeting for all modeling groups who have volunteered to quantify storylines, since this work needs to proceed in order for us to meet our original timetable and cannot be postponed until the next Lead Authors' meeting in the spring.

I hope that most of you can attend. Your input would be valuable in this early stage of modeling work. Furthermore, it would be good to also take the opportunity of this meeting to review the so-called zero-order-drafts (ZODs). The deadline for the submission of the final versions of the ZODs is 15 January (Thursday), so I expect that we will also have new material to discuss.

Although I realize that this meeting will take place on rather short notice and not all of you will be able to obtain the necessary approvals and visas to attend, I nonetheless believe that it is important at this stage to hold an informal meeting with the four modeling groups. I have funds available for the four lead authors from developing countries:

Matthew Luhanga, Zhou Dadi, Henryk Gaj, and Emilio La Rovere. As noted above, a more formal meeting of the complete writing team will be held sometime in March or April, at which time I hope everyone will be able to attend.

Please confirm your attendance for the February meeting with me as soon as possible (this week if you can), so that we can reserve sufficient hotel space in Berkeley.

Again, for those of you who are working on Zero Order Drafts, please remember that this Thursday is the deadline for completion. I look forward to receiving these.

Best Regards,

Naki

<center>Katalin Kuszko

Environmentally Compatible Energy Strategies

International Institute for | Email: kuszko@iiasa.ac.at

Applied Systems Analysis | Phone: +43 2236 807 319

A-2361 Laxenburg, Austria | Fax: +43 2236 71313</center>
</x-rich>

From: P R Shukla <shukla@iimahd.iimahd.ernet.in>
To: Nebojsa Nakicenovic <naki@iiasa.ac.at>
Subject: Re: Invitation to the SRES meeting in Berkeley
Date: Wed, 14 Jan 1998 09:10:12 -0800
Reply-to: shukla@iimahd.iimahd.ernet.in
Cc: "Joseph M. Alcamo" <alcamo@usf.uni-kassel.de>, "Knut H. Alfsen" <knut.alfsen@cicero.uio.no>, Dennis Anderson <dennis.anderson@ic.ac.uk>, Zhou Dadi <becon@public3.bta.net.cn>, "Gerald R. Davis" <Ged.R.Davis@si.simis.com>, Benjamin Dessus <benjamin.dessus@cnsr-dir.fr>, Jae Edmonds <ja_edmonds@pnl.gov>, "(although he cancelled) Joergen Fenhann" <j.fenhann@risoe.dk>, "Stuart R. Gaffin" <stuart@edf.org>, Henryk Gaj <Fewewar@tarnet.pl>, Ken Gregory <kennethgregory@msn.com>, "A. Gruebler" <gruebler@iiasa.ac.at>, Erik Haites <EHaites@netcom.ca>, William Hare <bhare@ams.greenpeace.org>, Michael Hulme <m.hulme@uea.ac.uk>, Michael Jefferson <jefferson@wec.co.uk>, Tae-Yong Jung <tyjung@his.keei.re.kr>, Tom Kram <kram@ecm.nl>, Emilio Lebre La Rovere <emilio@ppe.ufrj.br>, Mathew Luhanga <vc@udsmucc.gn.apc.org>, Douglas McKay <Doug.D.Mckay@si.simis.com>, Julio Torres Martinez <dpid@[169.158.128.138]>, Laurie Michaelis <laurie.michaelis@oecd.org>, Shunsuke Mori <mori@shun-sea.ia.noda.sut.ac.jp>, Tsuneyuki Morita <t-morita@nies.go.jp>, Richard Moss <rmoss@usgcrp.gov>, "Youssef H. Nassef" <nassef@hotmail.com>, William Pepper <wpepper@icfkaiser.com>, "Hugh M. Pitcher" <hm_pitcher@pnl.gov>, Lynn Price <lkprice@lbl.gov>, Hans-Holger Rogner <h.h.rogner@iaea.org>, Cynthia Rosenzweig <crosenzweig@giss.nasa.gov>, "Jim F. Skea" <J.F.Skea@sussex.ac.uk>, Priyadarshi Shukla <shukla@iimahd.iimahd.ernet.in>, Leena Srivastava <leena@teri.ernet.in>, Rob Swart <rob.swart@rivm.nl>, "H.J.M. de Vries" <Bert.de.Vries@rivm.nl>, "John P. Weyant" <weyant@leland.stanford.edu>, Ernst Worrell <e.worrell@nwsmail.chem.ruu.nl>

Naki,

Thanks for the invitation to the SRES meeting.

Given the funds situation at your disposal, I am opting out of attending the meeting. I would however like to offer any assistance on issues concerning developing / Asian countries. Specifically, I have data on structural changes of GDP and energy for countries in Asia-Pacific. The structural transitions in these countries offer interesting insights and directions for scenarios. I have passed an analysis of 12 countries to Tae. The countries include the important economies in Asia-Pacific, namely China, India, Japan, Korea, Indonesia, Malaysia, Thailand, Pakistan, Bangladesh etc. I think the structural changes in developing countries is a very vital aspect for specifying future emissions. Also, well documented and specified information on this shall help the policy exercises later which shall use our emissions scenarios as reference.

I think the modelling groups may also require some inputs (and insights) for handling developing country specifications in the models. In the past we have pointed out several lacunas - such as neglect of traditional biomass, disequilibrium, informal economy, geopolitical realities etc. These also influence technological assumptions and constraints. In fact our scenarios are very well suited to handle some

of these aspects differently. The modellers may have to be advised to handle these aspects suitably. This is vital since we aim to specify the emissions regionally.

An another issue I wish to bring to your attention relates to discount rates. I know your competence on this issue. However, the modelling difficulties (and paradigm itself) often stop us from using different discount rates. The persistence of high discount rates in developing economies is an observed fact. This may not equalize globally during the next half century (or more). Even if we may not want to have different discount rates (since this upsets the underlying neoclassical paradigm), we may just ask the modellers to ensure that the results are not sensitive to this.

A more interesting issue concerning the discount rates for our scenarios is that the different futures (scenarios) would have different associated discount rates. The sustainable development type scenarios (e.g. B1 scenario) may have lower discount rate than our A scenarios. If we run all scenarios with same discount rate, this would be a contradiction. I know there are no easy answers around this since we do not want to confuse the users of scenarios later on with too many different parameters. However it may be worth providing different specifications for important parameters or caveats where we anticipate contradictions.

Given the recent developments in East Asia, it may be worth to take a relook at A1 scenario and consider whether the Tiger World would transit to A1 or A2. This is just an aside.

Wishing you a very happy new year.

P.R. Shukla

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: frank.oldfield@pages.unibe.ch
Subject: Re: Poster competition
Date: Fri Jan 16 10:26:08 1998

Frank

I do not recall what Kyrdianov has worked on - sorry. However, Hantemirov has done outstanding work putting together and as yet preliminarily analysing what wii no doubt become a world famous sub fossil chronology in the Yamal area of northern Siberia. Indeed I will feature this work in my presentation.

Frank, an important point requiring your instant help! Some time ago I got a request to write something for a NERC(?) publication related to my talk in April. Now I can't find it and desperately need to contact the guy about length and deadline - which may have passed. Can you help? I know you coordinated with him.

Yes I know I'm a _anker!

Keith

At 10:12 AM 1/16/98 +0100, you wrote:

>Dear Keith,

>

>I'm trying to draw up a short list for the 5 young scientists who will
>receive financial support from UCL. I need to balance them for theme and
>region and it seems that one of them should probably be a former USSR
>dendro-person. I've consulted Gene who points to Hantemirov and
>Kyrdianov as the two most worthy. Do you have any advice? Both abstracts
>look good and Gene thinks highly of each piece of work. seems better to
>get a second opinion from the dendro-world than to leave it open or try
>to resolve the question from a non-specialist perspective.

>

>I look forward to hearing from you,

>

>Cheers,

>

>Frank

>

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>

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>

From: GERNER THOMSEN <gerner@get2net.dk>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Ph.D. in Sweden
Date: Mon, 19 Jan 1998 06:15:55 +0100
Reply-to: gerner <gerner@get2net.dk>

Dear Keith!

I contacted Hakan Grudd last week. He is also positive about a Ph.D. for me in Stockholm.

I have tried to make a formulation of a project. Please, read it and let me know what you think. Maybe the project is overlapping with that of Grudd or maybe you have better ideas. It could also be that I have misunderstood some points.

I have sent the project formulation to Schweingruber, Grudd and Kalen. I send it to Schweingruber because I already contacted him last week (before I got the message from you). He is also interested in the project and anyway he will get involved if I am going to train in Birmensdorf.

Best regards from:

Gerner Thomsen

Description of project

1. Background

Dendroclimatology can be defined as the use of tree rings to study and reconstruct past and present climate (Kaennel & Schweingruber, 1995). Global average surface temperatures have risen by 0.3-0.6 °C since the middle of the 19th century (Folland et al., 1990). Climatologists seek to establish the extent to which this rise may be attributable to an enhanced greenhouse effect and so need to distinguish anthropogenic from 'natural' climate fluctuations (those that would occur without anthropogenic influences) to help them make predictions of future climate changes (Briffa et al., 1996a). Clearly the century-long instrumental record is not long enough to accomplish this. Paleoclimatic fluctuations older than meteorological measurements can be inferred from a variety of data sources, including tree rings, records of vegetation processes (e.g. pollen in lake

sediments), records of ice layer in ice cores, historical records, etc. (Eddy, 1992). However, within a time frame of the last two millennia dendroclimatology has shown to be the most powerful tool available to provide globally distributed, annually resolved paleoenvironmental records (Luckman, 1996). The growing influence of dendroclimatology in paleoenvironmental studies can be seen in the fact that almost a third of Bradley and Jones' volume *Climate since AD 1500* (Bradley & Jones, 1992) deals with dendrochronology and dendroclimatic reconstruction.

Near the polar and altitudinal tree lines, tree growth is mainly dependent on summer temperature. As northern latitudes are regarded as being strongly affected by global climate changes, a network of chronologies is established along the polar tree-line in Eurasia (Briffa et al., 1996b). At specific locations in these northern high-latitude regions it is possible to extend the tree-growth record back beyond the life span of living trees by amalgamating the measurements from overlapping, absolutely-dated series of measurements made on dead wood from historical or archeological provenances or naturally surviving above ground, in peat or alluvial sediments, or preserved in lakes. The first pair of (ring-width and density) chronologies, made up from samples of Scots pine (*Pinus sylvestris* L.) at several locations adjacent to Lake Torneträsk, northern Sweden, have been used to reconstruct summer (April-August) temperatures representing a large region of northern Fennoscandia from AD 500 to 1980 (Briffa et al., 1990, 1992). The Fennoscandian temperature records show that marked high-frequency (interannual-to-century) timescale variability together with marked long-timescale (multicentury) variations in summer temperatures have been a characteristic feature in this region during the last millennium. Similar data from samples of larch (*Larix sibirica*) on the eastern slopes of the northern Urals have been used to reconstruct regional summer (May-September) temperatures representing a region of north-western Siberia for the period 914 to 1990 (Briffa et al., 1995b). As a part of developing the north Eurasian chronology network, two projects currently underway aim to build continuous multimillennial pine ring-width chronologies in northern Sweden and Finland, spanning 7000-8000 years (Briffa et al., 1995a). In Russia a similar project underway aim to build larch ring-width chronologies in Yamal Peninsula, also spanning 7000-8000 years (Shiyatov, 1997).

The application of radiodensitometry in the analysis of conifer rings throughout Europe (Schweingruber, 1985) show the considerable amount of additional information lying in density, as compared with total ring width. Obviously, external factors have a more uniform influence on cell wall growth in latewood (density) than on cambial activity (ring-widths). In trees of the northern and subalpine timberlines, maximum latewood density is essentially a measure of mean summer temperature (ibid.).

2. Purpose of this study

2.1. Main objective

The main objective of this study is to provide additional information for a more precise climate reconstruction based on the already existing Torneträsk-chronology in northern Sweden (AD 500 to 1980) and a future supra-long chronology (BC 7000 to 1996), based on ring-widths and maximum latewood density of Scots pine (*Pinus sylvestris* L.) from the same area.

2.2. Elaboration of the main objective

One of the most fundamental underlying principles in dendroclimatology is the assumption of uniformitarianism in the response of data to climate forcing. The uniformitarian principle implies that "the physical and biological processes which link today's climate with today's variations in tree growth must have been in operation in the past" (Fritts, 1976). However, it is a moot point whether the assumption of uniformitarianism holds when past climate variations are inferred from long chronologies. The problem arises because the extrapolation always is based on a regression model calibrated on very short meteorological records. Long chronologies, as those seen in northern Scandinavia and Siberia, are made up from trees of different ages growing under more or less uniform conditions. In such chronologies there must always be uncertainty regarding the long-term stability of (non-climate) environmental influences or differing climate sensitivity due to the inhomogeneity in the sampled material (Briffa, 1995a, Briffa et al., 1996a). The climate signals in chronologies may, to some extent, be affected by:

1.
Inhomogeneity in the site characteristics of the samples (soil fertility, water holding capacity of the soil, altitude, exposure of slope, etc.)
2.
Inhomogeneity in series length of samples (tree age)
3.
Inhomogeneity in tree growth form and population density of samples
4.
Anthropogenic influence (nitrogen deposition, raise in CO₂ level) producing enhanced tree growth in the recent part of the chronology

5. Series replication in the chronology

6. The technique used to remove the non-climatic, age-related bias in individual series (a technique known as standardization in dendroclimatology)

This study will focus on the influence of point 1-3 on the climate signal seen in densities of Scots pine from the area of Torneträsk in northern Sweden. It is well-known that the Torneträsk-chronology is subject to the inhomogeneity in samples described in point 1-3, but it is not clear to what extension these inhomogeneities affect the climate signal in the chronology. Thus, a study of the influence of inhomogeneity in the samples will provide valuable additional information for a more precise interpretation of the summer-temperature record inferred from the already existing Torneträsk-chronology. In the same way it will highly increase the value and confidence of climate reconstructions from future supra-long pine-chronologies in this region. The growth parameter under investigation is maximum latewood density. In this way the study will complement an ongoing similar study on ring-widths of Scots pine from the same region (Grudd, 1998).

2.3. Partial objectives of the study and publications

Methodologically, the project can be divided into three, but overlapping stages:

1. Building of density pine-chronologies around Torneträsk from different sites. Various site conditions (mainly soil fertility, water holding capacity of the soil, altitude, and tree population density) and different age classes must be taken into consideration. No less than 10-12 chronologies must be estimated.

2. Analysis of climate-growth relationships of the pine-chronologies, focusing on differences between high-frequency and low-frequency variability in the climate data. The results are compared and conclusions are drawn about the diversity of climate signal seen in density-chronologies from Scots pine growing under various conditions in the area around Torneträsk.

3.

Re-interpretation of the already existing Torneträsk-chronology on the basis of the new information provided by the study in case and the ongoing similar study of ring-widths from the same region (Grudd, 1998)

The results are published in three articles with the following provisional titles:

a)

"Site-induced differences in climate-growth response of *Pinus sylvestris* L." (The article focuses on differences in climate-growth response for trees growing on different soil types and for trees from stands with different population density)

b)

"Altitude and age as parameters of climate-growth response in *Pinus sylvestris* L." (The article focuses on differences in climate-growth response for trees growing at different altitudes and trees in different age-classes)

c)

"Possible site-induced changes in the climate-growth response of the 1,400 year tree-ring chronology from northern Fennoscandia" (A re-interpretation of the existing Torneträsk-chronology is made on the basis of the new information)

3. Methods

3.1. Sampling strategy

3.1.1. Selection of sites and stands

As already pointed out, various site conditions and different age classes must be taken into consideration. Site homogeneity largely determines the quality of the chronology. That is, the factor under investigation which is assumed to affect the climate-growth response must be constant all over the site, and other possible affecting factors are minimised. It is important that the stand have not been similarly damaged by fires, wind, or other catastrophic factors to extract reliable climatic information. Site characteristics will be noted (topography/geomorphology, soil conditions, vegetation description, signs of human impact, etc.).

3.1.2. Selection of trees

Trees should be in a dominant position (with the possible exception of stand density studies), without irregular growth which probably disturb the climate signal in the tree-rings. Individual variability in the final chronology decreases with an increasing number of samples. Consequently, two cores from at least 12 living trees are necessary to obtain a site-chronology of sufficient quality. It is best to sample a few more trees than necessary so that anomalous cores may be discarded. Trees of different age classes will be cored to allow for systematical studies on age-related bias in the climate-growth response.

Samples are taken at breast height with an increment borer. The cores are stored in air-dry conditions after labelling with a pencil. Growth irregularities (compression wood, wound tissue, etc.) are excluded by avoiding sampling in the vicinity of wound and of upslope and downslope sides of trees growing on sloping ground. Cores are taken as nearly perpendicular to the fibre orientation as possible. This can greatly reduce the variability owing to technical processing in densitometric studies (Schweingruber et al., 1990). Core characteristics will be noted (tree height, stem diameter at breast height, crown size and condition, injuries and irregular growth, coring direction and height, etc.). Sites and trees will be documented photographically.

3.2. Sample preparation, measurement, and chronology building

3.2.1. Preparation

Resins and heartwood substances must be chemically removed as they will influence on the X-ray absorption (Schweingruber, 1990). This is done through distillation in Soxhlett device; resins are extracted with alcohol, heartwood substances with water. After removal of resins and heartwood substances, laths of equal thickness have to be cut from the round cores. The Birmensdorf system may be used where the core is glued to a wooden support with the radial surface uppermost and a 1.25-mm-thick lath cut out with a small twin-bladed circular saw. To obtain comparable density values, the moisture content of the wood must be kept constant.

3.2.2. Measurement of density

The irradiation of film can be done with different methods. Two methods, which have proved to be useful are:

1.

Irradiation of a film (Kodak, Type R, single-coated industrial X-ray film)

resting on the moving stage. The film is transported at five cm/min under the radiation source, which is 31 cm above, and irradiated at 20kVh and 2mA (Vancouver system)

2.

Irradiation of a film (Kodak, Type X-Omat TL, double coated medical X-ray film) resting on a stationary stage at 11 kVh and 20 mA for 90 min. The source is 250 cm above the film (Nancy system)

The film is developed and the different gray levels produced on the radiograph by the wood samples are converted to wood density values. The basic instrument used is the densitometer (ibid). Analog or digital processing of the actual measurements produces a density profile from which the desired parameter (maximum density) is registered.

3.2.3. Dating and chronology building

For dating, chronology building and quality control, the program COFECHA (Holmes et al., 1986) may be used. In addition a manual dating control has to be done at the light table or monitor, comparing each curve with an existing master chronology. The procedure ensures precise dating of every tree ring.

3.3. Data processing

3.3.1. Standardization of tree-ring data

Before averaging tree-ring curves to mean chronologies which shall be used for dendroclimatological purposes, the raw values must be standardized to index values. In the same process, one has to remove the natural age trend of trees and eventual density variations caused by stand dynamics, and not representing climate. Also in this process, it is crucial to control the effect of detrending at the light table or on the monitor, comparing the original with the detrended curve. Much depends from this process, as the dendrochronologist here decides which portion of low frequency variation that is removed from the series. This in turn affects climate information inferred from the chronology. Therefore, several detrending methods have to be tested in this study.

3.3.2. Computing climate-growth response

Climate-growth models will be computed for all individual chronologies. The period selected for climate-growth modelling, is the period for which

climate data are available (the earliest series start in AD ??). Different techniques are existing for estimation of the climate-growth response. For example, simple correlation analysis may be used or a regression-technique based on principal component analysis. It may be relevant to detect non-linear relationships between climate variables and ring growth, as well as to study single years with special tree-ring (pointer years) and climate events. To detect changes in climate-response over time the Kalman filter can be used.

4. Time schedule

The project will be performed during three years (June 1998 to June 2001). The Ph.D. student will follow courses corresponding to 40 weeks of studies. >From earlier working, the following assumptions regarding time consume for field work and measuring can be made: It can take a number of days to become familiar with the localities and to find the most suitable pine stands. At each site, one to two days are needed for sampling and site description, provided that the pines do not stand too scattered, and long walking distances can be avoided. Time for measuring and chronology building should be estimated rather high (2-3 weeks per site).

1998:

Summer:

Preparing of a detailed sampling strategy for the whole project (2 weeks) and field work (6 weeks). The field work will focus on sampling of trees from about six sites with varying conditions (soil fertility and water holding capacity).

Autumn semester:

Training in use of densitometry equipment at the institute of Forest, Snow and Landscape in Birmensdorf, Switzerland. Measurement of samples collected in the summer.

1999:

Spring semester:

Continued measuring of samples at the university in Stockholm. Systematical analysis of standardization methods and construction of six site chronologies. Start of analysing climate-growth response in chronologies.

Summer:

Field work (6 weeks) which will put focus on sampling trees from about six sites in different altitudes and with different stand densities.

Autumn semester:

Measuring of the summer's material at the university in Stockholm.

Systematical analysis of standardization methods and construction of six new site chronologies. Analysing climate-growth response in chronologies.

2000:

Spring semester:

Analysing climate-growth response in all chronologies. Preparation of publication (a).

Autumn semester:

Analysing age-related climate-response. Preparation of publication (b).

Comparison of results with similar study on ring-widths (Grudd, 1998).

2001:

Spring semester:

Last statistics, preparation of publication (c), preparation of disputation.

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From: Lynn Price <lkpocd@dante.lbl.gov>
To: Nebojsa Nakicenovic <naki@iiasa.ac.at>
Subject: Confirmation of Attendance for Next IPCC SRES Meeting
Date: Tue, 20 Jan 1998 12:42:40 -0800 (PST)
Reply-to: Lynn Price <lkpocd@dante.lbl.gov>
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Hi everyone,

I need to have a firm number of attendees by the end of the day tomorrow (Wednesday January 21st) in order to hold rooms at the hotel. At the end of this e-mail I have listed the information that I currently have regarding who is planning to attend, who is not planning to attend, and who has not responded.

I will hold a room for each of the people listed below as attending unless I hear otherwise from you.

If you are in the list of people who have not yet responded and you plan to attend, please let me know ASAP.

If I have not heard from you by the end of the day tomorrow I will assume that you will make your own arrangements for accommodations.

For those of you who want me to hold a room for you, I will send information on how to make your reservations in a day or so.

Thanks,

Lynn

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Confirmed as attending:

Nebojsa Nakicenovic
Zhou Dadi
Stuart Gaffin
Henryk Gaj
Ken Gregory
Arnulf Gruebler
Erik Haites
Tae-Yong Jung
Emilio Lebre La Rovere
Alan Manne
Tsuneyuki Morita
Richard Moss
Hugh Pitcher
Rich Richels
Rob Swart
H.J.M. de Vries
Ernst Worrell

Not attending:

Knut Alfsen
Dennis Anderson
Joergen Fenhann
Laurie Michaelis
Priyadarshi Shukla
Jim Skea

Have not responded:

Joseph Alcamo
Ged Davis
Benjamin Dessus
Jae Edmonds
William Hare
Michael Hulme
Michael Jefferson
Tom Kram
Mathem Luhanga
Douglas McKay
Julio Torres Martinez
Shunsuke Mori

Youssef Nassef
William Pepper
Hans-Holger Rogner
Cynthia Rosenzweig
Leena Srivastava
John Weyant

From: Tom Wigley <wigley@meeker.ucar.edu>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: New MAGICC/SCENGEN
Date: Mon, 9 Feb 1998 15:48:15 -0700 (MST)
Reply-to: Tom Wigley <wigley@meeker.ucar.edu>
Cc: hm_pitcher@pnl.gov, o.brown@uea.ac.uk

Mike,

Thanks for the quick response. Responses to responses follows....

(1) I tried the composite GHG plus UIUC SUL on Norm's machine, in just the way you said. However, the results for the USA seem to be identical to those using **only** UIUC GHG input. I'll try again.

(2) You are right in saying one shouldn't scale GHG patterns by GHG+SUL dTs. However, to be strictly consistent one should never allow GHG patterns to be used alone. So you are **not** being consistent if you allow this---which you do. The point then is to minimize the extent of the inconsistency.

It is unarguably correct that the global-mean temperature to use is the one containing all forcings (i.e., column 6 in **DRIVE.OUT**). The choice then is what pattern(s) to use. If we had no SUL information, we would have to use GHG patterns; as in the original SCENGEN. Scaling these with the MAGICC GHG output would give both incorrect patterns and incorrect global-mean warming. Scaling with column 6 at least gets the global-mean warming correct (within MAGICC uncertainties). You seem to have chosen to get **both** things wrong, instead of just the patterns.

I can see some logic in your method; I just think (strongly) that it is wrong. At the very least, it will be confusing to the user. If the user selects only GHG model patterns, then won't they wonder why the global-mean temperature is inconsistent with MAGICC? To take an extreme case, suppose the full dT is 2degC and the GHG-alone dT is 3degC. Is it better to scale an approximate pattern (i.e., the GHG pattern) by 2degC or 3degC? In my view, GHG scaled by 2degC would be much closer to GHG+SUL scaled by 2degC than GHG scaled by 3degC. Surely the real issue (given that it is impossible to be entirely consistent in this case) is to get a result that is as close to the 'right' result as possible. I feel quite sure that scaling by column 6 is best on this basis---especially given that the patterns are much more uncertain than the global-means. I

think this is absolutely beyond doubt.

The bottom line here is that consistency is impossible if one uses only GHG patterns. Column 6 was included deliberately, and after some thought (along the lines noted above).

Of course, it is possible to get column 6 results by adding columns 2, 3, 4 and 5 as they now stand (and as they are in the version that you have). However, one cannot do this with the correct *raw* column 3, 4, and 5 output because of the nonlinear direct forcing effect. It just happens that, in your version, I 'faked up' column 5 as the difference between column 6 and the sum of columns 2, 3 and 4. I did this simply to get the code working; but (as you now know) I never got around to fixing it up until now. In the latest version, column 6 is again equal to the sum of columns 2, 3, 4 and 5 because I scale columns 3, 4 and 5 to ensure that this is so.

(3) Re HadCM2, again it is impossible to be consistent. What I said before is that the reason for adding these results is simply to make them readily available. I do *not* advocate using them in combination with any other model results. It is, I believe, perfectly reasonable to scale these results with column 6 data. Of course, this 'hides' an assumption about the relative magnitudes of the GHG and SUL components---i.e., it assumes that the HadCM2 relative magnitudes are okay. The point of scaling, however, is to account for other factors that change the global-mean temperature relative to HadCM2 results, such as different sensitivities.

I agree with you that it would not be an efficient use of time splitting the HadCM2 SUL results into GHG and 'aerosol' component patterns. The whole point of the sulphate part of SCENGEN is to look at the influence of different SO2 emissions patterns. Splitting up HadCM2 wouldn't help here at all.

I also think it would be valueless to hardwire HadCM2 dT results into SCENGEN---again, this would defeat the purpose of including these results. It would introduce an additional inconsistency; since HadCM2 patterns change with time, it would not be logical to scale the 2071-2100 pattern with (e.g.) 2031-2060 dT. Of course, you could argue that it is illogical to scale this pattern with (e.g.) 2031-60 dT from MAGICC; but this is a different issue that I don't think is worth discussing at this time.

(4) Thanks for explaining the UIUC 'other data' problem. I will ask Michael whether he can provide full global fields for the other variables, since it really would be valuable to include them. If he can give us these data, could you add them to SCENGEN? (re this, see below)

(5) I appreciate your problems with Olga and Mike Salmon. As far as I can see, incorporating the revised MAG.FOR code into MAGICC/SCENGEN shouldn't be too difficult. I can, however, get hold of some money to pay for some of Mike's time to do other work---perhaps \$5000 or so. Can we set something up? The contractual side would be easy---just a matter of agreeing a brief statement of work, and having CRU send a bill. If this is useful and possible, then can you check it out with Mike and Trevor?

Cheers,
Tom

On Mon, 9 Feb 1998, Mike Hulme wrote:

> Tom,
>
> Got your fax and email. Five responses:
>
> 1. UIUC SUL results *can* be combined with any GHG pattern (or
> combination). Simply click on the relevant GCMs in the GCMs menu. You can
> choose all 15 GHG patterns and also the UIUC SUL pattern simultaneously if
> you want. Not sure how you missed this one.
>
> 2. We do *not* allow GHG patterns to be scaled by GHG+SUL dTs from MAGICC
> (what you call 'global sulphate'); i.e., we never use column 6 in the
> *DRIVE files. We always follow the 'disaggregated sulphate' route by using
> columns 2, 3, 4 and 5. I still maintain it is not correct to scale GHG
> patterns by a global dT that results from GHG+SUL forcing. The way we have
> designed SCENGEN is so that the choice of what columns in *DRIVE to use is
> governed by what GCMs are selected in the GCMs menu. If only GHG patterns
> are chosen we use column 2. If only SUL patterns are chosen we use columns
> 3, 4 and 5 with the appropriate weightings applied (i.e., we have three
> UIUC SUL pattern files corresponding to the three SCENGEN regions,
> re-combined of course from Schlesinger's six original regions). If *both*
> GHG and SUL patterns are chosen then we combine the various patterns using
> columns 2, 3, 4 and 5. You will see that the global dT displayed in red on
> the main screen changes in keeping with these selections (i.e., GHG only,
> SUL only or GHG+SUL).

>
> If we allowed GHG patterns to be scaled by dTs from MAGICC that resulted
> from GHG and SUL forcing I believe that we break the consistency of our
> method. Column 6 is therefore redundant and serves only to check the
> summing of the other columns.
>
> 3. This parallels an earlier discussion about using HADCM2 SUL results in
> SCENGEN. Strictly, we should not use them since they are SO2 pattern
> specific. Allowing the user to scale HADCM2 SUL by a set of dTs resulting
> from *any* SO2 pattern is plainly wrong. A compromise would be to allow
> HADCM2 SUL to be scaled by the dT from the HADCM2 SUL simulation (i.e.,
> hard-wiring these dTs into SCENGEN and using only these if the user wants
> HADCM2 SUL). Of course, other GCM patterns should not then be added to
> this. There is another way of using HADCM2 SUL results more flexibly and
> that is by differencing HADCM2 GHG from HADCM2 SUL (2071-2100),
> standardising the result according to the dTs from the three SCENGEN
> regions and then treating these standardised HADCM2 SUL only patterns as
> independent aerosol patterns to be used in SCENGEN. This would be my
> approach but again requires more time and effort.
>
> 4. We only include T and P from UIUC for the very good reason that only T
> and P contain complete global fields (at least from the ftp site data).
> The other variables exist only for land areas. Since the UIUC grid is 4
> (lat) by 5 deg and SCENGEN is 5 by 5 we would need to regrid (and the
> longitudes are displaced by 0.5 a box as well which complicates matters).
> Regridding land only grids onto a different land only grid is non-trivial
> (possible, but would take some working at). For example, UIUC have no
> Iceland or Caribbean islands so what do we give to SCENGEN for these boxes?
> We have to tell SCENGEN something since we add other GCMs together.
> Faking up data here is very time-consuming. If UIUC have other fields
> apart from T and P for a full global grid but just not put them on the web
> site then fine, the problem is quite straightforward. If not, then we have
> a messy problem on our hands.
>
> 5. Points about revised MAGICC code noted and we will have a look at the
> new code when it is here. Please also note that apart from Olga not being
> paid by me now, neither is Mike Salmon. Indeed, Mike's contract is rather
> uncertain again. But I hope I can persuade him (and Trevor) to keep pace
> with MAGICC changes for all our sakes.
>
> Regards,
>
> Mike

>
> At 19:23 06/02/98 -0700, you wrote:

> >Dear Mike,

> >
> >Some rather urgent SCENGEN issues have arisen from my meeting with Norm
> >Rosenberg, Hugh Pitcher et al. at Battelle. While at Battelle, I had my
> >first chance to look at the new SCENGEN, since I have not had time to try
> >to get it working under NT. (I haven't had time to try your new batch
> >file yet.)

> >
> >The first thing is that you seem to have constrained things so that
> >Schlesinger's sulphate results can only be added to *his* ghg results.
> >This defeats the purpose of the method. The sulphate patterns,
> >appropriately scaled, can be added to *any* (or any combination) of ghg
> >(i.e., CO2 alone) results. I am at a loss to understand why you did this,
> >because it seems to me that the coding should be easier for the more
> >general case. The way it should work is this:

> >
> >First, the user selects the MAGICC output; low, mid, high or user climate
> >output. This determines which file to use to get the normalized pattern
> >weights, LODRIVE, MIDDRIIVE, HIDRIVE OR USRDRIVE.

> >
> >The user must then select whether to use global sulphate or disaggregated
> >sulphate. This determines whether to use the last column only in *DRIVE
> >(labeled SUM) to weight the ghg (or composite ghg) pattern (global
> >sulphate case); or to use the second, third, fourth and fifth columns of
> >*DRIVE (labeled GHG, ESO21, ESO22, ESO23) to weight, respectively, the ghg
> >(or composite ghg), region-1 sulphate, region-2 sulphate and region-2
> >sulphate patterns---and then sum these weighted patterns.

> >
> >What you seem to be doing now is to only allow SCENGEN to use
> >Schlesinger's ghg pattern for weighting with the GHG column. It should be
> >trivial to fix this. The ghg (or composite ghg) pattern should be
> >calculated no matter whether the user selects the global or disaggregated
> >sulphate case. You may have switched this calculation off for the
> >disaggregated case---but you *shouldn't*. As I noted above, the coding
> >should be easier for the proper working of the model.

> >
> >You may recall that I said earlier that I think there is still a glitch in
> >the sulphate pattern weights. On looking at the *DRIVE outputs again I
> >still think this is a problem. Have a look yourself and see whether you
> >think the numbers look reasonable or not. Ill check this out further over
> >the weekend.

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Subject: minutes of the SRES informal modelers' meeting
Date: Mon, 16 Feb 1998 16:48:49 +0100
Cc: kuszko@uea.ac.uk, naki@uea.ac.uk

Dear Colleagues,

Please find attached the minutes of the SRES informal modelers' meeting, 7-8 February 1998 in Berkeley, California. I would like to thank those who participated in the meeting and Lynn Price in particular, both for the excellent organization of the meeting and for drafting the minutes. Please note the deadlines detailed in our work plan; for those of you completing the next two rounds on model runs and storylines, this will be especially important. Additional submissions to the SRES scenario database would be also greatly appreciated. Finally, if anyone would like to receive a hard copy of the materials we discussed in Berkeley, please contact Anne Johnson at johnson@iiasa.ac.at. (The same material was sent to you by e-mail on January 30).

With best regards,

Naki

Attachment Converted: "c:\eudora\attach\draft-minutes1.doc"

Nebojsa NAKICENOVIC

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From: ???@??? Fri Feb 20 10:42:27 1998

Return-path: <dloberts@meto.gov.uk>
Envelope-to: f037@cpcall.uea.ac.uk
Delivery-date: Fri, 20 Feb 1998 10:41:40 +0000
Received: from mailgate3.uea.ac.uk [139.222.230.3]
by cpcall.uea.ac.uk with esmtp (Exim 1.73 #1)
id 0y5ptk-0005i2-00; Fri, 20 Feb 1998 10:41:40 +0000
Received: from thorn.meto.gov.uk by mailgate3.uea.ac.uk with SMTP (PP);
Fri, 20 Feb 1998 10:41:22 +0000
Received: from thorn.meto.gov.uk (MEADOW)
by thorn.meto.gov.uk (PMDF V5.1-9 #26370) with ESMTTP
id <01ITST3966TC0044ID@thorn.meto.gov.uk> for
m.hulme@uea.ac.uk;
Fri, 20 Feb 1998 10:40:27 GMT
Received: from hc0800 ([151.170.1.12])
by meadow.meto.gov.uk (PMDF V5.1-9 #26370) with ESMTTP
id <01ITST3LEWEW006LUJ@meadow.meto.gov.uk> for
m.hulme@uea.ac.uk;
Fri, 20 Feb 1998 10:40:44 +0000 (GMT)
Received: from hc1300 by hc0800 with ESMTTP (1.39.111.2/1.1) id
AA146051261;
Fri, 20 Feb 1998 10:41:02 +0000 (GMT)
Date: Fri, 20 Feb 1998 10:41:01 +0000 (GMT)
From: David L Roberts <dloberts@meto.gov.uk>
Subject: From dloberts@meto.gov.uk
To: m.hulme@uea.ac.uk
Message-id: <199802201041.AA146051261@hc0800>
Posted-Date: Fri, 20 Feb 1998 10:41:01 GMT
Received-Date: Fri, 20 Feb 1998 10:41:02 GMT
MIME-Version: 1.0
Content-type: text/plain; charset="X-roman8"
Content-transfer-encoding: 7bit
Status:

Dear Mike,

What is the current state of play regarding definition of improved sulphur emission scenarios? I have the 'zero-order draft' by Arnulf Grubler that you sent me at the beginning of November, as well as a shorter note by Hugh Pitcher. Have there been more developments since then?

As you can probably guess, this enquiry results from Geoff Jenkins's visit to Brussels (?) a few days ago. Geoff is now keen that we should use better emission scenarios than IS92a and is pressing me for action, even if this means using an interim scenario that has not yet been agreed by IPCC.

Best regards,
David

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Subject: Next SRES Meeting, week of 27 April in Washington
Date: Tue, 24 Feb 1998 19:01:16 +0100

Dear Colleagues,

I am writing to let you know that the next IPCC-SRES Full Authors meeting will be held the week of 27 April 1998 (instead the week of 6 April) in Washington, D.C. Bob Watson of the IPCC will attend. The exact dates during that week are not yet fixed, but I expect that we will have a full authors meeting for two days, preceded by a two-day modelers meeting. Please let me know soon--today if possible--whether you will be available during this week; it is critical that we finalize the dates early so there will be sufficient time to ensure funding for our colleagues from developing countries who need IPCC support.

I look forward to hearing from you very soon.

Best regards,

Naki

Prof. Dr. Nebojsa Nakicenovic

Project Leader

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: climat@ipcom.ru (L.Kitaev)
Subject: Re: for Proff.A.Krenke, Moscow
Date: Fri Feb 27 14:56:04 1998
Cc: eugene,stepan

Dear Prof. Krenke

I am happy to submit the proposal from here or to be associated with it in collaboration with our ongoing tree-ring development work (with Fritz Schweingruber, Eugene Vaganov and Stepan Shiyatov) but you will have to take the initiative in writing and organising the proposal. I am very tied up with meetings and I have to write and submit another INTAS proposal with the people I mentioned to continue development and analysis of the long chronologies at Yamal and Taimyr. The others need not be listed if you do not wish but I would ask you to discuss with Prof. Vaganov how he sees this being balanced with his priorities and our ongoing work. We will use our own transfer function approach (in our ADVANCE European project) to reconstruct circulation in summer based only on the tree-ring data but this is no worry for you. If you can get the draft to me soon - with details of all participants and money I will then look at it and revise and submit as you wish.If this is to happen you must take the initiative of putting it together.

please let me know what you intend
as soon as possible. I am here only for one more week!

Keith

At 09:56 AM 2/24/98 +0300, you wrote:

>

>

>Attachment Converted: "c:\eudora\attach\BRIFFA2.TXT"

>

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Subject: Tentative Attendance of IPCC SRES Meeting, 27-30 April 1998
Date: Fri, 27 Feb 1998 15:30:22 +0100

Dear Colleagues,

Thank you for your prompt response to my recent e-mail message regarding
the next IPCC SRES meeting. I am glad to hear that so many of you will
be
able to attend, since this will be a very important discussion. The plan
is
to hold the modelers' meeting on April 27 and 28, followed by the full
authors' meeting on April 29 and 30.

Below is a list of those who are planning to attend:

Joseph Alcamo <alcamo@usf.uni-kassel.de>
Dennis Anderson <dennis.anderson@ic.ac.uk>
Zhou Dadi <becon@public3.bta.net.cn>
Gerald Davis <Ged.R.Davis@si.simis.com> (part of the meeting)
Bert de Vries <Bert.de.Vries@rivm.nl>
Jae Edmonds <ja_edmonds@pnl.gov>
Joerg Fenhann <j.fenhann@risoe.dk>
Stuart Gaffin <stuart@edf.org>
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I will be in touch with additional details in the coming weeks.

Best regards,

Naki

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Subject: ZOD attached
Date: Wed, 04 Mar 1998 16:37:37 +0100
Cc: kuszko@uea.ac.uk

Dear Colleagues:

Naki has asked me to send you the attached IPCC Zero Order Draft by
Dennis
Anderson on the influence of social and economic policies on future
carbon
emissions. It is an updated version of the ZOD presented at the Berkeley
SRES meeting. The attachment is missing the last three charts, but these
will be available in time for the Washington, D.C. meeting. If you have
any comments, please send them directly to Dennis Anderson:

Dennis.Anderson@Economics.oxford.ac.uk

I have attached the ZOD in both rich text and MS Word formats.

Regards,

Anne Johnson

Attachment Converted: "c:\eudora\attach\anderson.doc"

Attachment Converted: "c:\eudora\attach\anderson.rtf"

Anne JOHNSON
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From: Padruot Nogler <nogler@wsl.ch>
To: k.briffa@uea.ac.uk
Subject: From Rashit Hantemirov
Date: Fri, 06 Mar 1998 14:05:21 +0100

Dear Keith,

I am in Birmensdorf now and will stay here until March 20s.
As far as I know Stepan Shiyatov has to translate the proposal into Russian because of this year there are two possibility to get grant. The one is just INTAS competition and other is joint INTAS-RFBR (Russian Foundation for Basic Researches) ones with the same requirements and grant amounts. For second one we have to submit russian version to RFBR. If proposal will reject by RFBR it will be automatically submit for INTAS competition.

Attached file is the ring-width series of subfossil (first letter is L in series number) and living larches from Yamal, used for mean chronology developing (best or the only ones for corresponding period).

Best regards,
hope to see you in London next month,

Rashit Hantemirov

Attachment Converted: "c:\eudora\attach\AB-XVII.RWM"

From: Anne JOHNSON <johnson@iiasa.ac.at>
To: Joseph Alcamo <alcamo@usf.uni-kassel.de>, Knut Alfsen
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Subject: new IPCC-SRES Zero Order Draft
Date: Tue, 10 Mar 1998 13:20:19 +0100

Dear Colleagues:

I am sending you a copy of Ged Davis' IPCC-SRES Zero Order Draft on storylines and scenarios. The text is appended below, but I am also attaching versions in MS Word and in Rich Text formats so that you can better view the graphics.

Please send any comments directly to Ged Davis at

Ged.R.Davis@si.simis.co

Regards,

Anne Johnson

Zero Order Draft

IS99
Storylines and Scenarios

February, 1998

Ged Davis et al

For Comment Only

Draft Paper for the IPCC Special Report on Emissions Scenarios

Contents

1. Introduction
2. Scenarios - overview
3. Golden Economic Age (A1)
4. Sustainable Development (B1)
5. Divided World (A2)
6. Regional Stewardship (B2)
7. Scenario comparisons
8. Conclusions

Appendix 1: Scenario quantification

1. Introduction

The IS99 scenarios have been constructed to explore future developments in the global environment with special reference to the production of GHGs.

These scenarios are being developed in three phases:

- Phase 1: the Special Report on Emissions Scenarios (SRES) team is preparing a set of scenarios for wide public discussion, which is the subject of this note,
- Phase 2: the scenarios will be placed on the World Wide Web, subject to public scrutiny, and suggestions for relevant modification of the scenarios will be sought,

- Phase 3: the scenarios will be finalised for peer review, incorporating suggestions received during the public review, by April 1999.

Phase 1 centred on a facilitated open process for Lead Authors at workshops

in Paris, Vienna and Utrecht. The scenarios developed allow for a broad range of GHG emissions and provide a basis for reflection on policy.

- 1.1 What are scenarios?

Scenarios are pertinent, plausible, alternative futures. Their pertinence, in this case, is derived from the need for climate change modelers to have a basis for assessing the implications of future possible paths for Greenhouse Gas Emissions (GHGs). Their plausibility is tested by peer review, in an open process, which includes their publication on the World Wide Web.

There are clearly an infinite number of possible alternative futures to explore. We have consciously applied the principle of Occam's Razor, seeking the minimum number of scenarios to provide an adequate basis for climate modelling and challenge to policy makers. The alternative futures constructed are not, and cannot be, value free since like any work they self-evidently reflect the team's view of the possible. The scenarios should not be construed as being desirable or undesirable in their own right and have been built as descriptions of possible, rather than preferred, developments. There can be no objective assessment of the probability of the scenarios, although in the prevailing zeitgeist some will appear to individuals to be more likely than others. Scenarios are built to clarify ignorance rather than present knowledge -- the one thing we can be sure of is that the future will be very different from any of those we describe!

2. Scenarios - overview

2.1 Scenarios: key questions and dimensions

Developing scenarios for a period of one hundred years is a relatively new field. Within that period we might expect two major technological discontinuities, a major shift in societal values and a change in the balance of geopolitical power. A particular difficulty is that people are not trained to think in these time-spans, are educated in narrow disciplines and our ability to model large-systems, at the global level, is still in its infancy. Additionally, most databases do not go back much further than 50 years and many less than that. How best to integrate demography, politico-economic, societal and technological knowledge with our understanding of ecological systems? Scenarios can be used as an integration tool, allowing an equal role for intuition, analysis and synthesis.

Terminology

Storylines, Scenarios and Scenario Families

Storyline: a narrative description of a scenario (or a family of scenarios), highlighting the main scenario characteristics, relationships between key driving forces and the dynamics of the scenarios.

Scenario: projections of a potential future, based on a clear logic and a quantified storyline.

Scenario family: one or more scenarios which have the same demographic, politico-societal, economic and technological storyline.

Scenario Classification

Our approach has been to develop a set of four "scenario families". The storylines of each of these scenario families describes a demographic, politico-economic, societal and technological future. Within each family one or more scenarios explore global energy industry and other developments and their implications for Greenhouse Gas Emissions and other pollutants. These are a starting point for climate impact modelling.

The scenarios we have built explore two main questions for the 21st century, neither of which we know the answer to:

- Can adequate governance -- institutions and agreements -- be put in place

to manage global problems?

- Will society's values focus more on enhancing material wealth or be more

broadly balanced, incorporating environmental health and social well-being.

The way we answer these questions leads to four families of scenarios:

- Golden Economic Age (A1): a century of expanded economic prosperity with

the emergence of global governance

- Sustainable Development (B1): in which global agreements and institutions, underpinned by a value shift, encourages the integration of ecological and economic goals

- Divided World (A2): difficulty in resolving global issues leads to a world of autarkic regions

- Regional Stewardship (B2): in the face of weak global governance there is

a focus on managing regional/local ecological and equity

Within these scenario families we examine plausible energy industry and other developments which will contribute to GHG emissions. Although the storylines cannot have explicit climate change policy measures in them there are examples of indirect mitigation measures in some of the scenarios.

The scenario quantifications of the main indicators related to growth of population and economy, the characteristics of the energy system and the associated greenhouse gas emissions all fall within the range of prior studies .

3. Golden Economic Age (A1)

This scenario family entitled "Golden Economic Age", describes rapid and successful economic development. The primary drivers for economic growth and development "catch up" are the strong human desire for prosperity, high

human capital (education), innovation, technology diffusion, and free trade.

The logic of successful development assumes smooth growth with no major

political discontinuities or catastrophic events. The scenario family's development model is based on the most successful historical examples of economic growth, i.e., on the development path of the now affluent OECD economies. Historical analogies of successful economic "catching up" can be found in the Scandinavian countries, Austria, Japan, and South Korea. "Intangible" assets (human capital, stable political climate) take precedence over "tangible" assets (capital, resource, and technology availability) in providing the conditions for a take-off into accelerated rates of development. Once these conditions are met, free trade enables each region to access knowledge, technology, and capital to best deploy its respective comparative economic and human resource advantages. Institutional frameworks are able to successfully sustain economic growth and also to handle the inevitable volatility that rapid economic growth entails.

The "intangible" prerequisites for accelerated rates of economic growth also offer long-term development perspectives for regions that are poorly endowed with resources or where current economic prospects are not auspicious, such as Sub-Saharan Africa. There, for instance, fostered regional trade and capital availability enhance the pull-effects of a strong South African economy. In other regions, growth may be fuelled by domestic know-how and high human capital valued at the international market. An example of this is the thriving software industry of the Indian subcontinent. In yet other regions, growth could be stimulated by the expansion of regional economic partnerships and free trade arrangements (e.g., extensions of NAFTA and the European Union).

The main difference with the historical OECD experience is a certain acceleration in time and space, (i.e., "leapfrogging") made possible by better access to knowledge and technology, a consequence of the high-tech and free trade characteristics of development. Successful catching up becomes pervasive; all parts of the "developing world" participate, though with differences in timing. The final outcome is that practically all parts of the world achieve high levels of affluence by the end of the 21st century, even if disparities will not have disappeared entirely. The current distinction between "developed" and "developing" countries will in any case no longer be appropriate.

As in the past, high growth (a "growing cake") eases distributional conflicts. Everyone reaps the benefits of rapid growth, rising incomes, improved access to services, and rising standards of living. The economic imperatives of markets, free trade, and technology diffusion (i.e., competition) that underlie the high growth rates provide for efficient allocation of resources. Efficiency and high productivity are the positive by-products of the highly competitive nature of the economy. They also provide the economic resources for distributive and social measures required for a stable social and political climate, vital for sustaining high growth rates in human capital, productivity, innovation, and hence

economic growth.

The economic development focus explains its central metric: the degree of economic development as reflected in per capita income levels (GDP at market exchange rates as well as at purchasing power parity rates). The principal driver is the desire for prosperity, all major driving forces are closely linked to prosperity levels, with actual causality links going in both directions. For example, demographic variables co-evolve with prosperity: mortality declines (i.e. life expectancy increases) as a function of higher incomes (better diets and affordable medical treatment).

In turn, changes in the social values underlying the fertility transition also pave the way for greater access to education, modernisation of economic structures, and market orientation. These are key for innovating and diffusing the best practice technologies underlying the high productivity, and hence economic growth, of the scenario.

3.1 Key Scenario Drivers and their Relationships

3.11 Population and Economic Development

High education, stable social relations, and incentives for innovation and

experimentation are the preconditions for productivity increases underlying

rapid economic development in this world-- as a result, social, economic, and demographic development are highly correlated .

The link between demographic and economic variables in the scenario corresponds to present empirical observations: the affluent live long and have few children. High per capita incomes are thus associated with both low mortality and low fertility. Together, this results in rather low population growth, characterised in addition by a considerable "greying" of

the population.

This family of scenarios combines high life expectancy with low fertility,

where OECD rates are assumed to stabilize at current (below replacement) levels, and developing countries follow a similar transition by the mid-21st century. Fertility rates range between 1.3 to 1.7 children per woman. Life expectancy can approach some 95 years, with a regional variation between 80 and 95 years. Global population grows to some 9 billion by 2050, and declines to 7 billion by 2100, the result of continued

below replacement fertility in all regions.

Population ageing results in economic growth rates somewhat lower than historical experience, especially in the OECD countries. Economic growth rates slow over time in proportion to the reduction of the potentially economic active population (age 15 to 65), which decline in some regions to

50 percent compared to the historical average of approximately 70 percent.

For "developing countries", economic growth is based on the most successful cases of economic "catch up" found in history. The economic growth profile of Japan after WW II served as a model to delineate the upper bounds of possible GDP growth for all regions. Consistent with growth theory, GDP expansion initially accelerates, passes through a peak, in which growth rates around 10 percent per year can be sustained for several decades, and then declines. Once the economic and industrial base is firmly established and the economy matures, growth rates decline with increasing income levels. This reflects saturation effects and a higher emphasis on quality rather than quantity at high income levels. The global economy in the "Golden Economic Age" expands at an average annual rate of three percent per year to 2100. This is about the same rate as the global average since 1850 and in this respect may simply be considered "dynamics as usual". Non-Annex-I economies expand with an average annual growth rate of four percent per year, twice the rate of Annex-I economies. By approximately 2030 Non-Annex-I GDP surpasses that of the Annex-I economies. Per capita income disparities are reduced, but differences between regions are not entirely eliminated. Non-Annex-I per capita income reaches the 1990 Annex-I level (14,000 \$/capita) by around 2040. By 2100 per capita income would approach 100,000 \$/capita in Annex-I countries and 70,000 \$/capita in Non-Annex-I countries.

3.12 Equity

Equity issues are not a major concern in the world, but is rather a by-product of the high rates of economic development. Existing per capita income gaps between regions close up in a similar way as between Western Europe and Japan compared to the US in the 20th century. Disparities continue to persist between regions, but more so within particular regions. Nevertheless, the high economic growth rates require a certain degree of income distribution. Extreme income disparities are found to be negative influencing factors for economic growth. Additionally, fair income distribution only assures the large consumer markets and the social cohesion and stability required for the realisation of high economic growth.

3.13 Settlement patterns/communication

Communication technologies and styles are highly homogeneous and extremely developed -- rather than a "global village" future, this is one of "global cities." Existing trends towards urbanisation continue, as cities provide the highest "network externalities" for the educational and R&D-intensive

economic development pattern underlying the scenario. Regional differences in settlement patterns persist. They range from fragmented, compact, but large (i.e., 20+ million inhabitants) cities that depopulate their respective rural hinterlands in Latin America to urban "corridors" connected by high capacity communication and transport networks (in Asia). Regional transport networks include high speed trains and maglevs, which ultimately fuse short- and long-distance transport means into single interconnected infrastructures. In some parts of the world high-tech cars take the place that high-tech trains occupy in other parts. The large urban agglomerates and the high transport demands of a high material growth economy generate vast congestion constraints. These are solved by applying market-based instruments (prices) rather than regulation. Economic instruments include access and parking fees, auctioning off the limited number of new car and truck licenses in megacities, much along the lines of the current stringent Singapore model. Therefore, even at very high income levels, car ownership rates could be comparatively low in parts of the world. In extremely densely populated areas, cars remain a luxury rather than a means of mass transport (viz. Hong Kong). In areas with lower population density, car densities are high (+1 car per inhabitant). Car fuels could be either oil, synfuels, electricity, or hydrogen. Intercontinental transport is provided by energy- and GHG-intensive hypersonic aircraft fuelled by methane or hydrogen. They are the physical transport equivalent of the high capacity virtual communication links of a truly global economy.

3.14 Environmental Concerns/Ecological resilience

Ecological resilience is assumed to be high. In and of themselves, ecological concerns receive a low priority. Instead, the valuation of environmental amenities is strictly in economic terms, e.g., a function of affluence. Non-congestion, clean water and air, and recreational possibilities in nature all assume increasing importance with rising affluence, although preferences for environmental amenities may differ across regions and income levels. For instance, urban air quality and human health are valued highly even at income levels lower than those prevailing in England, where stringent air quality measures were introduced after the "killer smog" of 1952. Reduced particulate and sulphur air pollution become a matter of major consumer preference at levels of \$2,000 - 3,000/capita income in Asia. Altogether, the concept of environmental quality changes from "conservation" of nature to active "management" -- and marketing-- of natural and environmental amenities and services.

3.2 Scenarios

The core bifurcation (with respect to GHG emissions) of the scenario family

unfolds around alternative paths of technology development in the agriculture and energy sectors. In the energy sector, the central question is how to manage the transition away from the current reliance on conventional oil and gas. In the agricultural sector, the key issue concerns land productivity. Alternative technology bifurcations lead to a number of scenarios embedded and consistent within the overall theme of "prosperity via high technologies". All scenarios provide the high quantities of clean and convenient energy forms and diverse, high quality food demanded in an affluent world. Because technological change is cumulative, it can go in alternative, mutually exclusive directions, i.e., changes become "path dependent". Alternative directions unfold around the interrelated cluster of variables of resource availability and conversion technologies in both energy and agriculture. For instance, new technologies may enable humanity to tap either the vast quantities of fossil resources existing in the form of coal, unconventional oil, and gas with technologies that are both highly economic, efficient, and clean in terms of traditional pollutants, such as particulates or sulphur. Alternatively, technological change could unfold favouring non-fossil technologies and resources, such as nuclear and renewables. A similar bifurcation unfolds in the agricultural sector. In one sub-scenario, only incremental improvements are achieved in farming practices and land productivity. This is combined with a gradual global diffusion of meat-based diets. Both of these trends are land- (and deforestation-) intensive. Alternatively, global agriculture could move in the direction of genetically engineered, high productivity crops and "sea-farming," combined with a quality- and health-oriented diet based on fish and vegetables, both of which are relatively less land intensive. As a result, GHG emissions range widely even for otherwise similar scenario characteristics.

3.21 Energy Resources/Technology

Resource availability and technology are tightly interrelated. The "Golden Economic Age" of high productivity growth results from substantial technological innovation. Both contribute to economic growth, expansion of accessible resources, and improved efficiency in resource use. Factor productivity improvements occur across the board for agricultural land, materials, and energy. Improvement rates largely follow long-term historical trends and are entirely technology- and income- driven. Energy intensity (total commercial and traditional primary energy use per unit of

GDP) improves at an aggregate global rate of 1.5 percent per year. Improvement rates vary across regions as a function of distance from the productivity frontier and the turnover rates of capital stock. *Ceteris paribus*, improvement rates are higher in regions with currently lower efficiency and greater than average GDP growth. This assumes no particular policy intervention or additional price regulation apart from the ones consistent with a free market environment (i.e. price subsidies are removed, and full costing principles are established).

Per capita final energy use gradually converges as income gaps close. Final energy use per capita in non-Annex-I countries would reach approximately 85 GJ (2 tons of oil equivalent) by 2050 and approximately 125 GJ (3 toe) by 2100, i.e., about the current average of OECD countries outside North America. Despite improvements in productivity and efficiency, the high income levels lead to resource use close to the upper bounds of the scenarios available in the literature. For instance, global final energy use would increase to approximately 1000 EJ by 2100.

The scenarios developed are a function of the different directions taken by technological change. The key question is which primary resources may become economically accessible in the future, and which technologies will become available to convert these primary resources into the final goods and services demanded by consumers. In the energy area, resources/technologies are key variables in determining the timing and nature of the transition away from currently dominant conventional oil and gas.

Four pathways are possible:

1. Progress across all resources and technologies.
2. "Clean coal" technologies: environmentally friendly except for GHG emissions and possible resource extraction impacts.
3. "Oil/Gas": smooth transition from conventional to unconventional oil and gas, tapping the vast occurrences of unconventional fossil fuels, including methane clathrates.
4. "Bio-Nuclear": rapid technological progress in non-fossil supply and end-use technologies, e.g. renewables, such as solar and biomass combustion, nuclear and hydrogen-fuelled end-use devices, such as fuel cells.

For the scenario quantification, a number of contrasting cases, characterised by the main energy form used in the second half of the 21st century, have been evaluated with the aid of formal energy models:

1. The dominance of Non-Fossil fuels -- the "Bio-Nuclear" scenario (A1R).
2. The dominance of unconventional gas, including hydrates, and oil (A1G)
3. The dominance of "Clean Coal" (A1C)

A brief scenario taxonomy is given below.

Scenario

Dominant

Oil/Gas Resource

Technology Improvements

	Fuel Availability	Coal	Oil/Gas	Non-fossil
AlR	Non-fossil Medium (<50 ZJ)	Low	Medium	High
AlG	Oil/Gas High (>75 ZJ)	Low	High	Low
AlC	Coal Low (<35 ZJ)	High	Low	Low

*

Depending on the assumed availability of oil and gas, (low/medium/high) and

corresponding improvements in production and conversion technologies for coal, oil/gas, and non-fossil technologies, different energy systems structures unfold. For instance, in the dynamic technology cases, liquid fuels from coal or unconventional oil/gas resources would become available

at less than \$30 /barrel, with costs falling further by about one percent

per year with exploitation of learning curve effects. Non-fossil electricity (photovoltaics, new nuclear) would become available at costs of

less than 10 mills/kWh (\$.01/kWh) and continue to improve further as a result of learning curve effects. The basic premise of the "dynamic technology" scenarios is that energy services could be delivered at long-run costs not higher than today, but with technologies having radically different characteristics, including environmental. In the event

that such technology dynamics do not materialise, energy costs and prices would be significantly higher than suggested above -- illustrative model runs suggest energy demand would be up to 20 percent lower for a fossil scenario without significant cost improvements .

3.22 Agriculture

In the agricultural sector, two contrasting scenarios of land productivity

could unfold, depending on the nature of advances in agricultural technologies. However, CO2 emissions from land use changes could range from 0.5 (low) to 1.5 (high) GtC by 2030 and from -1 to -2 (low) to zero (high) GtC emissions by 2100. In the latter case tropical forests essentially become depleted as a result of land-use conversions for agriculture and biomass fuel plantations. In the former case, land productivity gains are so substantial that ploughing of marginal agricultural land is no longer economically feasible and is abandoned, following recent trends in the OECD. The resulting expansion of forest cover leads to a net sequestration of atmospheric CO2.

3.23 Scenario Quantification

An initial scenario quantification in terms of population, GDP, energy use,

and CO2 emissions for the three energy resource/technology sub-scenarios is

summarised in Appendix 1 . The global scenario for 2100 is also summarised

in the form of a snowflake diagram. All scenario quantifications are

tentative and subject to revisions.
[Figure: "Snowflake" for A1 scenarios]

3.24 CO2 Emissions

The diverging pathways of resource availability and technological change characteristic of the three scenarios examined result in a wide range of annual CO2 emissions: from 10 to 33 GtC by 2100. It is interesting to note that the emissions of the two "fossil fuel" sub-scenarios, "clean coal" and "oil and gas," are quite close to each other (33 GtC versus 29 GtC). Continued reliance on oil and gas, coupled with demand growth, explain the emission patterns for the oil/gas scenario. Coal is the only fossil resource available in the "clean coal" scenario. Therefore, over time coal is increasingly required for conversion into premium fuels such as synliquids and syngas. This conversion "deepening" leads to a feedstock premium for coal and increases the market potential of non-fossil fuels. CO2 emissions are therefore not as high as in traditional coal-intensive scenarios.

4. Sustainable Development (B1)

The central elements of this scenario family include high levels of environmental and social consciousness, successful governance including major social innovation, and reductions in income and social inequality. Successful forms of governance allow many problems which are currently hard or difficult to resolve to fall within the competency of government and other organisations. Solutions reflect a wide stakeholder dialogue leading to consent on international environmental and social agreements. This is coupled with bottom-up solutions to problems, which reflect wide success in getting broad-based support within communities. The concerns over global sustainable development, expressed in a myriad of environmental and social issues, results in the eventual successful management of the interaction between human activities and the biosphere. While no explicit climate policy is undertaken, other kinds of initiatives lead to lower energy use, and clean energy systems, which significantly reduce greenhouse gas emissions. Besides cleaning up air quality, there is emphasis on improving the availability and quality of water.

4.1 Key Scenario Drivers and their Relationships

4.11 Technological Development

High levels of technological development focused on achieving sustainable development leads to high levels of material and energy saving, innovations in emissions control technology, as well as labour productivity. The latter is essential to support the rapid growth in personal income, given

that a major increase in labour force participation is implicit in the equity assumptions. Technologies tend to be implemented in an industrial ecology mode, implying a much more highly integrated form of industrial production than at present. Information technology achieves a global spread, and is fully integrated into production technologies. Advances in international institutions permit the rapid diffusion of new technologies -- R&D approaches two percent of GDP.

4.12 Population and Economic Development

Population -- reaches only 9 billion by 2100 -- due to a faster than expected completion of the demographic transition arising from a large increase of women in the labour force, universal literacy, and concern for the environmental impacts of high population levels. The potential impacts of ageing populations which emerge from this low level of population growth are offset by relatively high levels of immigration, which reduce the negative impacts of ageing populations on savings and the ability of societies to adapt and implement new and cleaner technologies. This world has a faster than expected transition from traditional to modern economic sectors throughout the developing world. In addition, widespread education leads to high labour productivity, and high labour force participation. Migration serves to sustain the size of the labour force in developed countries, which helps to maintain their growth in per capita income. Developing countries experience few institutional failures, enabling them to grow at or near the historical upper bounds of experience given their per capita incomes. This yields a world of high levels of economic activity, with significant and deliberate progress being made with respect to international and national inequality of income. The current order of magnitude differences in income between developing and developed countries are reduced to a factor of two, with moderate growth continuing to occur in OECD countries. Gross World Product (GWP) reaches \$350 trillion by 2100 and average global incomes \$40,000 per capita. Economic development is balanced and, given the high environmental consciousness and institutional effectiveness, this leads to a better quality environment, with many of the aspects of rapid growth being anticipated and dealt with effectively. Active management of income distribution is undertaken through use of taxes and subsidies. The composition of final demand will evolve to a mix reflecting lower use of materials and energy, thus easing the impact of high income levels.

4.13 Equity

In this world there is a preparedness to address issues of social and political equity. The increases in equity, reflect a shift in values which, with widespread education, leads to greater opportunity for all. New social inventions, such as the Grameen Bank's micro-credit schemes, are a significant contributor to an increase in institutional effectiveness and equity improvement.

4.14 Communications, Settlement Patterns and Environment

The social innovations and effective governance rest on high levels of communication, both in a passive (i.e. TV) and active sense. Governance systems reflect high levels of consent from those affected by decisions, and this consent arises out of active participation in the governance process.

Settlement patterns arise from design, and tend to reflect a distributed, compact, city design structure. This results in high amenity levels, and the careful design and location of these cities results in a lessening of the natural disasters which plague many cities today. Advanced hazard warning systems and careful design limit the impact of such disasters. Low emission technologies, and careful management of land use, preservation of large tracts of land, and active intervention to counteract the impacts of imprudent societal actions strengthen the resilience of the ecological system.

4.2 Scenarios

4.21 Energy Resources/Technology

Energy efficiency innovations, and successful institutional innovations disseminating their use, result in much lower levels of energy use relative

to historic patterns. The forward-looking nature of societal planning results in relatively smooth transitions to alternative energy systems as conventional oil and gas resources dwindle in availability. There is major

use of unconventional natural gas as fuel supply during the transition, but

the major push is towards renewable resources such as solar and wind.

The

impact of environmental concerns is a significant factor in the planning for new energy systems.

Two alternative energy systems, leading to two sub-scenarios, are considered to provide this energy:

1. Widespread expansion of natural gas, with a growing role for renewable energy (scenario B1N). Oil and coal are of lesser importance, especially post-2050. This transition is faster in the developed than in the developing countries.

2. A more rapid development of renewables, replacing coal and oil; the bulk

of the remaining energy coming from natural gas (scenario B1R).

4.22 Scenario Quantification

Per capita incomes in the developed world are close to ___ in 2100, while average per capita income in the developing world grows from ___ % of the developed world in 1990 to ___ % in 2100. Energy per unit of output continues to fall at about historical rates in the developed countries, resulting in total energy use of ___ EJ in 2100. Rapid spread of technology from developed to developing countries enables an energy growth

of ___ percent less than GDP, resulting in total energy use of ___ EJ in the developing part of the world

An initial quantification of the scenarios in terms of population, GDP, energy use, and CO2 emissions for the two energy resource/technology scenarios is summarised in Appendix 1. The global scenario for 2100 is also summarised in the form of a snowflake diagram. All scenario quantifications are tentative and subject to revisions.

[Figure: "Snowflake" for B1 scenarios]

4.23 CO2 Emissions

The range of carbon in CO2 emissions for the scenarios is 7.5 to 20 billion tons in 2100, reflecting 3 and 2 percent per year reductions in carbon per unit of GDP

5. Divided World (A2)

In a retreat from the globalising trends of the previous century, the world

"consolidates" into a series of roughly continental economic regions. Regions pursue different economic strategies based on the resources and options available to them. Trade within economic regions increases, while

trade between regions is controlled by tariff and non-tariff barriers to support the region's economic strategy. High income regions restrict immigration and impose selective controls on technology transfer to maintain high incomes for their residents.

High income regions encourage higher levels of education to increase the productivity of their labour force. They impose restrictions on immigrants,

except skilled immigrants, to keep per capita incomes high. They also try

to impose selective restrictions on technology transfer to maintain the productivity of their labour force.

Low income regions are only able to increase per capita incomes slowly. They do not have the resources to invest in educating the labour force or in research and development. Investment from other regions is constrained.

Thus exports are primarily products manufactured with low cost labour and

some natural resource-intensive products. Population growth is high relative to high income regions. Income inequality becomes more pronounced

within low income regions and increases between regions.

Regions use non-tariff barriers, such as differences in standards and

labelling requirements, to limit trade. Trade is also dampened by differences in tastes in products. These factors favour the use of resources found within each region. Regions that have abundant coal resources but very limited oil resources, for example, encourage use of "local" coal by heavy industries and electric utilities while allowing restricting free imports of crude oil and petroleum products .

5.1 Key Scenario Drivers and their Relationships

5.11 Population and Economic Development

Fertility rates vary among regions. North America, Northwest Europe and Asia experience falling fertility rates and populations. The Middle East, Africa, and to some extent, Southern Europe and South America see rising population although the rate of growth decreases. This leads to a shift in the world population balance from the Indian sub-continent and South East Asia to the Middle East and Africa by the end of the century. World population reaches 16 billion by 2100. Regional economies emphasise self-sufficiency with wide variations in growth levels. Average global economic growth is relatively low at around 2.5%/year, leading to a GWP of \$250 trillion by 2100. Trade across regions consists primarily of raw materials and semi-finished goods in a relatively low trust world where dependence on other regions is minimised.

5.12 Government and Geopolitics

National boundaries become less important within the regions as an increasing share of policy is agreed at the regional level. This allows considerable cultural diversity within regions. Governmental style is also diverse across regions. In some, government and religion strengthen their links, in others, secular democracy is maintained or consolidated. Education is strengthened in most regions with a deepening understanding of cultural history and religion. The growing strength of the economic regions, and their competing economic interests, lead to reduced international co-operation. Global environmental, economic and social issues are subject to relatively weak governance. Conflicts between ethnic and religious groups within economic regions become less violent as a result of economic pressures on the parties. Where ethnic and religious violence persists, the groups are excluded from the economic region. Thus wars occur in the boundary zones between economic regions. Wars may also occur near regional boundaries for control of scarce natural resources.

5.13 Technology Developments

While underlying science is conducted in all regions an information about scientific developments are available world-wide, consumption and

production patterns and hence, technology and practices, are determined by local circumstances.

Research activity increases in all regions; in high income regions due to the need to increase productivity with limited regional resources and in low income regions due to the growing size of the population.

Restrictions

on transfer of some technologies to other regions is widespread.

High income regions invest heavily in education to enhance labour productivity. Some high-income regions move towards broad-based education for a knowledge-based society. Others move towards practical education (lots of science and engineering) for an advanced industrial society.

Low

income regions are not able to invest as heavily in education, but the levels (and future rates of economic growth, vary significantly).

Technological change is rapid in some regions, slow in others, with industry adjusting to local resource endowments, cultural characteristics and education levels.

5.14 Communication and Settlement Patterns

Languages become more uniform within regions, but globally more diverse. Speakers of the main world languages are fairly evenly split.

Computerised

translation eliminates the language barrier to technology diffusion and economic development.

Urban concentration continues except in Europe and North America, which move towards larger numbers of smaller cities and towns. Urban shares of population in other countries rise to current OECD levels by 2020. While there is free movement within most regions, there is very little migration

among regions. Refugee problems are confined to edge areas, for example, Baltics and Tibet.

5.15 Environmental Concerns

Environmental management follow pragmatic paths: with rising incomes, people become increasingly concerned first about urban pollution, then about regional pollution, finally about global problems. In this world, global environmental problems are discussed extensively but the will to tackle them is lacking. Propensity to worry about the environment is regionally variable. Sulphur emissions are rapidly reduced in South and South East Asia due to the impacts on agriculture but increase in Africa with exploitation of coal and minerals there.

5.2 Scenarios

Divided World is explored through a single scenario.

5.21 Resource Availability

Regions try to use their resource endowment for their economic advantage. Regions with abundant energy and mineral resources use those resources domestically and to produce exports (surplus to expected long-term needs).

Regions poor in energy and mineral resources will minimise their dependence on these resources. High-income, resource-poor regions will develop as service-based, dematerialised economies, while low-income, resource-poor regions are forced to limit their consumption of resources.

High-income regions without indigenous oil and gas undergo a near-complete conversion to an energy economy based on nuclear or renewable based electricity and synthetic gases and liquids by 2050. India and China adopt these technologies at the largely exhausting domestic coal reserves by 2050. Renewable input, zero waste industry is pioneered in South East Asia and adopted in Europe, minimising mineral and fossil fuel requirements by 2050. Oil and gas-rich regions (North Africa, the Middle East, Central Asia, Russia) continue to use fossil fuels but towards 2050 the falling cost of renewable technology (wind and biomass in Russia, photovoltaic in the other regions) begins to make them competitive even in these regions

5.22 Scenario Quantification

An initial quantification of the scenario in terms of population, GDP, energy use, and CO2 emissions is summarised in Appendix 1. The global scenario for 2100 is also summarised in the form of a snowflake diagram. All scenario quantifications are tentative and subject to revisions. [Figure: "Snowflake" for A2 scenarios]

5.23 CO2 Emissions

The level of carbon in CO2 emissions for the scenario is 15 billion tons in 2100 as only oil and gas rich regions continue to use fossil fuels.

6. Regional Stewardship (B2)

"Regional Stewardship" is based on a natural evolution of the present institutional policies and structures. As such it does not incorporate major geopolitical power shifts or fundamental technological discontinuities. There is relatively low trust, global agreements are difficult to reach and the result is 'multiple islands' with inward looking policies.

This is a world of good intentions, which are not capable of being implemented. The late 20th century value shift towards environmental stewardship continues, for example as envisioned in the Cairo and Rio Programs of Action, with increasing recognition of the importance of human welfare and inequity. These concerns cannot be tackled at a global level and are resolved regionally or locally. Environmental solutions are tempered by the desire for balance with economic goals in many areas - but poor governance means that meeting the needs of the poor and future generations is hampered by limited prosperity. Families think seriously about the fact that their offspring may be dealing

with a more ecologically stressed world, moreover one with limited financial resources for dealing with such problems. Education levels are high so that the ability of families to internalise global concerns in their family planning decisions is also high. The relative stabilisation of world population growth after 2050 leads to general optimism about the ability of society to solve problems such as food and water supply.

6.1 Key Scenario Drivers and their Relationships

6.11 Population

Both local governance and environmental concerns limit population growth. The world largely supports efforts to reduce unwanted births both as a social service but also because there is an implicit belief that even increasing populations have severe environmental consequences. Education and welfare programs for the young and illiterate are widely pursued.

Population stabilises at 10.5 billion people by 2100. Since economic growth is relatively slow, fertility rates do not decline strongly. But, the effect of fertility rate declines on lowering population size outweigh those of mortality rate decreases increasing population size.

The stabilisation of global population (largely after 2050) leads to a new atmosphere for social planning. It becomes considerably easier than at present for education, health care and pension programs. Age cohort sizes are much more stable through time than at present, although of course, overall ageing continues.

6.12 Economic Development

GWP grows to around 240 trillion \$ in 2100 with a North/South income ratio of approximately 7/1 (presently 13/1). Concerns about the ecological costs of consumerist lifestyles receive wide attention and attempts are made, first in industrial countries, but later in developing countries, to seek satisfaction through community activities rather than high consumption. Overall people are eager to find alternatives to the high income world of materialism.

6.13 Governance

Governance is weak globally but strong nationally and regionally. Deliberate policies to limit trade for environmental and social reasons hinder the transfer of technologies. However pollution trading concepts catch on as a way of driving down the costs of pollution control. International alliances occur based on particular national circumstances, such as in the development of biomass technologies. This fragmentation gives rise to pockets of environmental and social justice activists. Environmental policies vary widely across regions, for example in acceptable sulphur emission levels. NGO and public interest groups are strong, influential and busy.

6.14 Equity

While strong redistribution policies are enacted within regions to reduce income disparity, income differences between regions persist globally throughout the century and even increases in absolute terms, although the relative inequity decreases. The mechanism by which global equity increases relates in part to population dynamics: as fertility rates decline in developing countries, the decrease in youth dependency ratios leads to an increase in savings rate and strengthened economic growth during the first half of the century. In the developed regions, by contrast, ageing becomes an increasing drag on economic growth in helping to converge global incomes, concerns about the persistence of income inequality world-wide are swamped by the local concerns and conscious policies to limit international trade.

6.15 Settlement Patterns

A strong deurbanization trend occurs in this world because of increasing concern about the marginalization of the very poor that accompanies massive urbanisation. There are also concerns about managing large transient populations that migrate seasonally to cities for short term employment, for example in the construction industry.

Immigration is controlled but accepted, partly to compensate for very low fertility rates in some regions and partly to help economic development worldwide without the problems of uncontrolled globalisation.

6.16 Environmental Policy

Environmental improvement is strongly pursued although regional policies vary widely such as with sulphur controls. Marked reductions in S, CH₄, deforestation, CFCs and N₂O occur and water quality is addressed. Ecological resilience is not seen as high. The environment is viewed as quite fragile and requiring careful policy stewardship. Resource extraction is viewed as intrinsically problematic and scepticism persists regarding the ability of society to prevent environmental disasters like the Valdez oil spill and Kuwaiti oil fires. Indeed the world is increasingly sensitive about and intolerant of such events and much tension exists concerning this aspect of development. Environment groups lobby hard on these themes and paint a picture of rapidly depleting natural resources.

6.2 Scenarios

6.21 Energy Resources/Technology

Because of the concern about ecological fragility, alternative and renewable energy systems are viewed with much hope and are socially and politically encouraged. Biomass technologies and policies are invigorated.

The labour and land intensive developing countries pursue biomass production while the capital intensive developed regions develop the required technologies. A degree of co-operation coalesces about such mutually symbiotic activities.

Consumers accept a rather long return in evaluating energy-efficiency investments. Mass transit systems are very successful and profitable. Advances in transportation technology are rapid.

Hydroelectric power is a constrained bag. Dams are viewed with disdain because there are soon no more wild rivers anywhere and the rights of indigenous people have been egregiously violated. Although they are relatively clean from the perspective of carbon emissions, their effects on indigenous people (mercury poisoning of fish, etc.) becomes unacceptable. Decommissioning dams is widespread to restore pristine ecological systems downstream.

Reduction in carbon intensity is not viewed as a policy goal but it declines for other reasons. It is a frugal world with limited resource availability and so the paradigm grows that it is less costly to save energy than it is to buy it and use it. This spurs the development of technologies that use carbon more efficiently. In addition the accompanying emissions of NOx and SOx and tropospheric ozone are increasingly viewed as unacceptable.

6.23 Scenario Quantification

An initial scenario quantification in terms of population, GDP, energy use, and CO2 emissions for the scenario is summarised in Appendix 1. Energy intensity declines at a rate of 1.3%/year to a value of 0.12 toe/\$1000 in 2100. This represents a total global energy usage in 2100 of 1250 EJ, of which 300 EJ is oil and gas; 100 EJ coal and 900 EJ is non-carbon renewables, with nuclear's role limited.

The global scenario for 2100 is also summarised in the form of a snowflake diagram. All scenario quantifications are tentative and subject to revisions.

[Figure: "Snowflake" for B2 scenario]

6.24 CO2 Emissions

By 2100 CO2 emissions 11.5 GtC/year, of which 5 GtC/year is emitted by the North and 6.5 GtC/year by the South. Carbon intensity declines at a rate of 0.8%/year to 2100, to a value of 0.3 tC/toe, some 50% of today's value.

7. Scenario Comparisons

[To be written]

8. Conclusions

[To be written]

Appendix 1: Scenario Quantification

[To be written]

Attachment Converted: "c:\eudora\attach\davis.rtf"

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From: Fritz Schweingruber <fritz.schweingruber@wsl.ch>
To: k.briffa@uea.ac.uk
Subject: No Subject
Date: Thu, 12 Mar 1998 11:43:51 +0100

Dear Keith

Yesterday we had the final meeting to a national research program climate and natural catastrophies. Local authorities and Grassel, WMO summarised the major open questions on which Switzerland could work:

- Changes of Forest and treeline borders eg. subalpine, or invasion of evergreen species in the chestnut forests in the Tessin
- long term chronologies (they spoke about climate)
- seasonal chronologies
- frequency and intensity of extreme climatic events.
- amount of anthropogenic input on climate and natural catastrophies.
- reconstruction of precipitations
- influence of natural phenomena as volcanoes and el nino on climate

Nowbody said anything about growth but few were aware of the local validity of the studies made in Switzerland.

Our actual studies fit perfectly to this topics. For the future (discussion in Kopenhagen) I see the following condensation points:

-continue millennial temperature sensitive chronologies. Some money should go to Taimyr and Yamal and perhaps French Alps.

-start with a precipitation sensitive network in Eurasia. Pinus, Juniperus in a transect from Spain to Tibet including dry sites in Sibiria. Partner could be Inst. of Geography, Bonn (Jan Esper) and Birmensdorf.

-Analysis of recovery of upper timberlines in Putorana mountains in north-central Sibiria, (similar study like Shiyatov in Polar Ural). A Vice director of the Inst. of Forest in Krasnoyarsk made a little Proposal (Dr. Abramov). I have a PhD Student who make the same in the Swiss Alps near St. Moritz.

-Growth-climate studies in a test region in central Sibiria. Very good is the baikal region. There is a very steep precipitation gradient, 200mm - 1800mm in a distance of 40 km. and in accordance a steep vegetation gradient from the steppe to pine forest to Abies sibirica stands. Victor Voronin made

a little proposal) At least one valley in the Abies region in the south of lake Baikal is heavily polluted An almost identical study has been made in southern Germany(Spiecker) in a transect from Lorraine to the black forest mill,(SO2).

-Reconstruction of extreme events in Central Europe (R. Vogels thesis shows how to do it) I am convinced that we could gather much mor material across Europe. That could be a topic for a thesis. It must not be part of an EU-proposal.

Can we discuss this suggestions at Kopenhagen?

Sincerely Fritz

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Subject: meeting next week
Date: Tue, 21 Apr 1998 15:53:20 +0200
Cc: kuszko@uea.ac.uk

Dear Colleagues,

Due to the large number of participants at the Lead Authors meeting, the location has been changed from IPCC WG II TSU offices to the World Bank, H Building, 600 19th Street, N.W.

The closest metro stop to this building is Farragut West on the orange and blue lines. Take the 18th Street exit from the metro and go one block to 19th Street and then two blocks over to G Street. You will need a badge to get into the meeting, but someone will be there to help you with this. In any case, it may be a good idea to come a bit early on the first day to get checked in. The meeting begins at 8:30 a.m. Wednesday morning.

The Modelers meeting will still be held at the WG II TSU office as originally planned. That meeting starts at 8:30 a.m. on Monday morning. The address, once again, is 400 Virginia Avenue S.W., Suite 750,

Washington, D.C.

We look forward to seeing everyone in Washington.

Best regards,

Anne Johnson

Anne JOHNSON

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Subject: RE: IPCC SRES Scenario Guidelines for Authors
Date: 08 May 1998 10:50:50 +0100

Find below guidelines on how to present the IS99 storylines and scenarios. Could you the nominated authors send me your first drafts as soon as possible.

In writing up your contribution could you cover the following areas, ideally structured as follows:

1. Scenario family narrative to discuss main themes, dynamics and a diagram showing 'grand logic'

2. Key Scenario Family Drivers and their Relationships

Topics you should cover include the following:

- * population
- * technology developments
- * governance and geopolitics
- * economic development
- * equity
- * communication and settlement patterns
- * environmental concerns/ecological resilience

3. Scenarios, include reasons for branches: this section should state clearly the reasons behind selection of scenarios and review the key highlights of the scenario quantification

- * energy resources/technology, include resource availability
- * land use and agriculture
- * scenario quantification, include snowflake
- * CO2 emissions

There may be other factors you wish to add to the paper.

Regards,
Ged Davis SI-PXG Tel: 0171-934 3226 Fax: 0171-934 7406
Shell International Limited, London
Scenario Processes and Applications

From: Keith Briffa <k.briffa@uea.ac.uk>
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Date: Fri Jun 12 12:36:49 1998

>Return-path: <m.baillie@qub.ac.uk>
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>X-Sender: mbaillie@143.117.30.62
>Date: Tue, 12 May 1998 16:42:31 +0000
>To: Keith Briffa <k.briffa@uea.ac.uk>
>From: Mike Baillie <m.baillie@qub.ac.uk>
>Subject: Re: report- edit this and send an email

>
>Keith, here are some thoughts on belfast work. Come back to me on this.
>Cheers Mike

>
>10K Belfast Report.

>
>All the remaining long chronology (prehistoric) oak data from Ireland,
>England, north and south Germany (including the major Hohenheim holdings
>(2827 tree series spanning 8239 BC to 841 AD) and the Netherlands (667
>series spanning 6025 BC with gaps to 1721 AD) has now been centralised and
>screened.

>Work has been progressing on calculating running statistics on and between
>these data sets and their constituent ring patterns. Additional attention
>has been paid to attempting to understand/interpret the data in various
>ways. During the year, three principal work packages have been explored
>with respect to assessing the oak data.

>
>work package i)
>signatures

>With such a wide grid of chronologies it is possible to review the
>occurrence of years of common growth trend. Signatures are normally
>defined as those years in which 80% or more of all trees in a 'region'
>exhibit the same trend towards wider or narrower growth. All sub-regional
>and overall European signatures have been isolated and the intention is to
>re-do the 1985 analysis of Kelly et al. comparing rainfall, temperature and
>drought index data with the occurrence of widespread signatures.

>
>work package ii)
>Stepped windows of correlation

>With the availability of the raw data from each laboratory all regional
>chronologies for Ireland, Britain, North Germany and South Germany have
>been reconstructed by standard means (initially fitting a 30-year spline to
>each individual tree-ring pattern). Using these standardised chronologies,
>stepped windows of correlation have been run comparing all regions across
>time back to 5000 BC. Notable changes are observed indicating periods of
>consistent, north-European-wide similarity and dis-similarity. The
>availability of the raw data then allows interrogation of anomalies. For
>example, there is a notable fall-off in correlation between the
>standardised Irish and English chronologies at AD 775 to 825. In the past
>this would have been attributed to aspects such as a) poor replication or
>b) narrow versus wide rings. In this case examination of these aspects
>showed that neither was the cause of the poor correlation; it appears that
>English and Irish trees were responding in completely opposite manner
>during this period. Such findings have important implications for both
>identifying and interrogating such episodes throughout the record.

>
>work package iii)

>Widest and narrowest rings.

>It had always been assumed that the widest (or narrowest) ring in any tree,
>in any year, would be idiosyncratic. This assumption produced the
>expectation that the information from such extremes would be largely
>meaningless. With the availability of the raw data it is now possible to
>create new chronologies of the 1st narrowest, and or the 2nd/3rd narrowest,
>the widest, etc, rings in each year, for each region, or for the entire
>regional dataset. The result of isolating these extremes turns out to be
>surprising in that plots of the extremes show remarkable coherence. Figure
>Z shows a section of the Irish chronology constructed from the widest (and
>narrowest) raw ring widths (the narrowest values being converted to indices
>for clarity). This presentation shows the 'maximum envelope of oak growth'
>year by year through time. This is a remarkable way to demonstrate periods
>when there are no narrow rings in any trees and others where there are no
>wide rings in any trees. Extreme events such as that in AD 540 can be seen
>as an overall downturn in the ring width envelope, not just a reduction in
>mean ring width.

>
>Extreme events.

>Work has continued documenting extreme events in the European oak, and
>other, records, partly as a preliminary to the detailed comparison between
>the oak and Fennoscandian and Finnish pine chronologies. Some of the
>events appear to be of a sufficiently global character that their effects
>should be apparent in the more temperature sensitive northern pine
>chronologies. Recently preliminary work has documented declines in the

>seventeenth century and twelfth century BC and in the later fifth century
>BC. Notable declines in the 1620s and 1120s in Foxtail pine chronologies
>from the Sierra Nevada (Scuderi 1993; Caprio and Baisan 1991) suggest
>reduced temperatures around the time of spaced events in the floating
>Fennoscandian record. With several exactly-spaced events available over
>several millennia it should be possible to link the major oak and pine
>holdings, with the additional possibility of using dated English and Irish
>sub-fossil pine chronologies to confirm linkages.

>Refs
>Caprio, A.C. and Baisan, C.H. 1992. Multi-millennial tree-ring chronologies
>from foxtail pine in the southern Sierras of California. Abstract in
>Bulletin of the Ecological Society of America 73, 133.

>
>Scuderi, L.A. 1993, A 2000-Year Tree-Ring Record of Annual Temperatures in
>the Sierra Nevada Mountains, Science 259, 1433-6

>
>
>Related applications:

>
>Interhemispheric Radiocarbon Calibration
>In addition collaboration has continued on a range of topics including
>interhemispheric radiocarbon calibration. Oak samples from Ireland and
>exactly contemporaneous samples of cedar from New Zealand have been measured
>in radiocarbon laboratories in Belfast and Waikato (samples from each
>hemisphere being dated in both laboratories). This work is showing
>interesting hemispheric changes through time with implications for carbon
>cycle modellers (related paper accepted for publication).

>
>Global tree-ring responses to environmental change.
>As part of our network of collaborators, it is possible to have access to
>tree-ring patterns and related temperature reconstructions from a wide grid
>of chronologies outside Europe. An example of the power of such grids is
>provided by the observed changes during the fourteenth century AD. Here
>chronologies from the EU oak group have been combined with those from Ed
>Cook (Tasmanian Huon pine); Keith Briffa (Fennoscandian and Polar Urals
>pine); Peter Kuniholm (Aegean oak and pine) and Xiong Limin (New Zealand
>cedar). When permed (random groups of five from seven chronologies) to
>show common responses, the overall pattern exhibits reduced growth in the
>1340s, the decade of the arrival of the Black Death in Europe, see Figure.
>Such a clear environmental context for the plague has never been available
>before.

>
>Comparisons with other proxy data.

>The strict annual character of tree-ring data is only truly comparable with
>precisely dated human records. For the early fourteenth century
>surprisingly complete records exist from England for crop yields and
>prices. In an attempt to compare two different but parallel proxy records,
>namely those for tree growth and for crop prices, collaboration with
>economic historians (Prof. Bruce Campbell Econ. and Soc. Hist. QUB) has
>been initiated. Preliminary plots of robust, screened European master
>chronologies against grain prices reveals surprising levels of common trend.
>
>Innundated trees
>As part of an effort to understand physiological response of oak to
>waterlogging, 21 oaks were sampled at Garryland Wood, County Galway. These
>trees grow in a limestone area which is flooded in some winters to depths
>of 10s of metres, for durations up to months. Some of the trees exhibit
>scar damage almost certainly from bark burst during submersion. Scars
>appear to coincide with winters of higher than average rainfall. The
>fact that the trees are not submerged during the growing season means that
>they do not show the extreme dieback and micro-rings associated with trees
>left standing in permanent water, such as examples from beside Loch Lomond,
>Scotland.
>
>Publications with Grant number
>
>Baillie, M.G.L. 1996 Chronology of the Bronze Age 2354 BC to 401 BC. *Acta*
>*Archaeologica* 67, 291-298
>
>Baillie, M.G.L. 1998 Evidence for climatic deterioration in the 12th and
>17th centuries BC. in Hänsel, B. Ed. *Man and Environment in European Bronze*
>*Age*, Oetker-Voges, Kiel, 49-55
>
>Baillie, M.G.L. and Brown, D.M. 1996 Dendrochronology of Irish Bog
>Trackways. (in) Raftery, B. *Trackway Excavations in the Mountdillon Bogs,*
>*Co. Longford. Irish Archaeological Wetland Unit, Transactions Vol. 3, Dept.*
>*of Archaeology, University College, Dublin, 395-402*
>
>
>In Press (with Grant number)
>
>Baillie, M.G.L. 1998 Putting abrupt environmental change back into human
>history, *Environments and Historical Change; The Linacre Lectures*, ed. Paul
>Slack, Oxford University Press
>
>Baillie, M.G.L. 1998 *Exodus to Arthur. Close encounters with comets and*

>the fiery dragons of myth. Batsford, London.

>

>Baillie, M.G.L. 1998 A View from Outside: Recognising the Big Picture.

>Proceedings of the Joint AEA/QRA Conference, Sheffield January 1996.

>

>Baillie, M.G.L. 1998 Hints that cometary debris played some role in

>several tree-ring dated environmental downturns in the Bronze Age.

>Proceedings of the 2nd SIS Conference, Cambridge July 1997.

>

>Baillie, M.G.L. 1998 Dendrochronology. in Jones, T. and Rowe, N. Ed Fossil

>Plants and Spores: Modern Techniques. Geology Society.

>

>Other

>Baillie, M.G.L. 1998 Bronze Age myths expose archaeological shortcomings;

>reply to Buckland et al. 1997 Antiquity, (forthcoming).

>

>

>Mike Baillie

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>

>

>

From: mann@snow.geo.umass.edu
To: p.jones@uea.ac.uk
Subject: Re: Something far more interesting
Date: Wed, 17 Jun 1998 12:03:13 -0400 (EDT)
Cc: t.osborn@uea.ac.uk

Dear Phil,

Of course I'll be happy to be on board. I think the opportunity for some direct collaboration between us (me, and you/tim/keith) is ripe, and the plan to compare and contrast different approaches and data and synthesize the different results is a good one. Though sidetracked by other projects recently, I remain committed to doing this with you guys, and to explore applications to synthetic datasets with manufactured biases/etc remains high priority. It sounds like it would all fit into the proposal you mention. There may be some overlap w/proposals we will eventually submit to NSF (renewal of our present funding), etc. by I don't see a problem with that in the least.

Once the collaboration is officially in place, I think that sharing of codes, data, etc. should not be a problem. I would be happy to make mine available, though can't promise its the most user friendly thing in the world.

In short, I like the idea. INclude me in, and let me know what you need from me (cv, etc.).

cheers,

mike

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From: mnoguer@meto.gov.uk
To: scenarios@meto.gov.uk
Subject: Scenarios issues
Date: Mon, 20 Jul 1998 18:00 +0000 (GMT)

Dear colleagues,

I will like to post here some correspondence which is clearly relevant for this "scenarios discussion group" regarding some issues related to the use of the new emission scenarios, simple models, etc. Please post any comments on these issues or any other issue that you may want to raise to the following address "scenarios@meto.gov.uk".

I have added the following experts to the list posted in my first Email:

P Wagner
R Watson
J Edmonds
S Smith
G Marland

Many thanks.

Maria Noguer

Issues raised by J Mitchell:

1. There are several uses for scenarios:
 - a) Conversion to concentration using chemistry models to produce forcing curves
 - b) Forcings for GCM runs
 - c) Use in simpler models to produce global mean curves of concentrations, forcing, temperature and sea level. This would requires a simple model which is documented and calibrated against one (preferably several) climate models. The final IPCC approved scenarios will not be available until February 2000, so we should decide now on which draft scenarios to use
2. The provisional emissions will be made available imminently. These need to be evaluated as there are four basic families and many variants. How is the median scenario defined?
3. What criteria are to be set for the simpler models used for global mean projections?

Issue raised by Tom Wigley and reponses:

Date: Mon, 13 Jul 1998 11:00:54 -0600 (MDT)
From: Tom Wigley <wigley@meeker.ucar.edu>
To: Sir John Houghton <jthoughton@ipccwg1.demon.co.uk>,
Patricia WAGNER <wagner@iiasa.ac.at>,
Hugh Pitcher <hm_pitcher@ccmail.pnl.gov>,
Robert Watson <rwatson@worldbank.org>
Cc: Jae Edmonds <ja_edmonds@ccmail.pnl.gov>, Mike Hulme
<m.hulme@uea.ac.uk>,
Atul Jain <jain@uiatma.atmos.uiuc.edu>,
Fortunat Joos <joos@phil.unibe.ch>,
Richard Richels <rrichels@msm.epri.com>,
Dave Schimel <schimel@ucar.edu>, ssmith@ucar.edu
Subject: IPCC CO2 Emissions Scenarios

Dear Bob, Hugh, Naki and John,

Mike Hulme has told me something that is quite alarming about the soon-to-be-released 'IPCC' CO2 emissions scenarios. If this is correct, you/IPCC should try to remedy it as a matter of some urgency. He said that the new 'IPCC' CO2 emissions scenarios will still begin in 1990 and will not use observed (Marland) emissions for the 1990s.

You may either not realize, or not remember, that during the preparation of the SAR and (especially) TPs 2 and 4, IPCC was frequently criticized for using out-of-date emissions data that were manifestly wrong during the 1990s. It would be extremely embarrassing to be subject to the same criticism with the TAR. Indeed, since the criticism is a justifiable one, it would be inexcusable not to have responded to it.

Equally embarrassing should be the fact that, in the published literature (my 1997 Nature and 1998 GRL papers), this 'error' has already been avoided.

How can you get around this problem? Ideally, the energy-economics models need to be revised to begin in or around 2000 instead of 1990. Indeed, in talking to Rich Richels about this issue, as well as echoing my concern, he noted that his model (MERGE) is currently being updated in just this way. He also pointed out that beginning an energy-economics model run in 1990 leads to considerable 'flexibility' in 2000 emissions; when, in fact,

the 2000 emissions will already be fixed and known by the time the TAR comes out.

It is probably impossible to make this ideal type of 'fix', but a 'fix' can still be made. What you could do is just what I have done in the above two papers. This is a simple procedure that CAN be used since it is in the published literature. All I did was use observed emissions to 1996 (as far as data were available), linearly extrapolate these to 2000 (under the assumption that this was a better projection than the corresponding IS92a projection), and then use IS92a CHANGES from 2000. You may be able to improve on the second step, but this is unimportant. The crucial thing is to get the beginning years of the record to match observed emissions as far as such data are available.

The above, by the way, does not have to be applied to emissions from land-use change because of the way we deal with initialization with the carbon cycle models. We do not use historical land-use- change emissions.

You may argue that, in terms of projected CO2 concentrations, incorrect 1990s emissions have only a minor effect. This is such an obviously specious argument that I won't bother to discuss it. Not least, it will not satisfy the critics.

A parallel issue does, however, arise with the CO2 concentration stabilization profiles. The 'S' profiles are already ludicrous, since their concentrations and implied emissions already diverge markedly from observations. The WRE profiles diverge less, but still enough for me to deem that they need revising. I have, in fact, already done this. I would be happy to pass the new profiles on to IPCC.

Best wishes,
Tom

=====
>From Robert Watson on July 13:

Tom: I appreciate you bringing this critical issue to the fore - you are absolutely right that we must not look naive. I assume that Naki and Jon et al. Will deal with this while I am on vacation for the next four days.

Bob

=====
Date: Wed, 15 Jul 1998 02:18:09 +0000
From: David Schimel <dave.schimel@mpi-jena.mpg.de>

To: Tom Wigley <wigley@meeke.UCAR.EDU>
Subject: Re: IPCC CO2 Emissions Scenarios

Tom,

I raised this issue at the scoping meeting in Bad (very bad) Munstereieffel, where it was greeted with general agreement but it appeared to come as a complete surprise to many that scenarios should have a relationship to reality.

There was also general mild surprise at the degree of non GCM-community interest in following Kyoto and stabilization rather than 1% per year and similar reactions to the fact that 1% year doubles the current rate of change.

But the wind is shifting

DS

=====
Date: Thu, 16 Jul 1998 09:46:49 -0500

From: Atul Jain <jain@uiatma.atmos.uiuc.edu> To: Tom Wigley <wigley@meeke.UCAR.EDU>

Cc: Sir John Houghton <jthoughton@ipccwg1.demon.co.uk>, Patricia WAGNER <wagner@iiasa.ac.at>, Hugh Pitcher <hm_pitcher@ccmail.pnl.gov>, Jae Edmonds <ja_edmonds@ccmail.pnl.gov>, Mike Hulme <m.hulme@uea.ac.uk>, Fortunat Joos <joos@phil.unibe.ch>, Richard Richels <rrichels@msm.epri.com>, Dave Schimel <schimel@ucar.edu>, ssmith@ucar.edu

Subject: Re: IPCC CO2 Emissions Scenarios

Dear Tom,

I got the same impression from Hugh's talk during the last week Community Meeting on IA, which was sponsored by NSF. It does not matter so much whether the starting point for the scenario calculations is 1990 or 2000. The main concern is that the emission scenarios should reflect the recent changes in fossil emissions, which show a decreasing trend from 1990 to 1995 in Annex B emissions. Using projected emissions that are incorrect, rather than updating them with observed emissions, is clearly not acceptable.

I agree with you that the effects of these emissions on CO2 concentration is minor. However, recent observed emissions will have a major impact on estimates of the cost of CO2 abatement, which depend mainly on cumulative emissions rather than on concentration. It is important, especially in light of Kyoto commitments, not to produce inaccurate emission pathways that overestimate emissions from 1990-2000, since they may be used as baselines for producing cost estimates.

Cheers! Atul

=====
Date: Thu, 16 Jul 1998 08:19:22 -0700
From: "Pitcher, Hugh M" <hugh.pitcher@pnl.gov>
To: "jain@uiatma.atmos.uiuc.edu" <jain@uiatma.atmos.uiuc.edu>, Tom Wigley <wigley@meeker.UCAR.EDU>
Cc: Sir John Houghton <jthoughton@ipccwg1.demon.co.uk>, Patricia WAGNER <wagner@iiasa.ac.at>, Hugh Pitcher <hm_pitcher@pnl.gov>, Robert Watson <rwatson@worldbank.org>, Jae Edmonds <ja_edmonds@pnl.gov>, Mike Hulme <m.hulme@uea.ac.uk>, Fortunat Joos <joos@phil.unibe.ch>, Richard Richels <rrichels@msm.epri.com>, Dave Schimel <schimel@ucar.edu>, ssmith@ucar.edu

Subject: RE: IPCC CO2 Emissions Scenarios

Dear Tom et al

In setting up the MiniCAM to do the scenario work for the SRES, we tuned the 2005 energy and hence emissions numbers to reproduce the latest IEA forecast, which explicitly incorporates the slowdown in 1990 to 1995. The only problem here is that informal feedback from within Russia(Igor Bashmakov) suggests the IEA data significantly overstate the reduction in energy use. Our scenarios all go through the short term forecast for 2005 and then diverge onto alternative paths.

Getting a good handle on recent historical data and a consistent/reasonable forecast for tuning the short term aspect of the scenarios is going to be increasingly critical as we try to sort out strategies and costs of strategies. This is a separate problem from the long term scenario work, and requires rather different tools.

cheers, hugh

=====
Date: Fri, 17 Jul 1998 14:27:51 -0600 (MDT)
From: Tom Wigley <wigley@meeker.ucar.edu>
To: "Pitcher, Hugh M" <hugh.pitcher@pnl.gov>
Cc: "'jain@uiatma.atmos.uiuc.edu'" <jain@uiatma.atmos.uiuc.edu>,
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Dave Schimel <schimel@ucar.edu>, Gregg Marland <gum@ornl.gov>,
ssmith@ucar.edu
Subject: RE: IPCC CO2 Emissions Scenarios

Dear all,

I appreciate the responses regarding my concern about the new 'IPCC' fossil CO2 emissions scenarios. However, no-one seems to be willing to grasp the nettle and suggest what can be done about it. From what Hugh says, all scenarios go through the same 2005 value, so this suggests an obvious 'fix'.

(I am curious to know what this 2005 value is, and how close it is to what I used in my Kyoto papers.)

Hugh also suggests the 'IPCC' 2005 value may be open to improvement, but I presume it is too late to do this now. So ... what should be done? The obvious solution would be to use Gregg Marland's 'observed' values as far as they go, and then linearly interpolate from his latest year to 2005.

When I did my work, I had Gregg's values to 1995, and was able to make a good guess from what he told me about what the 1996 value would be. By now, 1996 should be available, and a good estimate may be possible for 1997. If so, then the linear interpolation would go over 1997 to 2005.

Do you all agree with this strategy? ... or does someone have a better idea??

I'm copying this to Gregg to see what more recent data he can provide.

Cheers,

Tom

From: mnoguer@meto.gov.uk
To: scenarios@meto.gov.uk
Subject: Scenarios - SRES description 2
Date: Fri, 31 Jul 1998 10:09 +0000 (GMT)

As promised here is the second part of the SRES description:

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/p/ecs/general/admin/ipcc-sr/corr/open process/naki-short.doc 06/26/98,
11:34 AM

From: Nebojsa NAKICENOVIC <naki@iiasa.ac.at>
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Subject: Next SRES Meeting in Beijing, 7-9 October
Date: Tue, 25 Aug 1998 17:31:41 +0200
Cc: johnson@uea.ac.uk, kuszko@uea.ac.uk, dowds@uea.ac.uk

Dear Colleagues,

Zhou Dadi has been kind enough to organize the next SRES Lead Authors meeting in Beijing, China, to be held on 7-9 October, 1998. Dadi will provide us with more detailed information on meeting logistics in the near future, and I will send out a meeting agenda as we get closer to the meeting date. Basically, there are four items that need to be discussed at the meeting: 1) SRES progress to date; 2) the open process; 3) scenario revisions and additional work; and 4) planning the final report.

Please mark you calendars for this date and RSVP to both Zhou Dadi (becon@public3.bta.net.cn) and Anne Johnson (johnson@iiasa.ac.at) as soon as possible I will be out of the office 10-26 September and will not be able to receive messages during this time.

I look forward to seeing you in Beijing.

Naki

From: Nebojsa NAKICENOVIC <naki@iiasa.ac.at>
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<J.F.Skea@sussex.ac.uk>
Subject: Next SRES Meeting in Beijing, 7-9 October
Date: Wed, 02 Sep 1998 15:01:47 +0200
Cc: Dave Dokken <ddokken@usgcrp.gov>, Rob Swart <rob.swart@rivm.nl>,
"D.J. Griggs" <djgriggs@meto.gov.uk>

Dear Colleagues,

This is a follow up on the earlier announcement of the next SRES Meeting. First, I would like to thank all those of you who have confirmed that you will join us in Beijing. Unfortunately, some of our colleagues also had to cancel due to other commitments. Attached you will find the venue of the meeting and hotel that Dadi reserved for us at a special discounted price. My proposal is to convene at 13:00 hours on 7 October and try to finish on early afternoon on 9 October so that you have some free time left for sight-seeing before we all depart.

I will soon send to all of you formal invitation letters on IIASA letter-head just in the case you need it for travel approval (unless you cancel your participation in the meantime). Dadi will send you a similar invitation letter to use in order to obtain a visa for China.

Appended is my last e-mail concerning this meeting in case you did not receive a copy. In the attachment to this e-mail you will find two letters. One is from IPCC outlining the possible role of scenarios in IPCC assessment (Microsoft Photo Editor file). It is important for our work as it indicates possible uses of new IPCC emissions scenarios. One of the agenda items at the meeting will indeed be to discuss which of our marker scenarios we recommend be used in the interim period before our scenarios are approved by IPCC in early 2000. The other letter is also from IPCC announcing the SRES web-site (PowerPoint file). The web-site includes most of the scenario variants we have developed to date. Please circulate this second letter as widely as you can because we need as much feedback from the wider community of possible users as we can obtain.

Please let us know as soon as possible whether you are planing to attend.

I hope to see you all in China.

Regards, Naki

Venue:

National Meteorological Administration (No. 46 Baishiqiao Road, Haidian District, Beijing).

Accommodation:

Olympic Hotel (No. 48 Baishiqiao Road, Haidian District, Beijing, Tel: 086-10-62176688); discounted Price: US\$65+15% service costs.

Meeting Announcement:

Dear Colleagues,

Zhou Dadi has been kind enough to organize the next SRES Lead Authors meeting in Beijing, China, to be held on 7-9 October, 1998. Dadi will provide us with more detailed information on meeting logistics in the near future, and I will send out a meeting agenda as we get closer to the meeting date. Basically, there are four items that need to be discussed at the meeting: 1) SRES progress to date; 2) the open process; 3) scenario revisions and additional work; and 4) planning the final report.

Please mark you calendars for this date and RSVP to both Zhou Dadi (becon@public3.bta.net.cn) and Anne Johnson (johnson@iiasa.ac.at) as soon as possible I will be out of the office 10-26 September and will not be

able to receive messages during this time.

I look forward to seeing you in Beijing.

Naki

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: INTAS project
Date: Wed, 9 Sep 1998 10:38:59 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,

Some days ago I came back from the Polar Ural Mountains. I was there about 30 days making photos from the points where I have made photos 35-40 years ago and evaluating the changes which were happened during this period. Unfortunately, Rashit could not be able to go to the Yamal Peninsula for collecting subfossil wood this summer as a result of deficiency of money.

I am glad that we have been successful in INTAS proposal. Financial situation in our country so terrible that we will not work successfully without support from international grants.

Yesterday I have sent by post the signed form (official power of attorney). If you have any additional information concerning this grant, please give me know.

I wish the best to you, your family and Phil.

Sincerely yours
Stepan Shiyatov

stepan@ipae.uran.ru

From: gjjenkins@meto.gov.uk
To: m.hulme@uea.ac.uk
Subject: RE: WGI emissions/scenarios conference
Date: Wed, 16 Sep 1998 09:15 +0000 (GMT)

Mike

I think the problem is the same one as in 1988 and 1994. In order to answer the question: "what is IPCC's best estimate of climate change over the next hundred years, and the uncertainties?" we need a single best estimate of emissions (plus a range of uncertainty). In the same way as modellers say "here is our best estimate of climate sensitivity plus a range" then the SRES group should do the same thing. Of course they can make all the usual disclaimers and talk about surprises just as the climate modellers do. But NOT to come up with an estimate for a Business as Usual emissions scenario (plus a range, of 6GtC to 30GtC at 2100) seems to be ducking responsibilities. "Getting away from single number answers" is very laudable scientifically, but it presents policymakers (for whom the whole IPCC exercise is undertaken) with a problem. As long as there is a central estimate and a range, the surely both communities could be happy, as they ultimately were with BaU in 1990 and IS92a in 1995?

Geoff

-----Original Message-----

From: m.hulme@uea.ac.uk
Sent: 15 September 1998 20:23
To: scenarios
Subject: WGI emissions/scenarios conference

Dear All,

Here are three comments on the questions raised by WGI TSU on 7 Sept. and by some of the other contributions to the discussion about scenarios for IPCC TAR. I am commenting from the perspective of a climate scenario constructor servicing the impacts research community:

1. The SRES Working Group have identified 4 Marker Scenarios (out of a much larger range, although these 4 largely capture the range). I think the choice is good. I do not see why some modelling centres should not be able to run all 4 emissions scenarios through their GCM. From an impacts perspective I believe this would be very desirable and would enable a fair

range of climate change scenarios to be used in impacts work using direct GCM output (without the need for scaling). And if all four Markers could be run through more than one GCM (i.e., with different climate sensitivities) then impacts work would have an even better sample of the possible climate change space to analyse. These aspects of uncertainty seem to me to be critical for impacts people (and integrated assessors) to explore, to get us away from single number 'answers'.

2. If a single emissions scenario *has* to be adopted by some GCM groups, B2 seems to have the recommendation from Naki (and maybe SRES too - the storyline refers to it as 'dynamics as usual'). I think there are probably good reasons why SO₂ emissions fall so much in this storyline - regional rather than global solutions and the encouragement of environmental protection. The fact that the reduced C emissions relative to IS92a are offset by the big fall in SO₂ emissions (the net global warming in B2 is actually slightly higher than IS92a if aerosol effects are included) should simply be seen as a reflection of a more carefully worked out storyline than was the case with IS92a. I do not think it a good idea (indeed, I think it would be a very *bad* idea) for GCM centres to mix-and-match elements of IS92 and SRES98 scenarios - the TAR should try and stick with the SRES stories and emissions wherever possible. The internal consistency in these storylines (and hopefully emissions) is important to maintain (especially later on for impacts work), and the thinking behind the SRES scenarios is considerably better than was achieved in the IS92 scenarios.

3. The problem of different Markers having different 1990 emissions values (and the fact that 1990s C emissions diverge from those observed) is more serious. By 2000 the four Markers range in C emissions from energy sources from 6.6GtC (B1) to 8.0 GtC (A1). Given where we are right now (about 6.7GtC in 1997) it seems daft to have such a range for only 2 years hence (as Tom Wigley has pointed out). For example, by the time TAR is published we will know that A1 C emissions for 2000 are too high by, say, 15%. Surely we need to impose a 'fix' on all 4 Markers to account for this. Such amendment may occur as a result of the SRES 'open-process', but this will take up to 12 months to be agreed and published. Should not someone (WGI or WGIII TSUs) impose a temporary solution now for climate modellers?

Similarly, something needs to be done for CH₄ and N₂O 1990 emissions. CH₄ 1990 emissions range from 281 to 481Tg in the 4 Markers (compared with 506Tg in IS92). Surely this range is not defensible. I think at the least we need some assurance from SRES that there has been some investigation into these differences and that they will withstand scientific scrutiny in peer review. Again, maybe the open-process may lead to revisions, but what

do climate modellers do in the meantime? [By the way, the difference in global warming by 2100 that the SRES CH4 and N2O scenarios generates relative to those in IS92a is between 0.05 and 0.3degC - lower in all cases].

Mike

Dr Mike Hulme

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Climatic Research Unit fax: +44 1603 507784
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University of East Anglia web site: <http://www.cru.uea.ac.uk/~mikeh/>
Norwich NR4 7TJ

Mean temp. in Central England during 1998 is running
at about 1.2 deg C above the 1961-90 average

The global-mean surface air temperature anomaly estimate for the
first half of 1998 was about +0.60 deg C above the 1961-90 average,
the warmest such period yet recorded

From: mann@snow.geo.umass.edu

To: p.jones@uea.ac.uk

Subject: No Subject

Date: Thu, 17 Sep 1998 10:35:12 -0400 (EDT)

Cc: coleje@spot.colorado.edu, jto@ngdc.noaa.gov, k.briffa@uea.ac.uk, luckman@sscl.uwo.ca, mann@geo.umass.edu, mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu

Dear Phil,

Thanks for your message. I've chosen to "expand" the distribution list to include a few other individuals who can better address some of the key points you raise.

A meeting in January built around the AMS meeting (which should bring people into the Boulder vicinity) sounds like a good tentative plan. Peck? I'm assuming everyone on this list is a potential attendee...

As for your general comments, they get to some essential points. The modeling community leaders are probably about as skeptical about our paleo-reconstructions as we are of their sulphate aerosol parameterizations, flux corrections (or more worrying, supposed lack thereof in some cases!), and handling of the oh-so-important tropical Pacific ocean-atmosphere interface... So my personal philosophy is that more than one side here can benefit from extending the olive branch, and there are a few individuals in the modeling community who could benefit from slowing down on the stone throwing from their fragile glass tower :)

More to the point, though, I strongly believe the paleo community needs to present an honest but unified front regarding what we all agree we can definitely, probably, and simply not yet say about the climate of the past several centuries, and plan strategies that will allow us all to work towards improved reconstructions without stepping on each others toes. There's a challenge there, but one I'm sure we can all rise to. I am grateful to Peck for realizing that the time is ripe for a workshop in which we all strategize as a group towards these ends. I believe we all go into this in "good faith", and I'm very excited about what the workshop might produce, in particular, in terms of effective long-term strategies.

I share Phil's concern about getting things "straightened out" before the IPCC report. As one of the lead authors on the "observed climate variation and change" chapter for the 3rd assessment

report, a key goal of mine will be to present fairly and accurately all of our different efforts, and the common denominator amongst them...

I also understand all-to-well Phil's concerns about free data exchange. In fact, we've been working closely w/ Peck to get every aspect of our reconstructions, including calibration/verification statistics, etc., available on-line at NGDC. The one catch w/ the paleo network is that a few of the indicators we used were provided us under conditions that they not yet be passed along (this includes, I believe, the Moroccan tree rings, and some others. And at least one important indicator--Malcolm's Yakutia record--was as yet unpublished. Not myself knowing the details of the proprietary issues involved here, I have resisted simply putting our entire multiproxy network out their for public consumption. But working w/ Peck and Malcolm, I'm sure we can do this appropriately and quickly. That's an example of a key issue that would be on the table at the workshop in question.

-----PHIL'S MESSAGE TO PECK-----

Peck,

Thanks for the comments on the paper in The Holocene ! The paper stems from work Keith and I have been doing with the Climate Change Detection group headed by Tim Barnett. It is much toned down from some of the things about paleo data that Tim and Simon Tett wanted to say. Long paleo series (either the individual ones or regional/hemispheric averages) have got to be good before these sorts of people will begin to use them and believe they tell us something about variability in the past - something that cannot be got from long control runs of GCMs.

A small meeting would be a good idea, therefore. Mike Mann knows the next few times I'll be in the US. The first possible date for him is the AMS annual meeting in Dallas in Jan 99 - maybe we can tag something onto the end of this for a day or two. I'll let you and Mike work something out on this. I'm also in the US for a meeting on Climate Extremes which is tentatively scheduled for March 9-13 in Asheville.

Prsentation of the paleo data is the key in all this. Tim Barnett was somewhat horrified by the coherency diagrams he produced (fig 9). He then produced Fig 10 from the GCM and that was not much better. Hidden between the lines of the paper is the theme that a number of us have been saying for years (especially Ray and Malcolm) that the LIA and MWE were not that global and not that different from today's

temperatures. Mike's paper in Nature reiterates this. Keith and I have been thinking of writing a forum piece for The Holocene addressing in somewhat provocative terms what paleoclimatologists should be doing with regard the detection issue and to some extent with respect to science in general - should be continue using terms like LIA and MWE for example. We hope to address many of the issues you make in your email - seasonality, consistency of the proxy through time, goodness of the proxy etc. We need to come up with some agreed strategy on this especially with IPCC coming up.

What we did in the paper was one way of assessing proxy quality. Something like Tables 2 and 4 are what is required though to inform the uninitiated (modellers) about proxy data. For use in detection at the moment a paleo series has to be a proxy for temperature. I know proxies tell us about other aspects of the climate as well, but a clear, unambiguous temperature signal is what is needed.

Some other quick answers -

- 1) Happy to send to you all the series and the hemispheric values. I hope Mike will send all his as well, but the last time we discussed this he said that some could not be made freely available. This isn't Mike's fault but there are still some stumbling blocks to free exchange of data within the various paleo communities.
- 2) We all know the quality of proxies changes with time. Trees don't have dating problems but do have the reduction in sample depths you talk about. Dendro people are much more open about this though than the coral and especially the ice core communitites.
- 3) Trees may not grow everywhere but they are more global in extent than the others. There are also many more chronologies available and this is a factor. We had much more choice there than in the other paleo groups.
- 4) Whilst we are taking bets, proxies will never be better than instrumental data. Corals will eventually extend the SOI series but never be better than it for the years after 1850. Similarly with the NAO. Instrumental data exists to extend this to about 1750 and the fact that such data is sitting out there is only just begining to be realised. A great NAO

reconstruction could be produced if the real data extended over nearly 200 years, enabling the low-frequency aspects to be considered in much more detail than ever before (a la Stahle with the SOI).

That's enough for now.

Cheers
Phil

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Web: <http://www.geo.umass.edu/climate/mike>
Phone: (413) 545-9573 FAX: (413) 545-1200

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Jenkins, Geoff" <gsjenkins@meto.gov.uk>
Subject: Re: palaeo data
Date: Fri Sep 18 12:36:19 1998

Dear Geoff

it good to hear from you. By now you may know that we had a small working meeting to consider the current draft of the thematic bid yesterday in London. Simon Tett , Nick Shackleton , Paul Valdes and I really did get to grips with a lot of the important details concerning the way in which such a project might actually run. We are going for a joint Earth science/Atmospheric Science Board application for 8 million to run over 5 years. Simon told us about your offer of some support - perhaps as money , perhaps as some equivalent- and the spirit of the offer is much appreciated. Frankly, the fact that you consider this a worthy and valid scientific exercise is what really gives me cheer. We have a long way to go to really sort out many of the problems with the palaeo data and with the methodology of using them in a validation and/or detection context, but I genuinely believe this approach will yield rewards somewhere down the line. I think our support from the earth science side is very probable. The politics of the Atmospheric Board - and the potential clash with other initiatives coming from Reading - mean that their support (in any meaningful sense) can't be thought of as more than possible. I suppose we may have something like a near 50 % chance of eventually getting some money , but 50% is pretty good. I will now ammend the document to show an explicit requirement for formal supervisory input on the programme from the Hadley Centre and I acknowledge that there will be no blanket release of data whatever happens. I will forward the application to you soon. If we get through the outline agreement stage with NERC , we will surely revisit these practical details , along with others. For now I simply say thanks to you and John for your support , and thanks for the input of Simon and Peter Cox. I will stay in touch as and when things develop. Even if we fail here, the science imperative will mean that we find other means of working with you -most likely through an EC grant - on these issues.

Thanks again and I hope you are bearing up under the strain of recent troubles

Keith

At 11:53 AM 9/14/98 +0100, you wrote:

>Keith

>

>Im afraid I dont have your original email about your proposal for a thematic

>programme on palaeo data - we just got converted to Windows NT and I have

>wiped my old emails by mistake.

>

>We would be very supportive of a programme which delivered better estimates

>of natural variability of climate over the past 1000 yrs globally and

>regionally which, as I recall, is the main aim.

>

>What do you want me / us to do, ie a letter to someone in NERC or you
from
>me/ Dave Carson/ Paul Mason saying ho w important the topic is and that
we
>would be immediate users of deliverables etc?
>
>Let me know and I will draft something. Can you re-email what you set
please
>- sorry.
>
>Cheers
>Geoff
>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: rbradley@geo.umass.edu
Subject: Re: PAGES Open Science Meeting publication
Date: Fri Sep 18 12:57:16 1998
Cc: oldfield@ubecx01.unibe.ch

Ray

this is simply to say that I will get my paper to you as soon as I can. Frank knows that I am currently involved with writing a bid on behalf of the earth science community to try to extract 8 million pounds for a 5 year project from NERC to support Palaeo/Modelling validation work. I was not allowed to say no to this request and it is involving me in a lot of meetings and associated crap. I am now redrafting the proposal. Also I must write my application to NERC for a fellowship - if this fails Sarah and I are unemployed after December as things stand. God knows there is little chance of success but the application must be in by the end of September and I have not started it yet. This is a big deal for me and I am putting you down as my primary suggested scientific referee. The PAGES paper can only be done in mid October and I really need your and Frank's understanding on this. I had to do the Thematic bid proposal as Nick Shackleton asked me to , and I want to put him down as my primary Personal reference! In early October I have to attend a NERC Earth Science Board meeting to defend the Thematic bid; a meeting of PEP3 in Belgium;a UK CLIVAR meeting in London; an EC meeting to present our ADVANCE-10K results in Vienna. This is not bullshit. I will do the PAGES meeting paper as fast as I can and you must please allow me the leeway . Sorry - but this will not really hold the publication up . If I could sort out some funding I could afford to drop some of these things but with the EC future also up in the air at the moment , I have to try to juggle these things. Sorry again Ray

Keith

At 09:07 PM 9/12/98 -0400, you wrote:

>This is a reminder that the due date for your paper to be reviewed for the
>Special edition of Quaternary Science Reviews was August 31....unless
>you
>made a special deal with me (and have sent your checks to my Swiss bank
>account) you should send me your manuscript AS SOON AS POSSIBLE!!!
>
>Thanks
>
>Ray
>
>
>Raymond S. Bradley
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>
>

From: "Jonathan T. Overpeck" <jto@ngdc.noaa.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: climate of the last millennia...
Date: Thu, 1 Oct 1998 12:17:24 -0700
Cc: k.briffa@uea.ac.uk, ray bradley <rbradley@geo.umass.edu>, mann@snow.geo.umass.edu

Hi Phil - thanks for your detailed reply to my email. I look forward to working with you and the rest of the gang to really improve the state of paleo contributions to the detection/attribution issue. The earlier we get a small group together, the better, so I suggest we try to take you up on the AMS add-on idea. It would be ideal to have a 1 to 1.5day mtg in Boulder since we have many of the needed perspectives (ice core, coral, sed, data, etc) here. What would be the best dates for you (and Keith - I'm hoping he'll be up for this too). We can find the extra \$\$ to get folks to Boulder and have a quality time (do you ski?).

Once we set the dates with you (PLEASE SEND FAVORED DATES), Mike and Ray, we can set the agenda. The main thing is that it would set the stage for the extra degree of data sharing we'll need before the planned Santorini mtg (still no dates - please bug Jean-Claude!!). Sound ok?

As for the data from your paper, I'd like to get them up with the data from the other studies on the WDC www site asap. (JUST LET ME KNOW HOW!) The White House is interested in knowing the state-of-the-art, and if we can get everything together at one www site (including data and figs), I think I can get some needed visibility for the paleo perspective. You probably know this, but Henry Pollack's Borehole view of things (similar conclusions to the other recent papers) is about to appear in Science. Although each proxy and method does have it's limitations and biases, the multiproxy view is compelling with regard to the patterns of temp change over the past several centuries. The IPCC next time around should be much stronger than last on the paleo side of things (although still not as good as it can get!).

Of course, I'll continue to work with Mike and Ray to get the rest of the individual series out into the public domain. Santorini should be the goal - not allowed on the island without coughing up data first!

Aloha and thanks again! Peck

Dr. Jonathan T. Overpeck
Head, NOAA Paleoclimatology Program

National Geophysical Data Center
325 Broadway E/GC
Boulder, CO 80303

tel: 303-497-6172
fax: 303-497-6513
jto@ngdc.noaa.gov

For OVERNIGHT (e.g., Fedex) deliveries,
PLEASE USE:

Dr. Jonathan Overpeck
NOAA National Geophysical Data Center
3100 Marine Street, RL3, Rm A136
Boulder, CO 80303
tel: 303-497-6160

From: mann@snow.geo.umass.edu
To: jto@ngdc.noaa.gov, p.jones@uea.ac.uk
Subject: Re: climate of the last millennia...
Date: Thu, 1 Oct 1998 14:28:28 -0400 (EDT)
Cc: k.briffa@uea.ac.uk, rbradley@geo.umass.edu

Hi Peck,

Thanks for ccing the message. I was talking to Ed Cook at a NASA workshop we both attended a couple weeks ago, and he also expressed quite a bit of interest in attending the mini-meeting, since he'll be going to the AMS meeting to.

When is the meeting? Do other people prefer just before or just after the meeting for the workshop. Either probably works easily well for me at this point, since I won't have teaching committments at that point.

Looking forward to us finalizing a plan!

mike

Michael E. Mann
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Phone: (413) 545-9573 FAX: (413) 545-1200

From: Nebojsa NAKICENOVIC <naki@iiasa.ac.at>
To: scenarios@meto.gov.uk, sres@iiasa.ac.at
Subject: Meeting on SRES Scenarios, 1 October 1998
Date: Thu, 01 Oct 1998 21:57:23 +0200

Dear Colleagues,

A meeting was held today on SRES scenarios during the IPCC plenary session in Vienna. The meeting was organized by David Griggs, Fortunaat Joos, Richard Moss, and Rob Swart. Also present were a number of delegates including two Co-Chairs of IPCC, John Houghton from WGI and Bert Metz from WGIII. Attached is a document with issues discussed during this meeting.

The meeting was very productive in my view, even though it was quite brief. Two key issues were discussed that are listed in the attachment: (1) incomplete information concerning SRES emissions as reported on the website, and (2) consistency and plausibility of SRES scenarios and their emissions.

(1) Incomplete information

There appeared to be a general consensus that the range of CO₂ emissions (especially energy-related ones) are in quite good agreement across the SRES scenarios once one adds the missing emissions categories to all model runs. They are also in a relatively good agreement with the ranges given in SAR.

The SRES ranges of CH₄ and N₂O emissions did not appear to be a problem in themselves, but they are considerably lower than the ranges given in SAR.

It was agreed to ask the SRES writing team to further harmonize the ranges for the base year and the period 1990 to 2000 across the scenarios for CO₂, CH₄ and N₂O. At the same time, David Griggs will contact the colleagues from WGI to inquire whether the emissions ranges for these gases as given in SAR have changed in the mean time and will inform the SRES colleagues soon about the result. In particular, he will check whether the non-energy CO₂, CH₄ and N₂O emissions ranges are still appropriate as best guess for the 1990 situation and about any new numbers about the ranges for more recent years. It was also suggested that the SRES writing team discuss the reasons for relatively low CH₄ emissions in 1990 compared with the SAR range.

Most of the SRES models do not generate CFC and HFC emissions but these emissions are important for climate models. It was agreed that David

Griggs will inquire with climate modelers whether they really need all species of these gases or whether it is sufficient to report their joint emissions. SRES team is to report whether these emissions could be added to most of the model runs and over which time-scale. Joergen Fenhann is in touch with a number of colleagues on this issue already and he is planning to make a specific proposal how to handle this question across SRES scenarios.

SRES sulfur emissions are considerably lower than the IS92 range. There are a number of reasons for this difference that were discussed at the meeting. It was decided that this exchange should continue in the future so that there is a better understanding of all issues involved. This is a new aspect of SRES scenarios that represents an important change since IS92a, a change that was also suggested by the 1994 IPCC review of emissions scenarios.

The concern raised by Hugh Pitcher (in the WGI scenario discussion group) about high productivity growth in A1 scenarios was briefly mentioned. This issue is to be settled within the SRES writing team, possibly by including the formulation of alternative scenario variants.

(2) Consistency and Plausibility

Most participants of the meeting expressed the need to have emissions trajectories that are somehow normalized for all SRES scenarios for 1990 and that have the same trends through 2000 and diverge only thereafter across different scenarios. This would meet the need of climate modelers to work with the same starting points for all scenarios they model. One suggestion was that SRES team simply takes midpoints of emissions ranges in 1990 and renormalizes all SRES emissions. Another proposal is that climate modelers suggest their preferred values for 1990 to be used in renormalization. In any case, the method that is used would need to be well documented and cited in the relevant IPCC reports. This is necessary so as not to introduce an artificial impression that there is a full agreement on base-year emissions across SRES scenarios.

There were no specific suggestions how to harmonize short-term emissions through 2000. This issues is to be discussed within the SRES writing team and within the climate modeling community in order to collect emissions data for the last years that could be used for such harmonization.

The issue was discussed of generally lower CO₂ and SO₂ emissions across the range of SRES scenarios and in particular for B2 marker. This results in

lower GHG forcing and lower "negative" SO2 forcing. The total forcing remains roughly the same as in IS92a but has fundamentally different implications especially at regional level.

Most of the climate models will be in the position to use just a few scenarios, in some case, may be just two. Possible ways of avoiding the impression that there is a "preferred" scenario were discussed and there was a consensus that somehow the message needs to be conveyed that the whole set of SRES scenarios is plausible and that there is really no single "central" case that can be compared with IS92a.

Climate models need gridded SO2 emissions while SRES models generate SO2 emissions for a number world regions. Mike Schlesinger and Steve Smith will attend the next SRES meeting and it was suggested that Mike would use his method to produce gridded SO2 emissions and that Steve would use the method proposed by Tom Wigley to do the same. This way there would be two alternative gridded emissions patterns for all SRES scenarios available to user groups.

In conclusion, it was agreed that it would be useful to organize an informal meeting where SRES colleagues could meet with potential user groups from TAR (especially from WGI and WGII). Next possibility to do so would be on the occasion of the WGI meeting in Paris, 30 November to 3 December. I am not quite sure that I got the dates right. The next communication will be more precise.

Regards, Naki

Attachment Converted: "c:\eudora\attach\sres_w~1.rtf"

From: Keith Briffa <k.briffa@uea.ac.uk>
To: stepan@ipae.uran.ru, evag@ifor.krasnoyarsk.su
Subject: INTAS, Vienna and Norwich
Date: Fri Oct 2 10:51:37 1998

Dear Stepan and Eugene (and Fritz),

I have now received contracts from The EC for the INTAS work.

I have received the real signed Power Of Attorney form from Stepan , but not from Eugene.

It seems I must have both . I am a bit reluctant to forge Eugene's signature! We will need to think about how the money should be handled . Also please all go back and look at the document I wrote and be sure you are happy with the commitment. The most important new aspect is the biomass work and I think new , or additional collections need to be taken to look at the growth of young , medium and old trees separately through time. We have very few recent young and middle age trees in recent years. We could consider using data along north/south transects (how goes the status of the Siberian Transect?).

Also, I must go to Vienna in 2 weeks to present the results of ADVANCE10K . We have a meeting of this group here in Norwich in November but I am very sorry that I have no funds to invite you to attend this. Could you afford a meeting some time , perhaps in a neutral spot where we all (including Fritz) might get together to talk about the INTAS work and future EC work? A state of the art report of progress of the Taimyr and Yamal work is needed very soon (by email), also so that I can report on it in Vienna and Norwich. I am also writing a paper for PAGES for the book of the conference in London that Rashit attended. I will include a report of both projects , hopefully with some Figures of the data distribution or plots of the some version of the curves themselves (along with others at high latitudes) . I would appreciate new copies of the full dated raw data sets , in Tucson compact format, to produce some curves in a standard style. I would like to compare changing variance through time at different wave lengths and perhaps co spectra.

As for money on ADVANCE10K, I initially was awarded 50,000ECU to be split between Krasnoyarsk and Ekaterinburg. Because of exchange rate changes , which have gone against us continually since the start of the project, this is now worth between 0.2 and 0.25 LESS than it did then. I have looked at the remaining money and I think I can give you each a final payment of between 4000 and 4500 US dollars. This is not definite - but it is pretty definite! I hope this means you may be able to do this year's fieldwork. We need to think also about how and if this should be coordinated with the INTAS work - but maybe not? How about some discussion by email regarding these points. I look forward to a quick reply.

my best wishes

Keith

From: Michael Prather <mprather@uci.edu>
To: TAR_scenarios <scenarios@meto.gov.uk>, penner <penner@umich.edu>, Prentice <colin@planteco.lu.se>, Ramaswamy <vr@gfdl.gov>, derwent <rgderwent@meto.gov.uk>, isaksen <isaksen@halo.ps.uci.edu>, ehhalt <k.sieben@fz-juelich.de>
Subject: TAR/SRES urgent use scenarios
Date: Sun, 04 Oct 1998 14:17:34 -0700

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Prather's comments on SRES emissions regarding the four WGI chapters on radiative forcing.

THIS ADDRESSES ONLY THE URGENT NEED TO GET THE CLIMATE SCENARIOS STARTED.

OVERALL: It is CRITICAL that the WGI chapters are involved in and make decisions regarding the mapping of "emission scenarios" onto "trace-gas/RF scenarios" (to then be used in generating "climate scenarios"). This is needed so that the eventual chapters will back these preliminary (and hurried) approaches and present a consistent but updated (and more complete!) set of similar RF calculations in the TAR. We should not be adding new "volunteers" to calculate these forcings as has been suggested by last week's notes until we clearly agree on the rules/algorithms..

CO2: (WGI-Ch.3)

I have not heard from colleagues on Ch. 3 regarding carbon-cycle models for these scenarios that would be consistent with their pending chapter..

non-CO2 GASES: (WGI-Ch.4)

We need to make sure that the COMBINATION of adopted "atmospheric chemistry" and emissions is consistent with recent observations. It does not mean the total burden is on emissions. Once having chosen the chemistry (i.e., 120 year "lifetime" for N2O today), however, the current emissions are tied by observations. So we will do as already stated "make emissions match observations" but must be careful in the chapter to note this.

I see no obvious need to change the OH lifetimes (CH4, HFCs) and the N2O lifetimes from the SAR. The debate over a trend in OH is important for later analysis in the chapter. The key here is for consistency with the past decade. The budget of 560 Tg(CH4)/y is thus a balanced (steady-state) budget to match abundances of about 1710 ppb, and the current increase of about 1-2 ppb/y would then add about 3-5 Tg to this amount. Thus the rate of growth of CH4 emissions in the SRES is one concern, but the absolute level in the late 1990s is the most critical.

The IPCC97 Mosier & Kroeze N2O budget stands: natural = 9.0 TgN/y and anthrop = 7.2 TgN/y. Thus ALL of the N2O scenarios need to be scaled. Is this by a time-independent offset (e.g., + 5.5 TgN/y for B2)? or do we multiply the anthropogenic by a constant factor (e.g., 3 for B2)?

HFCs cannot be included as a bulk emission values since their lifetimes are so varied. What could be done is to focus on a single one as a surrogate, e.g., HFC-134a is the dominant RF from the IS92a options calculated in the SAR. Is this still so? We need to look at the projected HFC industry as in the last WMO

report.

O3 - as part of the IPCC/Aviation assessment (under SAR, now in final government review) we spent considerable effort in calculating the changes in O3 and the associated RF. This included both changes due to aircraft alone and that due to increases in CH4, CO, NOx, VOC described in IS92a. The 3-d tropospheric chemistry models generally agreed upon the O3 changes, and it looks as though we shall be able to take the SAR to the next step and predict changes in tropospheric ozone with a community consensus. (The results were only for IS92a 2015 and 2050 atmospheres, RF's not fully analyzed for background, of order 0.2 W/m2 for 2050.)

For the AOGCM scenarios I propose that we use these 2050 delta-O3 scenarios to "deliver" a zonal, annual mean O3 RF as a simple function of latitude. It would be easier that transmitting the perturbed O3 patterns to the AOGCMs and would accomplish the primary goal of including the O3 RF. The IS92a 2050 pattern would be scaled to the amount of NOx emitted and CH4 concentration (maybe). This is probably OK for now, but of course the correlation of NOx and CO emissions in generating O3 and OH changes is "current science" that needs to be evaluated in the chapter. Also the regional aspects of CO and NOx emissions affect the O3 perturbation.

I would PROPOSE that WGI-Ch.4 define the algorithms (e.g., CH4 lifetime @ 1700 ppb plus feedback factor and how to implement it) along with the constraints of the 1990s and then let the SRES scenario builders come up with a consistent set and send these on to the AOGCMs.

SULFUR & other AEROSOLS: (WGI-Ch.5)

The AOGCMs should NOT use their own sulfur cycle for the first of the climate scenarios. There is little doubt that all will produce vastly different negative RFs and hence different regional climate response. As I remember listening to the arguments for preparing these climate scenarios, the PRIMARY goal is to assess how well/consistently we can predict future climate and especially regional changes given a set of forcings. Likewise, we do not want these scenarios generated from different time lines for CO2, CH4, and O3 because the models have different cycle for these gases. So why S? While many of these models may have scientifically excellent S cycles and include indirect impacts on cloud formation, this task (i.e., comparison of S models in GCMs) should be the second tier of experiments.

Given the primary goals of these climate simulations by the AOGCMs, it would seem best to specify a simple albedo/RF by lat-long, ONE THAT Chapter 5 of the new TAR would advocate and support in its chapter. (e.g., what is suggested by Chapter 4 for O3 above) For example, the current geographic pattern of direct sulfate forcing has been studied and will obviously be reviewed/summarized by WGI - Chapter 5; this could be scaled to total S emissions, especially since they are dropping in most of the SRES emission scenarios. It would still provide a basic test of our predictions of regional climate across the AOGCMs.

There is nothing here to develop scenarios for other anthropogenic aerosol forcings that appear to be important (i.e.,

organics and soot).

summary RF: (WGI-Ch.6)

A potential issue here is the ability to de-convolve the
emissions and RFs per sector.

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Subject: Re: climate of the last millennia...

Date: Tue, 6 Oct 1998 11:06:20 -0400 (EDT)

Dear all,

I just wanted to thank Keith for his comments. They are right on target. There is indeed, as many of us are aware, at least one key player in the modeling community that has made overly dismissive statements about the value of proxy data as late, because of what might be argued as his/her own naive assessment/analysis of these data. This presents the danger of just the sort of backlash that Keith warns of, and makes all the more pressing the need for more of a community-wide strategizing on our part. I think the workshop in Jan that Peck is hosting will go far in this regard, and I personally am really looking forward to it!

cheers,

mike.

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Jonathan T. Overpeck" <jto@ngdc.noaa.gov>, p.jones@uea.ac.uk,
mann@snow.geo.umass.edu, rbradley@climatel.geo.umass.edu,
drdendro@ldgo.columbia.edu, coleje@spot.colorado.edu, Brian Luckman
<luckman@sscl.uwo.ca>
Subject: Re: climate of the last millennia...
Date: Tue Oct 6 13:38:33 1998

Hi Peck et al.

A little late but I'd like to put in my twopence worth regarding your original message and Phil's reply. I have been tied up with a load of stuff so don't interpret my lack of speedy response as a lack of interest in these matters.

My first comment is that I agree with all of your general remarks and with your implied rebuke to Phil that we should be very wary of seeming to dam certain proxies and over hype others when we all know that there are real strengths and weaknesses associated with them all. The truth is that all of this group are well aware of this and of the associated fact that even within each of these sub-disciplines e.g. Dendro, coral etc. there is a large range of value, or concern with the external usage of our data. However, my own and Phil's concerns are motivated, like yourself, by the outside world's inability to appreciate these points and the danger that we will all be seen as uncritical or naive about the real value of proxy data. The rationale for the recent Jones et al paper, and some things that I have written in the past is to inform would be users, particularly the modellers, that there are critical questions to be addressed about how the palaeo-data are best used in a 'detection' or 'model validation' context. Many in the palaeo-community understand these issues, but perhaps there has been some reluctance to air them in sufficient depth or in the right situations where they will be heard/seen by those people who now seek to use the data. I believe that many of the modellers, having been blissfully unaware for years of the need to work with the palaeo-community, are now expecting too much. This carries the danger of a backlash as they undertake simple assessments of the palaeo-series and conclude that they are all of very little use. The problem is that as we try to inform them we may get the balance between valueable self criticism and scientific flagellation wrong. The more so when the whip is seemingly aimed at others!

There is no doubt though, that many palaeo- types are not concerned with the 'bigger issues' of climate change, so it is up to those who do, such as this group, to try to sort out some sensible approach to how we do explore the good and bad, fairly, in our collective data and how we present this to the outside world. The meeting you propose is a good way forward. If he is already not included, I also urge you to invite Ed Cook.

I hate cold feet and I don't ski so I vote for anywhere away from snow.

To answer the question about the degradation in tree-ring chronology confidence back in time - yes, we (that is several of us in tree rings, and rising out of them, in average temperature or rainfall series, have

suggested a basis for quantifying chronology error as a function of series replication and time-dependent changes in the correlations of the series that go to form the mean chronology. The problem is tricky because the error is timescale (i.e frequency) dependent also. This is just the chronology. Calculating confidence limits on reconstructions derived from one or more chronologies must take account of the regression error (again likely to be timescale dependent) while incorporating the additional uncertainty associated with the chronology. When the reconstructions are derived using a spatial transfer function (such as in canonical correlation or our similar Orthogonal Spatial Regression technique)the reconstruction at each point in the predictand network has some ,different, uncertainty relating to the error in each predictor series and the magnitude of its influence in the specific regression equation relating to that point. Finally, as regards this issue, if you have detrended or high-pass filtered the original predictor series in some way (i.e. tree-ring standardisation) , you have some potential long-timescale uncertainty around the final reconstruction which can not be represented by any analyses of the remaining predictors or their association with a relatively short instrumental predictand series. I have a half drafted paper on this which I intended to submit to Tree-Ring Bulletin - perhaps one day!

Your question about Jasper, the sample depth, in my opinion , IS responsible for the early high values. So don't put much faith in the early warmth. We have devised a simple method of scaling down the variance in average series to take account of the inflated variance that occurs when a reduced number of series are averaged - such as at the start of this chronology . We used this in our recent Nature paper looking at a possible volcanic signal in the density data averaged over the northern network. Ed has incorporated this in the latest version of his super tree-ring standardisation/chronology construction program , but it was not used in the Jasper work .

I agree that we must be careful not to appear to be knocking other proxies- even if this is not intended . We must also be explicit about where problems lie and in suggesting the ways to overcome them. I for one do not think the world revolves only around trees. The only sensible way forward is through interpretation of multiple proxies and we need much more work comparing and reconciling the different evidence they hold. Let's have more balance in the literature and more constructive dialogue /debate between ourselves.

Keith

At 02:38 PM 9/14/98 -0700, Jonathan T. Overpeck wrote:

>Hi Phil et al. - just read the Jones et al. Holocene paper (v. 8, p.
>456-471) and had a couple comments/questions....
>
>1) nice paper
>

>2) would you like to archive the reconstructions at the WDC-A for
Paleo??
>It would be great to add them to existing recent ones (Cook et al. -
>drought; Mann et al. NH temp; Briffa et al. NH temp, Overpeck et al.
Arctic
>temp). It would be ideal to get each of the 17 proxy records PLUS the
>hemispheric recons.
>
>3) regarding proxies, I wonder how much of the "quality" issue regarding
>ice cores and some other remote proxy records is due to there not being
any
>instrumental stations near them (and at the same altitude)? Also, with
>respect to coral records, I get the feeling most in the coral community
now
>think there is something "funny" about long Galapagos record (age model,
>maybe more - I think a new record is being generated). Also, many coral
180
>records (e.g., New Caledonia) are influenced by both temp and salinity
>variations. This is a solid reason why the fit of such a record to temp
>won't be as good as you'd like (or as good as a buffo dendro record). I
>think Terry Quinn is generating the trace metal data to sort temp out.
>Lastly, I've now seen a number of coral records (most not published, but
>Tarawa is an example I think) where the proxy does as well as local
>instrumental data (in this case ppt) in getting the regional signal, AND
>the local instrumental record only go back to the war. I'm guessing,
just
>between us, that ENSO recons based on proxies will soon be better than
>instrumental ones before 1950 - not just before 1850! In fact, I'd bet
on
>it (using some of the money Ray still owes Julie!). Thus, I worry that
it
>might not be wise to dismiss reconstructions on a proxy basis,
particularly
>since trees lack one important trait - they don't work for all parts of
the
>globe.
>
>4) About trees.... (Keith are you still reading?? - I sent this to Ed
and
>Brian too, since they might have insights). Has anyone examined how a
>tree-ring recon degrades as a function of sample size back in time. I
>always see the quality of dendro recons cast as GREAT vs.other proxies
(and
>they are) based on comparison with instrumental records. But, the dendro
>records usually have the best sample replication in this same
instrumental
>period, and then tail off back in time. For example, Brian's Jasper
recon
>has a sample depth of ca 28 trees in the last century, but drops off to
ca.
>5 in the 12th century and 1 (?) in the 11th century. The "quality" of
the
>recon must degrade too?? In contrast, some non-dendro reconstructions
may

>not verify as well as dendro vs the instrumental record, but they might not
>degrade with time either since the sample density doesn't change with time.
>Thus, could it be that at some point back in time, the dendro records
>degrade to the same quality (or worse) than other proxies???

>
>5) Talking specifically about Jasper, it is interesting that the 20th
>century is as warm or warmer than everything in the last 1000 years
EXCEPT
>before ca. 1110 AD. Since the sample depth before this time is 5 or less,
>how much faith should we put in those warmer than modern temps??

>
>6) I went to the trouble of all this mainly to A) get some feedback (and
>data into the WDC) and also B) to highlight that we need to extra careful
>in judging the quality of one proxy over or under another. If a well known
>group of paleo scientists suggest that, for example, corals are not that
>useful, then it might mean more years before we have a mutli-century
>record of tropical climate variability. I think it is clear that each proxy
>has limitations (and I like the table 2 idea of Jones et al), but the real
>need is to understand that each record (not just each proxy) has pros and
>cons, and that wise use requires knowing these pros/cons. Some coral, ice
>core and sediment records are no doubt better than some dendro records
>(also, for example, with respect to reconstructing low frequency variations
>in climate). I'm NOT trying to dis tree-rings, but rather to suggest more
>balance in what we all say in the literature.

>
>7) Lastly, I think there is a need to have a small workshop to put together
>an expanded version of Jones' et al. table 2, and, more importantly, to set
>some guidelines for data generators in terms of the kinds of data and meta
>data that need to be archived to ensure best use of the data (for example,
>information of the nature of the climate signal and what might bias it -
>like the salinity effect on a coral record or method of standardization on
>a dendro record). Also, we need guidelines on what info should be archived
>with a climate reconstruction (for example, are error bars available; if
>not, why not - there are often good reasons, but the interdiscipinary user

>might not get it). It might be best if the database could be upgraded,
>so
>that users would know, for example, that a proxy record or recon they
>want
>to use has some recently discovered problem or verification.
>
>I've asked Mike Mann if he'd like to help put together such a workshop
>with
>me, and I think I have some US funding for it - it would be small, with
>just a couple folks from each proxy plus some folks like Phil and Mike
>who
>are well-know users of paleo data. Like the idea??
>
>Thx for reading this far. Cheers, Peck
>
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From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Short report on progress in Yamal work
Date: Fri, 9 Oct 1998 19:17:12 +0500
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith,

I apologize for delay with reply. Below is short information about state of Yamal work.

Samples from 2,172 subfossil larches (appr. 95% of all samples), spruces (5%) and birches (solitary finding) have been collected within a region centered on about 67030'N, 70000'E at the southern part of Yamal Peninsula. All of them have been measured.

Success has already been achieved in developing a continuous larch ring-width chronology extending from the present back to 4999 BC. My version of chronology (individual series indexed by corridor method) attached (file "yamal.gnr"). I could guarantee today that last 4600-years interval (2600 BC - 1996 AD) of chronology is reliable. Earlier data (5000 BC - 2600 BC) are needed to be examined more properly.

Using this chronology 1074 subfossil trees have been dated. Temporal distribution of trees is attached (file "number"). Unfortunately, I can't sign with confidence the belonging to certain species (larch or spruce) of each tree at present.

Ring width data of 539 dated subfossil trees and 17 living larches are attached (file "yamal.rwm"). Some samples measured on 2 or more radii. First letter means species (l- larch, p- spruce, _ - uncertain), last cipher - radius. These series are examined for missing rings. If you need all the dated individual series I can send the rest of data, but the others are don't corrected as regards to missing rings.

Residuary 1098 subfossil trees don't dated as yet. More than 200 of them have less than 60 rings, dating of such samples often is not confident. Great part undated wood remnants most likely older than 7000 years.

Some results (I think, the temperature reconstruction you will done

better than me):

Millennium-scale changes of interannual tree growth variability have been discovered. There were periods of low (5000-2800 BC), middle (2800-1700 BC) and high interannual variability (1700 BC - to the present).

Exact dating of hundreds of subfossil trees gave a chance to clear up the temporal distribution of trees abundance, age structure, frequency of trees deaths and appearances during last seven millennia.

Assessment of polar tree line changes has been carried out by mapping of dated subfossil trees.

According to reconstructions most favorable conditions for tree growth have been marked during 5000-1700 BC. At that time position of tree line was far northward of recent one.

[Unfortunately, region of our research don't include the whole area where trees grew during the Holocene. We can maintain that before 1700 BC tree line was northward of our research area. We have only 3 dated remnants of trees from Yuribey River sampled by our colleagues (70 km to the north from recent polar tree line) that grew during 4200-4016 and 3330-2986 BC.]

This period is pointed out by low interannual variability of tree growth and high trees abundance discontinued, however, by several short (50-100 years) unfavorable periods, most significant of them dated about 4060-3990 BC. Since about 2800 BC gradual worsening of tree growth condition has begun. Significant shift of the polar tree line to the south have been fixed between 1700 and 1600 BC. At the same time interannual tree growth variability increased appreciably. During last 3600 years most of reconstructed indices have been varying not so very significant. Tree line has been shifting within 3-5 km near recent one. Low abundance of trees has been fixed during 1410-1250 BC and 500-350 BC. Relatively high number of trees has been noted during 750-1450 AD.

There are no evidences of moving polar timberline to the north during last century.

Please, let me know if you need more data or detailed report.

Best regards,
Rashit Hantemirov

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From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Your data- a reference?
Date: Tue, 13 Oct 1998 12:46:54 +0500
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith,
below is the list of publications concerning Yamal chronology.

References of russian articles are in three forms:

- a) original russian text. I am afraid you will be not able to read (see) it without any russian driver. Therefore, if you need this form of reference, please see attached file as well (.doc file) using attached russian font;
- b) russian words written by english letters;
- c) english translation (excuse me for my english).

1. Hantemirov, R.M. A 2,305 year tree-ring reconstruction of mean June-July temperature deviations in the Yamal Peninsula //Int. Conf. on Past, Present and Future Climate: Proc. of the SILMU conf. Helsinki, Finland, 22-25 August 1995 /Publication of the Academy of Finland 6/95.- Helsinki, 1995.- P. 124-127.

2. U`mrelhpnb P.L., Qspjnb @.^. 3243-kerm dpebeqmn-jnk|veb` pejnmqrpsjvh jkhl`rhweqjhu sqknbhi dk qebep` G`o`dmni Qhahph // Opnake{ naye{ h ophjk`dmni }jknchh (L`reph`k{ lnkndefmni jnmtpevmvh).- E{j`rephmaspc, 1996.- Q. 266-278.

Hantemirov R.M., Surkov A.Yu. 3243-letnyaya drevesno-kol'cevaya rekonstrukciya klimaticheskich usloviy dlya severa Zapadnoy Sibiri // Problemy obshchey i prikladnoy ekologii (Materialy molodezhnoy konferencii).- Ekaterinburg, 1996.- S. 266-278.

Hantemirov R.M., Surkov A.Yu. A 3243-year tree-ring reconstruction of climatic conditions for the north of West Siberia // Problems of general and applied ecology (Proceedings of young scientists conference).- Ekaterinburg, 1996.- P. 266-278.

3. Xh rnb Q.C., U`mrelhpnb P.L., L`geo` B.Q. Onbeqr|_l`k|qjhu ker. Kernohq|hgmemhi jkhl`r` m`_l`ke g` onqkedmhe rph r{q wekerh , g`ohq`mm` b cndhwm{u jnk|v`u depeb|eb. //_l`k - qnjpnbhymhv`

Pnqqhh.- 1996.- N 4.- Q. 6-7.

Shiyatov, S.G., Hantemirov, R.M., Mazepa V.S. Povest' Yamal'skich let. Letopis' izmeneniy klimata na Yamale za posledniye tri tysyacheletiya, zapisannaya v godichnykh kol'zakh derev'ev // Yamal - sokrovishchnica Rossii.- 1996.- N 4.- S. 6-7.

Shiyatov, S.G., Hantemirov, R.M., Mazepa V.S. The tale of Yamal's years [summers]. A chronicle of climate changes on Yamal during last three millennia recorded in tree rings. // Yamal - the treasury of Russia.- 1996.- N 4.- P.6-7.

I am sorry, it is difficult for me to translate properly the title of this article in the popular magazine.

4. Shiyatov, S.G., Hantemirov, R.M., Schweingruber, F.H., Briffa K.R. and Moell M. Potential long chronology development on the northwest Siberian plain: Early results // Dendrochronologia.- 1996.- V. 14.- P. 13-29.

5. B`c`mnb E. @., Xh rnb Q.C., U`mrelhpnb P.L., M`spga`eb L.L. Hglemwhbnqr| kermei reloep`rsp{ bngdsu` b b{qjhu xhpnr`u Qebepmncn onksx`ph g` onqkedmhe 1.5 r{q. ker: qp`bmhrek|m{i`m`khg d`mm{u cndhwm{u jnkev depeb|eb h kednb{u jnkmmnj // Dnj. @M.- 1997.- R. 358, 9 5.- Q. 681-684.

Vaganov E.A., Shiyatov, S.G., Hantemirov, R.M., Naurzbaev M.M. Izmenchivost' letney temperatury vozducha v vysokich shirotach Severnogo polushariya za posledniye 1.5 tys. let: sravnitel'nyy analiz dannykh godichnykh kolec derev'ev i ledovykh kolonok // Doklady Akademii Nauk.- 1997.- T. 358, N 5.- S. 681-684.

Vaganov E.A., Shiyatov, S.G., Hantemirov, R.M., Naurzbaev M.M. Variability of summer air temperature in high latitudes of the Northern Hemisphere during last 1.5 thousand years: comparative analysis of tree-ring and ice core data // Proceedings of the [Russian] Academy of Sciences.- 1997.- V. 358, N 5.- P. 681-684.

Papers in press expected to be published this year:

6. U`mrelhpnb P.L. Dpebeqmn-jnk|veb` pejnmqrpsjvh kermhu reloep`rsp m` qebepe G`o`dmni Qhahph g` onqkedmhe 3248 ker // Qha. }jnk. f.-

1998.-R. 5, N 5 (b oew`rh).

Hantemirov R.M. Drevesno-kol'cevaya rekonstrukciya letnich temperatur na severe Zapadnoy Sibiri za posledniye 3248 let // Sibirskii ekologicheskii zhurnal.- 1998.- T. 5, N 5 (v pechati).

Hantemirov R.M. Tree ring reconstruction of summer temperatures on the north of West Siberia during last 3248 years // Siberian Ecological Journal.- 1998.- V. 5, N 5 (in press)

There is English version of this journal

7. U`mrelhpnb P.L. 4309-kerm upnmknch dk _l`k` h ee hqonk|gnb`mhe dk pejnmqrpsjvhh hqrnphh jkhl`rhweqjhu hgmemhi m` qebepe G`o`dmni Qhahph. // Opnake{ }jknchweqjncn lnmhrnphmc` h lndekhpnb`mh }jnhqrel.- QOa.: Chdpnlrenhg`r, 1998.- R. 17.- (b oew`rh)

Hantemirov R.M. 4309-letnyaya chronologiya dlya Yamala i yeyo ispol'zovaniye dlya rekonstrukcii istorii klimaticheskich izmeneniy na severe Zapadnoy Sibiri // Problemy ekologicheskogo monitoringa i modelirovaniya ekosistem.- SPb.: Gidrometeoizdat, 1998.- T.17 (v pechati).

Hantemirov R.M. A 4309 year chronology for Yamal and its use for reconstruction of climatic changes history on the north of West Siberia // Problems of ecological monitoring and modelling of ecosystems.- S.Petersburg: Gidrometeoizdat, 1998.- V.17 (in press)

8. U`mrelhpnb P.L., Xh rnb Q.C. P`dhnsckepndm{e h demdpnupnmknchweqjhe d`rhnbnjh onkshqjno`elni dpebeqhm{ m` _l`ke h hu hqonk|gnb`mhe dk hgswemh dhm`lhjh keqnrsmdpnb{u }jnhqrel. // Ahnr` Ophsp`k|qjni Qsa`pjrjh b ongdmel okeiqrveme h cnknveme. Ej`rephmaspc, hgd-bn "Ej`rephmaspc", 1998 (b oew`rh).

Hantemirov R.M., Shiyatov S.G. Radiouglerodnyye i dendrochronologicheskiye datirovki poluiskopayemoy drevesiny na Yamale i ich ispol'zovaniye dlya izucheniya dinamiki lesotundrovych ekosistem // Biota Priural'skoy Subarktiki v pozdnem pleistocene i golocene. Ekaterinburg, izdatel'stvo "Ekaterinburg", 1998 (v pechati)

Hantemirov R.M., Shiyatov S.G. Radiocarbon and dendrochronological datings of subfossil wood from Yamal and their using to study forest-tundra ecosystems dynamic // Biota of [near]Ural Subarctic

during the late Pleistocene and the Holocene. Ekaterinburg, publishing house "Ekaterinburg", 1998 (in press)

9. Xh rnb Q. C., U`mrelhpnb P. L. Demdpnupnmknchweqj` d`rhpnbj` dpebeqhm{ jsqr`pmhjnbg hg `puenknchweqjncn onqekemh _pre-6 m` onksnqrpnbe _l`k // Dpebmnrh _l`k`. Rnank|qj, 1998 (b oew`rh).

Shiyatov S.G., Hantemirov R.M. Dendrochronologicheskaya datirovka drevesiny kustarnikov iz archeologicheskogo poseleniya Yarte-6 na poluostrove Yamal // Drevnosti Yamala. Tobol'sk, 1998 (v pechati)

Shiyatov S.G., Hantemirov R.M. Dendrochronological dating of shrubs wood from archeological settlement "Yarte-6" on the Yamal Peninsula // Antiquities of Yamal. Tobolsk, 1998 (in press).

I am not quite get your question about fieldwork. You mean "this year" is 1998? If so it is too late now, on southern part of Yamal yesterday was about -10 C. Next year we plane fieldwork, final decision about where and when we will make in the beginning of next year. I would like to go to Yuribey River, northward of our usual research area.

Best regards,

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Attachment Converted: "c:\eudora\attach\Timcyr.ttf"

From: Sarah Raper <s.raper@uea.ac.uk>
To: scenarios@meto.gov.uk
Subject: Scenarios Conference - Simple Models
Date: Wed, 14 Oct 1998 13:25:07 +0100

3. Use of simple climate models

3.1 Simple models used only as tools for extrapolation/interpolation GCM results to estimate the effect of different scenarios or sensitivities?

1-D UD/EBMs (upwelling-diffusion energy balance models), such as the Wigley and Raper (1992) model updated in Raper et al. (1996), in my opinion, come into this category. I along with Jonathan Gregory and Tim Osborn have completed a very detailed comparison of this and several alternative 1-D models with HadCM2 results. With the addition of a sea ice parameter the Raper et al. model reproduces well the HadCM2 results for global mean surface temperature and thermal expansion out to 2100, for several scenarios.

However, the distinction between 3.1 and 3.2 below is not clearcut. By the end of the 900 year 2xCO₂ experiment the thermal expansion for the HadCM2 model is nearly 5 times larger than that simulated by the fitted (over 1860-2100) UD/EBM, and unlike the UD/EBM shows no sign of coming to equilibrium. In our analysis we conclude that it is not immediately obvious which if either model is correct. The difference serves to highlight the uncertainty in the thermal expansion commitment. Incidentally a fitted pure diffusion/EBM gives good simulation of the HadCM2 results in both the short and long term.

3.2 Simple models used to offer independent climate predictions?

It would probably be difficult to use 2+D models for 3.1, so they may belong here.

I think, 3.1 and 3.2 serve different purposes. Both may be desirable.

3.3 Depending on the answers to 3.1 and 3.2.....

Whichever 3.1, 3.2 or both is adopted the results and the attendant simple model versus A/OGCM comparisons should be given in the projections chapter. A selection of the results should then carry over to the sea level chapter. This consistency is very important.

It is a separate question as to whether the simple climate model results should subsequently be used as scaling factors for regional scenario development in the scenario chapter.

3.4 How many simple climate models are needed...

For 3.1 in order to fit the A/OGCM results extensive comparisons using alternative parameter values/models (for example, UD versus pure diffusion) will be necessary. As well as my HadCM2 comparison mentioned above a comparison with ECHAM3/LSG results is also well underway. In both cases the work shows that it is advisable to calculate the effective climate sensitivity of the A/OGCMs for use in the simple model. We found that the effective climate sensitivity is non-constant but apparently varies with the surface temperature in these models. For this calculation and for comprehensive model comparisons a specific list of A/OGCM output is required. This includes decade ocean mean temperature profiles, a measure of the strength of the thermohaline circulation, the A/OGCM forcing change for 2xCO2 etc. I am keen to continue these comparisons specifically as input to the new IPCC assessments. Unfortunately, and I think mistakenly, the US DOE have recently decided to discontinue this line of research. An endorsement of the need for this work by the IPCC would help my attempts to acquire funding elsewhere.

For 3.2 there would be no need of tuning to A/OGCM results and many model results could be used to give a range. This would serve a different purpose to 3.1 where A/OGCM results are interpolated/extrapolated for different sensitivities and forcings.

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Subject: scenarios e-conf., session 3

Date: Thu, 15 Oct 1998 18:22:30 +0100

> 3. Use of simple climate models:

> 3.1 Simple models used only as tools for

> extrapolating/interpolating GCM results to estimate the effect of different
> scenarios or sensitivities?

> 3.2 Simple models used to offer independent climate
> predictions?

> 3.3 Depending on the answers to 3.1 and 3.2, where will
> the assessment of simple model results be located within the TAR (under the
> projections or the scenarios Chapter or under an Appendix?)

> 3.4 How many simple climate models are needed (again
> depending on 3.1 and 3.2)?

I wish to pick up on two of the points raised by Sarah Raper and Jonathan Gregory which, while not directly answering the questions posed above, need a clear position being taken upon by IPCC. These two points are:

>From Gregory

"The presentation of a wide range of scenarios and sensitivities (3.1) will be a very important output of the TAR. Tom Wigley argues that it would be inappropriate to relegate it to an Appendix. Nonetheless it is different from the discussion and assessment of models which produce the basic projections of climate change and sea-level. I think both climate change and sea-level chapters should have separate, final, sections devoted specifically to showing the full range of uncertainties and the best estimates - an appendix to each chapter. The figures given there will be brought together in the summary of the TAR."

This is a very important concern from the perspective of how Chapter 13 (climate scenarios) is written and how WGII will look over their shoulder to WGI. For many reasons which have been well-articulated elsewhere, it is too much to expect complete consistency from WGIII emissions, to WGI models and to WGII impacts - the lags in the knowledge creation and ratification are too great. However, bear in mind that most GCM results used for climate scenario construction will be 1% per annum forcing (plus a few with 0.5% forcing, stabilisation forcing or one or more of the new SRES forcings, but these latter GCM results are unlikely to feed forward into (much) impacts work in time). However, for much impacts work to be

properly assessed and interpreted by IPCC it is necessary to have used a range of climate scenarios spanning a range of risk. This is difficult, nay impossible, without resorting to simple climate model results. If WGI can Fast-track this generation of headline projections spanning a range of forcings and sensitivities, then this information may be made use of by climate scenario developers and impacts analysts. If not, then WGI (Chapters 9 and 11) will be saying one thing, and all the impacts work is in danger of saying something else (e.g. using IS92 forcings with the SAR Chapter 6 simple model projections). At worst, some careful post-hoc re-interpretation of WGII results may be necessary in light of WGI for the policymakers summary and most importantly for the Synthesis Report.

>From Raper

"It is a separate question as to whether the simple climate model results should subsequently be used as scaling factors for regional scenario development in the scenario chapter."

This is indeed a separate question and one on which Chapter 13 can and will 'assess' the science. Scaling of GCM results has been widely used by impacts/integrated assessors since CRU started using this methodology in the early 1990s. Whether or not to adopt/recommend scaling methods for the IPCC TAR was side-stepped by the TGCIA, although it was clearly stated within the TGCIA that basing all impacts work on 1% p.a. forced GCMs which represented a narrow range of climate sensitivities, would skew impacts results in a particular (and not altogether desirable) direction. Chapter 13 will also recognise this problem and will assess the pros and cons of scaling based on simple models, but given the short length of Chapter 13, its remit now is not to convert any headline simple model results from Chapters 9 and 11 into scaled regional scenarios for impacts work - by mid-late 1999 it will be too late for that anyway. So, different impact studies will now adopt different approaches, and WGII can assess the resulting science, but what will help the writing of Chapter 13 and WGII will be as clear a statement of intent (and ideally some preliminary results) of the sort of exercises that Sarah and Jonathan refer to, preferably using the new SRES emissions scenarios.

Mike

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Mean temp. in Central England during 1998 is running
at about 1.05 deg C above the 1961-90 average

The global-mean surface air temperature anomaly estimate for the
first half of 1998 was about +0.60 deg C above the 1961-90 average,
the warmest such period yet recorded

From: From <evag@ifor.krasnoyarsk.su>
 To: k.briffa@uea.ac.uk
 Subject: No Subject
 Date: Sat, 17 Oct 1998 10:09:48 +0400 (MSD)

trwcrn.rwm

Tree-ring widths (TRW) chronology:

Ident., No.	Trees, No.	Inent. N (trees)
1)	118	all living and dead 2209-years chronology
2)*	4	MAY,925,927,928, CHA044
3)*	1	CHA-H1
4)*	1	MAY702
5)*	1	NOV001
6)*	1	CHA-H6
7)*	1	NOV078
8)*	1	NOV-A02
9)*	1	CHA005
10)*	1	NOV029
11)*	5	CHA060,012,009,017,001

* - calibrated radiocarbon age

1) all living and dead 2209-years chronology

2209=N -212=I 1) 118 samples -5(13F6.0)~

23000	24000	42000	14000	27000	21000	13000	28000	20000	30000	38000	65000	58000
54000	66000	65000	16000	55000	46000	56000	53000	68000	29000	21000	48000	15000
29000	25000	32000	22000	31000	29000	18000	27000	53000	41000	35000	47000	66000
89000	52000	28000	34000	39000	33000	25000	28000	36000	32000	43000	47000	63000
49000	49000	50000	56000	40000	42000	46500	65000	28000	30500	55000	40500	44500
24500	24500	50500	6500	22500	39000	37000	54000	30000	47500	41000	23000	52000
56000	46000	35000	44000	71000	53000	73000	87000	64000	53000	44000	52000	48500
41000	45000	50000	61500	42000	48000	58500	44000	50000	78500	62500	46000	73500
45000	90500	64000	99000	64000	53500	90000	80000	45000	64000	87500	37000	55500
74500	88500	61500	58500	66000	88500	76500	116500	84500	88500	44500	70500	26000
46000	51000	15000	42000	55000	81000	76000	67000	61000	34000	28000	24000	54000
34000	46000	27000	37000	33000	53000	56000	51000	52000	52000	64000	58000	39000
48000	35000	51000	49000	37000	43000	55000	32000	39000	57000	34000	29000	45000
49000	11000	33000	45000	36000	36000	32000	32000	41000	43000	30000	15000	43000

16000 23000 50000 46000 30000 23000 10000 38000 26000 28000 26000 19000 21000
27000 27000 18000 11000 20000 12000 16000 12000 24000 16000 20000 22000 11000
28000 22017 28065 21856 12882 14098 22014 25112 35140 14161 1219 19996 13315
13517 10704 15207 19702 19134 16446 3039 17992 25257 15901 23191 23203 1000
27479 15919 11296 19473 10860 19530 15335 26299 9411 5291 31041 12069 4539
27818 7493 10411 7919 10605 9945 17887 14974 13111 12423 8397 3226 22759
13618 10784 12556 15426 18972 22968 16454 17000 19389 10860 16583 17472 16410
29186 14931 19302 13833 21143 7466 21325 6210 18981 19848 12337 19850 26400
18285 20246 31337 23294 13450 17941 34285 38733 27586 32435 25338 21392 32594
28435 35517 40156 18777 23268 28298 30149 19095 25926 42906 39255 34173 22065
29118 17902 27172 38119 37347 25090 6500 34301 26855 5941 38507 35826 14832
22651 22197 36162 32763 24581 31479 25689 34191 36718 42915 26990 26878 43824
34625 32174 57385 51360 55039 55054 37906 18168 34882 34761 41604 12657 13161
16197 31916 24132 21855 30630 36385 30745 24153 40741 30006 33620 26577 33367
26186 38229 29349 52789 47438 35978 47997 17548 51853 46033 28743 12085 27608
34020 17925 32088 34944 33101 4081 30879 17446 15978 28435 18335 35868 22251
21528 34309 2773 6384 9014 19779 23547 26701 11470 22866 13911 18834 21164
20124 10157 23354 23804 25057 14675 20483 14798 8351 21108 8335 10598 17069
23246 30087 13235 14254 15864 2164 9347 19932 7031 20000 12181 12757 3687
20469 14247 10620 8746 28494 27058 13708 17022 20529 15788 28236 10115 19326
18135 23963 15390 7162 17279 32849 31069 16989 24420 13018 25653 14928 27235
23283 18571 29915 27266 33951 24041 47844 47675 44769 46163 46952 19771 23019
38639 34723 33079 33469 21124 29181 20774 26725 29081 34518 17204 28940 37208
32775 58976 10594 42606 48863 36946 32213 41849 27432 39733 16259 35834 34341
62407 42028 44445 35859 29798 36765 23502 18434 20274 45121 21526 24560 31877
34800 38334 20428 8781 37238 19716 7604 19439 30829 32487 20464 29784 31750
31928 23184 25438 32931 32310 39233 32585 27749 35201 28107 26776 28485 12709
15027 33760 11325 31204 31662 30223 36039 40012 25509 8772 19157 35361 17630
29531 29212 31187 24300 4562 21532 31632 10503 29400 31222 25730 28030 26917
4688 12078 26173 26710 9482 10246 28444 24912 24827 28289 17974 20492 7018
21514 34516 33310 36256 44727 45114 28650 23419 33516 11778 43465 20220 25175
23955 21139 26410 28461 35890 14156 38692 4772 28678 23572 30616 34457 38619
34856 26276 23577 22361 19873 37267 34284 15317 24184 48975 37987 31429 35273
18054 43859 16763 36500 38608 21093 31207 32854 30413 13416 33594 19433 30082
19389 15758 27999 39612 44671 37417 39594 37086 28268 12974 30605 29249 37753
33663 11363 36143 21306 46288 16113 30107 18372 38803 28205 29546 14434 46587
26678 41108 43586 37374 30224 28331 31544 30825 32838 26578 33066 20678 36675
25315 28839 26035 37685 30226 24501 28528 33510 51162 13581 21995 29417 32967
23924 14920 20805 20512 25141 25598 25703 18462 17040 21751 16046 21996 18069
20342 35014 28332 35165 35442 33871 33850 27816 23579 31256 28535 12889 22552
32800 23463 18168 23192 13514 12918 18007 9645 12635 13072 21150 14148 23932
9018 12498 18710 16600 21805 7213 22851 15759 15814 15200 23895 13963 25953
17684 20987 27162 17110 30437 32360 29106 16759 32655 33595 19709 33258 6052

26222 17722 34334 39148 11789 42244 36821 1797 4814 13594 26070 12939 6916
23229 4446 10246 7540 13714 22299 20476 19088 13476 18404 3900 19064 32509
18843 22990 28820 26310 43229 39537 31840 28824 37437 49123 36642 26598 35534
22271 52498 57130 24689 41995 27017 30140 37749 57837 25520 46108 54090 49658
45089 24465 57550 46258 47711 57767 40029 55404 32947 54873 46590 58746 34993
54879 61748 27910 32067 31872 7046 36295 37264 37901 26789 30777 43434 37700
29501 43272 44470 25658 40156 29332 20015 29524 36727 36948 31928 29953 19737
41447 12328 39805 22439 26927 23239 39549 7098 15840 20929 23772 15353 28007
22955 21463 24290 8873 17708 27278 21769 28332 18403 23837 14195 28935 20013
26065 23293 17814 25742 24984 18238 28932 31088 8914 36008 13544 29850 32271
43589 40051 23543 16407 22265 30511 16002 27839 32794 22413 10217 39905 27802
20776 26814 33852 34807 22456 20637 4815 21855 37894 25930 1818 6596 23364
29193 17672 24675 23853 5993 1193 29426 28114 14413 24810 26160 25576 11685
23679 28930 27702 26763 11733 36410 22337 39023 39591 5069 35118 21200 20396
8735 31218 18536 17272 31415 7196 22859 27298 25531 19425 10399 23570 12696
8352 15032 18992 14626 15444 18765 19280 16423 13234 21223 18692 21367 30821
15418 19031 27041 18009 33393 21949 9369 17344 27753 26670 14494 37218 36654
23904 16576 15594 29869 8638 29094 10394 19081 16729 39305 24061 16216 18959
35626 30247 34454 27558 23983 33922 24609 29676 30460 18236 15331 13953 11694
24988 14321 19124 20936 4785 26340 29808 33539 20732 42390 43144 48471 35663
44234 58963 18491 38119 42704 34253 30509 45563 28242 40627 26959 19787 30831
17054 29454 14203 11907 23517 12541 22802 33360 23233 45317 36219 25209 18721
16921 19920 27720 26663 34059 49228 40157 24209 39570 35193 26808 7585 20873
18554 23309 30212 16812 20517 23079 11592 18401 30493 24638 26735 13995 36813
48920 40278 28927 47026 31865 20986 39037 34740 33252 38718 22690 19176 35577
2580 11231 25408 24867 15897 22064 19354 25936 36895 19666 28904 37001 44624
50833 37233 36536 10932 23639 22069 37132 32183 18924 14646 39770 48286 41257
61823 38685 48732 17881 14121 42920 48009 43173 31532 31883 41708 27496 35394
21644 60068 59735 39445 37137 52625 62747 31373 22840 37017 4124 18122 16422
23362 11732 27172 29596 28005 12863 26527 33936 40401 25001 19088 35345 14541
41458 29285 35867 35215 41416 40820 28276 29922 54348 48932 45189 27202 40680
22536 12274 11911 15438 31847 15518 32623 16994 6958 28295 12391 20124 15200
3872 19066 6154 16046 21994 9883 13803 18203 7738 24826 25802 41665 39420
10082 23138 36672 23293 15780 30997 32336 56083 52873 12327 57182 35586 39138
25576 20923 11457 19626 15096 25439 32855 38655 44821 46423 37342 52026 68594
56632 41091 56548 10213 47772 55599 47835 50947 38083 45772 33426 42712 34339
15275 23257 18921 15782 25821 22725 11567 21104 29535 19800 39800 27438 11278
22770 28603 19851 33342 52927 32471 27769 46087 43229 17067 37574 15950 24974
27458 24971 20471 11634 36141 41870 25253 34853 36198 40878 37941 32716 14577
17551 29037 15527 27155 30336 16565 13659 17427 17985 15333 28674 31912 33061
21280 39694 16494 20841 27794 13885 2565 19240 20764 14003 15234 17235 32861
32447 37592 43724 40821 49210 38946 15957 19545 27864 13492 27344 42029 37682
27146 11498 40925 31045 29398 27439 38022 32927 49087 49043 49449 35359 36962

24378 36666 31602 50729 24814 62188 46992 57665 8994 31133 41369 49188 49729
34232 51923 44904 33188 44207 52862 39145 33680 38792 39171 26952 39862 49060
53489 47236 46363 57434 40117 62997 50508 35887 11315 40273 26840 11259 22813
29683 23477 9655 45503 48217 39129 57846 36584 54067 27905 34950 53044 49242
6346 26682 60001 42356 39453 38095 28854 32367 43999 43182 76475 9980 26734
37163 32521 56584 52948 47244 45685 57133 54086 22626 57892 25345 31469 14592
13839 30265 43116 32693 39278 17673 47578 16717 40561 32427 46271 70501 45415
38845 32493 52634 28619 43829 41729 49462 16162 21579 36292 22750 23572 28727
33052 21509 13640 26083 3888 31372 24734 25512 27071 31571 24681 30374 24534
33169 12625 20093 53206 17146 4073 38241 27796 10701 20413 24781 13696 14691
28574 14242 17157 26778 36497 29876 10106 36171 30465 36840 30557 36331 47945
55943 49453 23943 40887 18421 35653 7226 20925 16290 39092 30054 33829 21976
27694 36015 25317 41708 28307 18924 28561 25760 21980 32029 24752 35831 31853
29046 13943 13463 29012 27598 46246 15320 14192 32116 25571 32122 37198 21309
24451 7718 29199 25351 12890 36031 18695 18575 27989 21409 22320 26659 23255
6836 11240 14685 10076 20696 16281 8961 25734 18756 22537 6889 12456 22313
13306 15308 15505 17235 10645 30885 6203 18640 26682 10589 16431 28376 17826
6304 17627 27904 30156 32025 27955 46022 22728 14528 20370 26056 21896 28926
34096 22612 41428 48536 56094 40957 53286 50459 32060 44338 44482 41154 13807
11326 3297 11426 7576 26075 16469 17875 40687 38680 42653 4189 15048 13883
31267 30324 8534 19704 27760 28691 32492 52563 38623 37560 27673 33206 25809
42342 53294 36139 40726 48492 45376 34414 24610 35000 20567 34436 15964 36710
32282 31438 38330 35359 34186 40505 38729 11085 23307 29585 45856 30278 35038
28031 61066 71654 75101 44821 39539 5015 23301 32435 16773 42902 38394 35250
38140 50031 34522 47063 39538 37625 44671 19419 26965 49777 39253 37798 39380
60435 32630 31485 47937 32777 43802 30784 47142 37691 30497 28847 43470 41648
37101 40726 16574 33858 35558 42346 32535 35480 39239 19817 17962 40171 19816
37158 48680 26345 38391 56809 20909 43281 26427 28300 10520 11234 37255 28329
33247 37494 15393 17142 30050 24568 30868 25822 26042 29408 8962 34690 23488
22172 23157 30993 11397 34005 23622 34032 29107 33019 39124 10729 45917 29799
21484 11154 19750 10963 25793 29698 16148 30739 40478 28837 11405 28409 42056
28589 36331 30851 14922 29795 35541 30907 29046 30087 31996 24960 10172 22222
47286 32457 29091 29240 24873 14528 23808 17266 29730 13252 16810 11011 21315
17198 20894 28959 19943 11296 13434 9382 17430 13696 25412 27865 23093 7885
13852 25494 22304 5032 21311 21766 32202 24233 32537 34665 21149 4541 17197
21595 10014 17248 23052 9932 26619 24058 31319 24079 32681 26048 23140 12880
14733 33067 20015 18721 29651 26843 21754 30090 35288 33385 22382 30894 14728
26071 25792 23771 32227 27265 24298 26117 10108 33626 11545 13202 32819 20454
20939 12584 32712 11446 29923 24529 21244 2000 29325 10270 18780 14979 29992
17247 22835 19369 36933 31079 14026 18997 22716 11568 16741 26364 20229 24592
20948 24879 29982 19867 19478 31888 17419 22989 28106 21737 4936 12040 15016
18961 5235 18167 24849 18367 27222 23919 14306 20386 33748 14910 22044 19999
22210 27410 29148 38037 12635 33100 44025 36026 9169 22049 10997 26327 23360

15028 14360 25476 19163 18067 32330 14489 31136 28690 24305 13269 27592 35264
13291 29446 26123 19894 18688 21564 28586 40368 33895 36981 22843 28835 25897
31387 15225 17297 21077 21867 12440 14398 19166 13061 11008 20385 14993 7768
23283 20160 17045 26833 22701 26387 23256 27723 21111 32775 7176 14600 6560
4525 15770 14353 9099 15162 24470 36183 31308 31823 19556 30681 26487 20038
29204 35066 17925 36458 32013 32462 8064 28601 25226 27308 24907 17930 24206
28880 26326 46087 11387 42678 40037 31112 25112 32453 36598 19521 23389 36012
27063 36490 35092 22232 9785 37702 38043 30604 35077 43926 47220 41646 34102
26212 39082 25302 17634 23170 24958 41060 25989 17794 6167 29321 25024 31646
23853 40694 40252 44804 27458 47022 24027 27829 24725 8566 25765 37958 26832
44567 34530 16105 27248 26055 34928 27453 39903 34871 26434 37469 24709 39487
30218 22976 28462 41952 49108 25851 32901 22448 18331 29066 10315 28571 29070
32664 32870 28318 40625 10357 35280 25849 23649 19720 8395 12389 17508 13577
16774 11858 18736 6479 12156 19628 23655 22221 18061 6689 17556 20901 29764
30796 7261 32870 14236 5948 23671 27600 21503 20273 15348 15678 27342 22366
27979 21643 19756 20343 27883 18753 21325 9415 21976 11436 37690 27274 28101
25355 33940 30386 34422 25320 52582 45733 36687 35368 37902 42693 22233 16666
45695 7105 21338 22127 26892 13168 12589 29874 19946 38389 42508 38118 44281
34808 34262 42548 20033 17134 18463 34504 32362 18734 22133 37281 30119 18316
28807 26584 45163 45681 23834 13205 14869 29485 27289 22233 23254 13266 19679
17399 43549 17745 22862 19067 10631 18321 26515 32895 29419 28948 38780 37180
30926 21697 33762 31089 41763 25857 40686 14920 39838 35513 36599 27497 43523
38081 35011 32143 40349 36135 43614 51856 50396 67195 57225 61241 41440 65260
48097 28219 48738 24261 40273 29658 36309 51236 32544 40954 36983 33193 27788
32247 29070 28358 30658 23016 35060 22024 25796 37168 21417 28881 28177 23317
24240 15012 13238 22566 26812 17797 23989 38457 13285 22011 26412 25138 40011
18164 32288 21720 33763 9829 29992 21171 21000 10000 24000 12000 19000

2) MAY,925,927,928, CHA044

296=N -670=I 2) 4 samples (MAY925,927,928, CHA044) -5(13F6.0)~

42000 76000 35000 37000 35000 27000 47000 24000 95000105000128000 94000102000
51000 37000 26000 38000 30000 35000 20000 37000 19000 16000 39000 50000 44000
78000 44000 69000 79000 66000 31000 55000 32000 34000 17000 51000 22750 26000
55000 55250 43500 37750 28250 61250 33750 55500 28750 51750 38500 42000 22250
39250 46750 41750 32500 39250 23000 34000 15000 22750 3250 9250 21000 24250
15250 26500 13250 15500 33750 50750 27500 9750 48000 67500 71500 70500 54500
59000 54500 44750 31000 23250 42250 26250 44750 54750 56000 44750105500 44000
33250 43250 30750 47250 40750 34000 33750 50750 59250 43500 72000 42000 35250
42500 37500 47750 51000 84750 47000 73000 28500 59000 56750 46000 58000 28000
23500 16000 13500 25500 27000 49500 31500 58000 80000 83000 73000 35500 74750
43750 16750 12000 43000 27750 24750 26250 45250 43250 35250 38000 49500 35000
14250 41000 44000 36500 39000 16750 54750 60250 65500 54250 36750 57250 63778
58250 88583 83250 72250 97563117313 75875 75750 76250 38000 85563 82938 92500

77313 98125 28250 33313 51688 42063 73438 49938 92375 32563 65125 48188 55125
51688 63250 48125 82688 57188 99813 74313 63750 85625 39063 58563 87750 77438
19063 79563 58750 35063 47750 36188 56750 63125 65938 49917 91833 41833 60833
55917 94083 58083 66417 84167 84250115167102750103417 52583 72333 52000 86917
92167 58167 37750 41583 43000 40333 65417 43750 58667 18667 34250 52667 77583
46917 48417 37583 54500 45833 64417 41000 28167 44417 39000 33500 23111 7167
27667 40000 14500 13833 34500 20667 28833 35333 36000 16167 29667 32667 28500
23167 32833 33667 21167 29500 18167 23500 29333 23167 19167 18750 14750 12750
27250 19500 19000 12250 8250 21000 24000 7000 21000 26000 28500 24000 18000
10500 21000 9000 10000 7000 8000 16000 3000 13000 8000

3) CHA-H1

306=N -1398=I 3) 1 sample (CHA-H1) -3(20F4.0)~

710 520 595 790 500 760 775 425 640 410 295 115 295 405 335 305 245 575 465 365
11701070 710 630 430 315 615 640 625 540 365 310 755 295 665 550 460 90 565 735
905 310 755 425 660 590 690 640 765 770 640 405 645 475 595 490 705 480 760 840
375 415 675 650 650 390 770 935 815 465 660 705 980 645 595 920 715 280 490 400
925 7501015 890 740 9201085 595 685 755 440 260 450 160 215 430 235 515 695 505
300 605 395 530 120 60 420 500 480 260 510 485 420 515 765 475 395 675 265 475
455 465 740 690 280 705 670 795 480 465 455 940 5551210 855 805 740 790 85 465
405 340 615 735 280 115 510 685 610 165 280 500 765 760 960 685 715 385 300 555
325 365 235 305 55 215 410 415 600 65 415 315 130 35 200 135 500 295 360 330
510 415 755 765 490 305 185 145 45 225 315 215 335 325 200 165 270 255 305 280
315 160 410 345 415 340 325 385 340 185 405 100 365 250 315 320 415 355 125 410
425 235 270 540 415 340 470 295 525 375 385 235 320 320 125 175 140 80 155 225
265 255 50 30 170 150 80 50 135 80 65 230 285 430 295 195 245 340 245 255
285 405 290 395 390 450 250 400 225 250 385 325 285 400 325 315 475 170 85 55
95 235 180 290 235 400 495 585 640 465 280 510 350 740 5601100 930 380 400 580
350 650 500 540 510 580

4) MAY702

270=N -2456=I 4) 1 sample (MAY702) -2(26F3.0)~

83 71101 76 62 66 89124144164 11 95 99 74 70 78 85122 55178202161102130153109
123128153124147152 68173 97131147134111 94 80106 55 72 98 22 58 28 66 76 36 73
2 65 27 32 48 43 56 39 18 43 33 17 33 36 30 31 15 39 28 37 33 27 23 32 30 16
29 21 17 25 29 18 35 35 36 15 28 25 26 23 21 26 25 16 14 16 21 31 26 2 20 16
30 32 26 23 25 40 9 20 35 41 17 12 20 17 13 26 18 25 13 29 20 12 23 15 14 10
10 19 11 15 12 17 12 10 16 9 10 2 18 14 10 16 5 13 5 4 9 7 12 10 19 21
13 16 14 32 6 16 27 18 14 9 18 7 11 21 17 11 13 5 16 14 21 17 14 12 14 19
16 18 15 21 22 17 13 26 4 21 7 9 14 7 23 26 29 8 15 17 13 18 13 12 12 12
13 16 16 16 7 21 6 7 4 16 18 5 10 11 9 24 9 18 13 10 11 7 3 12 5 9
11 7 11 10 12 40 34 9 16 2 10 13 13 2 8 5 2 6 3 9 2 8 4 6 8 10
6 3 14 6 13 9 6 2 5 9

5) NOV001

246=N -2923=I 5) 1 sample (NOV001) -2(26F3.0)~

2 24 4 46 49 46 31 20125114115 71 33115148130 81 58 75107104 57119179106182
169117127160187162143170102174 60112 93 34 17 72 76 86100 94109125137 62104133
139 89 99 61 92 40 94 67 16 93 86136 90 60 60 40 78 79 79133 74 81120159 82103
70 52 72 36 83 65 39108 68 79142127 56 83116138133 62 71 51 77 49113128103158
106 51 54 71 88 70149 60 14 26 43 23 89 35 64100 84 67108 78 48 52 44 22 52 52
57 13 64 29 43 22109 71 47 37 89 74 93 82 29 52 50 34 64 53 16 8 32 19 38 18
20 46 40 36 49 15 17 47 43 15 19 31 49 26 29 36 19 25 53 8 36 35 52 46 22 29
26 43 31 42 22 14 46 48 17 30 49 17 60 51 48 43 32 42 33 21 21 13 28 24 20 38
40 73 37 36 41 48 47 13 73 28 45 24 46 18 34 33 13 59 21 38 51 22 28 24 31 28
25 17 10 10 16 23 14 26 14 20 11 18

6) CHA-H6

345=N -3178=I 6) 1 sample (CHA-H6) -2(26F3.0)~

71 90 55 99 41 94 87138157143113 98188184168144147136 66 91 65 26 95 87 62 58
93 21 50106 79 61 68 50 85 21101 68 96 73 94 84 65 71 78 46 91 81 79 64 73 33
49 39 71 42 82102 67 23 26 49 11 55 60 71103 91 65 61 68 38 42 47 42 50 33 37
63 50 62 90104 87 26 58 72 52 17 9 32 22 18 44 67 78 40 76 29 62 63 57 29 4
20 31 30 16 31 51 55 52 42 28 15 50 72 58 73 59 71 67 34 29 48 29 51 41 61 20
31 11 34 43 40 31 45 19 44 39 48 56 29 41 11 40 44 30 40 27 32 58 5 81 18 16
31 6 38 6 44 67 15 52105 63 97 67 33 29 43 47 87 70 39 76 63 79 54 83 33 43
57 4 24 55 85 68 72 75 40 44 27 42 29 54 67 43 47 31 33 19 4 20 26 34 38 47
13 17 30 24 38 5 20 19 15 12 29 19 43 25 24 31 4 20 19 20 3 34 2 52 26 42
28 46 31 42 36 17 31 6 28 34 64 35 33 34 7 22 14 31 7 22 5 20 7 15 4 15
13 2 37 24 8 22 34 32 19 27 31 56 27 2 28 10 21 37 18 20 9 27 18 27 9 7
1 11 5 27 26 36 52 40 50 42 14 23 4 25 10 38 26 40 56 35 72 38 74 80 32 42
39 20 14 28 25 8 23 28 23 44 29 54 79 28 29 36 39 45 86 94 11 51 8 3 28 5
13 15 11 6 18 1 26

7) NOV078

299=N -3358=I 7) 1 sample (NOV078) -2(26F3.0)~

55 86139 68 20 40136142152115153161154170 95134136113106101 83119184 81166118
92 18 78160117118 84 90132114 43112123 60 52 34 30 30 8 52 9 21 30 13 49 3
58 52 18 25 22 33 24 60 27 44 32 39 18 33 43 60 72 81 75 67116 87 25 81 38 41
41 71 80 93 53 34 78 67 75 82 48 66 18 49 36 41 21 6 10 44 90 53 23 63 98 33
68 83 50104 88 70 66 60 82 65 41 80 88111 41 45 48 60 29 47 46 50 58 73 50 90
39 73 46 68 27 68 93 55 51 83 80 40 43 72 23 40 77 86 91 60 67 47 20 20 32 50
32 37 41 30 31 30 23 19 40 12 27 46 56 58 31 30 20 16 33 30 48 25 22 36 41 50
24 42 28 20 46 44 19 18 25 23 22 8 37 42 25 6 22 10 20 18 25 29 20 22 23 13
17 18 36 20 33 32 6 25 26 37 37 33 16 27 38 18 45 41 29 44 42 57 27 53 17 16
32 17 7 20 22 20 17 22 7 14 7 18 16 6 18 25 24 19 34 14 14 17 15 10 29 24

40 53 10 47 29 15 35 27 39 32 22 63 28 49 50 64 47 21 34 9 29 27 12 21 34 20
43 31 34 31 32 14 41 59 42 34 25 33 24

8) NOV-A02

286=N -3457=I 8) 1 sample (NOV-A02) -5(13F6.0)~

83000 84500 67500 62000 50500 64500106500 96500 75500 82500 83000 83000 84500
94000 73000 73000 64500 72000 75500 94000 93000 52667 66667 56333 53000 57000
35333 46000 8000 5500 24000 32500 30000 19000 22000 37000 27500 37500 22500
29500 33000 32500 54500 70500 42000 61000 69000 84000 68000 73500 52000 70500
77000 91000112500 59000 14500 80000 47000 74500 64000116500 56500 88000 89500
54500 56000 83000 58000 21000 67500 86500 85500 97000 86000 94500109000 70500
65500 52000 82500 50500 39500 48500 49500 55000 54500 57000 47500 45000 66000
77000 78000 76000 54000 68000 58500 21000 28000 14500 46500 29000 48500 37000
41500 19000 28000 29500 31000 38500 22000 11500 28500 25500 28000 27500 34000
22000 30000 62500 49500 38500 38000 47000 43000 46500 39500 39000 44000 40500
45500 38500 74500 38500 42000 22500 30000 46000 41000 22500 37000 31500 19500
4000 12500 26000 32000 43000 37500 43000 53000 72500 62500 46000 58500 7000
25500 40500 51000 64000 89000 70000 81000 47500 77500 20500 70000 84000 71000
76000 56000 54500 76500 59500 35000 51000 62500 39500 41500 28500 48000 23000
25500 28500 36000 4000 21000 20000 13500 6500 12500 5500 21000 14500 21500
14000 5000 12500 2000 32500 28000 26500 29000 9000 29000 37500 22500 14000
41000 22000 1500 5000 23000 11500 19000 20000 26000 24000 29000 15000 11500
28000 21500 26500 42000 22000 22000 8500 22000 18000 8500 7500 16500 20500
30500 18500 39500 22000 17000 28500 21000 30000 49500 35500 54000 34500 65500
53000 55500 44500 43500 75000 76000 56000 63500 39500 37000 10500 38000 48000
53000 67500 82000 71000 89500103500 85500 83000107000 67000105500117500 78000
123000139500 97500122000 99500 78500 60000 69000 76000 66500 67500 44500 11000

9) CHA005

198=N -3513=I 9) 1 sample (CHA005) -2(26F3.0)~

28 66 47 28 20 50 50 36 44 38 29 38 25 22 19 17 10 18 9 16 9 10 16 19 18 19
13 14 16 12 10 22 17 17 23 34 38 40 37 67 92 56 41 52 60 47 57 52 77100 90103
80 49 50 56 38 47 34 44 25 31 47 65 94 91 39 29 62 40 60 44 34 33 43 41 49 34
63 56 38 43 44 41 33 38 37 38 48 30 46 31 15 13 16 30 41 43 51 50 43 56 69 67
30 37 52 59 43 44 53 43 64 52 40 47 17 34 35 35 52 26 32 52 43 44 16 10 37 44
28 39 33 39 38 56 27 58 33 58 79 67 38 24 38 30 38 39 44 19 34 32 28 25 29 27
25 30 57 55 40 34 47 49 51 37 34 35 24 17 28 35 43 38 56 62 88 79 81 69 85 38
60 73 78 52 73 38 53 81109121 93 85124116145141

10) NOV029

306=N -3634=I 10) 1 sample (NOV029) -2(26F3.0)~

129159235264201202138213132154 98111136129125115106 62100126101107108104175111
43 15 47 53 54108 83119 57 64 81 71 74 44 30 72 82 43 38 82 43 41117 98 98102

68 74 88 57 47 78 61 94124168 58 41 32 51 45 44 33 37 35 33 19 62 51 65 78132
77 90 94 79 60 60 21 16 8 21 57 61 45 67 47 64 21 53 58 59 86 50 62 60 52 27
74 73 76 61 52 67 45 30 27 25 17 12 11 2 12 9 29 12 23 17 9 18 2 35 17 31
58 41 67 50 52 22 60 40 13 42 28 31 46 60 34 37 23 31 55 32 59 53 27 37 18 36
23 27 27 13 8 34 35 24 23 27 20 13 28 33 17 42 31 37 32 35 38 35 35 52 42 54
33 35 36 45 19 20 20 18 32 33 26 46 30 53 24 55 25 46 57 39 35 69 55 37 42 41
22 34 59 51 49 53 67 46 19 26 47 45 45 60 46 25 39 47 38 24 47 21 30 46 34 57
30 18 21 18 39 28 34 21 26 26 15 32 16 29 25 13 33 28 29 17 25 14 31 14 39 37
33 3 23 5 25 13 19 25 14 26 31 13 7 8 36 15 22 21 3 20 19 24 24 23 13 17
35 18 26 24 13 31 30 37 17 23 17 10 16 12 7 21 13 12 9 9

11) CHA060,012,009,017,001

685=N -3964=I 11) 5 samples (CHA060,012,009,017,001) -5(13F6.0)~
29500 20500 15000 24000 33000 36500 10000 11000 12500 13500 10500 5500 7000
7000 10000 4500 3000 6500 9500 16000 13000 16000 5500 7000 9000 11000
13500 22000 15000 15500 13500 12500 4500 6000 7000 9000 21000 23000 42000
14000 17000 5000 5000 14000 22000 21000 17000 15000 12000 15000 17000 16000
10500 10000 15000 6000 22000 8500 17500 15000 34000 27000 12500 9500 14000
14000 13500 9500 15500 17000 9000 5000 8000 8000 8000 9000 7500 4500
7500 12500 15000 26500 19000 20500 32000 39500 23500 35000 29000 27000 19000
21000 20500 22500 24500 26500 23500 41500 32500 43000 56000 33000 44000 77000
45500 59000 29000 55500 35500 24000 41500 51500 48000 44500 42500 48500 33000
26500 22000 30500 30500 32000 50500 40000 29000 11000 19000 21000 13500 25500
25500 30000 5000 5500 18500 6500 31000 14000 35000 32500 15000 42000 50500
57000 22500 50000 57000 53000 64099 21470 9631 18304 35842 46483 57075 55743
62066 81774 72528 56319 37556 34971 50015 39598 21283 53422 56443 68633 77002
39117 41629 35335 29859 38102 46170 39393 53294 51532 57480 43041 48908 45052
22796 30368 71920 47418 38804 16721 18342 30597 39246 54877 44497 63724 47343
56569 41014 35417 57015 38640 55746 40256 38815 28450 28771 35747 40459 40367
43102 37881 33733 53481 52421 41144 57534 49544 62108 48135 32065 49386 40716
19883 31000 43000 54000 65000 30000 19000 53000 34000 31000 51000 38000 30000
39000 54000 31000 41000 24000 5000 26000 30000 37000 28000 27000 36000 52000
51000 83000 94000 57000 27000 28000 40000 34000 53000 47000 35000 30000 51000
60000 53000 45000 26000 22000 15000 47000 40000 41000 36000 29000 41000 32000
30000 46000 28000 21000 44000 47000 61000 26000 39000 31000 23000 27000 34000
17000 17000 14000 22000 26000 27000 42000 39000 37143 36479 34282 15973 46985
41586 45817 35541 34462 33297 57851 38141 39830 58005 52402 64245 61268 95274
77879 79103 44527 73461 67818 51382 66915 48836 58044 48542 60188 50493 34297
21814 30343 27318 19330 31028 37674 22448 25890 20938 27414 34284 36175 22814
22155 18932 34119 28429 46027 39944 28606 37674 58716 57737 33924 59131 47706
67784 57924 47264 56184 23589 35398 50320 59990 41211 56298 48331 56917 46614
60352 73078100871 72100100826 46340 71674 67785 70748 65034 57059 83787 82437
97654102262 97603 96917110019 72257 67592 83499 86173107194 53370 83050 71618

72105 62601 70925 75670 71983 92814 83718 69543 58714 54920 59474 43291 66602
48121 79532 69034 25023 50577 63493 77587 77307 56182 45723 67844 49108 75721
46890 84507 77881 70337 46438 52629 60915 50684 57532 58031 43993 32527 61223
52640 49079 42544 53483 61960 79030 66823 73806 32689 35046 18242 62750 55673
61686 52388 77760 33551 53130 63936 65666 51292 68383 50993 61192 60891 43838
56876 51626 58651 36797 26491 52839 38990 52762 33637 22651 49848 53290 66765
72486 53265 79909 30593 34434 40624 45162 24607 27409 45092 66972 71704 67281
33133 53007 43198 57953 62357 38773 42726 57282 60859 38621 38300 31630 48192
51651 34748 43513 36436 50128 54668 29234 31987 36751 31569 37721 13337 42200
40125 59482 44299 19273 30587 26770 18675 28352 53830 37686 33647 20975 22003
24719 35767 26587 26669 18037 25899 19415 22622 31868 31603 11966 28692 25282
11026 24117 27808 12843 15031 12381 21029 14078 17673 24989 21396 13818 36290
32305 30660 18314 40216 43074 55488 30400 52655 48880 80052 64740 40598 78201
38192 61936 43419 22177 17147 17388 5300 24236 32535 13552 16430 13265 21525
11911 36666 10407 31224 29079 21922 39323 27000 26000 45000 41000 55000 41000
57893 51388 45397 21782 37218 35585 31277 19650 31069 5221 17963 30678 39867
11885 34455 16000 12500 12500 31000 47500 19000 33500 24500 13000 42000 29000
17000 44000 37000 34500 31500 55500 28500 32500 32000 25500 17000 26000 29000
10500 27000 34000 32000 14000 11000 24000 27000 8000 14000 10000 16000 24000
18000 26000 20500 26000 35500 48500 42500 38000 43500 29500 32500 44000 47000
56000 55000 74500 66000 67000 80000 31000 52000 64000 81000 34000 39000 17000
43000 29000 61000 60000 47000 65000 79000 114000 22000 68000 59000 113000 54000
103000 106000 110000 58000 82000 107000 133000 180000 178000

tem-rcs.rwm

Temperature reconstructed:

- 1) Early summer temperature reconstructed, RCS-RES chronology
- 2) Early summer temperature reconstructed, RCS-RES chronology (5-years moving average)
- 3) Annual temperature reconstructed, RCS-chronology (5-years moving average)

1) Early summer temperature reconstructed, RCS-RES chronology

$$2072=N \quad -77=I \text{ TJJ} \quad -4(13F6.0)\sim$$

150043131332106186 94108 55646 60349 66041119600 86633105443 73367 90395 86782
 117175117224102770101186 98365116284103958 74753 94355 75545106681103513 82673
 95246111730 74902 91385117818 77971 77228104255107077 48370 92672109750 93019
 95197 84505 85990103166104602 81089 59210111680 63814 83614128906111334 82673
 70942 52181110096 87623 92177 87623 73565 82475 94058 92969 76634 66437 86039
 72971 84208 77228 99058 81683 88564 90890 68665103562 92771104008 87970 67279
 74753 93910 99701117719 67031 44410 91930 77575 83911 78119 85594 95345 91138
 83564 55795 92128105196 82426 97919 92524 45944109750 79654 73268 93712 71041
 93316 82921104057 66338 61735117719 72377 62576112423 62972 76931 72724 78367
 80693 95741 86782 81188 79505 71437 64358107324 83218 77872 81485 84950 93118
 98662 81634 82624 87277 70348 86089 87227 84257110047 75100 86485 74951 91583

64804 96830 63913 94108 94108 75595 92672101335 82030 87227106928 86287 68566
80792113017117521 89851 95147 79010 75446101137 90742104849109205 64556 79505
90742 95296 76486 90544120244104503 93068 69308 85940 69259 92029110789103909
80050 48469105592 90148 55894119551103958 66190 85346 81386109057100097 81931
100048 85198102919105097107423 75991 79010111631 92029 89851132965111581115442
110245 75842 49112 89851 93167108611 55993 62032 73714104057 90395 84752 97721
103414 91435 80000108364 86138 94058 81733 93761 82574104701 86831126778109700
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107077 91979 94999 65893107918

2) Early summer temperature reconstructed (5-years moving average)

$$2068=N \quad -75=I \text{ TJJ5} \quad -4(13F6.0)\sim$$

107463 89524 76466 79149 77654 87613 90217 95088 88524 94632 96988102869105027
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3) Annual temperature reconstructed, RCS-chronology (5-years moving average)

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VARIABILITY OF LARCH RADIAL GROWTH IN THE EAST OF TAYMIR AND PUTORAN FOR THE LAST 2000 YEARS

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Abstract

Regional tree-ring chronology with extension 2209 years (since 212 B.C. till 1996 A.D.) was built for the east of Taymir according to wood of living trees, well preserved residues of dead trees and semi-fossil wood from alluvial bank deposits by the cross-dating method. In addition the "floating" tree-ring width chronology for the period of Holocene Optimum (3300-2600 B.C.) was built with extension 685 years and supported by several radiocarbon dates. High values of synchrony and correlation of individual tree-ring series show a prevailing effect of one external factor on radial tree growth change in the studied region of Siberian subarctic. It was established that the main factor of growth variability the early summer and annual temperature is which explains up to 70% of tree growth rate variability. Cyclic components stable for two millennia were revealed at analysis of the tree-ring chronology: double secular (about 180 years), secular (78-90) and intrasecular (44, 28, 11 and 6,7-6,9 years) variations. Models for reconstruction of the early summer and annual air temperature were obtained according to tree growth variability. Temperature dynamics in the eastern part of Taymir for the last two millenia agrees well with temperature variations in the northern hemisphere obtained according to other indirect sources. The warming of the middle of the 20-th century is not extraordinary. The more long in time, and close in amplitude the warming at the border of the first and the second millennia was.

Key words: radial growth, tree-ring chronologies, temperature change, dendrochronology, climate, growth cyclicality, temperature reconstruction, response functions.

Introduction

The leading dendrochronological groups began their work in some key regions of circumpolar zone of the northern hemisphere

on building the superlong (several millennia, and for the whole Holocene period if to use subfossil wood) tree-ring chronologies for the quantitative reconstruction of natural temperature variations [6,8,19,20,35,36]. The high latitudinal regions in the northern hemisphere are of greatest interest for assessing natural and anthropogenic variations of air temperature, forest-tundra ecosystem growth and productivity, regeneration regime as well as of polar timberline dynamics because the ecosystems of high latitudes have the highest sensibility to the expected global climate warming [4,15,18,19,22,43]. Owing to accessibility and great amount of well-preserved wood of dead trees as well as of subfossil wood from alluvial river deposits and wood buried in bogs several regions in high latitudes of Russia turned to be promising for building millennial chronologies: the Polar Urals [15,31], Yamal peninsula [32], the east of Taymir and Putoran [7,8,14] and the lower Indigirka river[34]. The following problems were solved in the given paper: 1) obtaining of the absolutely dated 2000 year tree-ring chronology suitable for quantitative reconstruction of climate changes; 2) revealing of the main climatic factor responsible for the year-to-year and long -term growth variability; 3) building of models of climate change reconstruction for the whole period of long tree-ring chronology.

Material and methods

Dendrochronological material was collected in Kheta-Khatanga plain as well as in Moyero-Kotuy plateau regions of the Middle-Siberian forest zone within the northern stripe of the northern taiga subzone [1] (Fig.1). The wood samples were taken with the help of a borer or chainsaw from the living trees, from the well preserved residues of a dead and subfossil wood. The whole sampled material is from trees from three types of conditions: 1) from the contemporary northern timberline of larch in the stow (urotchishche) Ary-Mas of the Taymir biospheric reservation (latitude of 72 28' N.); 2) from contemporary upper timberline with absolute marks 200-300 m above sea level in the Kotuy river valley (latitude of 70 30'-71 00' N.); 3) from alluvial deposits of flood-land and over-flood-land terraces of large tributaries of the Khatanga river (latitude of 70 30'- 73 00'N.). Measuring of the tree-ring width was made with the help of automatized devices with resolution up to 0.001 mm, and later the measured individual tree-ring chronologies were treated in the standard software package for dendrochronological and dendroclimatological analysis [26,33]. Owing to the high year-to-year variability, high

synchrony of individual series between each other the results of the cross dating gave a chance to build the continuous chronology since the year 212 B.C. till 1996 A.D., it means the total length 2209 years. Besides, according to the well-cross-dated discs of subfossil wood for which the series of radiocarbon dates was made at the University of Bern (Switzerland) and at the Joint Institute of Geology, Geophysics and Mineralogy SB RAS (Novosibirsk) the "flooding" chronology of 685 years long was obtained which according to the dates agreed with climatic optimum of the Holocene (3300-2600 years B.C.).

Standardization method is used to treat individual series for the best revealing of climatic signal. This method is intended to remove the changes caused by age or by factors of the non-climatic nature (for example, effect of phytocoenotic factors) from tree-ring width variability. For standardization two approaches were used: 1) an approximating curve of age variations is tried for every individual series [15,26]; 2) an age curve is used which is built according to the entire totality of analysed individual curves of growth [3,11,21,23]. As the special studies showed, the long climatic changes (or super-secular variations) remain more reliably at the second standardization method [21]. Therefore, it was chosen for standardization of individual series and obtaining of the long chronology of tree-ring indexes. Obtaining of regional tree-ring chronology (C1) and of the so-called "residual" series (C2), from which autocorrelation was removed [26,40], was as a result of individual series standardization. The main statistic characteristics were calculated for the obtained chronologies: inter-series coefficient of correlation (as an index of synchrony of individual series), sensitivity coefficient, standard deviation, 1-st order autocorrelation etc.[30,41].

Analysis of the frequency-temporal structure of obtained chronologies at the entire period and at the 500-year intervals was carried out by Fourier method of direct transforming (Blackman-Tyuki method) and Fourier method of "fast or inverse transforming" (Kuli-Tyuki method) [10]. The methods of graphical assessment of smoothed curves [15,16], analysis of autocorrelational function [2,15], a narrow-striped filtering of series [12] were used at the revealing of long (super-secular) cycles in growth variability. Revealing of the main climatic factors of growth variability was based on response function assessing and interpreting [30,42]. The quantitative reconstruction of climatic factors according to variability of growth indexes was made on the base of calculated regression model

at which building one part of climatic series was used for calibration, another part - for verification [6,7,41]. Adequacy of reconstruction model was assessed by standard statistic indexes: correlation coefficient, Fisher's criterion, autocorrelation of residues - criterion of Darbin-Watson [17].

Results

In the result of the cross dating (its quality was checked by statistic estimates according to the COFECHA program [26,33]) of *Larix gmelini* living trees and trees dead long ago from the upper timberline as well as of subfossil wood from alluvial deposits the regional tree-ring chronology since the year 212 B.C. till 1996 A.D. was built for the eastern part of Taymir and Putoran. The total number of wood samples being dated was 118, including 27 living and 91 dead trees. The average age of the used trees made 300 years, the maximum age was 798 years. The percentage of the missing rings is not very large - only 0,5% because the discs were analysed, mainly, but not wood cores [44]. The tree number in the regional chronology is not homogeneous in calendar scale and has a tendency to decrease when moving to the past: 3 and more models since the year 135 B.C., 5 and more models since the year 81 B.C. Dating of dead trees showed that in the upper timberline under continental climate the dead tree residues can remain on the day surface during more than 1900 years. Inter-series correlation coefficient for the whole time period is rather higher than the threshold value (0,62- 0,75, $p < 0,01$). It confirms a stable and strong external influence which synchronizes growth variability of individual trees.

The main statistic C1 chronology for the whole period and in 500- year intervals are given in the Table 1. The high and close coefficient values of sensitivity and standard deviation for the different time intervals show that tree growth variability under these conditions is controlled by one and the same factor during two millennia. Autocorrelation of the first order which shows the growth effect of the previous year on the growth in the next year is of great importance as well. Autocorrelation is not significant after treating it by autoregression model in C2. The average tree-ring width for two thousand years is at the level 0,28 mm but it greatly varies in time, the average tree-ring width is more high (0,40 mm) for the Holocene optimum period (Fig.3). Radiocarbon datings agree well with absolute dendrochronological data.

Analysis of spectral density allowed to show the frequency

stripes important by their contribution to the total growth variability and to assess their amplitude (Table 2). The contribution of millennial cyclic component makes about 4%. The large contribution is brought by double secular cycle (in frequency stripe of 120-220 years) up to 20,6% and secular cycle (in frequency stripe of 60-120 years) - up to 13,3%. The cross-spectral analysis of the 500 year old fragments of chronology showed that during two millennia the double secular (180 years), secular (78-90 years) and several intrasecular (44,28,11 6,7- 6,9 years) cycles are steadily present. Hereat, re-distribution in the capacity of separate cyclic components is observed. Such a re-distribution was already noted in the papers on dendroclimatic data from boreal taiga forests and forest-tundra regions [2,12,15]. Positive anomalies of growth, the most strong in amplitude and long in time, fall on the 4-th century, the border of the 6-th and 7-th centuries, the border of the 1-st and 2-nd millennia, the middle of the 20-th century. However, the negative anomalies fall on the 1-st century, the border of the 13-th and 14-th centuries and the first half of the 19-th century. These anomalies can be explained by superposition of cycles of different length. So, growth increase in the middle of the 20-th century agreed with positive periods of the double secular, secular, and several intrasecular cycles (44, 11, and 6,7 years).

Since at the polar timberline the summer temperature is the main factor of growth limiting [6, 15,35,43], then the dendroclimatic analysis of relation of growth variability was carried out, first of all, with air temperature data for summer period. However, some changes were brought to the traditional searching scheme of correlation with average monthly temperature data. In order to reveal the key interval of the season when temperature mainly affected on radial tree growth the value of correlation coefficient between C2 growth indexes and air temperature for every five days beginning from the 8-th of May was calculated. Everyday data of the Khatanga meteorological station since 1933 to 1989 were used. All the calculations are given in the Fig.4. As we see, the significant positive connection ($p < 0,01$) between air temperature for every five days and growth indexes is observed for the period since June 17 to July 11 and it falls on the interval of stable temperature rise in the season. The temperature of the more late intervals of the season does not show large connection with growth variations. The temperature for the period June 17 -July 11 we called as an early summer temperature. Temperature sum for this time period

shows the most correlation with C2 tree-ring indices ($R=0,77$). Thus, variability of C2 tree-ring indexes is determined by the early summer temperature variability in the east of Taymir and Putoran by 60%. The smoothed (the 5-year moving average) C1 tree-ring indexes and instrumental values of average annual air temperature show high agreement too ($R=0,72$). At the same time the smoothed annual temperature shows the significant relationship with the concordance coefficient calculated for the same period of 5 years based on all wood samples available. This relationship is positive ($R=0.44, p<0.01$) and shows that in cool periods the synchrony in tree-ring variations among all trees measured becomes lower, in warm periods it becomes higher, but has a non-significant relation to tree-ring width variations. Therefore, at searching of quantitative models of reconstruction of leading climatic variables using tree-ring chronologies it was conventionally taken to use C2 for the early summer temperature reconstruction, and C1 - for the annual temperature reconstruction.

The results of calibration and verification of obtained models of the early summer and average annual temperature reconstruction according to 2000-year chronologies data are given in the Table 3 and in the Fig.5. As we can see, the early summer temperature variability is well explained by tree-ring indexes variability in C2 ($R^2 = 0,59 - 0,72$), the average annual temperature variability is described by the model with two variables: by smoothed values of tree-ring indexes in C1 and by concordance coefficient values between individual series ($R^2 = 0,67$) (Fig.5). Comparison of calculated values of the early summer and average annual temperatures with the real ones for the period of instrumental observations shows (Fig.5) that the calculated values of the early summer temperature agree well with the year-to-year variability of real values repeating the most large positive (1940-41, 1953, 1967, 1979, 1984) and negative (1947, 1949, 1980, 1989) extremes. Hereat, in the curves of the early summer temperature variability the long fluctuations are not expressed. To the contrary, the periods of large temperature rise (1938-1956, 1983-1989) and temperature fall (the end of (19)50s and (19)70s clearly agree in reconstructed and real values of average annual temperature.

Based on the obtained models according to two chronologies the reconstruction of the early summer and average annual air temperature was made for the east of Taymir and Putoran for the period since the year 81 B.C. to the present time (it means, for the period provided

by 5 and more samples). The curves of variability of the reconstructed early summer temperature (smoothed by the 5-year and 57-year moving average) are given in the Fig.6. The average value of the early summer temperature equals to 9,6 C for the instrumental observation period. The most large fall of the early summer temperature is marked in the 1-st century ($T=8,4$ C), and in the end of the 13-th century (8,4 C). The most warm periods with the raised average early summer temperature are the end of the 3-rd century ($T=9,7$ C), the border of two millennia (9,6 C), the middle of the 20-th century (9,9 C). The middle of the 20-th century is characterized by the most rise of the early summer temperature, but the 11-th and 12-th centuries are characterized by the long period with high early summer temperatures.

Long variations of the average annual temperature range from minus 14 C to minus 12,5 C. It was of great interest for average annual temperature to compare the reconstruction data with other indirect data on dynamics of average annual air temperature of the northern hemisphere in order to make clear whether temperature variations in the east of Taymir and Putoran reflect global temperature changes in the northern hemisphere. As such the data on reconstruction of temperature variation in high latitudes according to ratio of oxygen isotopes in ice cores of Greenland were used [25,29]. In the Fig.7 both reconstructions are matched in the calendar scale since the late of the 12-th century. Their good agreement is well seen, especially in positive (the 14-th and 15-th centuries, the end of the 18-th and the middle of the 20-th centuries) and in negative (the late of the 13-th and of the 17-th centuries, and the first half of the 19-th) extremes. It means, the long fluctuations of average annual temperature in the east of Taymir and Putoran agree well with global air temperature variations of the northern hemisphere for the last millennium, and hence the tree-ring chrnology of this region can be used to analyse both regional peculiarities and global temperature variations in the northern hemisphere.

Discussion and conclusions

The results of analysis of the super-long tree-ring chronology of the Taymir and Putoran east show that the information on the main climate changes in the northern hemisphere for the last 2000 years is reliably fixed in it: fall of temperature in the first century, climate warming in the 3-rd and 4-th centuries, warming in the Medieval Warm Period (?) or "the small climatic optimum" at the border of two millennia, the long fall of temperature in the 17-th and

19-th centuries ("the small glacial period") and the present climate warming in the middle of the 20-th century [27]. Since the obtained regional chronology has good correlations with other chronologies of subarctic zone within 500- 600 km [6,43], then we can believe that similar regularities of the early summer and average annual temperature variability are typical of large sector of Siberian subarctic. It was shown earlier that the long growth variations agree well for the entire Siberian subarctic [8,24]. The studied region (and this is shown by subfossil wood samples and by obtained "floating" chronology) has a high potential to build the tree-ring chronology for the whole Holocene period and to study in details temperature variations for this period of the Earth history.

Two important consequences from reconstruction analysis should be noted especially. First, the analysis of frequency structure of series and of their separate fragments illustrates a constancy of the main environmental factors limiting growth. It is confirmed also by comparing reconstructions with other indirect evidences. Second, the warming in the middle of the 20-th century, marked as extraordinary [22], has the analogs in the past. So, the warming at the border of millennia shows a close amplitude and was more long [27,38]. Historical evidences on climate of this Medieval Warm period say about the more large climate warming than the present one [13]. The obtained data demonstrate that temperature variations in high latitudes for the instrumental period (1850- 1990) do not go far beyond limits of natural variations revealed during two millennia.

Ratio of natural and anthropogenic components in the present and future climate changes is especially discussed. It is proved in some papers based on the long tree-ring chronologies of North America that the influence of anthropogenic component becomes large and can be separated and assessed quantitatively [39]. Hereat, the trees growing above the upper or polar timberline reflect stable temperature rise in the northern hemisphere [28,35,37]. However, a direct correlation between temperature and growth is marked only for trees from growth regime especially chosen [36,37]. The stable trend of summer and especially winter temperatures for the last decades is connected with the increase of anthropogenic component share, mainly, at the expense of atmospheric green-house gases [4]. At the same time, on large areas of high latitudes (mainly, in subarctic zone) tree growth, correlating well with temperature rise till (19)60s, begins to stunt after this period from the rise of temperature [24]. We can believe that the direct

temperature effect is combined with other factors which influence growth rate of trees in polar latitudes. For instance, increase of winter precipitation can shift the dates of snow cover melting to the more late time even at summer temperature rise [9]. In polar latitudes the conditions of the first season half play the leading role in radial tree growth and tree-ring forming [5,6]. Therefore, tree growth response at the polar timberline will be more complex than we can expect only from summer temperature variations.

Conclusions

1. The long 2209-year (since the year 212 B.C. till 1996 A.D) regional tree-ring chronology was obtained for the east of Taymir and Putoran, suitable for quantitative dendroclimatic analysis and climate change reconstruction. Numerous radiocarbon data of sub-fossil wood and several built "floating" chronologies show a high potential of dendrochronological material of the given region for the building of absolute tree-ring chronology for the main Holocene period (more than 6000 years).
2. The main factors were established which determined the year-to-year variability as well as long-term variability of larch growth in the study region. They are the early summer and average annual air temperature and they determine up to 70% of the total growth variability.
3. In long-term growth variability during two millennia the millennial, the double secular and secular cycles as well as some intrasecular cycles which are met the most often in tree growth variability in polar latitudes of the northern hemisphere are steadily seen. The most large warmings and falls of temperature can be explained by matching particular cycles.
4. Reconstruction of the early summer and average annual temperature variations in the east of Taymir and Putoran showed good agreement of temperature variations in the given region with temperature variations in the northern hemisphere obtained in other indirect sources. The warming in the middle of the 20-th century is not extraordinary. The warming at the border of the 1-st and 2-nd millennia was more long in time and similar in amplitude.

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44. Vaganov E.A., Naurzbaev M.M., Schweingruber F.H., Briffa K.R., Moell M. An 840-year tree-ring width chronology for taymir as an indicator of summer temperature changes. "Dendrochronologia", 1996,14:193-205.

Tabl.1. The main statistical characteristics of C1 chronology

Period	Years	Statistical parameters				
		Mean index	Sensit.	St.dev.	1-st autocorr.	Coef.var.
212 BC-						
1996 AD	2209	1.016	0.421	0.443	0.41	43.6
212BC-287AD	500	1.014	0.411	0.482	0.53	47.5
0-499AD	500	0.963	0.426	0.421	0.38	43.7
500-999AD	500	0.982	0.457	0.441	0.38	44.9
1000-1499AD	500	1.015	0.427	0.433	0.37	43.6
1497-1996AD	500	1.039	0.339	0.441	0.44	42.4

Tabl.2. The relative power of different cyclic components in C1 chronology
(in % to common variation)

Period	Spectral window			
	<600 years	220-600 years	120-220 years	60-120 years
81 BC-1996 AD	4.0	6.1	11.3	6.7
0-499 AD	2.8	20.6	2.6	
500-999 AD	3.7	6.9	11.0	
1000-1499 AD	12.0	2.3	4.9	
1497-1996 AD	5.9	8.8	13.3	

Tabl.3. Statistical evaluations of model for reconstruction early summer temperature based on C2 chronology and annual temperature based on C1 chronology

Period	Calibration			Verification			
	R ²	F value	D-W statistics	Period	R ²	F value	D-W statistics
early summer temperature							
1933-1989	0.59	79.6	1.914				
	(p<0.00001)						

1960-1989	0.72	72.7	1.907	1933-1959	0.45	20.5	1.877
	(p<0.00001)				(p<0.001)		

annual temperature (average)							
1933-1993	0.67	46.0	2.51				
	(p<0.000001)						

FIGURES

Fig.1. The map of territory where wood samples were collected: 1- sites of living old trees; 2- sites where wood remains of dead trees and subfossil wood were collected; 3-recent polar timberline.

Fig.2. The variability of average tree-ring width (smoothed) in absolutely dated (upper) and "floating"(below) Taymir chronology. The according radiocarbon dates shown at right column.

Fig.3. Correlation of C2 index chronology with pentad temperatures (asterisk shows the significant value of correlation coefficient). The many years average temperature curve also shown as wide line.

Fig.4. Comparison between observed and calculated early summer (1) and annual (2) temperature for Taymir. Empty columns indicate the residuals.

Fig.5. Reconstructed early summer temperature from C2 chronology: 1- 5-year smoothing, 2- 57-year smoothing.

Fig.6. Comparison of long-term changes in annual temperature reconstructed from Taymir C1 chronology (2) with oxygen isotopic ratio in Greenland ice cores (2)(according to Burroughs,1992).

From: Keith Briffa <k.briffa@uea.ac.uk>
To: evag@ifor.krasnoyarsk.su
Subject: transfer
Date: Wed Nov 18 11:04:42 1998
Cc: stepan@ipae.uran.ru

Eugene

I am told that the money transfer (5000 u.s. dollars) should have gone to the bank account you stated. Please let me know if this is received by you. I now also have the contract signed by INTAS and we must organise future work and I will talk to Fritz about us visiting Ekaterinburg next year. In the meantime I wish you and Stepan to organise major review papers of the Yamal and Taimyr long chronology staus for inclusion in the Holocene ADVANCE-10K Special Issue. These need to be completed by June at the latest . They will each be 10-12 pages of print. I can suggest content, do some analyses and help with editing these . I am also sending Stepan's 5000 dollars to Switzerland now to be carried back by his colleague. I have yet to sort out how claims on the INTAS money will be handled. Have you received the details of the final contract?

best wishes
Keith

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Paul Valdes <P.J.Valdes@reading.ac.uk>, Nick Shackleton
<njs5@cam.ac.uk>
Subject: Re: Thematic Proposal
Date: Thu Nov 26 10:51:57 1998
Cc: sfbtett@meto.gov.uk

Paul and Nick

at this point it would be unwise to consider the proposal dead. Yes

it has received mixed receptions in different quarters but this was always to be expected. Each of the boards has its own family to protect , or at least this is the way science funding is now perceived, so that the only consideration in the discussion (especially of proposals from alien boards) is whether or not there will be enough on the carcass for ones own. The strength of our proposal lies in the potential for true cross-Board participation and the real scientific and strategic advantage of the focus on the Hadley Centre work. In my mind the problem has always been to get real enthusiasm from ASTB , and if COAPPEC had not been on the table this may have been more forthcoming. I can not see that we could have done anything more in the circumstances to overcome this hurdle than by enlisting Hadley Centre support. The decision to go jointly only with ESTB and ASTB was already made. The issue of 'no money anyway ' typifies the unsatisfactory nature of the system - but in this case I hear things may not be so bleak. Apparently some millions more pounds are now available than was the case earlier! At this point NERC will say nothing - but they are equally not saying ' sorry and goodbye' . Let us wait and reconsider when we hear something definite.

Incidentally, I have seen a copy of a project funded in Germany where they have millions of marks to compare model and palaeodata to verify and otherwise explore the natural variability in the Hamburg model! They are looking forward to using our data in this exercise!

I will be in touch as soon as I hear more.

best wishes

Keith

At 06:41 PM 11/25/98 +0000, Paul Valdes wrote:

>Keith, Nick,

>

>Have you had any news about the thematic proposal.

>

>I gather that things did not go well for it in the ASTB.

>The story I have heard is that it was tabled along with

>the other proposals, but also tabled was the proposed

>expenditure for the next 5 years. Moreover, apparently

>it was then said (or perhaps just implied) that there

>was no point looking at some thematic proposals because

>all money was already committed!

>

>If only half of this were true, then it is disappointing.

>Apparently, more atmospheric chemistry was recommended,

>plus COAPPEC (the coupled ocean-atmosphere project).

>

>Hopefully it faired better at ESTB but it clearly cannot

>be argued to be a joint proposal!

>

>Perhaps we should consider recycling it into an EC framework

>5 proposal.

>

>Paul

>

>-----

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>

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>

>

From: Bob Keeland <Bob_Keeland@USGS.GOV>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Re: verification and uniformitarianism
Date: Wed, 2 Dec 1998 16:13:08 -0700
Reply-to: grissino@VALDOSTA.EDU

Frank is correct in that we need to define 'abrupt climatic change' or even just 'climate change.'

Using Jim's Schulman Grove example suppose that the area supported a stand of bristlecone pine 9,000 or more years ago, hence the scattered remnants. Either a major catastrophic event or a fluctuation in climate (call it climate change if you want) resulted in conditions that killed the mature trees and eliminated any further recruitment for up to 1,000 years. This site may be near the limits of recruitment and with a major (or minor perhaps) change in climate it could easily be beyond the limits of recruitment. About 8,000 years ago climate again became favorable for bristlecone pine recruitment and a new stand(s) developed and have existed ever since. Some or most of the material remaining from the original stand may be buried down in the valley, or the original stand may have been small or sparse. The amount of time between the loss of the original stand and the beginning of the new stand would depend on the period of unfavorable weather and the amount of time needed for bristlecone pine to re-invade the area. I am out on a limb here, so to speak, as I am somewhat ignorant of prehistoric climate patterns for the area and of bristlecone pine ecology, but this seems like a relatively reasonable scenario.

I guess that my point is that climate continues to fluctuate within broad bounds. Everything that we are now calling 'climate change' is well within the bounds observed within the prehistoric record of climate fluctuations. Do we call any variation 'climate change' or should we limit the term climate change for anything considered to be caused by humans? To my mind it is not so much what we call it, but rather that we keep a clear idea of what we actually talking about.

Bob Keeland
USGS, National Wetlands Research Center
Lafayette, LA
bob_keeland@usgs.gov

From: Bryson Bates <bryson@per.clw.csiro.au>
To: Barrie Pittock <barrie.pittock@dar.csiro.au>
Subject: Re: uncertainties guidance paper
Date: Mon, 14 Dec 1998 18:58:01 +0800 (WST)
Cc: "'econf.part2@usgcrp.gov'" <econf.part2@usgcrp.gov>

Dear All --

On Mon, 14 Dec 1998, Barrie Pittock wrote:

- > 1. Two issues are being addressed and partially confused:
- > (a) the confidence we have in the science (which seems to be the main concern of the paper);
- > (b) the quantitative uncertainty regarding specific results such as: by what percentage will the rainfall change at 2050 in region/location A? or, how much will changes in tropical cyclones cost in percent of GNP (or additional? lives lost)? My reading of the comments from WG1 authors reported by Neil Leary was that they were focussing more on (a), whereas WG2 authors may want to focus a bit more on (b).

I wholeheartedly agree. While I agree with the probabilistic approach in general, there are a number of practical factors that will mitigate against it. Barrie has listed most, I have added one below.

- > 2. Authors will be limited largely by what is in the literature, especially on the second class of uncertainty. So the guidance needs to go from the authors, or IPCC in some other way (as soon as possible), to the researchers to encourage greater attention to quantifying their uncertainties, and to the authors to put their fingers on misleadingly "precise" estimates by pointing out the basis of such estimates, eg., "this estimated crop yield change is based on only one simulation with one GCM and should be considered in the light of the range of results from other GCMs and for other realisations".

Another source of uncertainty is the different methods used to derive climate change scenarios at regional and local scales. Some authors apply perturbations (based on changes indicated by several GCMs) to historical climate series, some use results from limited area models, while others use one of a wide variety of stochastic approaches that are based on results from one or more GCMs. The important point here is these methods would produce different estimates of uncertainty for the same region and

the same suite of GCMs.

> 6. Regarding para. 67, I am more concerned about the "best" or "central"
> estimate for climate sensitivity of 2.5 deg.C for 2xCO2 than about the
> range. Several lines of evidence (paleo-evidence, fitting models to the
> last 100 years, the distribution of improved model results) all suggest
> that the "best estimate" for this increasingly dated and artificial
> notion should be raised from 2.5 to nearer 3.5. This would be
> controversial, but I believe it would also be giving the best advice
> possible. Whatever you believe is the correct number, the level of
> concern such a change would raise is in itself evidence for the
> importance of central estimates in the climate change debate.

This could be investigated and quantified in a Bayesian framework.

> 7. I share Martin Manning's problems with the use of the term "Bayesian"
> and equating it with "subjective". Personally I think this paper should
> avoid such specialist technical terms if possible, especially if there
> is disagreement about what they mean!

Yes: Bayesian methods provide a means of combining prior (expert) knowledge with data to quantify the posterior distribution. The prior knowledge may be based on the results of previous experiments and need not be subjective. Another point is that formal application of Bayesian methods usually leads to problems that are analytically intractable. The recent development of Markov chain Monte Carlo methods has largely overcome this.

> 8. I repeat my concern re too much spatial aggregation of results if it
> hides important regional differences, as these are very important for
> questions of intragenerational equity. I think the paper should
> specifically warn against this. Averaging is notorious as a way of
> hiding important differences.

I share this concern: the average of a large negative and a large positive number is close to zero.

Regards
Bryson Bates

From: Rob Swart <Rob.Swart@rivm.nl>
To: oadegbul@oaife.edu.ng, oadegbul@cerd.edu.org, dahuja@worldbank.org, cna@meteo.go.ke, cna@elci.gn.apc.org, 110217.3046@compuserve.com, alcamo@usf.uni-kassel.de, knut.alfsen@cicero.ui.no, j.aloisi@unep.fr, amano@ksc.kwansei.ac.jp, amous.apex@gnet.tn, dennis.anderson@economics.oxford.ac.uk, applebpg@bp.com, mapps@nofc.forestry.ca, l.arizpe@unesco.org, robert.ayres@insead.fr, frtca@fy.chalmers.se, Jan Bakkes <Jan.Bakkes@rivm.nl>, gil_bamford@toyota.com, banuri@tellus.com, barbour.wiley@epamail.epa.gov, terry.barker@econ.cam.ac.uk, richard.baron@iea.org, cenef@glas.apc.org, jeannett.beck@rivm.nl, lenny_s_bernstein@email.mobil.com, root%CpCb@ernet.in, k.blok@nwsmail.chem.ruu.nl, pb@ne.su.se, bbolin@osteraker.mail.telia.com, bert@misu.su.se, JC.Bollen@rivm.nl, jbond@erols.com, idbouille@inbox.servicenet.com.ar, british@proaxis.com, british@heart.cor.epa.gov, jpbruce@sympatico.ca, bruggink@ecn.nl, ecalvo@mail.cosapidata.com.pe, ocanz@arrobbba.com.ar, kapros@softlab.ece.ntua.gr, ccarraro@unive.it, caccerri@pintado.ciagri.usp.br, cerri@cena.usp.br, renete.christ@dg11.cec.be, john.christensen@risoe.dk, criqui@iepe.upmf-grenoble.fr, becon@public3.bta.net.cn, partha.dasgupta@econ.cam.ac.uk, ogunlade.davidson@risoe.dk, devra@wri.org, ged.r.davis@si.simis.com, emilio@ppe.ufrj.br, dearing@wbcsd.ch, Yhding@Public.Bta.Net.Cn, rdixon@igc.apc.org, ddokken@earth.usgcrp.gov, tom.downing@ecu.ox.ac.uk, duchin@rpi.edu, ja_edmonds@pnl.gov, ellerman@mit.edu, osp@intouch.com, sfankhauser@worldbank.org, tibor_farago@mail.mata.vu.hu, PMFEARN@INPA.GOV.BR, zhoufq@public3.bta.net.cn, j.fenhann@risoe.dk, bfisher@abare.gov.au, brian.p.flannery@exxon.com, louise.fresco@fao.org, fujimori@ffpri.affrc.go.jp, fewewar@tarnet.pl, gilberto.gallopini@sei.se, cgay@chajul.ine.gob.mx, ft-geng@correo.dnet.com.pe, pghosh@mail.asiandevbank.org, a.m.gielen@minez.nl, jglenn@igc.apc.org, goldemb@iee.usp.br, estrukova@hotmail.com, jgrant@ipieca.org, kennethgregory@msn.com, dJgriggs@meto.gov.uk, mgrubb@riia.org, gruebler@iiasa.ac.at, jgu@ens.dk, joyeeta.gupta@ivm.vu.nl, sujatag@teri.res.in, pcutman@erols.com, ehaites@netcom.ca, david.hall@kcl.ac.uk, kirsten.halsnaes@risoe.dk, allen@wri.org, bhare@ams.greenpeace.org, theller@leland.stanford.edu, matthijs.hisschemoller@ivm.vu.nl, michael.hoel@econ.uio.no, hogan.kathleen@epa.gov, hohenstein.william@epa.gov, hohmeyer@uni-flensburg.de, chl1@eng.cam.ac.uk, leen.hordijk@wimek.cmkw.wau.nl, rhoughton@whrc.org, xuhging@public3.bta.net.cn, m.hulme@uea.ac.uk, saleemul@citechco.net, image-ers@rivm.nl, imura@ies.kyushu-u.ac.jp, Bert.Metz@rivm.nl, ogunlade.davidson@risoe.dk, ejo@isi.fhg.de, munasinghe@eureka.lk, ecalvo@mail.cosapidata.com.pe, Fabio@cidea.unepnet.inf.cu, depas3lh@cbn.net.id, lorents.lorentsen@fin.dep.telemax.no, ishi@globalenv.t.u-tokyo.ac.jp, patricia.iturregui@conam.gob.pe, hjacoby@mit.edu, fuj.jaeger@magnet.at, ajaffe@nber.org, janzen@em.agr.ca, jaszay@eta.enrg.bme.hu, jefferson@wec.co.uk, c.j.jepma@eco.rug.nl, gjenkins@meto.gov.uk, ejo@isi.fhg.de, johnson@iiasa.ac.at, joos@climate.unibe.ch, tyjung@ccmail.keei.re.kr, lijf@public.bta.net.cn, stephen_karekezi@elci.gn.apc.org, kasiwagi@cc.tuat.ac.jp, kates@ecology.coa.edu, Pekka.Kauppi@Helsinki.FI, hskhesh@erenj.com, ger.klaassen@dg11.cec.be, alexey.kokorin@g23.relcom.ru, kolstad@econ.ucsb.edu, kopp@rff.org, kram@ecn.nl,

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Subject: IPCC Emissions Scenarios
Date: Fri, 18 Dec 1998 15:34:41 +0100

LS

As you may recall, the Intergovernmental Panel on Climate Change (IPCC) is in the process of preparing a Special Report on Emissions Scenarios (SRES).

Recently, it has been agreed that these scenarios are to play an important role in IPCC's Third Assessment Report. The Terms of Reference of this Special Report include a so-called Open Process to stimulate input from a community of experts much broader than the writing team. This Open Process has started in August 1998 and was planned last until the end of the year.

Because of the late date of this message we decided to extent this deadline

until January 10 now. A website (sres.ciesin.org) is managed by the Center

for International Earth Science Information Network (CIESIN) in the United

States in collaboration with the Energy Research Foundation (ECN) in the Netherlands, the Technical Support Unit (TSU) of Working Group III on Mitigation of IPCC in the Netherlands, and the International Institute of Applied Systems Analysis (IIASA) in Austria, the home institution of the co-ordinator of the SRES Report, dr. Nebojsa Nakicenovic. Three types of input are invited: (a) new scenarios (preferably from the peer-reviewed literature) that have not been taken into account by the writing team,

(b) new quantification of the proposed SRES scenarios based on storylines, and

(c) suggestions for improvements of the material developed until now.

Several of you have responded to an earlier request for input into this open process. Thank you for that input. Amongst other things on the basis of input received so far, recently the information on the website has been

improved considerably. The writing team of the report has now started to actually draft their report, but can still take into account reactions to this new information as published through the website, in principle until 31 December 1998. Herewith I would like to invite you to explore the site (again) and provide us with your comments.

PLEASE DO SO USING THE FACILITIES OF THE WEBSITE, DO NOT USE THE EMAIL ADDRESS OF THE SENDER OF THIS MESSAGE OR THE EMAIL GROUP LIST ABOVE!!!!

On behalf of Dr. Nakicenovic, thank you very much for your support to this important endeavour!

Dr. Rob Swart

Head, Technical Support Unit
Intergovernmental Panel on Climate Change Working Group III: Mitigation
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From: Bill Hare <Bill.Hare@ams.greenpeace.org>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: MAGICC
Date: Fri, 18 Dec 1998 18:05:59 +0100

Dear Mike

Please send the details etc to me.

Thanks

Bill

\
On 18 Dec 98 at 9:43, Mike Hulme wrote:

> Date: Fri, 18 Dec 1998 09:43:31 +0000
> To: Bill Hare <Bill.Hare@mail.nli.g13>
> From: Mike Hulme <m.hulme@uea.ac.uk>
> Subject: Re: MAGICC

> Bill,

>

> The version of MAGICC we are distributing is the IPCC SAR 1996
> version. You can get that from me under Licence for \$50. If you
> wish to proceed let me know and I can send it you with invoice.

>

> Regards,

>

> Mike

>

> At 17:59 16/12/98 +0100, you wrote:

>>Dear Mike

>>

>>I would like to know how to get the most recent version of MAGICC and
>>of COMICC (carbon cycle model). I heard from a colleague that you
>>may be distributing MAGICC??

>>

>>I look forward to hearing from you,

>>

> >Regards

> >

> >Bill Hare

> >

> >Bill Hare

> >Climate Policy Director

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From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Scientific cooperation
Date: Mon, 21 Dec 1998 11:00:33 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,

Thank you for the money transfer via Fritz Schweingruber. I received 5000 USD. Is it necessary to give you a receipt for this sum of money? Money will be used for organization of field works in the Yamal Peninsula and Polar Urals next year. Of course, this sum is not enough. I hope we shall have an additional money from the INTAS project and the Russian Funds.

I received two copy of the INTAS contract from Fritz and one copy I sent to E. Vaganov. We would like to know your opinion concerning transfer money.

Also, I need to know exact time you and Fritz intend to visit Ekaterinburg next year. The new rules demand to make application to the Russian officials before 6 months of your arriving. Do you want or not to travel in the area of Southern Ural Mountains after meeting in Ekaterinburg? Fritz wants to travel over this area (the Taganai and Iremel Mountains).

Best wishes to you, your family and your colleagues.

Marry Christmas and Happy New Year!

Sincerely yours,

Dr. Stepan G. Shiyatov

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Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

From: Janice Darch <J.Darch@uea.ac.uk>
To: env.faculty@uea.ac.uk, env.researchstaff@uea.ac.uk
Subject: EN99:04 UKRO - European News (29 January 1999) (fwd)
Date: Fri, 29 Jan 1999 16:09:54 GMT

Dear All, The most pertinent document is item one on copyright. Some ENv policy documents are also included as item5.

#Janice

Forwarded Message:

From: Helen Self <H.Self@uea.ac.uk>
Date: Fri, 29 Jan 1999 14:32:36 GMT
Subject: EN99:04 UKRO - European News (29 January 1999) (fwd)
To: d.chadd@uea.ac.uk, dean.wam@uea.ac.uk, Dora.K@uea, e.banakas@uea, e.doy@uea, f.littlewood@uea, g.turner@uea.ac.uk, h.brownlee@uea, j.casey@uea.ac.uk, j.darch@uea, j.johnson@uea.ac.uk, j.schostak@uea, j.steward@uea, j.watson@uea.ac.uk, m.silbert@uea, m.stallworthy@uea, mrs@sys.uea.ac.uk, odg.gen@uea, r.mcbride@uea, r.mclarty@uea.ac.uk, r.sales@uea.ac.uk, r.sassatelli@uea.ac.uk, t.prime@uea.ac.uk, v.koutrakou@uea

Forwarded Message:

From: ukro.ukro <ukro.ukro@BBSRC.ac.uk>
Date: Fri, 29 Jan 1999 12:45:25 +0000
Subject: EN99:04 UKRO - European News (29 January 1999)
To: g.l.a.jones@reading.ac.uk, geoff.g.wood@vla.maff.gov.uk, costas.kaldis@britcoun.gr, david.elliott@britcoun.org.il, shabtay.dover@skynet.be, elosuniv@BBSRC.ac.uk, eoscmemb@BBSRC.ac.uk, elosresc@BBSRC.ac.uk

=====
EN99:04 UKRO - European News (29 January 1999)
=====

News on non-Framework Programme 5, programmes & policy
=====

GENERAL:

1. ESF on Copyright Law
2. GENERAL - Policy documents

LIFE SCIENCES:

3. DG V - Newsletter on Alzheimer's Disease
4. Microbiology - Industrial Platform

ENVIRONMENT:

5. ENVIRONMENT - Policy documents

ENERGY:

6. Synergy - International Cooperation in Energy

INFORMATION TECHNOLOGIES:

7. Public-Sector Information

INDUSTRY & TECHNOLOGIES:

8. Results - Pilot Projects on Benchmarking

9. Communication on Industrial Policy

EDUCATION:

10. Leonardo Database on Cordis

REGIONAL FUNDS:

11. Mid-term Review for Structural Funds

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1. ESF on Copyright Law

The European Science Foundation is warning that current plans for new EU copyright laws, if left unchanged, could harm the international competitiveness of European research. The Commission's draft Directive harmonising aspects of copyright will shortly be debated by the Council of Ministers. The ESF is calling for changes to be made to the wording of one of the Directive's key articles which deals with 'exceptions' to the proposed laws to ensure that it doesn't cause legal and financial headaches for Europe's researchers.

The Foundation supports the Commission's objectives of improving the protection of intellectual property as technological developments make it ever easier for pirates to duplicate and distribute copyright material. But it warns that this should not be at the expense of Europe's ability to carry out research. Reflecting widespread concern in its Member Organisations, the Foundation argues that the draft Article 5, which deals with 'exceptions' to the proposed laws, "could result in research being treated differently in different countries across Europe". As presently written, the Article sets out an exhaustive list of permissible exceptions to the directive, but it leaves to Member States the interpretation and implementation of these 'exceptions'. The effect of this could be that some researchers might find themselves in a worse position than at present regarding their access to and use of published material. Given the differences in national legislation between Member States, the ESF recognises it may be difficult to draft and agree prescriptive legislation for 'exceptions'.

The Foundation is recommending, therefore, that a clause be added to the Directive allowing for the inclusion of all current 'exceptions' set out in national legislation. Other suggested revisions include the need to ensure that 'scientific research' is interpreted in a broad sense, with research in the humanities and arts being explicitly included. In addition, the ESF suggests that the current reference to 'non-commercial' research could cause confusion, as it would be very difficult to differentiate between commercial and non-commercial research in most academic settings. To avoid this, it recommends the introduction of a 'public good' definition of research, which could form an 'exception' to the Directive. The Foundation's statement also points out that the Directive's current reference to the possibility of Member States exempting the use of work "provided that such use exclusively serves the purpose of illustration for teaching or scientific research" is ambiguous. It could be interpreted that there is such a thing as 'illustration for research' and that any 'exception' did not apply to research in general. A simple rewording of the sentence to read "sole purpose of scientific research or for illustration for teaching" would clarify the 'exception'.

The European Commission's draft "Directive on the Harmonisation of Certain Aspects of Copyright and Related Rights in the Information Society" is available on-line at <http://europa.eu.int/comm/dg15/en/index.htm>

FURTHER INFORMATION: Johanne Martinez, Information Officer, European Science Foundation, tel 0033 3 8876 7114, fax 0033 3 8837 0532, email: jmartinez@esf.org, URL: <http://www.esf.org>

2. GENERAL - Policy documents

Recent policy documents issued by the European institutions. Full titles and details appear on the UKRO web site under the subject listings:

- * Community action programme in the field of Civil Protection
- * Action programme for customs in the Community
- * Further actions in the fight against trafficking in women
- * Further actions in the fight against trafficking in women
- * Better lawmaking 1998: a shared responsibility Commission report to the European Council
- * Determination of the person liable for payment of value added tax
- * Legal aspects of electronic commerce in the internal market
- * General framework for Community activities in favour of consumers
- * Action programme for customs in the Community

3. DG V - Newsletter on Alzheimer's Disease

The first edition of the Alzheimer Europe quarterly newsletter has been

published by DG V (Public Health). The newsletter is intended to draw attention to the aims and activities of Alzheimer Europe, a grouping of national organisations dealing with Alzheimer's disease. The newsletter includes news of research, events and conferences relevant to the field. It will focus on important developments in the European institutions which affect people with dementia and is also intended to be a platform for the exchange of ideas between organisations and institutes active in the field of Alzheimer's disease. Each issue will include reports on EC-funded transnational projects, beginning in the first edition with London's Institute of Psychiatry EUROCARE project. The next edition of the newsletter will be published towards the end of March 1999.

FURTHER INFORMATION: Alzheimer Europe, tel 00352 297 970, fax 00352 297 972, email: info@alzheimer-europe.org, URL: <http://www.alzheimer-europe.org>

4. Microbiology - Industrial Platform

The Industrial Platform for Microbiology, a ginger group of EU-funded companies and researchers, has decided to change the focus of its activities. It will now aim to provide a forum for EU industrial microbiologists to discuss research and development strategies, scientific aspects of regulatory developments in applied life sciences, and professional issues such as education and training in the field. The Industrial Platform for Microbiology was originally established to organise information exchange between EU-funded companies interested in using the results of EU funded projects and academics working on microbiology research and development projects. Its members will meet again in Brussels in February 1999 to discuss a draft "code of conduct" for companies involved in bioprospecting activities.

FURTHER INFORMATION: Anne-Marie Prieels, Tech-Know Consultants, tel 0032 58 513 953, email: anne.marie.prieels@skynet.be, URL: <http://www.tech-know.be>

5. ENVIRONMENT - Policy documents

Recent policy documents issued by the European institutions. Full titles and details appear on the UKRO web site under the subject listings:

- * Present situation and prospects for radioactive waste management
- * Minimum criteria for environmental inspections in the Member States
- * Cooperation in the field of accidental marine pollution
- * Limitation of emissions of volatile organic compounds due to the use of organic solvents
- * Review clause Environmental and health standards four years after the accession of Austria, Finland and Sweden to the E. U.
- * Application of aerial-survey and remote-sensing techniques to the

agricultural statistics for 1999-2003

- * Financial instrument for the environment
- * Forestry strategy for the E. U.
- * Control of transboundary movements of hazardous wastes and their disposal
- * Voluntary participation by organisations in a Community eco-management and audit scheme
- * Remote sensing applied to agricultural statistics during the period 1994-1998

6. Synergy - International Cooperation in Energy

The Council has announced a Decision (1999/23/EC) adopting a multiannual programme to promote international cooperation in the energy sector (1998-2002). According to the Decision, within the European Union's Energy Framework Programme (see EN39:98, item 11), a specific programme for reinforcement of international cooperation in the energy field will be implemented from 1998 to 2002 ('Synergy programme').

The objectives of this programme are to provide assistance to third countries with the definition, formulation and implementation of energy policy, and to promote industrial cooperation between the Community and third countries in the energy sector. The main tasks of the Synergy programme are to help achieve the Community's energy objectives: competitiveness, security of supply, and protection of the environment.

The financial reference for the Synergy programme will be ECU 15 million. Of this, ECU 6m will be for the period 1998 to 1999. The finances for the period between 2000 and 2002 will be reviewed if the amount ECU 9m is not consistent with the financial perspective for that period.

Supported activities are:

- * Energy policy advice and training;
- * Energy analyses and forecasting;
- * Energy dialogue and exchanges of information on energy policy, notably by means of organisation of conferences and seminars;
- * Support to regional transboundary cooperation;
- * Improvement of the Framework for industrial cooperation on energy.

According to this Decision, **NO FUNDING MAY BE GRANTED TO RESEARCH, development, or demonstration projects.**

FURTHER INFORMATION: OJ L 7 of 13 January 1999, p.23.

7. Public-Sector Information

The European Commission has decided to publish a Green Paper on how the information gathered by government departments and other public bodies can be used to provide the greatest benefit for citizens and businesses in Europe. A lot of information gathered by public bodies for carrying out their duties could be used by the multimedia industry for developing new products and services. Citizens could make better use of their rights if, for example, information was readily available on the conditions for working, studying or living as a pensioner in other Member States. Many people would like to have full information on the tax regulations for cross-border purchases. The competitiveness of businesses could be increased if they had a quick and easy means of finding out what the regulations and procedures are for exporting to other countries. All this information exists, but the technical and legal procedures and terms under which the Member States make it available are uncoordinated and therefore not very transparent for citizens and business. The Green Paper calls for these matters to be discussed and asks questions about how the situation can be improved.

FURTHER INFORMATION: <http://ww.echo.lu/legal/en/access/access.html>

8. Results - Pilot Projects on Benchmarking

Results from four pilot projects on benchmarking framework conditions - in the fields of professional qualification, logistics, the impact of new information technologies (NITs) on company organisation, and the financing of innovation - are reported in the January issue of the newsletter of the European Association of Development Agencies (EURADA).

The lessons drawn included:

- * Companies located in peripheral regions suffer from the poor quality of infrastructures, expensive logistical services and weaknesses in the field of transnational cooperation;
- * The authorities should support the effective use of NIT and the enhancement of NIT-related structures;
- * SMEs lack NIT qualifications and skills;
- * Business Angels play a lesser role in innovation in Europe than in the US, probably due to tax- and revenue-related problems;
- * Even though it remains below the number of such companies operating in the US, the number of venture capital companies operating in Member States of the EU is rising (750 versus 1800);
- * Generally speaking and in comparison with the US and Israel, Europe suffers from a deficit in terms of the ability to evaluate technological risks and from a lack of initiatives to support faster interaction between universities and companies;
- * Education policies should be more practical and in-company training

should be fully integrated in the programmes of higher education institutions;

* Closer links should be promoted between industry and the educational system;

* The skills which new workers lack most upon entry to the labour market are (a) knowledge of English, (b) computer literacy, (c) knowledge about the industrial world, and (d) adaptability.

FURTHER INFORMATION: EURADA, Avenue des Arts 12/7, B-1210 Brussels, tel 0032 2 218 4313; fax 0032 2 218 4583, email: info@eurada.org, URL: <http://www.benchmarking-in-europe.com>

9. Communication on Industrial Policy

The Commission has adopted a communication launching an open debate with the EU's different political, economic and social players on the orientation of a new industrial policy with a view to addressing the challenges of globalisation and accelerated technological changes. The communication diagnoses European industry's weaknesses and proposes a series of measures to promote industrial competitiveness.

The communication diagnoses the weaknesses of European industry:

- * Europe does not have a strong presence in the services sector;
- * European enterprises resort to insufficient externalisation;
- * Specialisation remains underdeveloped in sectors with high growth, highly differentiated products and requiring a strong marketing strategy;
- * The European audiovisual sector is in an unfavourable competitive position;
- * European enterprises form relatively few alliances in advanced technology areas;
- * The amounts invested by risk capital funds are insufficiently oriented towards new and high- technology industries;
- * European enterprises can access financial markets only with difficulty;
- * The level of R&D spending in terms of EU GDP is still below that of its principal global economic partners;
- * The exploitation of research results is not efficient enough;
- * The EU suffers from high costs and the complexity of procedures for achieving intellectual property protection in Europe;
- * European enterprises put very few joint research projects in place.

To counteract this situation and stimulate European competitiveness, the communication emphasises the following proposals, among others:

- * Reinforce intangible investment, by adapting the systems of accrediting competencies and by improving the level of and return from research resources, especially through a better system of intellectual property protection;

- * Develop human resources by acting on the educational system, by encouraging the spirit of enterprise and various forms of social innovation and social cohesion;
- * Promote the access of European enterprises to the world market, by accelerating the exploitation of the competitive advantages of the Single Market;
- * Promote fair rules of the game at a world level in view of the new round of WTO negotiations (that is by developing an observation system for public support to research in industrialised countries);
- * Develop the dialogue between industry and public authorities and forms of self-regulation (protection of consumers and users);
- * Improve financing by eliminating institutional and regulatory barriers to the development of venture capital and improving the tax regime applied to venture capital;
- * Adaptation of the rules to the context of the information society and electronic commerce (agreements such as the "International Charter").

FURTHER INFORMATION: Press release IP/99/33.

10. Leonardo Database on Cordis

DG XXII and Cordis have approved plans to include the products database of the Leonardo da Vinci Programme on the Cordis service. The publication of the Leonardo Da Vinci products database on Cordis should allow its continual update. This is hoped to improve interaction between the owners of products and their users. The schedule for the launch of this database on Cordis has yet to be confirmed.

FURTHER INFORMATION: DG XXII, fax 0032 2 295 5699, URL:
<http://europa.eu.int/en/comm/dg22/leonardo.html>

11. Mid-term Review for Structural Funds

The European Commission approved a report on the mid-term review for the poorest (Objective 1) and sparsely populated regions (Objective 6) in the European Union (EU) for the present programming period (1994 1999). It gives an overview of the mid-term evaluations carried out for the Structural Funds programmes during the current programming period. The report shows important achievements, e.g. when it comes to reducing disparities in basic infrastructure, energy diversification or environmental improvements.

URL: http://www.inforegio.org/wbdoc/docoffic/official/repor_en.htm

=====
Commission press releases (reference 'IP/year 2 digits/number') can be obtained from 'RAPID' at <http://europa.eu.int/en/comm/spp/rapid.html>
Log in as 'guest' with password 'guest'.

European documents (ISBNs) and Official Commission documents (reference 'Com (year 2 digits) number') are available from your local European Documentation Centre at: <http://www.cec.org.uk/relays/relhome.htm> or from the Stationery Office, Tel 0171 873 8372, fax 0171 873 8463.

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??

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From: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re[2]: IPCC Chapter 13 - invitation to contribute
Date: Tue, 2 Feb 1999 20:21:47 +0100
Reply-to: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>

Dear Mike,

thanks for your message. I am sure we can work with these files as soon as we know how the grid is organized. Is it line by line from the North to the South, starting at the dateline? Or something different?

Yes indeed, it would be the best to work with **your** 61-90 baseline for this. Does the baseline also contain cloudiness? If not, then I intend to generate that from our own files, and we will make the assumption that, on the level of monthly means, this does not change as much as to significantly affect the sensitivity of vegetation to the other forcings.

As for a minor point, please remember to use my pik-address whenever possible. The other two (csi and t-online) are both used for sending mail while I am on the road (csi) or at home (t-online), and particularly t-online has the drawback that I can ONLY access it from home (presently) and not from the lab. Unfortunately, I cannot convince my mail sending software to always pretend the mail comes from PIK...

Yes, I will come to the ACACIA meeting, at least until the second day in the afternoon - after that I have to juggle two other meetings in Holland and Germany. With some luck, I should be able to present some results there.

Best wishes!

Wolfgang

PS: I saw your correspondence with Kinne and am interested to follow up - but not today.

On Dienstag, 2. Februar 1999, you wrote:

> Wolfgang,

- > Martin is dragging his feet, but you have convinced me we should distribute
- > them anyway. I have got someone onto it today and with luck may have the
- > minimum (8 realisations for 4 scenarios and for 3 timeslices and for Tmean,
- > Precip and DTR on the HadCM2 grid for the entire world) completed and on an
- > ftp site by Friday. I will also let Nigel know about this. Presumably you
- > will use the 1961-90 0.5deg baseline data? Our files will present
- > *changes* from 1961-90 on a mean monthly basis on the 2.5 by 3.75 grid.

- > Let's keep in touch on this since it opens up a number of other
- > issues/applications. Will you be coming to the ACACIA meeting in early March?

- > Mike

- > p.s. the files will be in the same format as the attached file to this
- > email - just so that you can start thinking about what you need to do.

mailto:Wolfgang.Cramer@pik-potsdam.de

From: "Jonathan T. Overpeck" <jto@ngdc.noaa.gov>

To: Frank Oldfield <frank.oldfield@pages.unibe.ch>

Subject: Re: Finances and futures

Date: Thu, 4 Feb 1999 11:43:09 -0700

Cc: messerli@giub.unibe.ch, domraynaud@glaciog.ujf-grenoble.fr, pedersen@eos.ubc.ca, k.briffa@uea.ac.uk

Hi Frank and friends - I'm happy to see the budget looking sound and feel Franks suggestions are good ones in terms of money to spend this year. Building on the Swiss paleoclimate course is a good idea, and, of course, we should decide on future REDIE investments at future SSC's. My gut feeling is that REDIE will have to continue to be a lower priority in the future, BUT that we should stay committed to getting scientists (including youngsters) from developing countries to our science mtgs - makes more sense than training probably, given tight budgets. Thanks,Peck

>Dear colleagues,

>

>I now share with you some ideas about our financial situation in PAGES. I

>think the information should be treated confidentially at this stage and

>certainly with some discretion.

>

>During the course of last year, it was very difficult to keep track of our

>financial position from month to month, partly because it took our

>financial contacts in the University of Bern an inordinately long time to

>sort out the financial implications of the OSM, partly because, in the

>course of doing this, they made some understandable but very significant

>and confusing errors. Niklaus has now managed to sort these out and we also

>have our confirmed budget for 1999 - which means that we can begin to do

>some real planning.

>

>The first significant point is that we are carrying over into 1999 a

>surplus some US\$15k greater than we began with in 1998. In fact we have

>been building up our 'carry-over' steadily since the beginning of 1996 and

>it is now around \$67k - between 13% and 14% of our annual budget and a much

>higher proportion of that part of our budget that is uncommitted each year.

>Whilst I believe it would be unwise to eliminate it entirely, I do think we

>should aim to reduce it significantly provided there is a good rationale

>for the means we choose.

>

>I have attached a summary of how I see things for 1999. You will see that

>even if we spend all the funds committed to workshops at our Pallanza

>meeting, we still have a very healthy surplus. On past experience, I do not

>think this sum will be exceeded during 1999 - even if we have one or two

>more urgent requests, they are more than likely to be offset by delayed

>workshops, so I think this is actually likely to be an over-estimate.
>Moreover, I have assumed that ALL the money allocated by IGBP for Synthesis
>will be spent in 1999. We are under some pressure to do this, but the pace
>of the exercise makes me suspect that we may have difficulty.

>
>At the end of the Table, I list 3 additional commitments I would like to
>propose for prioritizing and I discuss each briefly below:

>
>1. REDIE (which you may remember stands for Regional Educational and
>Infrastructure Efforts (about which we have, so far, said very little and
>done even less).

>
> In this area, one of the ideas gently simmering on the back burner has
>been the notion of winning support from START to run something like a
>Summer School for selected young scientists from developing countries. This
>emerged from an informal discussion between ourselves in the Office, Bruno
>and Roland Fuchs, the Director of START, when he was over here on a visit.
>At the time, he seemed quite keen on the idea, but has since been silent.
>No matter, I still feel it is an idea worth working towards at least up to
>the pre-commitment stage and I have been exploring informally the
>possibility of basing such a course in London.

>
>This coming summer, I think we may have a chance to do a kind of partial
>trial run. Thomas Stocker and Andy Lotter (a first class paleolimnologist
>here in Bern) plan to run a Summer School nearby this year. Thomas
>approached me some time ago to see if PAGES could support participation by
>any overseas students and my reply was a very cautious one to the effect
>that we would normally expect to be approached and have an input at the
>planning stage and that we would only really consider such a possibility in
>the context of training for scientists from developing countries. Having
>discussed the whole thing more fully with him, I begin to wonder whether
>it may offer quite an interesting possibility. My plan would be to seek
>nomination of/applications from say 3 to 5 young scientists from different
>parts of the developing/former eastern bloc world (representing each of the
>PEP Transects) and bring them to Bern both for the course and for a short
>period linked into the PAGES Office. The ideas behind the latter part of
>the suggestion would be to
> - support their participation if need be,
> - give them some sense of PAGES and its role in international global
>change science/IGBP etc and
> - solicit feedback and advice about what the shape of an ideal course for
>developing country scientists interested in PAGES activities might be.

>
>I believe that even if we did not have something like REDIE in our
>Implementation Plan it should be an important commitment; since we do, it

>is an absolute obligation which we ignore at the risk of serious
>allegations of bad faith.

>

>2. I feel there will be a need to follow up my PEP II visit to Australia
>with something positive there. John Dodson is responding well to
>suggestions about more co-ordination and bringing in more colleagues to
>share the responsibility, but I think that if whatever we agree in Perth is
>actually to work, there will be a need to fund a WORKshop (as distinct from
>a mini-symposium) of thematic and/or regional co-ordinators to get their
>act together. We should offer money for this.

>

>3. The difference it has made having Cathy Stickley (based at UCL) working
>for PEP III is fantastic, but we risk losing her input unless something can
>be done. I'm negotiating with ESF, but it will be over a year before their
>finely grinding mills deliver anything. Rick and Francoise are also going
>to apply to EC for Framework 5 funding, but that will be no quicker. I am
>seriously considering asking Zimmie to help bridge the gap since he did not
>quite close the door when I last talked this through with him, but I feel
>that if I do this, PAGES might need to put up a bit more colateral, the
>more so since we are in credit.

>

>Both 2 and 3 reflect my view that the PEP's remain an absolutely vital part
>of the PAGES structure and need to be supported if that is the only way
>they can achieve their objectives.

>

>All three of the above suggestions require some endorsement in principle
>before I take them any further. If we were to spend all the funds envisaged
>before the end of 1999, our budget credit would be very much reduced -
>probably by too much, but I believe the PEP funding would probably be paced
>over a longer period and that the other items in our budget are more likely
>to be marginally under- than over-spent, so I do not feel we are proposing
>any unreasonable risk.

>

>I look forward to any reactions members of EXCOMM may have to these
>suggestions.

>

>Withh all good wishes,

>

>Frank

>

>

>Attachment converted: Macintosh HD:Budget for 1999 (RTF /MSWD) (0000B314)

>

>Frank Oldfield

>

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tel: 303-497-6160

From: Simon Tett <sfbtett@meto.gov.uk>

To: Peter Stott <pastott@meadow>, Gareth Jones <gsjones@meadow>, Myles Allen <allen@wobble.ag.rl.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>

Subject: Tuesday Meeting

Date: Wed, 17 Feb 1999 23:01:45 +0000

Dear all,

thank you for the meeting on tuesday. I think it went well. Here as promised and slightly late is a summary of what we discussed. Myles can you forward the message to Michael. Can you let me know if you are all happy with this and once I have made any corrections you want I'd like to send it to John, Geoff and Tim Barnett -- anyone else you think should get it?

Proxy Climate forcing.

Solar -- Beer has a Be based proxy reconstruction of Solar ACTIVITY which can be converted to irradiance changes. [Is it different from LBB or H&S ?] Has the LBB dataset been updated? Has Svensmark got a better handle on his proposed physical mechanisms to amplify solar irradiance changes? [Someone to check at RMS meeting which I won't be able to attend] Want forcing back to 1600?? though HC would find it hard to justify doing runs that early -- me to see if John/Geoff think useful or not.

Me to check with William the source of the rumour about problems with the H&S dataset.

Volcanoes. Volcanoes are an important climate forcings [Issue for IPCC??] Do volcanic eruptions cluster? Myles to "persuade" a student to look at Phil/Keith's dataset and see if there is evidence for this? Are there other indices of volcanic activity? Is climate response to volcanoes sensitive to mean state?? i.e. in cooler climate get bigger response. [Gareth could see from our model if Krakota response significantly different from Pinatubo]

Proxy Climate data + comparison with obs and models.

Keith/Phil have 400 sites of high quality tree ring density data which there are willing to let HC (Mat) use to do a crude model/data comparison. Mat and Tim to liase on what they are doing. Note that funny things are happening in the density data post 1950. Also available may be some borehole data [Phil to talk to Pollock/Wang about possibility] which could use to compare with model -- should consider using lower soil temperature rather than 1. m temp. There are a few sites with data from 0A to 2000 as well as many sites with data for 1700 to 2000 -- should consider both. There may be some other tree ring data which tells us something about SW USA precip and thus ENSO.

Tim wants to compare patterns of temperature var from the proxy data and compare that with the models i.e compare "observed" and modelled covariance structure rather than just the variability. Also Tim wants to try and unpick Mann's stuff.... HC to provide solar forced run from 1700 -- Me to check if it goes from 1700!

Our approach will be to compare model data "directly" with Proxy data rather than do Interpolation a la GISST or Mann et al.

EU proposal

Not clear if in this years framework 5 call there will be room for Detection/Attribution proposals (which is how we'd like to frame a model/proxy comparison). Other issue is that QUARCC 2 and model/proxy comparison could involve similar institutions which could cause problems. Phil to check if room this year for proposal. Keith pointed out that we can't just recycle the NERC thematic proposal (PRESIENT). There is good news on that front which suggests the proposal will go through with an 8 million pound budget!!!

Ad Hoc detn group.

Not much said on that (or at least I didn't note it) Phil -- you have some advice for me on that?

CLIVAR/PAGES

In the next 1-2 years there may be new reasonable quality ice core and sedimentation data available. Data availability from the proxy and modelling groups is an issue (another reason for an EU proposal!).

Phil pointed out that there is a lot of instrumental data (in "funny" units) which could be digitised in Europe.

Keith is planning on writing a "call to arms" paleo data paper.

I think I need to come up with a list of actions.... Anyone want to volunteer.....

Simon

From: Eugene Vaganov <evag@ifor.krasnoyarsk.su>
To: k.briffa@uea.ac.uk
Subject: No Subject
Date: Fri, 19 Feb 1999 13:55:20 +0300 (MSK)

From: <dndr@ifor.krasnoyarsk.su>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Some information about the super-long tree-ring chronology
the East of Taymir and Putoran

Dear Keith

I sent two variants of letter by mail few days ago.
Hope that you received fax copy of it.
There are the references you ask:

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Regards, Gene.

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Fred.Semazzi@soc.soton.ac.uk
Subject: Some things of possible CLIVAR interest
Date: Thu Feb 25 17:08:21 1999
Cc: t.osborn@uea,p.jones@uea

Dear Fred,

The following legends refer to the appropriately titled post-script files that will be sent to you separately by my colleague Tim Osborn.

Please note that these results are products of the European Community funded project ADVANCE-10K (Analysis of Dendrochronological Variability and Associated Natural Climates in Eurasia - the last 10,000 years). Environment and Climate Programme Contract ENV4-CT95-0127. See also <http://www.cru.ac.uk/cru/research/>

As I said on the 'phone , due acknowledgement of the above is important to us!

Figure 1

Annually averaged tree-ring density data from 400 high-latitude or high-elevation sites around the Northern Hemisphere. This series represents interannual and multidecadal summer temperature variability from A.D.1400 onwards. This series shows circum-hemispheric summer temperature variability on interannual and multi-decadal timescales and demonstrates the relative cooling effect of known, and some probably as yet unknown, large explosive volcanic eruptions.

Figure 2

Normalized tree-ring -density anomalies around the Northern hemisphere showing patterns of likely summer temperature changes year by year through the relatively cool decade of the 1810s, in part caused by major volcanic eruptions in 1809 and 1815.

Figure 3

Decadally-smoothed timeseries of standardized radial tree growth at three high northern latitude regions during the last 2000 years : Tornetrask, N.Sweden (20E); Yamal(70E) and Taimyr(102E), Russia. Positive and negative values of these data represent relatively warm and cool summers, associated at each location with the strength and position of large-scale atmospheric circulation features.

I have asked Phil Jones here to send you a post script file and reference for the mean 1000-year Northern Hemisphere curve. His email address is shown above.

You may be also interested in some reconstructions of the NAO made by various people. If so ask Tim about these.

best wishes
Keith

From: Mike Hulme <m.hulme@uea.ac.uk>
To: Jose Caicedo <jdpabon@bacata.usc.unal.edu.co>, cubasch@dkrz.de,
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Subject: URGENT - IPCC DDC consultation
Date: Thu, 01 Apr 1999 12:46:01 +0100

Dear TGCIA'ers,

I have two questions to raise with you regarding the IPCC Data
Distribution
Centre. The first one concerns advice regarding a GCM submission to the
DDC and the second concerns mirror web sites for the DDC.

1. GCM submission.

The LMD (through Herve Le Treut) has requested the runs from LMD coupled
GCM be lodged with the DDC. His original request (July 1998) is appended
below as text ATTACHMENT 1. We originally rejected the submission on the
grounds that the runs were not historically forced, i.e., they were
cold-start experiments with 1% p.a. forcing being introduced from
'current'
baseline and different to all other DDC runs.

However, LMD have re-submitted their request for reasons outlined in
ATTACHMENT 2 which is an email from my DDC Co-Manager Michael
Lautenschlager (dated 12 February 1999). In this ATTACHMENT Michael
makes
a proposal to include the LMD model runs, but as 'related modelling
results' rather than as 'full status' DDC results.

We need to take TGCIA soundings on this. Strictly, the LMD runs do *not*
qualify according to the criteria the TGCIA established back in May 1997.
The question is how flexible are we prepared to be and whether including
model runs with a different experimental design may either a) confuse
impacts users and/or b) invalidate inter-model comparisons. Bear in mind
also that if/when new GCM results forced by SRES forcings are generated
this summer and beyond, we will need to consult again about how the DDC
handles/presents these new SRES runs. At present the DDC does not have a
mandate for these either.

Please would you submit your opinions to me by Monday 12 April. I will
then compile the views expressed and make a recommendation.

2. DDC mirror web sites.

With the DDC web site now fully operational (and the CD-ROM about to be released) we need to consider our idea for mirror sites around the world. Users are picking up data and information from both the Yellow Pages (full GCM archive site) and Green Pages (synthesised GCM results, observed data, and other scenario data and visualisation), but for some users/regions/operations access is very slow.

Proposed mirror sites might include: CSIRO (Victoria), IIT (Delhi), NCAR (USA) and Cape Town (S.Africa). Maybe a Japanese site also.

The mirror sites could consist only of the Green Pages (about 0.5GB requirement) or both Green and Yellow Pages (several GB requirement, but I have not checked exactly how much with DKRZ). I know that we can arrange for the mirror sites to automatically refresh every 24 hours therefore reflecting perfectly any developments on the host mother-site (i.e., the mirror sites must be perfect mirrors).

Could I also ask for your views on the desirability of these options, whether Green only or Green plus Yellow, how many mirrors and where they should be? Please let me have your views on this also by Monday 12 April.

In considering both these questions it is perhaps worth thinking about the longer-term future of the DDC beyond TAR and into 4th IPCC Assessment. Although TG CIA and the DDC has now only a mandate through the lifetime of TAR, for us to really learn from our experiences and to achieve full benefits for IPCC, then we need to be thinking ahead beyond year 2000.

Mike Hulme

—
—

ATTACHMENT 1

—
—

Subject:

From: Herve.Letreut@lmd.jussieu.fr at internet

Date: 9/7/98 9:08 pm

Dear Maria,

At the IPCC meeting a week ago, I spoke with M. Hulme concerning the possibility of having our simulations being integrated in the IPCC

data base (DDA?)

I think that our simulations meet a number of the criteria:

- the control simulation is 200 years long
- the model has participated to CIMP1 and CMIP2
- it is described in details (description posted on the WEb in the Euroclivar Web site: <http://www.knmi.nl/euroclivar>)

Our main problem concerns the definition of the experiments. We have used a model without flux correction and have decided to start from observed Levitus data. The coupled model has some drift but it stabilizes rather quickly and the thermohaline circulation is quite stable

Accordingly our initial CO2 value corresponds to a recent past: 320 ppm. >From that value we have increased directly the CO2 concentration of 1 percent per year. We have therefore not allowed for an 'historic' increase of the CO2 before the actual 1percent increase, which is due to a lack of understanding of the IPCC rules.

My feeling is that scientifically this is not too important (we have no 'cold start' symptom when we look at the difference between the perturbed and controled run). I have realized that in the context of the IPCC, however, people may think otherwise.

My question is two-fold:

- Can our experiment nevertheless be integrated in the IPCC data base. This is important to us: if it cannot we will not realize the sulfate experiment we had planned to do, and wait for the future scenarios to be decided.
- I hope that I will be more easily aware of the IPCC initiatives in the future. But is there any procedure through which we can make sure in advance that a given experiment we decide to carry out does get approved by the IPCC?

Sincerely yours

Herve

Herve Le Treut

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ATTACHMENT 2

Hamburg, den 12. February 1999 (15:00)

Dear Maria and Mike,

last week I have a discussion with Herve LeTreur from LMD in Paris about the DDC rejection of the French contribution to the climate scenario calculations. He informed that the climate modellers are running into political difficulties because no French data are contained in the DDC.

We have rejected the data last year because they design of his experiments are not directly comparable to the DDC requirements. A recalculation is not possible within short term.

In order to prevent the French colleagues from difficulties I suggest to install an additional section in our DDC page which may be entitled 'DDC related modelling results'. In this section Herve`s data as well as data from other groups can be disseminated. The processing priority is certainly lower than for the direct DDC data.

Do you agree with my suggestion?

Best regards, Michael

Dr Mike Hulme
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Climatic Research Unit fax: +44 1603 507784
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University of East Anglia web site:
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Norwich NR4 7TJ

Annual mean temperature in Central England during 1999
is about +1.5 deg C above the 1961-90 average

The global-mean surface air temperature anomaly for 1998
was +0.58 deg C above the 1961-90 average, the warmest year yet recorded

From: Keith Briffa <k.briffa@uea.ac.uk>
To: mann@snow.geo.umass.edu
Subject: Re: ipcc update
Date: Mon Apr 12 13:22:40 1999

Mike

I am off to Finland for a week but I am sending you (via Tim) a copy of a draft perspectives piece for Science on your recent 1000-year reconstruction paper. They want to run it in early May I think and I have been told I will see their edited draft on my return. The idea was to make a wider comment that just report on your latest curve so I decided to mention uncertainties in tree-ring data while pushing the need for more work on high-resolution proxies and especially interpretive work in the very recent context of high temperatures and other possible anthropogenic environmental disturbance. The trouble is that they would only give us 1000 words and one Figure. Anyway this Figure now contains a selection of various large-scale temperature average series - all recalibrated against northern warm season (April-Sept) average land data north of 20 degrees north. This is just to provide a convenient common scale - all the original season /area references are given. You will see that this brings Phil's curve nicely back in line and the correct (low frequency) density curve now fits better also. I have taken the opportunity to put our new longish (2000-year) tree-ring width curve in representing the north of Europe/Siberia. This is the average of Tornetrask (Sweden) and Yamal and Taimyr (Siberia) - all processed to retain low-frequency variance. These curves and a similar average incorporating all the Northern tree-ring data (not including the large density set) are in my paper for the Pages open science meeting publication. Tim and I will produce a short paper describing the new low-frequency density curve, probably for Geophysical Research Letters. For the meantime I hope you think the perspectives piece is O.K. Let me know if you have any problems with it - but remember that they are going to hack it about anyway. By the way, how did you compare the high-elevation (PC1) timeseries with Jacoby and D'Arrigo's northern treeline data in your paper when the latter only go back to 1671? Did you use their reworked Gidding's dataset for Alaska?

Thanks for the message on the IPCC stuff. I am happy to write any additional bits or make suggestions. Sorry I did not get back to you last time but I was confused about the timetable. Thanks for putting my name on the list. I will make comments again as soon as I see the next draft. Cheers

Keith

At 06:20 PM 4/11/99 -0400, you wrote:

>

>Dear Phil, Keith,

>

>An update on IPCC. Almost done w/ my revisions, taking into account
>yours and Phil's comments, and included the *correct* briffa et al
>series. Keith--added your name in the contributor list. Sorry for
>the earlier omission (I hadn't heard from you at the point I
>wrote the initial draft)...

>

>A couple things--Phil can you send a copy of the in-press Rev
>Geophys. article as soon as possible? I'd like to have a copy
>for my own records...

>

>Also, I'm going to have to leave it to you to insert some
>of the references you mentioned in your comments which I'm
>not familiar with. Also, you'll need to supply an updated
>reference for the Briffa et al series as soon as it is
>ready.

>

>I'll send you the revised draft when I finish it within a day
>or two, at the same time I send it to Chris, Jim, and Jean. We'll
>need to incorporate Pfister's contribution (if it ever comes in),
>and Jim and Jean's suggestions at the next stage. I believe it
>will be Chris' responsibility to coordinate this. Anyways, more
>from me soon...

>

>best,

>

>mike

>

> Michael E. Mann

> Current	Starting Fall 1999
>Adjunct Assistant Professor	Assistant Professor
>Department of Geosciences	Dept. of Environmental Sciences
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> <http://www.geo.umass.edu/climate/mike>

>

From: mann@snow.geo.umass.edu

To: k.briffa@uea.ac.uk

Subject: No Subject

Date: Tue, 13 Apr 1999 15:05:02 -0400 (EDT)

Cc: juppenbrink@science-int.co.uk, mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, t.osborn@uea.ac.uk

Dear Keith

(Tim, please get this to Keith by FAX or other means, if he is unlikely to have received this at his own email while traveling).

It's a good piece overall. As you might suspect, I do have several comments. Ray and Malcolm may send along a few of their own. Malcolm in particular may want to comment on some of your points regarding dendroclimatic series and our ITRDB PC#1 series which figures so prominently in our millennial reconstruction.

1) page 2, top paragraph:

It's is very misleading to make it sound as if we are strictly reconstructing northern hemisphere mean temperature, and then say "4 of the records are actually from the southern hemisphere locations". This is misleading for a number of reasons. First of all, if one is going after true northern hemisphere areally-weighted mean temperature 0-90 degrees (as we are), then the southern hemisphere tropics are actually more relevant than the high-latitudes of the Northern Hemisphere. Careful diagnostics of surface temperature covariances by Alexey Kaplan, Mark Cane and others have shown this clearly to be true. BUt more than that, we are reconstructing the full 20th century surface temperature domain shown in Figure 1 of our '98 Nature paper. This is a GLOBAL domain, albeit sparse outside the southern hemisphere tropics/subtropics, particularly the southern oceans, for obvious regions. THE proxy network roughly overlaps the spatial domain of surface temperature we are reconstructing (ie, compare Nature '98 figure 1a and figure 1b). We choose to diagnose from this spatial domain the northern hemisphere mean only because that is the hemisphere for which we can meaningfully talk about a true hemispheric mean. But both the predictor and predictand have a global distribution. Without going on and on, I think its clear why your comments here are a bit unfair in how they represent why we use southern hemisphere data. This is probably the most important point that needs to be revised here.

2) page 2, 2nd paragraph

A minor point, but an important one: It is incorrect to say the our uncertainties are based only on "a consideration ...goodness of fit...over the calibration period"! This is not correct. A key point is that the verification period (1854-1901) diagnostics (though based on a somewhat sparser distribution of gridpoint data from which NH mean temp can be estimated) give very nearly identical diagnostics in terms of unresolved reconstructed NH mean temp variance. So our uncertainties are based both on 20th century calibration and independent confirmation from 19th century data. PLEASE MAKE SURE this is clear.

On the bigger point being made here, I agree w/ you in principle, and this is a point that Phil has raised too: what we *DONT* take into account (though I challenge anyone to really ever be able to take this into account!) is the unknown potential bias due to degradation from diminishing quality of the underlying proxy data back in time. However, on some of the specific points in that regard, it is very likely not a significant concern in our reconstructions. We closely examined the spectra of the underlying proxy data to insure that those upon which our reconstruction ultimately relies have the amount of millennial scale trend/variability that would be expected for a climatic series for at least the null hypothesis of red noise. Malcolm independently examined the tree ring chronologies underlying our ITRDB PC #1 to verify that the standardization was appropriately conservative for a millennial-scale reconstruction. Furthermore, Malcolm verified that the ITRDB PC #1 is made up of heavily replicated chronologies as far back as we use them. So neither of the points you raise appear to be all that relevant to our reconstruction.

With regard to this point, I have some issues with your Figure that accompanies the piece. It is quite ironic given your comments about the potential impacts of standardization on the long-timescale variations. For our millennial reconstruction we have verified as carefully as has ever been verified, that the millennial scale trend is likely to be meaningful. I don't think you have done so for the 2000 year-long trend in the series you show, and if you have not verified that it is likely to have retained 2000 year long trends, it is VERY misleading to show this series along with the others. I don't believe that it is likely to accurately represent the 2000 year long trend in Northern Hemisphere mean temperature, as you imply by showing it here. I think this series needs to be removed from the plot. I have a related comment

below (point #5).

3) page 3, 1st paragraph:

Remove "this is a moot point" and replace with more appropriate language. It is not "a moot point" because the problem you point out has largely been shown to apply to tree ring density data (which you have largely been using), and much less so tree ring width data (which we are using). Furthermore, the comparison only goes through 1980 at which point there is little evidence that there is a significant decline in tree ring width response, although more evidence that there is already a problem at that point with density data. Your criticism is not quite fair here, and the statements should be revised to reflect more accurately on what we have done.

4) page 3, 2nd paragraph:

When you talk about proxy-based ENSO reconstructions, you should mention our NINO3 reconstruction! This is complementary to Stahle's SOI reconstruction in a number of ways. The appropriate references here are both our Nature '98 papers, and the chapter in Henry Diaz's latest book (in the press):

Mann, M.E., Bradley, R.S., and Hughes, M.K.,
Long-term variability in the El Nino
Southern Oscillation and associated teleconnections, Diaz, H.F. & Markgraf, V.,
(eds) El Nino and the Southern Oscillation: Multiscale Variability and
its Impacts on
Natural Ecosystems and Society, Cambridge University Press, 321-372, Cambridge,
UK, in press, 1999.

if you care to, you can download the galley version here:

<ftp://eclogite.geo.umass.edu/pub/mann/ONLINE-PREPRINTS/ENSO-recon/>

in either pdf format (chapter-diaz.pdf) or postscript (chapter-diaz.ps)

5) accompanying figure (see also my point #3):

There are problems with the 2000 year series in terms of your definition of the baseline for comparing with the other series, and this differs quite a bit from what we are likely to be showing in IPCC. It appears that both the density NH reconstruction and your 2000 year long

series fall at least 0.1C below the other series during the 20th century, and are probably running at least that much too cold the whole way through.

Also, correct "global temperature and non-temperature proxies" in your description of our series to "global climate proxies" which is a more honest way of describing them given our methodological approach, and make sure it is clear to the readers which series are extratropical and warm season, and which are full northern hemisphere/annual mean estimates (ours). Such discussion will, again, figure prominently in IPCC, and it would be a shame for Science to be publishing something that is misleading in that respect. In part, it was this issue that forced the publication of a followup to Phil's perspective by me, Ray, Malcolm, and Phil a year ago, and it would be nice to avoid that scenario this time around...

Thanks for your consideration of the above comments. I believe your piece will make an excellent "Perspectives" article for Science, once these comments are appropriately taken into account. I'll leave it to the Science editor in charge to determine if that is the case.

best regards,

mike.

Michael E. Mann

Current

Starting Fall 1999

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From: Brian Luckman <luckman@julian.uwo.ca>
To: K.BRIFFA@UEA.AC.UK
Subject: GROVE REVIEW
Date: Tue, 13 Apr 1999 16:33:08 -0400

Keith,

The attachment is in WORD and better formatted.

Brian

Dear Keith,

Enclosed please find my comments on Jean Grove's paper. It gives the impression of a cut and paste job written in haste with several minor annoying errors. It lacks the synthesis I would have expected and reads like a catalogue. The paper is also not as comprehensive as would appear from the title. Six months ago I reviewed a paper by her (for Astrid) on "The Initiation of the Little Ice Age in regions round the North Atlantic". The paper she submitted to you is clearly complementary and reviews " the rest of the world" for comparison with the classic areas discussed in the earlier paper. Yet the earlier paper is only alluded to once (rather coyly) and does not appear in the references. This surely has to be significantly recognised in the title and body of this paper, because as it stands, the review of this earlier (best dated) material is far from adequate.

I cannot speak for most of these data directly but the North American material I am familiar with is not particularly up to date (though in fairness most of Greg Wiles's stuff is still in press). I have sent her under separate cover copies of my Little Ice Age in the Rockies paper (about 6 months ago) and more recently the Luckman and Villalba review paper on glacier fluctuations of the last Millennium along the PEP-1 transect. (copies are on their way to you too).

I think her mixing the discussion of ice core records and glacier histories significantly muddies the waters on whether the term LIA should be used to refer to a glacier or a climate event. I feel this should be addressed and the paper needs a more effective conclusion. She must also decide whether she wants diagrams or tables.

I don't know how she will take these criticisms but, as she is just finishing revising the book, I would have thought she could have presented

a better synthesis. I leave it to your judgement as to how to deal with these comments. The paper could be much better but that depends on how much she is willing to reorganise and to some extent rethink what she has written.

I am sending you this e-mail. Do you want me to return the manuscript to you? If you wish I can also e-mail WORD copies of the two papers to you (and her) if you wish a rapid turnaround. But you will only get the diagrams by mail. If I don't hear from you tomorrow I'll just put everything in the mail.

Cheers

Brian

Review of "The Initiation of the Little Ice Age" by Jean Grove

This paper is a useful summary but needs significant fine-tuning and possibly retitling before it should be accepted. The title promises a comprehensive review that the text does not deliver. When I first read this paper I kept asking myself- where is the discussion of all the well-dated early LIA material from Switzerland, Canada and Alaska? Then I remembered the paper by the author that I reviewed 6 months ago entitled "The Initiation of the Little Ice Age in Regions round the North Atlantic". The present paper is not a global review of evidence but a companion paper that compares the "Rest of the World" with the "European/North Atlantic record" discussed in that earlier paper. The crux of the problem is the first sentence after the title "Little Ice Age Initiation ..." at the top of page 3. I initially read this to mean that Holzhauser had submitted a paper on the European record to Climatic Change. Careful re-reading suggests that the author is actually referring to her own review paper. This misunderstanding could be avoided by explicitly acknowledging, in the

introduction to the present paper, that the evidence for the circum North Atlantic Region has previously been reviewed by Grove (in press), giving the full citation in the references, and that the section entitled "LIA initiation in regions around the North Atlantic" is a brief summary of that review.

There are a number of general points that need to be made before discussing specifics.

1. This discussion begs the question of how one would recognise the beginning of the LIA (A question I raised in my earlier review) Why, for example is the line drawn between the 8-9th century medieval glacier advances and the 12-13th century ones? Possibly this is related to the author's definition of the so-called Medieval Warm Period which has recently been extensively discussed (Hughes and Diaz 1994). It might be useful to insert a brief discussion of the rationale for this boundary and a definition and defence of the use of the term Medieval Warm Period.in either the introduction or the final discussion section.

2. I also feel that there is a logical inconsistency in the way the author uses the ice core evidence in this paper. In her abstract Dr Grove indicates that "the term LIA refers to the behaviour of glaciers, not directly to the climatic circumstances causing them to expand " (abstract lines 3-4). I agree strongly agree with this usage to differentiate between a glacier event and a climatic event. However, the discussion of the definition of the LIA from the ice core work is based on either periods of greater annual snow accumulation or inferred paleotemperatures from isotopic records. i.e. these definitions are based on climatic events not glacial events. The author should perhaps address this dichotomy and discuss it more fully. If one wishes to argue for retaining the term LIA for the glacial event, it is inconsistent to identify it in ice core records based on temperature (or snowfall) records.

3. The author appears to have an implicit faith in the veracity of ^{14}C dates which I do not share and a disdain for minimum age dating based on lichenometry or dendrochronology. There is a strong emphasis on calendar dated ^{14}C ages throughout this paper and age determinations by other techniques are often significantly downplayed. The paper never specifically addresses the relative errors involved in age determinations by these various techniques. Lichenometry and minimum age tree-ring dating of moraines are disparaged yet, in this timeframe the error terms are almost certainly less than ^{14}C dates from equivalent situations (i.e. dates above glacier deposits or on moraine surfaces). The comments made in this paper

about lichenometric dating and dendrochronological dating of moraines (from minimum tree ages) only stress the likelihood of large errors through the use of these dating techniques. These comments may be appropriate for some moraines that date from the 12-13th centuries but they should not be unqualified, universal statements cannot remain couched in those terms. In most situations lichen and tree-ring minimum ages for moraines of the last 500 years or so are considerably more accurate than 14C ages would be.

4. In my review of her earlier paper I commented that I did not consider that sites in the Canadian Rockies could be described as "around the North Atlantic". In this paper, it makes no geographical sense to review the results from the Rockies separately from adjacent areas in British Columbia and Alaska which they closely resemble (see Luckman and Villalba, in press). I have no objection to the comment that the Rockies material was discussed in a previous paper (and will therefore not be repeated in detail) but surely in the context of this paper these results should be presented in the discussion of evidence from Western North America. Having recently reviewed the literature for North America I also note there are omissions of significant recent material that is recently published or in press (see Luckman and Villalba attached).

5. The Tables and diagrams appear identical except for Table 10. Tables 1-9 should be deleted?

More detailed and specific comments follow.

Page Para Line

1 3 4 why is lichenometry excluded?

1 4 1 Reference to Grove in press??

1 4 3. In this paper evidence from.....???

2 2 1-2 Is dating within the last millennium considered to be the critical defining factor in identifying a glacier advance as belonging to the LIA? See comment about the inception of the LIA, above.

2 3 1 delete orphan period before text

3 2 3 Holzhauser 1998 not in the references.

- 3 2 5 change phrase within brackets to (Grove, in press) and insert in references.
- 3 3 1 ... Rockies dating derived from ring width and.....(revise)
- 3 3 6 Also Stutfield after 1272 (Luckman , in press)
- 3 3 11 Luckman 1995, 1996a and b??? (there is no 1995 a and b)
- 3 3 14 Luckman 1991 not in references. Could be Luckman 1993? Luckman et al. 1997 (never referenced) or Luckman 1996
- 3 4 3-4 Given the dispute about the universality of the Medieval Warm Period (see Diaz and Hughes 1994) perhaps it would be better to indicate the dating here e.g. 10-13th centuries?
- 4 2 1-5 based on what evidence? Lichens, historical data , 14C?
- 4 4 What are these moraine dates based on?
- 5 2 1 delete comma
- 5 2 3 1991a or b?
- 4 1 8-9 snow cover extended? = period of snow cover lengthened between these dates?
- 5 3 end of several lines truncated in xerox copy sent to me
- 6 1 as above
- 5 3 3 not in references, Haeberli ?? Kuhn references also missing.
- 6 1 19 reference for Swiss example?
- 6 1 end negative summer temperature anomalies or negative annual anomalies?
- 7 1 2 said claimed ? = said or claimed?
- 7 1 5-8 admitted by who relative to what? This somewhat disparaging comment seems dismissive. Perhaps lichenometry is the only available technique. Is the author aware whether or not these glaciers ever extended into forested areas. Is there any wood associated with these moraines? Does the evidence

presented by these authors and their lichenometric dates indicate the presence of early LIA moraines?

7 2 7 delete end bracket

7 2 last What is being implied here? Were the samples dated of the same species, were the records long enough to crossdate?

8 Table 1 etc Are these Tables or Figures? The Tables within the text seem almost identical to the diagrams appended at the end.

9 2 1 and Footnote 5; Rothlisberger 1986 not in refs. Rothlisberger and Geyl??

9 3 2 Figure 2 and Table 2 seem identical which will be used?
References should be R and G 1986 not Rothlisberger 1986?

10 1 6 is thought?

10 1 9 geographically close or close in age?

10 1 last sentence surely should come after the next section?

11 1 last The glaciers or monsoon cover 46,000 square kilometers?

12 2 13 Why must it have preceded the LIA? based on a 14C age?

13 Table/Figure 3 explain XXXs

13 1 8 "The Dunde record shows the Little Ice Age clearly" This section needs to differentiate clearly between the glacier fluctuation record, the snow accumulation record and the isotopic temperature signal. If the term LIA is being used to define/describe glacier events then it cannot also be used- without qualification- to describe climatic events. The author is describing climate signals here not glacier advances. This section and the discussion on page 14 needs more clarification and discussion.

14 1 5 after 1264 based on what evidence?

15-16 Apart from a conference abstract listed in the references but not cited, there are no references to the spectacular work of Wiles in tree-ring dating of overridden forests in this area. In addition, the discussion of the abstract by Yager et al., is somewhat confusing. (how can

one have a floating chronology from 911-1992?; are tree-ring dates or calendar equivalent 14C dates being cited here?) This section on Alaska is quite dated (see Luckman and Villalba and several references by Wiles and Calkin cited therein).

16 2 This section needs to be reworked. The data presented for Klinakini Glacier

and Franklin Glaciers are presented and then queried without reaching any conclusion. Both indicate glacier advance after the dated materials and the comments qualifying these dates apply equally well to many other dates cited in this paper. (Lag time is ignored at several other sites in the discussion). The reporting of the Bridge Glacier site is incorrect. Ryder and Thomson only identify one advance here, not two and consider both 14C ages provide limiting dates for the same event. The till described is between the paleosol and the present surface not between two paleosols. Although scattered, there are several other papers on this region- Ryder 1987, Desloges and Ryder 1990, Clague and Mathews, 1992 etc - see Luckman and Villalba, in press).

16 As stated earlier, discussion of the Canadian Rockies should be included with western North America. There are also early LIA moraines on Mount Baker in Washington.

18 1 Rothlisberger and Geyh?.

19 1 1-2 Rationale for this statement?

20 1 1-2 See earlier discussion. The ice core data provide information about snow accumulation and climate- not necessarily glacier advances

20 1 end in-situ trees at what site? Again Thompson is referring to a climate event not a glacial event

20 Footnote 13 Based on what data? 1970 predates the 1976 Pacific Climate shift.

21 2 13-14 Again, is this bias? In my experience dating based on the oldest tree for most moraines has far smaller error terms than radiocarbon dating. In this specific case the moraine may be older but this does not justify the statement "approximate at best"

21 2 20 why is Rothlisberger's date of 1000-1220 cal AD acceptable in this circumstance but Ryder and Thomson's date of 1040-1210 (p16) not?

24 footnote 14 although the survey may have delimited glacier area, I assume it was an aerial survey !!

24 3 1 sub-fossil trees.

24 3 5 see comment on 21 2 20 above.

24 3 8 14C dates do not sample! Sample HV.xxx taken from a stump.....etc

26 footnote 16 see Gordon and Harkness, 1992 Quat Sci Rev, 11 697-709 for a comprehensive review

28 2 see earlier comments on ice core discussions.

28 3 4-5 what specifically is meant here? Warmer and cooler intervals for which dates?

29 2 5-6 see above. Lack of obvious period of significantly cooler temperatures?

30 2 1-2 But you don't present any "precise dates" in this table, nor are any of the calendar dendro dates from Alaska included . If this table is intended to be a summary should not it show all of the data being discussed?

31 1 3-4 The implication here seems to be that a 14C date from an in-situ log gives a more precise limiting date for the subsequent glacier event than date from a log that is not in-situ? Is this the case? Or is it that dates on wood are better calibrated than dates on soils, bones or other materials?.

32 1 4 Luckman 1995 I think.

32 1 10-11 These are not dates from moraines but dates from forests overridden by glacier ice. Are there any examples of moraines dated to the 13th century presented in this paper.

Table 10 is never referred to in the text. It needs a caption. Does 13=13th

century or 1300s?

33 1 1-3 NO. decreased temperatures or increased accumulation correlated with the

LIA have been identified in these cores.

This is not a very synthetic conclusion.

34 Barlow et al., 1997 delete in press

35 Bjork Antarctic

36 Corte CONICET not CONISET

36 Eglinton Font

36 Fushimi Initials

36 Fuhimi 1978 delete reference to 1977!

37 include Grove in press

38 Holzhauser 1998??

39 Luckman 1993a should be Luckman B.H., Holdsworth, G and Osborn G.D., 1993
reorder Luckman 1993b as Luckman 1993

40 Luckman 1996b Dendroglaciology not Dendrochronology
Alberta not British Columbia

41 Nesje and Dahl 1991b delete)

Nesje et al., Jostedalsbreen ???

Nesje and Rye Geografiske ? capital G

42 Thompson 1980????

45 Wardle Omoeroa (capital)

Attachment Converted: "c:\eudora\attach\grove.norwich.doc"

From: mann@snow.geo.umass.edu
To: k.briffa@uea.ac.uk, mann@geo.umass.edu, mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, t.osborn@uea.ac.uk
Subject: oops typo. disregard previous message
Date: Wed, 14 Apr 1999 16:06:45 -0400 (EDT)

Dear Tim,

Thanks for your comments. Some responses to them are given below. I'll be too busy for further correspondance as I prepare for travel, leaving Friday morning for a week.

Since I will be away and unreachable through next wednesday. I would thus request that you and Keith correspond with my co-authors Ray Bradley (who should be able to respond upon his return from current travel on Sunday /Monday) and Malcolm Hughes on the revisions (please cc to me so I can read upon my return), as I will be unreachable.

I'm sure we can come up with something mutually agreeable to all of us with this piece, as is my goal with IPCC, as long as there is proper communication and mutual understanding by all concerned. Lets strive for this--choice of language is a nontrivial element...

best regards,

mike.

COMMENTS

One additional new comment:

0) 1st page, "In attempting to do this...Mann et al...exemplifies" is unacceptable language to us. We confront the very problems that are being discussed here, so it is a disservice to us to say our paper "exemplifies" these problems. It "exposes" or "confronts" would be fair language, but "exemplifies" is unacceptable.

responses to your responses to my original comments:

1) I'm not sure how to interpret your response vis-a-vis my original comments here. My point is that our use of southern hemisphere records in the reconstructions is fundamentally sound, from the point of view of some very basic principles of optimal interpolation, etc., and given the domain we are reconstructing, which is not NH only, although we diagnose NH from our pattern reconstructions as a key index. There is no basis for what sounds like

a criticism of our use of such data. I couldn't tell if you were agreeing with this or not from your comments.

2) The uncertainties are determined from the uncalibrated variance given a certain predictor network. The predictor network is unchanged from 1820 to present, so the verification period (1854-1901) unresolved variance is an independent check on the calibration period unresolved variance. Both gives numbers in the range of 30% for the NH mean temperature reconstruction, meaning that the error bars we determine from verification period are essentially the same as those we determine from the calibration period. IN this sense, the error bars as determined from calibration and verification are essentially identical, The bottom line, if we had used the verification period to estimate the error bars, the eye would barely see the difference.

There may be a considerable misunderstanding on your/Keiths part, regarding regarding what is actually shown by the spectrum of calibration residuals in our GRL paper. It does not in any way conflict with what I indicate above. What this particular diagnostic shows is that there is no evidence of any increase in unresolved secular variance (ie century-scale and longer) in our reconstructions at least back to 1600. In contrast, there is evidence that such frequencies are not as well resolved as higher frequencies with the sparser predictor network available before 1600. Our estimates of uncertainty TAKE THIS FACT *EXPLICITLY* INTO ACCOUNT. Our uncertainties estimates are made up of two components that add in quadrature, including a component of uncertainty in the lowest-frequency variability as estimated from the spectrum shown, and a component of the highest-frequency variability from the spectrum shown. These are approximated as a step-wise break in the mean (white noise) level of unresolved variance at the edge of the secular band. Unlike any previous study, we have actually estimated the increased uncertainty due to the loss of low-frequency variability as it can best be estimated, and this is explicitly incorporated into our error bars, which is why those error bars expand considerably before 1600. This is discussed in the GRL paper, and is a VERY important fact. It would be very unfortunate if this fact were misrepresented!

3) I'll leave this to Keith and Malcolm to discuss (Malcom?). I think it is pretty clear in the paper what our assumptions are here, and what the justification is of those assumptions. There is of course room for differing opinions on this stuff, as it is all somewhat speculative, and we indicate that this is so in our paper.

4) good enough

5) I really doubt that the 2000 year trend is meaningful and, unlike the results we have shown, there is no confirmation that these 3

sites accurately reflect northern hemisphere mean temperatures to any reasonable level during the modern era.

Work by us and others looking at similar data would suggest that series in such regions are not adequately representative of the largest-scale trends. There is, further, no verification of the frequency-domain attributes pass any satisfactory test. For these reasons, I have informed Julia Uppenbrink directly that I don't believe this series should be shown in this context. I agree it is an important series, and it will be appropriate to discuss it in IPCC. But it should not be considered on a par with more statistically-verified true Northern Hemisphere mean temperature reconstructions, and it is very misleading to show it along with the NH mean reconstructions. The 2000 year trend runs absolutely counter to everything we know about the mid holocene. Extratropical Northern Hemisphere summer temperatures should have been at an absolute peak 4000-6000 ybp, and the 2000 year trend *ought* to at least be heading in that direction. The fact that it doesn't, and that the trend hasn't been verified in the sense discussed above, causes me real concern. It would be misleading to argue we have any reason to believe that NH mean temperatures have done what that series does 2000 years back in time...

Re, the adjustment of the series, I believe it is fundamentally unsound. Essentially, agreement over the period we can best constrained (20th century) has been sacrificed for agreement during the period we can't constrain, apparently for the sake of getting the different series to align during the 19th century. Please download the figures I have prepared for the latest IPCC report.

<ftp://eclogite.geo.umass.edu/pub/mann/IPCC/nhemcompare-ipcc.gif>

OR

<ftp://eclogite.geo.umass.edu/pub/mann/IPCC/nhemcompare-ipcc.ps>

You will see how I have aligned the series based on a 1961-1990 reference period for the instrumental series, and a 20th baseline adjustment for the alignment of all series. To me, this is the most reasonable adjustment of the series if they are to be shown together. It also shows the different that latitudinal variations make EXPLICITLY by showing the difference between our TRUE (0-90 lat weighted) NH annual mean temp series, and an extratropical (30-70 deg lat) average from our pattern reconstructions, which approaches quite closely the Overpeck et al '97 and Jones et al '98 series. Seasonal distinctions then the key remain difference. This is, I believe, the best approach to the comparisons, and the one I will favor

in IPCC.

The alternative is that true NH mean temperatures and extratropical NH mean temperatures must be shown on separate plots, because adjusting them the way Keith has provides a misleading picture, and one that I don't believe can be justified for the purposes of IPCC, regardless of what you choose to do with your Science piece.

From: "Raymond S. Bradley" <rbradley@geo.umass.edu>

To: k.briffa@uea.ac.uk

Subject: CENSORED!!!!

Date: Mon, 19 Apr 1999 10:41:31 -0400

>Date: Mon, 19 Apr 1999 10:06:52 -0400

>To: juppenbrink@science-int.co.uk

>From: "Raymond S. Bradley" <rbradley@geo.umass.edu>

>Subject: Climate warming prespctives article

>Cc: mann@snow.geo.umass.edu, mhughes@ltrr.arizona.edu

>

>I have just returned from Finland and have now read all the correspondence regarding the Science perspectives article you asked Keith Briffa & Tim Osborn to write. I've sent Tim Osborn & Keith Briffa a few suggestions re their perspectives article. If you would like to see them, let me know.

>I would like to diasassociate myself from Mike Mann's view that "xxxxxxxxxxxx" and that they "xxxxxxxxxxxx". I find this notion quite absurd. I have worked with the UEA group for 20+ years and have great respect for them and for their work. Of course, I don't agree with everything they write, and we often have long (but cordial) arguments about what they think versus my views, but that is life. Indeed, I know that they have broad disagreements among themselves, so to refer to them as "the UEA group", as though they all march in lock-step seems bizarre.

>As for thinking that it is "Better that nothing appear, than something unacceptable to us"as though we are the gatekeepers of all that is acceptable in the world of paleoclimatology seems amazingly arrogant. Science moves forward whether we agree with individual articles or not....

>

>Sincerely,

>

>

Raymond S. Bradley

Professor and Head of Department

Department of Geosciences

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<<http://www.geo.umass.edu/climate/climate.html>

From: mann@snow.geo.umass.edu
To: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk
Subject: Ray's coments
Date: Tue, 20 Apr 1999 09:12:04 -0400 (EDT)
Cc: mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu

Dear all,

Ray accurately (though w/ not the same level of detail) obviously recapitulates my main concerns here. As for the one area of disagreement (not understanding the reason for expecting an overall cooling during the 1st millennium), I'll refer Ray to the appropriate areas of his Paleoclimatology text book, and show him some additional recent work relevant to this, upon my return.

Thanks again to all for working to make the final product one we can all be happy with.

best regards,

mike

Michael E. Mann

Current Starting Fall 1999

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<http://www.geo.umass.edu/climate/mike>

From: "Connie Woodhouse (by way of \"Henri D. Grissino-Mayer\" <grissino@valdosta.edu>)" <woodhous@NGDC.NOAA.GOV>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Re: Problem with "az510.crn": No Correlation
Date: Mon, 26 Apr 1999 16:26:13 -0400
Reply-to: grissino@VALDOSTA.EDU

Dear Steve,

AZ510.crn is a bristlecone pine chronology. I suspect the others you are working with are ponderosa pine or Douglas-fir. In this region, these lower-elevation species have quite a different response to climate than the bristlecone. I haven't worked with the AZ510 chronology, but I would guess that bristlecone tree growth at this site would be favored by warm winter temperatures and perhaps somewhat drier conditions, while the ponderosa and Douglas-fir do well under cool, wet winter conditions. This may be the reason for your poor correlations.

regards,

Connie Woodhouse

Connie Woodhouse
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Campus Box 450
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Boulder, CO 80309
ph: (303)497-6297
fax: (303)497-6513
email: woodhous@culter.colorado.edu

From: Matthew Salzer <msalzer@POSTAL.AERO.UND.EDU>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: AZ510: No Correlation
Date: Tue, 27 Apr 1999 11:05:47 -0500
Reply-to: grissino@VALDOSTA.EDU

Steve:

I've had some experience with bristlecone pine on the San Francisco Peaks and you are correct in noting their lack of correlation with precipitation records and with other precipitation sensitive tree-ring chronologies like Slate Mtn. Ponderosa. There is no "problem" with the AZ510 chronology; it is, as suggested by Dave, Connie, and Jim, a chronology constructed from trees whose growth is not primarily limited by precipitation. Site location and tree species are critical when comparing chronologies and evaluating climate - tree growth relationships.

We've collected in the Peaks recently as part of an ongoing archaeological and paleoclimate project and have built a chronology extending back to 663 BC, more than 1200 years longer than the AZ510 chronology collected by Don Graybill in the early 1980's. We're working on a temperature reconstruction from this chronology that should prove to be a valuable addition to the already extensive archive of southwestern USA paleoenvironmental research.

Matt Salzer

Laboratory of Tree-Ring Research
University of Arizona
msalzer@ltrr.arizona.edu

Upper Midwest Aerospace Consortium
msalzer@aero.und.edu

From: "Mitchell, John FB" <jfbmitchell@meto.gov.uk>
To: 'Mike Hulme' <m.hulme@uea.ac.uk>
Subject: RE: GEC paper
Date: Fri, 30 Apr 1999 17:23:15 +0100

see inserts

jfbmitchell@meto.gov.uk
Hadley Centre for Climate Prediction and Research
The Met. Office, Bracknell
RG12 2SZ UK
Tel +44 1344 856613/6656
Fax+44 1344 856912

> -----Original Message-----

> From: Mike Hulme [SMTP:m.hulme@uea.ac.uk]
> Sent: Friday, April 30, 1999 12:31 PM
> To: Mitchell, John FB
> Subject: RE: GEC paper

>

> John,

>

> Could you have a quick look at this paragraph (see below) from the GEC
> fast-track paper. I do not understand:

>

> a) why CO₂-doubling forcing for CM2 is cited (see your original email at
> the end of this message) as 3.26Wm⁻² when I thought it was 3.471Wm⁻² (I'm
> sure I've seen 3.471Wm⁻² cited elsewhere for HadCM2).

[Mitchell, John FB] 3.471 in longwave, 3.26 when shortwave also
taken into account. Unfortunately modellers do not always make clear how
they have estimated their CO₂ forcing.

> and

>

> b) why the forcing curves in the plot William Ingram sent show higher
> forcing in CM2 than CM3 (by about 0.5Wm⁻²) when the CO₂-doubling forcing
> is
> *lower* in CM2 cf. CM3.

[Mitchell, John FB] HadCM2 is 1%/year increase in CO₂ which is only
approximately equivalent to IS92a. Hadcm 3 is "95a" - in fact "95a" I think
differs only from in the conversion of the 92a emissions to concentrations,
so strictly speaking is not an emissions scenario. As far as I know, Tom
never did explain why his concentrations in 1995 were different from the

ones Jonathan and I derived using his 1992 model- I think CH4 lifetimes and the CO2 sink were the main factors.

> [is this solely due again to the difference between IS92a and IS95a
> concentrations?]

>

> and

>

> c) why the global-mean warmings in CM2 and CM3 are quite similar when CM3
> has a higher sensitivity than CM2 (3.3 to 2.5K over the next century) and
> CM3 also has a higher CO2-doubling forcing (3.74Wm⁻² to 3.26Wm⁻², or
> 3.47Wm⁻² - see a)). Surely this should lead to faster warming in CM3 cf.
> CM2?

[Mitchell, John FB] See above - HadCM2 uses 1%/year increase in CO2, which gives a greater forcing than HadCM3, even after the effect of explicit trace gases is added in.

(about 0.5Wm⁻² by 2100). The greater climate sensitivity does not make as big a difference as one would expect. The difference in CO2 forcing per doubling is not the issue- the net forcing is, and that has been calculated taking the difference in CO2 response into account

Maybe I have misinterpreted something here.

> Thanks,

>

> Mike

>

> _____

>

> Paragraph from GEC paper

>

> "In HadCM3, greenhouse gas concentrations were increased from their 1860
> values up to present (1990) as observed and then following the IPCC
> emissions scenario IS92a (Leggett et al., 1992) from 1990 to 2100. Only
> one simulation was carried out. The increase in radiative forcing during
> the twenty-first century is very similar to HadCM2, being only 0.5 Wm⁻²
> (about 10%) smaller by 2100 than in the HadCM2 experiment (Figure 2).

> Note

> that the ratio of the increases in CO2 concentration (HadCM2/HadCM3) is
> much greater than the ratio of the changes in radiative heating. There is
> a greater increase in heating in HadCM2, so a greater increase in CO2 is
> required to produce the same fractional increase in heating. Also,

> because

> the heating due to doubling CO2 in HadCM2 is less than in HadCM3 (3.26
> Wm⁻²

> compared to 3.74 Wm⁻²), a larger increase in CO₂ is required to give the
> same change in heating. Note also that the increase in forcing varies as
> the logarithm of the change in CO₂ concentration."

>
>
> At 14:54 09/04/99 +0100, you wrote:

>>Hi Mike.

>>

>>2xCO₂

>>HadCM2 3.26 Wm⁻² including stratospheric adjustment and allowance for
>>solar absorption.

>>hadCM3 3.74 Wm⁻² as above.

>>

>>

>>Gordon C., C. Cooper, C. Senior, H. Banks, J. M. Gregory, T.C. Johns,
> J.F.B.

>>Mitchell and R. Wood, 1999. Simulation of SST, sea ice extents and ocean
>>heat transports in a coupled model without flux adjustments. Climate
>>Dynamics (provisionally accepted)

>>

>>Note year is 1997

>>Gregory, J. M. and J.F.B Mitchell, 1997. The climate response to CO₂ of
> the

>>Hadley Centre coupled OAGCM with and without flux adjustment, J Geophys
>>Lett., 24, 1943 -1946.

>>

>>I will try and look at then text now

>>John

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>>

>>> -----Original Message-----

>>> From: Mike Hulme [SMTP:m.hulme@uea.ac.uk]

>>> Sent: 09 April 1999 14:11

>>> To: Mitchell, John FB

>>> Subject: RE: GEC paper

>>>

>>> John,

>>>

>>> Here is a Word 6 version of the GEC paper. You need to give me two
>>> references (Gregory and Mitchell 1998 and Gordon et al 1999?) and check
>>> through the bits I have added. See especially what I have worded about
>>> CO2

>>> concentrations in Section 7 - quite what we cite for HadCM3 I'm not
> sure.

>>> It depends what the impacts people say about the sensitivity of their
>>> results to CO2 concentrations. I also have a question in the text in
>>> Section 5 for you.

>>>

>>> Figure 10 is not made yet - I thought I would produce this inter-model
>>> comparison plot for the Amazon given the interesting results we were
>>> getting there.

>>>

>>> I will wait for your comments before sending it to Martin and the other
>>> impacts people, but I must do this by the 19th April at latest.

>>>

>>> I think I understand where the various CO2 numbers come from now.

>>>

>>> Regards,

>>>

>>> Mike

>>>

>>>

>>> At 11:59 09/04/99 +0100, you wrote:

>>> >Dear Mike,

>>> > I think we have traced where the different CO2 values have come from

	HadCM2		HadCM3	
	assumed	'correct'	assumed	'correct'
>>> > 2020s	441	470	457	434
>>> > 2050s	565	590	574	528
>>> > 2080s	731	770	712	638

>>> >

>>> > The left hand HadCM2 value we think comes from SA90 - Peter Cox will
>>> >check. The second HadcM2 value is notional- I don't think the
>>> inconsistency

>>> >between the the columns matters that much, since there is no "correct"

>>> >HadCM2 value.

>>> > The HadcM3 values do matter. The right hand side value is

>>> >what was used in the model, and what Willaim took from the TOM Wigley
> as

>>> >being the SAR IS95a values. I do not know where these are publicaly

>>> >available, and I have asked Dave Griggs that if we use new scenarios

> (eg
>>> SRES) in the TAR, they are publicly available and well documented. The
>>> left
>>> hand column appears to be from the 1992 IPCC supplement.(The annex by
>>> Mitchell and Gregory). This used the then current UEA energy
>>> balance/carbon
>>> cycle model to convert CO2 emissions to concentrations. I presume the
>>> discrepancy comes from changes to the carbon cycle model and anything
>>> else
>>> affecting the conversion from emissions to concentrations.
> Unfortunately,
>>> as
>>> far as I can tell, the SAR never refers to these or explains why the
>>> concentrations are different.
>>>> This could easily happen again. The situation with the new
>>>> SRES scenarios to me seems rather chaotic, and again they are
> emissions
>>>> scenarios, not concentration scenarios. The initial GCM runs will use
> CO2
>>>> concentrations from one particular model. The TAR may report (probably
>>>> will
>>>> report) different values since they will use a different model. The
> best
>>>> thing is to talk to the people who set up the GCM run to find out
> exactly
>>>> what was used in the model
>>>>
>>>> With best wishes
>>>> John
>>>>
>>>>
>>>> jfbmitchell@meto.gov.uk
>>>> Hadley Centre for Climate Prediction and Research
>>>> The Met. Office, Bracknell
>>>> RG12 2SZ UK
>>>> Tel +44 1344 856613/6656
>>>> Fax+44 1344 856912
>>>>
>>>>> -----Original Message-----
>>>>> From: Mike Hulme [SMTP:m.hulme@uea.ac.uk]
>>>>> Sent: 08 April 1999 17:35
>>>>> To: N.W.Arnell; Sari Kovats; Matt Livermore; parryml@aol.com;
>>>>> Andrew

> > > White; jfbmitchell@meto.gov.uk; gjjenkins@meto.gov.uk;

> > > r.nicholls@mdx.ac.uk

> > > Subject: HadCM3 CO2 concentrations

> > > Importance: High

> > >

> > > Dear Fast-trackers,

> > >

> > > In putting the scenario paper together for the GEC issue, John

> Mitchell

> > > and

> > > I have come up with slightly different CO2 concentrations for HadCM2

> > and

> > > HadCM3 to what we had earlier assumed. These CO2 concentrations

> will

> > > really have to appear in the scenario paper to be consistent with

> the

> > GCM

> > > experiments. Given the differences from the values (I think) you

> have

> > all

> > > used in the impacts work, what significance does this have for your

> > work?

> > >

> > >

> > > HadCM2 HadCM3

> > > assumed 'correct' assumed 'correct'

> > > 2020s 441 470 457 434

> > > 2050s 565 590 574 528

> > > 2080s 731 770 712 638

> > >

> > >

> > > The difference is that the assumed HadCM2 concentrations are

> 20-30ppmv

> > too

> > > low while the assumed HadCM3 concentrations are 20-70ppmv too high.

> > >

> > > The assumed HadCM2 concentrations came from Cox and Friend (they had

> > > already run Hybrid with these concentrations before the FT work got

> > under

> > > way, so we adopted their values). I cannot yet trace where the

> assumed

> > > HadCM3 concentrations came from, but the 'correct' values are what

> both

> > > John Mitchell and the IPCC (1996 report) have calculated for the
> IS92a
> > > scenario.
> > >
> > > Your suggestions on how best to handle this inconsistency would be
> > > appreciated. How big a difference do these differences make to your
> > > impacts?

> > >
> > > Thanks,
> > >
> > > Mike

> > >
> > >
> > >
> > >
> > >

> *****

> > > ***

> > > Dr Mike Hulme
> > > Reader in Climatology tel: +44 1603 593162
> > > Climatic Research Unit fax: +44 1603 507784
> > > School of Environmental Science email: m.hulme@uea.ac.uk
> > > University of East Anglia web site:
> > > <http://www.cru.uea.ac.uk/~mikeh/>
> > > Norwich NR4 7TJ

> > >
> > >

> *****

> > > ***

> > > Annual mean temperature in Central England during 1999
> > > is about +1.5 deg C above the 1961-90 average
> > > *****

> > > The global-mean surface air temperature anomaly for 1998
> > > was +0.58 deg C above the 1961-90 average, the warmest year yet
> > recorded

> > >
> > >

> *****

> > > ***

> > > << File: gec.fasttrack.doc >>

> >

From: Trevor Davies <t.d.davies@uea.ac.uk>

To: m.kelly@uea.ac.uk,j.palutikof@uea.ac.uk,k.briffa@uea.ac.uk, m.hulme@uea.ac.uk,p.jones@uea.ac.uk

Subject: Re: CRU Board

Date: Tue, 04 May 1999 09:08:24 +0100

Mick,

CONFIDENTIAL

I think I'm missing out on something here (refer also to Keith's email where he talked about "CRU being railroaded by ENV"). My recollection was that it was agreed that I should approach Reading to see if they are up to anything & sound out if they might be interested in talking about a joint bid. The suggestion may have been mine originally, but I do not have absolute recollection over that. Southampton have approached us via the Registrar and via Peter Liss. As far as I am aware, nobody from UEA has approached them (although I have certainly argued with Jean that we should at least talk with them).

I now have a leaked document which spells out some of the research councils' thinking. I will get a copy over to CRU today. Please keep this document within the CRU5, since it may compromise the source. NERC and EPSRC are signed up. ESRC are not yet. Given the EPSRC stake, it will certainly be useful to get RAL etc involved. The funding might be 2million per year. That might imply that the Councils favour multi-site, clusters, etc, but they stress they have no preconceptions.

Given some of their requirements, the JIF bid may be useful.

An important requirement seems to be to attract an "internationally renowned and charismatic scientist" to be overall Director. Do you think we should sound out Schneider? Watson? ??

Trevor

At 11:17 01/05/99 +0100, Mick Kelly wrote:

>I can't make the re-arranged date so here is my input on some of the items

>I know are on the agenda:

>

>National Climate Centre:

>

>1. I feel even more strongly after learning more of the opposition that we

>should make a single site bid and capitalise on our proven track record as
>the only UK university which has covered and can cover all aspects of the
>climate issue from hard science to policy and philosophy.
>We should
>continue to firm up our links with NERC institutes, Hadley Centre, etc.
>But if we reach out to other universities we will:
>a) reveal what we see to be our sectoral weaknesses - a very bad strategic
>move
>b) have to split what is a limited pot of cash
>c) create a potential administrative monster that we know ERSC don't like
>from CSERGE experience
>d) weaken our comparative advantage as the place where all aspects of the
>issue are covered.
>It's my understanding that the CRU 5 have already decided in previous
>discussions that this is the way we should go? Trevor - do you want to
>argue against this? It's notable that we haven't been approached by other
>universities!
>
>2. Kerry reckons that likely limited lifetime of ESRC presence
>(Global Env programme office) at SPRU means it's not worth approaching
>them - so I haven't.
>
>3. I propose a working group be set up to move forward the centre proposal
>and ensure coordination/representation of views. 2 from CRU Bd,
>2 from CSERGE (Kerry and Neil?), Dean. Chair from CRU would be my vote -
>this should not all be loaded on Trevor's shoulders.
>
>Studentships
>To report on situation re my proposals:
>1. Craig Wallace (ex MSc) is reserve candidate (joint with Tim Osborn).
>2. My candidate for my solo topic was switched to the ESRC/NERC
>interdisciplinary bid by the studentship committee even though I'd told
>them we definitely couldn't put him forward for this - so that's
>scratched. They thought my topic was not NERC-friendly - but didn't tell
>me this till after the event. A number of phrases spring to mind but maybe
>they were just having a bad day.
>3. My feeling is best tactic for next year
>if we want more students - do we or are we at saturation point? - is to
>advertise early (now?), advertise applicants must have/be in line for a
>first or MSc with distinction, ensure we get feedback on topics from the
>committee and submit candidates early on in the process. Obvious, really.
>
>CRU 5 employment/salaries situation
>What is the current situation?

>
>AOB: Desk space for students
>Can I repeat that I think we should have policy on registration only ie
>post three year grad. students to be adopted when Nick finishes and before
>we hit the next late submitter? My feeling is a desk for 6 months then
>they move out to our overflow rooms in ENV. We should prioritise desk
>space in CRU for first year students. What does ENV do in this situation?

>
>Regards
>Mick

>
>_____

>
>Mick Kelly Climatic Research Unit
>University of East Anglia Norwich NR4 7TJ
>United Kingdom
>Tel: 44-1603-592091 Fax: 44-1603-507784
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>_____

>
>
>
>

++++
Professor Trevor D. Davies
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United Kingdom

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++++

From: mann@snow.geo.umass.edu
To: k.briffa@uea.ac.uk
Subject: Re: Perspective Science piece
Date: Tue, 4 May 1999 10:47:47 -0400 (EDT)
Cc: mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, t.osborn@uea

Hi Keith,

Thanks very much for the update. Sounds like everything should be good here. I'm sorry If I might have seemed to over-react, but it was just to make sure we avoided the scenario of last year where we had to end up publishing a followup letter because we and Phil hadn't had adequate communication before the piece was published. I'll look forward to seeing the piece in print. It sounds like you guys have a done a very good job. Indeed, Tim and we had a very constructive dialogue about things in your absence. Will be in touch.

best regards,

mike

p.s. I mentioned to Phil it would be nice to get at least one spatial pattern of your summer dendro temperature estimates into IPCC, along with a pattern or two from our multiproxy recons. I haven't heard back to Phil, but perhaps you can make a specific suggestion, and send me an appropriate postscript file? It's not too late to get this to Chris Folland for inclusion in the initial draft. Thanks in advance...

Michael E. Mann

Current _____ Starting Fall 1999 _____

Adjunct Assistant Professor		Assistant Professor
Department of Geosciences		Dept. of Environmental Sciences
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e-mail: mann@geo.umass.edu; memann@titan.oit.umass.edu (attachments)

Phone: (413) 545-9573 FAX: (413) 545-1200

<http://www.geo.umass.edu/climate/mike>

From: mann@snow.geo.umass.edu

To: p.jones@uea.ac.uk

Subject: Re: Straight to the Point

Date: Thu, 6 May 1999 13:09:36 -0400 (EDT)

Cc: k.briffa@uea.ac.uk, mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, t.osborn@uea.ac.uk

Hi Phil,

SOrry that you have taken such a negative spin from this. I had hoped it was all resolved pretty amicably, and emphasized to Keith and Tim that I was being perhaps overly picky this time PRECISELY to avoid the misunderstanding that happened last time around w/ Science.

Trust that I'm certainly on board w/ you that we're all working towards a common goal. That is what is distressing about commentaries (yours from last year, and potentially, without us having had appropriate input, Keith and Tim's now) that appear to "divide and conquer". The skeptics happily took your commentary last year as reason to doubt our results! In fact, your piece was references in several commentaries (mostly on the WEB, not published) attacking our work. So THAT is what this is all about. It is in the NAME of the common effort we're all engaged in, that I have voiced concerns about language and details in this latest commentary--so as to avoid precisely that scenario.

Please understand the above to be a complete and honest statement about the source of my concerns. It really doesn't have anything to do about who did what first, etc. I trust that history will give us all proper credit for what we're doing here.

The millennial-scale trend issue appears to be a source of contention. Malcolm can address the replication issue better than any of us--it's not a problem w/ our reconstruction. Furthermore, WE HAVE EXPLICITLY TAKEN INTO ACCOUNT THE LOSS OF LOW-FREQUENCY VARIANCE IN OUR ESTIMATES OF UNCERTAINTY. I don't know how many times I need to stress this. It is of fundamental importance in framing our conclusions. Our own analysis convinces me that things are already quite uncertain a millennium back in time. With regard to longer timescale variations, the evidence is all over the place. At EGS I saw some convincing evidence that many new paleo proxies indicate steadily decline at least over several millennia, and so do, in large part, the available long borehole estimates (though we should all take that w/ a good dose of NaCl). So I'm skeptical of estimates more than a millennium back in time until we have multiple proxies we can trust at that timescale, and can verify somehow the DC component of the estimates, or at least replicate them. This was my concern about the latest 2000 year recon that was shown.

You are right, the Milankovitch forcing argument is ONLY A NULL HYPOTHESIS. I hope I haven't argued anything more than that. That our millennial scale trend, which

we reasonably trust, and have some idea of the uncertainties in, is in line w/ that null hypothesis is information that cannot be ignored. That Kutzbach, Berger, and others are showing increasingly convincing model integrations over several millennia suggesting this, is more evidence. In the real world, anything *could* have happened. But lets not loose site of the appropriate null hypothesis here.

I hope the above clears things up somewhat. I'm sorry things have been construed in more negative light than I had ever intended. Call me anytime to discuss, here at the office (not sure how well our schedules overlap though).

Thanks, and sorry for the miscommunication here,

mike

Michael E. Mann

Current

Starting Fall 1999

Adjunct Assistant Professor		Assistant Professor
Department of Geosciences		Dept. of Environmental Sciences
Morrill Science Center		Clark Hall
University of Massachusetts		University of Virginia
Amherst, MA 01003		Charlottesville, VA 22903

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<http://www.geo.umass.edu/climate/mike>

From: mann@snow.geo.umass.edu

To: p.jones@uea.ac.uk

Subject: Re: Straight to the Point

Date: Thu, 6 May 1999 13:48:25 -0400 (EDT)

Cc: k.briffa@uea.ac.uk, mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, t.osborn@uea.ac.uk

Hi Phil,

Thanks for your message. I'm with you 100%, and honestly am very much looking forward to moving towards close collaboration between us. I've already talked a bit w/ Tim about those plans and the possibility of him spending some time in Charlottesville, etc. Will be in touch w/ you guys soon about trying to solidify some of these plans...

Yes, I will be in the Lion's den, so to speak. Not sure how much must stands behind his roar though... We do have to deal w/ the skeptics here somewhat directly. At least, to the extent that I do presentations on capitol hill for USGCRP (I do one w/ Jim Hansen and Malcolm on the 17th of this month), I'm a bit in the fray. Mostly, though, I've been trying to help Mike McCracken and company behind the scenes. We all know what happens when a U.S. scientists becomes a thorn in the side of big business...

Anyways, I'm really happy that the air is cleared. More soon,

mike

Michael E. Mann

Current

Starting Fall 1999

Adjunct Assistant Professor		Assistant Professor
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<http://www.geo.umass.edu/climate/mike>

From: Phil Jones <p.jones@uea.ac.uk>

To: mann@snow.geo.umass.edu

Subject: Straight to the Point

Date: Thu, 06 May 1999 17:37:34 +0100

Cc: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk,mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu

Mike,

Just back from two weeks away and from discussions with Keith and Tim and some emails you seem quite pissed off with us all in CRU. I am somewhat at a loss to understand why. It is clear from the emails that this relates to the emphasis placed on a few words/phrases in Keith/Tim's Science piece. These may not be fully resolved but the piece comes out tomorrow. I don't want to open more wounds but I might by the end of the email.

I've not seen the censored email that Ray has mentioned but this doesn't, to my way of working, seem to be the way you should be responding - ie slanging us all off to Science. We are all trying to work together for the good of the 'Science'. We have disagreements - Ray, Malcolm, Keith and me have in the past, but they get aired and eventually forgotten. We have never resorted to slanging one another off to a journal (as in this case) or in reviewing papers or proposals. You may think Keith or I have reviewed some of your papers but we haven't. I've reviewed Ray's and Malcolm's - constructively I hope where I thought something could have been done better. I also know you've reviewed my paper with Gabi very constructively.

So why all the beef now ?

Maybe it started with my Science piece last summer. When asked to do this it was stressed to that I should discuss how your Nature paper fitted in to the current issues in paleoclimatology. This is what I thought I was doing. Julia Uppenbrink asked me to do the same with your GRL paper but I was too busy and passed it on to Keith. Again it seems a very reasoned comment.

I would suspect that you've been unhappy about us coming out with a paper going back 1000 years only a few months after your Nature paper (back to 1400). Ray knew all about this as he was one of the reviewers. Then the second Science comment

has come out with a tentative series going back 2000 years. Both Science pieces give us a chance to discuss issues highly relevant to the 'science', which is what we have both tried to do.

Anyway that's enough for now - I'll see how you'll respond, if at all.

There are two things I'm going to say though :

- 1) Keith didn't mention in his Science piece but both of us think that you're on very dodgy ground with this long-term decline in temperatures on the 1000 year timescale. What the real world has done over the last 6000 years and what it ought to have done given our understanding of Milankovic forcing are two very different things. I don't think the world was much warmer 6000 years ago - in a global sense compared to the average of the last 1000 years, but this is my opinion and I may change it given more evidence.
- 2) The errors don't include all the possible factors. Even though the tree-ring chronologies used have robust rbar statistics for the whole 1000 years (ie they lose nothing because core numbers stay high throughout), they have lost low frequency because of standardization. We've all tried with RCS/very stiff splines/hardly any detrending to keep this to a minimum, but until we know it is minimal it is still worth mentioning. It is better we (I mean all of us here) put the caveats in ourselves than let others put them in for us.
- 3) None of us here are trying to get material into IPCC. I've given you my input through the review of the chapter in Asheville. I may get a chance to see the whole thing again at some stage, but I won't be worried if I don't.

I can't think of a good ending, but hoping for a favourable response, so we can still work together.

Cheers
Phil

Prof. Phil Jones

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From: Phil Jones <p.jones@uea.ac.uk>

To: mann@snow.geo.umass.edu

Subject: Re: Straight to the Point

Date: Thu, 06 May 1999 18:51:01 +0100

Cc: k.briffa@uea.ac.uk,mhughes@ltrr.arizona.edu,rbradley@geo.umass.edu, t.osborn@uea.ac.uk

Mike,

We'll differ a bit on a few points, but let's wipe the slate clean and get back to improving our estimates of past changes over the last millennium.

I must admit to having little regard for the Web. Living over here makes that easier than in the US - but I would ignore the so-called skeptics until they get to the peer-review arena. I know this is harder for you in the US and it might become harder still at your new location. I guess it shows though that what we are doing is important. The skeptics are fighting a losing battle.

Cheers

Phil

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From: James Hansen <jhansen@giss.nasa.gov>

To: D Parker <deparker@meto.gov.uk>

Subject: Re: Temperatures

Date: Fri, 07 May 1999 10:30:21 -0400

Cc: ckfolland@meto.gov.uk, imacadam@meto.gov.uk, p.jones@uea.ac.uk, makis@giss.nasa.gov

Hi, David,

I don't think that Antarctic is the principal source of differences. When we compare only the common areas it doesn't really come into play. There are areas in Mexico and Northern Africa that seem to contribute more to the differences. Makiko will put the plots that you requested at <http://giss.nasa.gov/~cdmss/Parker>

Regards, Jim

At 05:35 PM 5/5/99 +0100, D Parker wrote:

>To Jim Hansen jhansen@giss.nasa.gov

> (& copies to Chris Folland, Ian Macadam, Phil Jones)

>Jim

>

>Thanks for the mailed illustrations comparing your surface temperature data set with Phil Jones's.

>

>We are trying to understand the cooling of your data relative to Phil Jones's in the Southern Hemisphere during the 1990s (Table 1 below) in the annual series you sent to Ian Macadam. Plots of these were shown at the IPCC meeting in Asheville in March and showed the same relative cooling, but Figure 2 of your mailed illustrations does not show it. I note that the comparison in Figure 2 was made over the common area. If you use all available grids, do you get the relative cooling in the GISS dataset? I expect you will, because I have been perusing your web site and have noted that most recent years are cold over Antarctica in your dataset. This could be the focus of the problem, as your stations (with 1200km influence) will have more weight than Phil's unless you use common grids.

>

>As an aside, recent cooling over Antarctica could be partly forced by ozone losses, though I note that the cooling is strongest in March-May, not in Sept-Nov when the ozone hole occurs. If Antarctica cools, there will be consequences for Southern Hemisphere atmospheric circulation patterns, conceivably even contributing to the recent cooling of marine air temperature

>relative to sea surface temperature.

>

>To help further, can you provide annual maps, 1989 through 1998, of Jones
>(land), GISS (stations, 1200 km) and Jones minus GISS in the format of Figure
>3 of your mailed illustrations? Web or ftp access would be better than
paper,
>if possible.

>

>Thanks and regards

>

>David 5 May 1999

>

>*****

>

>Table 1. Annual Southern Hemisphere Anomalies (deg C) Relative to 1961-1990

>

	GISS	Jones
> 1990	0.250	0.30
> 1991	0.265	0.32
> 1992	0.023	0.14
> 1993	-0.027	0.24
> 1994	0.033	0.35
> 1995	0.069	0.37
> 1996	0.191	0.23
> 1997	0.033	0.34
> 1998	0.317	0.60

>

>

>*****

>

>

>

>

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>
>

James Hansen
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212-678-5500 fax (678-5622)

From: "Raymond S. Bradley" <rbradley@geo.umass.edu>
To: k.briffa@uea.ac.uk
Subject: vomit
Date: Fri, 14 May 1999 07:25:34 -0400

Excuse me while I puke...

Ray

>From: mann@snow.geo.umass.edu
>Date: Wed, 12 May 1999 13:00:09 -0400 (EDT)
>To: juppenbrink@science-int.co.uk, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk
>Cc: mann@geo.umass.edu, mhughes@lrr.arizona.edu, p.jones@uea.ac.uk,
> rbradley@geo.umass.edu

>Dear all,

>Thanks for working so hard to insure a final product that was
>acceptable to all. I think that Keith and Tim are to be
>commended on a fine job w/ the final version of the
>Perspectives piece that appeared, and I thank Julia for her
>especially difficult editorial task.

>I appreciate having had the opportunity to respond to the
>original draft. I think this opportunity is very important
>in such cases (ie, where a particular author/groups work
>is the focus of a commentary by someone else), and hope
>that this would be considered standard procedure in the
>future in such instances.

>I think we have some honest disagreements amongst us about
>some of the underlying issues, but these were fairly treated
>in the piece and that's what is important (The choice of
>wording in the final version was much better too. Wording
>matters!).

>Thanks all for the hard work and a job well done. I like
>to think that my feedback helped here--so I take some
>pride here as well.

>best regards,

>mike

>
>
> _____
> **Michael E. Mann**

> _____ **Current** _____ **Starting Fall 1999** _____

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>Department of Geosciences | Dept. of Environmental Sciences
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>
>
>
Raymond S. Bradley

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From: Dave Schimel <schimel@cgd.ucar.edu>
To: Shrikant Jagtap <sjagtap@agen.ufl.edu>
Subject: RE: CO2
Date: Mon, 17 May 1999 09:21:35 -0600 (MDT)
Cc: franci <franci@giss.nasa.gov>, Benjamin Felzer <felzer@ucar.edu>, Mike Hulme <m.hulme@uea.ac.uk>, schimel@ucar.edu, wigley@ucar.edu, kittel@ucar.edu, nanr@ucar.edu, Mike MacCracken <mmaccrac@usgcrp.gov>

I want to make one thing really clear. We ARE NOT supposed to be working with the assumption that these scenarios are realistic. They are scenarios-internally consistent (or so we thought) what-if storylines. You are in fact out of line to assume that these are in some sense realistic-this is in direct contradiction to the guidance on scenarios provided by the synthesis team.

If you want to do 'realistic CO2 effects studies, you must do sensitivity analyses bracketing possible trajectories. We do not and cannot not and must not prejudge what realistic CO2 trajectories are, as they are ultimately a political decision (except in the sense that reserves and resources provide an upper bound).

'Advice' will be based on a mix of different approaches that must reflect the fact that we do not have high confidence in GHG projections nor full confidence in climate system model projections of consequences.

Dave

On Sun, 16
May 1999, Shrikant Jagtap wrote:

> Friends,
>
> I'm enjoying the current debate about CO2 levels. I feel that we are using
> the GCM scenarios, and we MUST use exactly those CO2 levels for crop model
> runs, so all data is consistent. So if we are wrong, we are uniformly wrong
> and adjust our explanations accordingly whenever we agree on things. Now to
> use different data will be hard to explain.
>
>
> Shrikant
>
> Dr. Shrikant Jagtap
> 104 Rogers Hall, Ag & Biol. Engineering
> University Of Florida
> Gainesville, FL 32611
> Tel: 352 392 7719 (Work) & Fax: 352 392 4092 (Work)
> <http://www.agen.ufl.edu/~sjagtap/ssj/>
>
> Tel: 352 379 0698 (Home)
>
>
>
>
> -----Original Message-----
> From: franci [mailto:franci@giss.nasa.gov]
> Sent: Saturday, May 15, 1999 3:58 PM

> To: Benjamin Felzer
> Cc: Mike Hulme; schimel@ucar.edu; wigley@ucar.edu; kittel@ucar.edu;
> sjagtap@agen.ufl.edu; nanr@ucar.edu; Mike MacCracken
> Subject: Re: CO2
>
>
> dear ben,
>
> You just showed that the Hadley transient run we are supposed to use for the
> national assessment is too high, forcing-wise, because it assumes an overall
> 1.2% increase in total forcing.
>
> My question is then the following:
>
> -why are we using a 1% annual increase in GHG forcing (corresponding to the
> 1.2% increase) as a criteria for GCM simulations to then be used for the
> national assessment? Is it because of the possible confusion you refer to
> below? If so, that criteria needs to be revised.
>
> I still have a problem with the real CO2 calculations, in connections to
> hadley or CCCM. It seems to me it is still arbitrary to use one or another
> CO2 curve.
> However, in this arbitrariness, two easy solutions are possible (i am just
> summarizing previous e-mails, at the cost of being highly repetitive and
> obvious):
> -one is dave's, i.e. assume no change i GHG forcing mix from today, and
> apply 1% compounded increase to the 1990 actual levels.
> That gives a concentration of real CO2 in 2100 that is > 1050 ppm. THAT'S
> 50% higher than projected by IS92a, and even 17 % higher than the worst
> emission case devised in IS92f.
> -the second is tom's. Just use the co2 in IS92a, and assume that all other
> further changes necessary to get the hadley forcing (whatever they are)
> happen in GHG other than CO2.
> I will repeat that I like the latter solution.
>
>
> Whatever the consideration of self-consistency and physics are when you make
> this decision, I do not think we should carry out the national assessment by
> using "unrealistic" CO2 numbers. I thought the numbers that come out of our
> exercises (from the impact side of things) were supposed to serve as some
> basis to be used in the process of decision making at the national and
> regional level. Am i out of line here? There are dozens of people right now,
> out there, including our group at giss, who are gathering data, fine-tuning
> models, making connections among physical and socio-economic variables,
> etc., at a very low "effort spent/retribution received", and then we are
> going to run things at 1000 ppm in 2100?
> As far as my specific contribution is concerned, it surely might make a big
> difference in crop yield changes under climate change whether I use 700 ppm
> in 2100 (the IS92a) curve, or >1000 ppm (the 1% compounded increase).
>
> The problem is the same for the 2040's (the other decade we have decided to
> simulate), although possibly not as bad as the 2090's case.
>
> Either solution we opt for, we have to make clear to whomever will receive
> our results that the climate forcing scenario is on the "high" side of
> things.
>
> Ah! It was so nice and easy when we were working with doubled-CO2

> equilibrium runs!

>

> cheers,

>

> francesco

>

> PS what about the CCCM scenario?

>

>

>

>

>

>

>

>

>

>

>

> -----Original Message-----

> From: Benjamin Felzer <felzer@ucar.edu>

> To: franci <franci@giss.nasa.gov>

> Cc: Mike Hulme <m.hulme@uea.ac.uk>; schimel@ucar.edu <schimel@ucar.edu>;

> wigley@ucar.edu <wigley@ucar.edu>; kittel@ucar.edu <kittel@ucar.edu>;

> sjagtap@agen.ufl.edu <sjagtap@agen.ufl.edu>; nanr@ucar.edu <nanr@ucar.edu>;

> Mike MacCracken <maccrac@usgcrp.gov>

> Date: Friday, May 14, 1999 8:12 PM

> Subject: Re: CO2

>

>

>>Please disregard the previous message and replace with this message (1st

>>paragraph is unchanged).

>>

>>On Fri, 14 May 1999, Benjamin Felzer wrote:

>>

>>> Going back to some of the original radiative forcing values, it would

>>> appear that the 1% increase is true of RADIATIVE FORCING, whether of CO2,

>>> CH4, etc, or the total (to an approximation). However, once we convert

>>> back to CO2 concentration (using the exponential relationship), the

>>> actual

>>> increase in concentration is more along the order of 0.7% (all

>>> compounded). Is it possible that the original 1% assumption was

>>> mistakenly applied to CO2 concentrations for the modelers when it was

>>> actually meant for radiative forcing??

>>>

>>Therefore for the ecological models we should use Dave's original

>>suggestion, because the models really did use a 1% increase in equivalent

>>CO2, which approximates a 1% increase in CO2 alone. The point here is

>>that this 1% increase is much higher than IS92a, but that might be because

>>of the confusion between radiative forcing increase and concentration

>>increase discussed above. In fact a 0.7% increase in equivalent CO2 might

>>have been a more realistic assumption for IS92a, but the 1% increase in

>>concentration is what was actually used in these earlier models. The CO2

>>concentrations used in the ecological model should correspond to those

>>used in the GCMs, not to what we think they should be.

>>

>>

>>> Any other thoughts?

>>>

> >> **Ben**

> >>

> >>

> >>

> >

> >

>

>

From: David Viner <d.viner@uea.ac.uk>
To: m.hulme@uea.ac.uk, s.raper@uea.ac.uk
Subject: Fwd: Re: Climate Sensitivity
Date: Tue, 18 May 1999 11:48:40 +0100

Mike

The climate sensitivity of HadCM2.....pick a value between 2.5 and 4.1K

D

>Envelope-to: f046@cpcal1.uea.ac.uk
>Date: Tue, 18 May 1999 11:27:48 +0100 (BST)
>From: T Johns <tcjohns@meto.gov.uk>
>Subject: Re: Climate Sensitivity
>To: d.viner@uea.ac.uk
>Cc: tcjohns@meto.gov.uk
>Status:

>

>Hi David,

>

>I have just got back from leave today - sorry for the lack of response
>to your emails.

>

>On climate sensitivity, the equilibrium sensitivity in HadCM2 was difficult
>to get a definitive answer for initially as the conventional slab experiment
>was unstable, so we estimated it from part of a transient coupled run
>instead. We quoted 2.5 K in the original Nature paper. Recently we
>have done a HadAM2 slab experiment (modified sea ice and slab ocean physics)
>which indicated 4.1 K rather than 2.5 as an equilibrium value. This is
>quoted in a paper submitted as a CMIP study. The HadAM3 conventional
>slab experiment gave the 3.3 K figure I think. The HadCM2 discrepancy
>indicates the perils of this yardstick; other research here suggests that
>the effective climate sensitivity does respond to climate change feedbacks
>in transient experiments (with HadCM2 particularly). The early 2.5 K
>estimate has been revised upwards based on a long coupled run of HadCM2 to
>be closer to the 3.3 K we got from HadCM3 equilibrium slab experiments.

>

>Comparing transient temperature responses to similar time-varying forcing
>may be a better indication of real sensitivity, but so long as we quote

>single climate sensitivity numbers I fear that there is scope for confusion.

>

>Tim.

>

>PS: I will try to get an update on the HadCM3 references sorted out for you.

>

>> Tim

>>

>> I'm a bit confused as now I have seen a number of different values, in

>> HCTN2 you mention that HadAM3 has a climate sensitivity of 3.3 degrees K

>> and that this is similar to HadCM2. Is this the case and is such a value

>> available from a comparable HadAM2 experiment.

>>

>> Many regards

>>

>> David

>>

>> PS Did you get my message about references?

>

#-----

Dr. David Viner

Climate Impacts LINK Project

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#-----

From: Tom Wigley <wigley@meeker.ucar.edu>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: CO2 concentrations
Date: Wed, 19 May 1999 16:21:51 -0600 (MDT)
Cc: Mike MacCracken <mmaccrac@usgcrp.gov>

Dear Mike,

Yes, I am aware of the confusion surrounding what the Hadley Centre did and why. It is even messier than you realize. I have forcing data sets (more than one!) from Jonathon Gregory that differ from the numbers you gave in your email!! The Hadley people have clearly screwed things up, but their "errors" don't really matter given all of the uncertainties. I didn't mention this because I thought that opening up that can of worms would confuse people even more.

In my view (trying to keep things as simple as possible), the key points are these:

- (1) The HadCM2 run purports to be IS92a, and it is a good approximation to this.
- (2) Their use of 1% compounded for CO2 **is** a reasonable approximation to the IS92a GHG forcing (which, itself, is uncertain).
- (3) The climate model output is also uncertain.
- (4) The pure CO2 input to IS92a is what I have distributed from the Bern model.
- (5) Hence, the best and simplest combination is to use HadCM2 climate output with these (point (4)) **a priori** defined "pure" CO2 concentrations for IS92a.

On Wed, 19 May 1999, Mike Hulme wrote:

- > Tom,
- >
- > Thanks for clarifying your thinking on this.
- >
- > I still have a problem with HadCM2 forcing and making sense of what Hadley
- > have published, esp. the numbers in the Feb. 1997 J.Climate paper by

> Mitchell and Johns. There, they make it clear that the model was presented
> with CO2-equiv. rising from 473ppmv in 1990 to 1414ppmv in 2100, i.e., a 1%
> p.a. increase. This *seems* precise and unambiguous, so I don't think they
> do adjust the CO2-equiv. growth ratio (C2100/C1990) to 3.127 (i.e., about
> 1.05% p.a.) as you suggest.

>
> This concentration scenario yielded a 1990-2100 model forcing of 6.5Wm-2
> (sic), "close to that reported by Mitchell and Gregory in 1992" [Mitchell
> and Johns, 1997] using STUGE (my estimate for that is about 6.2Wm-2). Both
> of these are quite a bit higher than the 5.8Wm-2 forcing in IPCC SAR for
> IS92a. With this (apparently) higher forcing, I reasoned that all else
> being equal, the actual CO2 concentrations that are consistent with HadCM2
> should also be *higher* than those cited in IPCC SAR and hence we could not
> just use the CO2 concentrations from MAGICC (or the Bern model). Hence my
> somewhat higher CO2 estimates of 790ppmv by 2100 were arrived at by using:

>
> $pCO_2 = 279ppmv * (\exp(F/(3.47/\ln(2))))$ where F is the proportion in
> MAGICC of total forcing due to CO2 alone for IS92a.

>
> The Mitchell/Johns J.Climate paper is confusing, however, because it also
> presents results in their Table 1 which shows a 1990-2100 HadCM2 forcing of
> only 5.5Wm-2 (sic), a value that relates to their text-cited value of
> 6.5Wm-2 only by using DQ of 5.05Wm-2 (i.e., the sensitivity of HadCM2)
> rather than DQ = 6.3Wm-2. Yet the text of the paper continues to imply the
> HadCM2 forcing is '12% higher' than Kattenburg, rather than 5% lower.

>
> The bottom line ... the IS92a SAR forcing of 5.758Wm-2 and DQ of 6.3Wm-2
> only yields a CO2-equiv. growth rate of just over 0.8% p.a., rising to
> nearly 0.9% p.a. if the HadCM2 DQ of 5.05Wm-2 is used. These are still
> some way short of 1% p.a.

>
> Regards,

>
> Mike

>
> p.s. this is now more a matter for my own curiosity since I agree that for
> most assessment purposes the Wigley/Joos numbers are the best to use.

>
> At 15:36 18/05/99 -0600, you wrote:

>>Dear all,

>>
>>I've just read the emails of May 14 onwards regarding CO2. I must say
>>that I am stunned by the confusion that surrounds this issue.

>> Basically, I and MacCracken are *right* and Felzer, Schimel and (to a
>> lesser extent) Hulme are *wrong*. There is absolutely, categorically no
>> doubt about this. Let me explain.

>>
>>(1) The Hadley Centre run is meant to simulate the climate change
>> consequences of the full IS92a emissions scenario.

>>
>>(2) In this scenario, there are the following concentration and forcing
>> changes over 1990-2100:

Item	C(2100)	DQ(1990-2100)
CO2	708	4.350
CH4	3470	0.574
N2O	414	0.368
Halos		0.315
TropO3		0.151

GHGs		5.758
SO4 (dir)		-0.284
SO4 (indir)		-0.370

TOTAL		5.104

>>
>> These are the numbers I used in Ch. 6 of the SAR. They do not agree
>> precisely with numbers in Ch. 2, because I used the models and formulae
>> embedded in MAGICC. The differences between Ch. 2 and Ch. 6 are
>> irrelevant to the present issue.

>>
>>(3) How does one simulate the combined effects of all the GHGs in a
>> climate model that only has CO2? The standard way is to take the GHG
>> radiative forcing (ending in 5.758W/m**2 in 2100 in this case) and
>> convert this to *equivalent* CO2 concentration changes. If one uses
>> the old (IPCC90) forcing formula for CO2 (which is what was used in the
>> SAR), viz $DQ=6.3 \ln(C/C_0)$, then $C(2100)/C(1990)$ is 2.494. Note that the
>> 1% compounded change would be $C(2100)/C(1990)=(1.01)**110=2.988$. Thus,
>> 1% compounded CO2 gives roughly the correct *forcing*.

>>
>> NOTE THAT THE ACTUAL CO2 CHANGES ARE *NOT* THE CO2 CHANGES USED IN THE
>> MODEL. THE MODEL USES ARTIFICIAL CO2 CHANGES, SCALED UP TO ACCOUNT FOR
>> FORCING FROM OTHER GHGs.

>>
>> NOTE THAT THE ACTUAL CO2 CHANGE IS FROM 354ppmv IN 1990 to
>> 708ppmv IN 2100. THIS IS *NOT* A 1% COMPOUNDED INCREASE.

>>

>>NOTE, FURTHER, THAT WHAT MIKE HULME SUGGESTS IN HIS POINT 8 IS ALSO
>>WRONG. IT IS WRONG TO *BACK OUT* THE CO2 FROM FORCINGS. THE CO2 WAS
>>SPECIFIED A PRIORI.
>>
>>NOTE FINALLY THAT MIKE *DOES* GIVE THE 708ppmv VALUE IN HIS POINT 9.
>>USING THIS WOULD BE OK, BUT I RECOMMEND USING THE SLIGHTLY DIFFERENT
>>BERN MODEL RESULTS (SEE BELOW).
>>
>>(4) Now, some minor wrinkles. In the Hadley Centre model for CO2,
>> $DQ=5.05 \ln(C/C0)$. Hence, to get a forcing of $5.758W/m^{**2}$, they need to
>>use $C(2100/C1990)=3.127$. Note that this is a little closer to the 1%
>>compounded result than my above calculation. The Hadley Centre may well
>>have used a slightly different total 1990-2100 GHG forcing than mine, so
>>they may have backed out a compounded CO2 increase rate even closer to
>>1% than the above. In any event, if they decided to go with 1%, then
>>this was a perfectly reasonable choice in order to capture the total GHG
>>forcing.
>>
>>(5) The 708ppmv C(2100) value is what comes out of my carbon cycle
>>model. In the SAR, in Ch. 2, we considered results from three different
>>carbon cycle models; mine, the Bern (Joos) model, and Atul Jain's
>>model. For illustrations in the SAR, we used the Bern model. The
>>mid-2100 value with this model, for IS92a, was 711.7ppmv. A later
>>version of this model, used in IPCC TP4, gives 711.5ppmv. Jain's model
>>gave 712.3ppmv.
>>
>>(6) The bottom line here is that, for a consistent pairing of Hadley
>>Centre climate and CO2, one MUST use the ACTUAL CO2 numbers that went
>>into calculating the radiative forcing, NOT the equivalent CO2 numbers.
>>The climate response reflects all GHGs, whereas the plants are
>>responding only to CO2.
>>
>>(7) I am attaching the Joos CO2 time series. I recommend using the
>>actual values rather than trying to fit a compound CO2 increase to
>>them---which, in any event, should not be done using just the end point
>>values. This, however, is your choice. Differences will be negligible
>>in terms of plant response.
>>
>>I hope this clarifies things. It has always seemed pretty obvious and
>>clear cut to me. I hope it will now to all of you.
>>
>>Cheers,
>>Tom

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Orson Vandeplasseche <ovdplasseche@mail.wesleyan.edu>
Subject: Re: tree rings
Date: Thu May 27 10:57:56 1999
Cc: k.briffa@uea

Dear Orson

Very sorry for such a slow reply.

The individual curves (Tornetrask, Taimyr and Yamal) have not been calibrated against their local temperature records yet, and so only exist as standardised (or normalised) anomalies.

For the calibrated Tornetrask record of Briffa et al. (1992), the calibrated reconstruction made use of both tree-ring width and tree-ring density and so it will look different to the ring-width only record shown in the PAGES newsletter recently. For the earlier extension to this record, only ring-width will be available - which is why the calibrated record cannot be simply extended with the new data. Instead, a new calibration needs to be made, using ring-width only. This hasn't been done yet, and - while it *might* be a simple linear regression - sometimes ring-widths from one year and from the previous year are used together as predictors, so I cannot guarantee that it will be a simple rescaling of the uncalibrated curve. Nevertheless, the uncalibrated curve *is* correlated with summer temperature, so it certainly provides useful information.

The average of the three series was calibrated *after* they were averaged, and was calibrated against the April-September mean temperature over all land north of 20N. This was purely for comparison with the other curves shown in our Science piece; for this curve, this region is by no means the optimum, and the temperature anomalies would no doubt differ in magnitude if a regional temperature from northern Eurasia had been used instead. This offers one explanation of why the 650-750 warming differs from Briffa et al. (1992). The second is that only ring-width has been used. The third reason is that it is the average of 3 curves - if the other two don't show the warming, or not as strongly, then of course the signal will be less pronounced in the average. So, you can still use the Briffa et al. (1992) calibration - it is certainly not wrong.

Hope this helps with your choice of what to use.

We will send you a reprint to your Middletown address when they arrive. I am now going to mail you hard copy (black & white) of the Tornetrask uncalibrated ring-width record (annual and 50-yr smoothed) from the PAGES article, and also a hard copy of the calibrated northern Eurasia record from the Science paper. The northern Eurasian record should preferably be referenced using both Briffa & Osborn and Briffa et al.

Best regards

Tim

From: Phil Jones <p.jones@uea.ac.uk>
To: "Folland, Chris" <ckfolland@meto.gov.uk>
Subject: RE: VARIANCE PROBLEM
Date: Thu, 10 Jun 1999 15:48:05 +0100
Cc: d.parker@meto.gov.uk,t.osborn@uea.ac.uk

Chris,

Sorry to be flooding you with another email, but I was discussing this with Tim. Tim reminded me of a paper that he'd written in that well known journal Dendrocronologia ! I've sent down a copy of the proofs to you both. The paper has been in press for the last 2 years ! This must be the slowest journal in the world. This has some more theory in it and some variance corrections for tree-ring and temperature series.

We are going ahead with the method I've outlined over the last few emails. Tim and I have modified a couple of things slightly :

1) Using the present combined dataset (Jones, 1994 and Parker et al. 1995) we will calculate monthly rbars for each 5 by 5 box. The grid-box time series will be filtered with a 30-year Gaussian filter. rbar will be calculated from the residual grid-box time series. Tim reckons that a longer filter is better (an analysis in the paper). He suggests 40 years, but this involves more problems with the ends, so we'll go with 30. I don't think 20,30,40 will make that much difference to the rbar values.

We are using the combined dataste for the estimation as this should produce better rbar values around coasts and islands. If we used the land only dataset we would have real problems with isolated islands and with some coasts (where all neighbouring boxes will be in one direction from the coastal box).

2) Having got fields of the monthly rbars we'll then apply the formula to the land-only dataset. As you're doing something similar with the marine dataset, we can remerge the two variance corrected datasets using David's merging (growing land and neighbour checking) program.

3) We will then write this up as a small paper for GRL, about the land only results. Both of you can be on this if you want. We can decide later what to do about the merged dataset.

4) applying the correction in real time in the future will mean that we will always be slightly changing approximately the last 15 years data - because of the filter end effects. Best would seem to be to maintain the present version we have and apply this variance correction every few years (eg the IPCC cycle !).

Cheers
Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Paul Valdes <P.J.Valdes@reading.ac.uk>
Subject: Re: PRESCIENT
Date: Mon Jun 14 16:33:37 1999
Cc: njs5@cam.ac.uk

Paul

I have been told PRESCIENT is positive. It has been factored into NERC finances -for the full 8 million I believe. No official written statement has been declared as far as I know but someone from NERC visited here while I was away in Russia last week and talked of a first call for proposals in April 2000. At present this is all I know. Will keep you informed if I here more.

best wishes
Keith

At 04:41 PM 5/29/99 +0100, you wrote:

>Hi Keith,

>

>I met Simon Tett the other day and he said that you thought that the
>thematic proposal had definetely been funded. Is that true? The
>last thing I heard was very promising but not the final word!

>

>Best Wishes

>

>Paul

>

>-----

>Dr. Paul Valdes
>Email: P.J.Valdes@reading.ac.uk
>Phone: + 44 118 931 6517
>Fax: + 44 118 931 8905

Dept. of Meteorology,
University of Reading,
Earley Gate, Whiteknights,
PO Box 243
Reading. RG6 6BB. UK

>

>-----

>

>

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Density data from Polar Urals
Date: Wed, 16 Jun 1999 16:32:32 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,

I am reminding your promise to send me raw density data from Polar Urals remnants of larches as soon as possible, as I must prepare samples for Fritz until the end of June. Leonid Agafonov will bring them to Slovenia to Fritz.

Tomorrow I will lie down in hospital for 7-9 days, as I get the infection from a tick in Iremel area, not encephalitis, but a new kind of infections from ticks, namely "lime-borrelious" (I do not know its name exactly in English). The sign of this disease is red field approximately 5-8 centimeters in diameter around the point where a tick bite a body. This place itches greatly. If you have such characteristics, you must apply to doctor. This disease is not so dangerous as encephalitis and can be easy recovered from antibiotics. I hope that your tick did not contained such infection.

I wish you the best.

Sincerely yours,

Dr. Stepan G. Shiyatov

Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
Subject: Re: Density data from Polar Urals
Date: Fri Jun 18 11:21:10 1999

Stepan

I am attaching the raw density measurements (max. latewood den.) for the Sob River site as we extracted them from Fritz data bank. The format is Tucson like (index) except for a different header on each sample series. For your purposes the start and end date of each series are shown as the 2 I4 fields in columns 5-12 of these identifier lines. I hope this is all you need. You may also refer to Figure 2a in our paper in the NATO ASI Volume edited by Phil. The article on Low Frequency Signal problems that you are a co author on. This Figure shows the number of density samples through time in this chronology - very low before 1200 and between 1400 and 1600!!

I am sorry to hear of your tick infection. This is no laughing matter and you should ensure that you are well treated and rested. As of yet I have no problems other than worrying about how we will organise future proposals to the EU. Thankyou again for your hospitality and the warm reception from your excellent group. I sincerely hope we will be able to continue our collaboration for many years to come. I hope too that Eugene also feels committed to this working relationship. Perhaps he was tired but I got the impression his priorities were not so much concerned with our work.

I await detailed description of the full network - locations and correspondence with the density network positions and names - that I believe Valerie will work on. Perhaps the outline and draft of something from Rashit would also be forthcoming soon.

Meanwhile I send my best wishes to you and I await news of your continued health

Keith

At 04:32 PM 6/16/99 +0500, you wrote:

>Dear Keith,

>

>I am reminding your promise to send me raw density data from Polar Urals
>remnants of larches as soon as possible, as I must prepare samples for
>Fritz until the end of June. Leonid Agafonov will bring them to
>Slovenia to Fritz.

>

>Tomorrow I will lie down in hospital for 7-9 days, as I get the
>infection from a tick in Iremel area, not encephalitis, but a new
>kind of infections from ticks, namely "lime-borreliosis" (I do not know
>its name exactly in English). The sign of this disease is red field
>approximately 5-8 centimeters in diameter around the point where
>a tick bite a body. This place itches greatly. If you have such

>characteristics, you must apply to doctor. This disease is not so
>dangerous as encephalitis and can be easy recovered from antibiotics.
>I hope that your tick did not contained such infection.

>

>I wish you the best.

>

>Sincerely yours,

>

>Dr. Stepan G. Shiyatov

>

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>Institute of Plant and Animal Ecology

>8 Marta St., 202

>Ekaterinburg, 620144, Russia

>e-mail: stepan@ipae.uran.ru

>Fax: +7 (3432) 29 41 61

>Phone: +7 (3432) 29 40 92

>

>

>

>

From: sdecotii@ncdc.noaa.gov
To: christy@atmos.uah.edu, clarkea@mar.dfo-mpo.gc.ca, climate@cabel.net,
pfrich@meto.gov.uk, pgroisma@ncdc.noaa.gov, jwhurrell@meto.gov.uk,
m.hulme@uea.ac.uk, p.jones@uea.ac.uk, Jouzel@obelix.saclay.cea.fr,
mann@snow.geo.umass.edu, j.oerlemans@fys.ruu.nl, deparker@meto.gov.uk,
tpeterse@ncdc.noaa.gov, drind@giss.nasa.gov, drobins@rci.rutgers.edu,
j.salinger@niwa.cri.nz, walsh@atmos.uiuc.edu, swwang@pku.edu.cn
Subject: Plan of action for Chapter 2
Date: Mon, 21 Jun 1999 13:12:34 -0400

Below is the text and attached is a file in MSWord regarding a plan of action for Chapter 2 leading up to the IPCC Meeting in Arusha, Tanzania.

June 21, 1999

Dear Lead Authors and Key Contributors,

This note is to outline a plan of action for Chapter 2 leading up to the IPCC meeting in Arusha, Tanzania to take place 1-3 September. As you know, we are now in the midst of a "friendly review" from our colleagues of the strawman draft of our chapter. We expect to receive comments from these reviews through middle or even late July. These reviews will include some from people other than our nominated reviewers, like Sir John Houghton, from whom we have just had a brief review. Please check regularly with the Tar02.meto.gov.uk email site to cover this aspect.

Accordingly we ask each of the individuals listed below to revise the draft section as suggested below, and to indicate their response to reviewer's comments. The first person listed is to take the lead, and individuals with an asterisk by his name are to prepare the material for presentation in Arusha. We would ask that a provisionally revised part of your chapter be completed by 20 August and emailed to Tom Karl or placed on the website so that Sylvia Decotiis can create a new version of Chapter 2 for Tom to bring to Tanzania. Tom will bring one paper copy of the provisional new "Arusha" version of chapter 2 to Tanzania, and a complete series of electronic files which can be input to PCs via 1.4MB floppy disks. It would be a considerable advantage for attendees to bring portable PCs, though we expect some IPCC PCs to be available at the Arusha International Conference Centre.

Chris Folland will be leaving for Tanzania early (24 Aug) whereas Tom Karl

will still be available until 29 Aug for urgent interactions. We will decide later as to whom, and how many of us, should actually make presentations, noting that Hans Oerlemans is not likely to be present.

But

all attendees be prepared, and bring appropriate visual material and of course, further suggestions. We have listed assignments next to each section.

Section 2 ----- Tom Karl* and Chris Folland* Executive Summary ù
total
revision and update
Section 2.1 ---- Chris Folland* Changes needed regarding uncertainty
guidelines
Section 2.2.1 ---- Chris Folland* Okay for now
Section 2.2.2 ---- David Parker, Phil Jones, Tom Peterson, Chris Folland*
Length okay, but reduce number of figures.
Section 2.2.3 ---- John Christy* Check for accuracy
Section 2.2.4 ---- John Christy* Check for accuracy
Section 2.2.5 to 2.2.6 ---- Oelermans*, Nick Rayner, John Walsh, David
Robinson, Tom Karl and Chris Folland. Glacier section needs to be updated
Section 2.2.7 ---- Oelermans, Tom Karl* Check for accuracy
Sections 2.3 through Section 2.3.5---- Mike Mann*, Phil Jones Reduce in
size by about 10%
Section 2.4 through Section 2.4.5 ----Jean Jouzel* Reduce in size about
10%
Section 2.5 through 2.5.4 ---- Jim Salinger*, Pasha Groisman, Mike Hulme,
Wang. Provide a better context for why this section is important, more on
upper tropospheric water vapor if possible
Section 2.5.5 ---- Steve Warren, Dale Kaiser, Tom Karl* Add new analyses
of
cloud amount
Section 2.5.6 ----Jim Salinger*
Section 2.6 through 2.6.6 ----Jim Salinger*, George Gruza, Alynn Clarke,
Wang. Reduce in size by at least 50%. Identify a rationale section at the
beginning. IPCC 1995 will help here. Some material may go elsewhere. May
need to consult Mike Mann or Jean Jouzel. Please send revised section to
Chris Folland to finally review (even if not complete) by 16 August.
Chris
will feed back changes to Jim by 23 August. Jim Salinger should interact
with Chris during this work too. Jim should prepare presentational
material
Section 2.7 through 2.7.4 ----David Easterling, Pasha Groisman, Tom Karl*
Review for accuracy
Povl Frich: please interact and be prepared to present extremes parts.
Jim
Salinger: you may have more material on extremes in the South Pacific.
Please feed this to Tom Karl and Povl Frich.
Section 2.8 ---- Tom Karl, Chris Folland* Develop a summary, including
strawman cartoon

In addition we have about twice the number of figures that will be
allowed

so everyone should identify figures that can be removed or combined to
reduce the size. The latter can sometimes be very effective. At the

present time we are about 1/3 over our word limit so everyone will have to respond to the reviewers (often requesting more), and yet being more judicious in the words we use. Please consult the 1995 IPCC Report as a guide.

Please do not hesitate to comment on these plans, preferably as soon as possible, so that holiday arrangements etc do not cause problems.

Cheers and thanks,

Chris and Tom

(See attached file: ARUSHA INSTR LEAD AUTHORS.doc)

National Climatic Data Center

Attachment Converted: "c:\eudora\attach\ARUSHA INSTR LEAD AUTHORS.doc"

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: State of health
Date: Wed, 30 Jun 1999 16:56:43 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,

I recovered from tick's infection, at any case I do not have high temperature during the last week. I hope that your health is also good. Now I am preparing for field work.

I selected 32 new samples of dead larch trees from the Polar Urals and sent them to Fritz via Leonid Agafonov. A new version of the chronology will be up to 170 years longer and a better replicated between 1400-1700 AD.

The hard disk is working perfectly, thank you very much.

My best wishes to your family and Phil.

Sincerely yours,

Dr. Stepan G. Shiyatov

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Phone: +7 (3432) 29 40 92

From: Trevor Davies <t.d.davies@uea.ac.uk>
To: c.bentham@uea,p.jones@uea,j.palutikof@uea,p.liss@uea,m.hulme@uea, r.k.turner@uea,k.brown@uea,j.darch@uea
Subject: Climate change centre info.
Date: Fri, 02 Jul 1999 12:51:51 +0100

>Envelope-to: t.d.davies@uea.ac.uk
>From: "Andrew Watson" <a.j.watson@uea.ac.uk>
>To: "Trevor Davies" <t.d.davies@uea.ac.uk>
>Subject: Climate change centre info.
>Date: Fri, 2 Jul 1999 11:11:01 +0100
>X-MSMail-Priority: Normal
>X-Mailer: Microsoft Outlook Express 4.71.1712.3
>X-MimeOLE: Produced By Microsoft MimeOLE V4.71.1712.3

>
>Hi Trevor
>I was with John Shepherd earlier this week. He told me he
>was phoned up last Friday by Tariq Ali at Imperial College,
>seeking to sign him up to the IC bid; it seems that IC's
>relations with Oxford may have gone sour. If that is the
>case, IC will probably make strenuous efforts to detach some
>of the members of the consortium that UEA is trying to put
>together.

>I was attending a meeting on the "miilliesym" proposal, and
>we were treated to a talk from Ian Dwyer of NERC (new
>position to co-ordinate global change research) on the
>climate change centre. Two things I picked up that I didn't
>know before (but you may) were

>(1) All the decisions, both on the outline proposals and
>full proposals, will be taken by a panel of experts
>(academics from overseas and industry). There will not be
>the normal peer review system. I asked if there would be the
>opportunity to suggest names for this panel, but the answer
>appeared to be no; the panel will be selected and organised
>by the research councils, chiefly NERC.

>(2) The split of funding for the centre will be (per year) 1
>million NERC, 0.75 million EPSRC, 0.25 million ESRC.

>Cheers, Andy

>*****

>Prof Andrew J. Watson
>email: a.watson@uea.ac.uk
> or : a.j.watson@uea.ac.uk
>phone: (44) 1603 593761 direct
> 1603 456161 switchboard
> 1603 507719 fax
>School of Environmental Sciences
>University of East Anglia
>NORWICH NR4 7TJ
>U.K.
><http://www.uea.ac.uk/~ajw/ajw.htm>

>*****

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>

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Professor Trevor D. Davies
Dean, School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

Tel. +44 1603 592836

Fax. +44 1603 507719

+++++

From: Janice Darch <J.Darch@uea.ac.uk>
To: env.faculty@uea
Subject: Modeling & Data Analysis Research NRA-99-OES-04 <fwd>
Date: Wed, 14 Jul 1999 11:00:10 +0100 (GMT Daylight Time)
Reply-to: J.Darch@uea.ac.uk

--- Begin Forwarded Message ---

Date: Tue, 13 Jul 1999 16:45:56 -0400
From: OES Comments <oescomm@hq.nasa.gov>
Subject: Modeling & Data Analysis Research NRA-99-OES-04
Sender: OES Comments <oescomm@hq.nasa.gov>
To: OESCOMM@caffeine.public.hq.nasa.gov

Reply-To: OES Comments <oescomm@hq.nasa.gov>
Message-ID:
<3.0.32.19990713164217.0069a378@mail.hq.nasa.gov>

Investigations that Contribute to the NASA Earth Science Enterprise's
Modeling and Data Analysis Research

General Information

Solicitation Number: NRA-99-OES-04
Response Date: Sep 27, 1999

Description

NASA is soliciting proposals for investigations that will contribute to modeling and data analysis research that is supported by NASA's Earth Science Enterprise. This NRA solicits proposals directed to the interests of disciplinary research and analysis, interdisciplinary science, and data analysis programs that include global and regional modeling activities and large-scale data analysis, especially model-driven analysis. It also solicits proposals from instrument science teams and/or guest investigators being newly competed or re-competed in which global and regional modeling and/or model-driven data analysis constitute major elements of the proposal. This NRA is expected to result in research funding of approximately \$65 million over three years. The individual program elements included in this NRA, and the responsible NASA Program Managers are:

Program Element

Manager

- a. Global Modeling and Analysis Program (GMAP) K Bergman
- b. Atmos. Chemistry Modeling & Analysis Pgm. (ACMAP) J Kaye
- c. Phys. Oceanogr. Research & Analysis Pgm. (PORAP) E Lindstrom
- d. Ocean Vector Winds Science Team (OVWST) E Lindstrom
- e. Pathfinder Data Set & Associated Science Pgm.(PDSP) J Dodge
- f. EOS Interdisciplinary Science Program (EOS/IDS) J Dodge

In keeping with overall NASA goals and those of the Office of Earth Science, research supported by this NRA will be directed toward demonstrating successful use of data from satellite observing systems, in conjunction with other kinds of data, to improve models and assimilation systems for the Earth system or one or more of its components.

Participation in this program is open to all categories of domestic and foreign organizations, including educational institutions, industry, non-profit institutions, NASA centers, and other U.S. agencies. In accordance with NASA policy as described in Appendix C, all investigations by foreign participants will be conducted on a no-exchange-of-funds basis, i.e., investigators whose home institution is outside the United States cannot be funded by NASA. Proposals may be submitted at any time during the period ending September 27, 1999. Proposals submitted to NASA will be evaluated using scientific peer review. Proposals selected for funding will be announced in November, 1999.

All prospective proposers are strongly encouraged to submit a letter of intent (LOI) to propose to this Announcement by August 27, 1999. This letter should contain a brief description of the research to be proposed. Please see Appendix E of the NRA for details.

Point of Contact

Name: Kenneth H. Bergman
Title: Manager, Global Modeling and Analysis Program
Phone: (202) 358-0765
Fax: (202) 358-2770
E-mail: kbergman@hq.nasa.gov

--- End Forwarded Message ---

Dr. J.P. Darch
Research Administrator
School of Environmental Sciences
University of East Anglia
Norwich
NR4 7TJ
U.K.

Tel : 01603 592994

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Edward R. Cook" <drdendro@ldeo.columbia.edu>
Subject: Re: Vagonov et al. Nature paper
Date: Fri Jul 16 16:57:47 1999

Ed

to be really honest, I don't see how this was ever accepted for publication in Nature. It is a confusing paper that leaves me asking what actually have they done and what is the so-called testable Hypothesis of which they speak. Why didn't they do the testing? Yes Sob river is the Polar Urals site and I don't know why they get the results they do for it. Their precip. trends are dubious and our detailed regional response functions do not show a significant effect of high precip. in winter. I really have not had time to fully digest their message but I can't see why either they or Nature did not ask my opinion of it. My instinctive first reaction is that I doubt it is the answer but we do get results that support a recent loss of low-frequency spring temperature response in our data that may be consistent with their hypothesis of prolonged snow lie in recent decades. I have not spoken to Iain yet about the isotope data but I will. If you get any detailed thoughts on the Nature paper please let me know, as I don't know how to respond, if at all.

best wishes

Keith

At 04:11 PM 7/14/99 EDT, you wrote:

>Hi Keith,

>

>What is your take on the Vagonov et al. paper concerning the influence of

>snowfall and melt timing on tree growth in Siberia? Frankly, I can't

>believe it was published as is. It is amazingly thin on details. Isn't Sob

>the same site as your Polar Urals site? If so, why is the Sob response

>window so radically shorter than the ones you identified in your Nature

>paper for both density and ring width? I notice that they used Berezovo

>instead of Salekhard, which is much closer according to the map. Is that

>because daily data were only available for the Berezovo? Also, there is no

>evidence for a decline or loss of temperature response in your data in the

>post-1950s (I assume that you didn't apply a bodge here). This fully

>contradicts their claims, although I do admit that such an effect might be

>happening in some places.

>

>Cheers,

>

>Ed

>

>

>

From: Sarah Raper <s.raper@uea.ac.uk>
To: tar13@meto.gov.uk
Subject: Chapter 13 review
Date: Fri, 23 Jul 1999 19:52:44 +0100
Cc: mnoguer@meto.gov.uk, pvanderlinden@meto.gov.uk

COMMENTS ON CH. 13 (SCENARIOS) FROM TOM WIGLEY
(Page and line numbers are from the May 14 zero order draft.)

Dear contributors to Ch. 13,

Here are my comments on your chapter. I think you all know me well enough that you will not be offended by my occasional bluntness. The chapter needs a lot of work (not surprisingly), but it has at least touched most of the bases. It suffers from a lack of overview perspective, making the detail hard to wade through. I was disturbed by the lack of credit given to MAGICC/SCENGEN, since this software already addresses many of the key issues that arise in scenario development.

Apologies for not proof reading this. By the time I got to the end of typing it, I'd had enough.

- Page 3 (lines 86-89) : Critically, this information doesn't give a full assessment of uncertainties.
- 3 (110-115) : Sentence too long.
- 3 (117) : State 'illuminate uncertainty' earlier, since this is a primary purpose of, e.g., MAGICC/SCENGEN.
- 3 (118) : 'indeterminate' is far too strong.
- 4 (124-125) : Not clear.
- 4 (155) : What is 'integrated assessment'? Define and/or explain earlier.
- 5 (170) : Clumsy grammar.
- 5 (171-172) : Silly! Scenarios per se do not have ANY uncertainty associated with them, by definition. They are, however, a very (if not the most) useful tool for assessing and quantifying uncertainties. For example, a primary purpose of MAGICC/SCENGEN is to quantify uncertainties. Major text revision is needed to

clarify this point.

Part of the problem here is that the boundary between scenarios and predictions/projections is indistinct (as is the distinction between predictions and projections -- this too needs to be clarified). One could argue that 'scenarios' developed using MAGICC/SCENGEN are actually better predictions of some aspects of future climate change than any O/AGCM results. Certainly, 'scenarios' based on scaling are much more than just scenarios as defined here -- they are true predictions, based on some assumed scenario (this is the correct word here!) for future emissions.

Substantial work is required to the present text to clarify these issues -- they are the crux of the matter.

5 (178-179) : Note earlier that scenarios (a word I will continue to use even though it may be inappropriate in many cases) usually define CHANGES in climate. They are not, in these cases, 'scenarios', but 'scenarios of change'. Strict (i.e., absolute) scenarios are then constructed from them by adding the changes to a baseline climatology. This needs to be explained up front.

5 (187) : Delete '(and art)'. This is a derogatory term, likely to be misinterpreted/misrepresented.

6 (220) : Comma after 'scenarios'. The text contains many stylistic and grammatical errors (the most common being the failure to isolate parenthetical clauses). I will assume that someone with a better grasp of grammar will catch all these at some stage, so I will not comment further on them.

6 (225+) : A critical item missed here is inter-variable consistency. Later, consistency between climate and CO2 is mentioned; but there is no mention of consistency between, e.g., temperature and precipitation, etc. This is a major issue!

7 (257) : Instrumentally-based analogue scenarios were first introduced by Wigley et al. (Nature, 1979). Credit should be given. Also, the USDOE 'State of the Art (sic)' reports (1985) and the Bolin et al. SCOPE report (1986) both review this and other methods. This reviews should be cited.

7 (267-268) : What does 'extrapolating ...' mean?

7 (296) : Wigley et al. (1979) should also be cited here.

8 (306) : Nevertheless, they may do a better job of getting the inter-variable correlations 'right' than GCMs!

8 (315) : Delete 'questionable'. This word is entirely unnecessary here. More importantly, the authors need to be more careful in their choice of words, since there are many critics out there who

will be looking for things that can be taken out of context, misinterpreted, or misrepresented.

8 (344-345) : Control run? So what? This is only relevant if the control is used in scenario development. This raises the issue of 'Definition 1' versus 'Definition 2' for defining climate change (a terminology introduced by Santer et al., 1994, JGR). (Later, this difference is attributed to Cubasch et al., but it was first clearly enunciated by Santer et al.) The difference is whether or not one subtracts the control from the perturbed result. More needs to be said about this. It is often assumed that subtracting the control will remove any spurious drift in the perturbation experiment. This, of course, is clearly wishful thinking, both a priori, and as shown by Raper and Cubasch (1996). Basically, there is no way to reliably remove drift in a perturbation experiment; which makes it all the more important to have drift-free models. Flux adjustments do not necessarily remove drift -- just look at some of the ECHAM control-run results. There are some very important issues here, central to the use of O/AGCMs in scenario generation. They need better coverage. More is said later, but this is still inadequate.

9 (357) : Yes, they can be different, but so what? The issue is whether the differences are statistically significant. To my knowledge, no one has addressed this issue properly.

9 (358) : I'm sure (at least I hope) you don't mean 'observed'. The issue is the difference between the equilibrium PATTERNS of change and the MODELLED (NOT 'observed') transient patterns of change.

9 (to 361) : You've missed the most important point! The advantage of an equilibrium result over an O/AGCM result is that the former is pure signal.

9 (to 376) : The Definition 1 versus Definition 2 issue is relevant here.

9 (379) : Please don't propagate garbage. The issue here is natural internally generated variability. There is no need for such variability to be chaotic, so you should eschew use of this word.

9 (to 387) : I presume here that you are talking about O/AGCMs. You should not use just 'GCM' -- you must be specific. Also, you've missed some vital points: the natural internal variability problem (i.e., output is signal plus noise -- noted elsewhere, but must be stated here); and the model-specific nature of the climate sensitivity.

10(399) : Please give credit to the first work on this (Santer

- et al., 1990). I should point out that this was actually my idea.
- 10(404-406) : Totally unclear.
- 10(420-421) : Poor wording. Should be '.. to which changes are added'.
- 10(423) : Delete 'appropriate'.
- 10(429) : Insert 'based' after 'period'.
- 10(431) : 'weather generators' comes as a non sequitur here. In any event, you haven't said what they are!
- 10(435-437) : So what? The issue is what period one is measuring the impacts from. In most cases it will be some nominal 'present-day', so the baseline climatology must refer to the same period. Whether or not the period has some sulphate effect in it is utterly irrelevant.
- 10(437-438) : What garbage. See above.
- 11(448-450) : More garbage -- think about it! The reason 1990 is not so useful as a reference 'period' is because the impacts variable is probably not adequately definable over a single year. You have really messed up this issue.
- 11(460) : Yet more garbage! Given what I have tried to explain above, it is ludicrous to consider daily data as part of the baseline climatology. The impacts variable may require daily data from a baseline period in order to define ITS reference level (but probably not), but this is NOT the same thing. Either all this is very badly worded, or you don't know what your doing.
- 11(468) : No!! Think about it!
- 11(470) : No!! This is NOT the reason.
- 11(473) : No!! Not 'observed' (which is past or present), but FUTURE data.
- 11(482-483) : Duplication.
- 12(to 492) : This is a very confused paragraph.
- 12(497-499) : Wrong. For upper air, there is a major paper by Santer et al. (JGR, 1999), which also touches on some surface issues. There are also a number of papers by Trenberth that are relevant.
- 12(507) : Again, introduction of an undefined term/concept (downscaling).
- 12(510) : At last, mention of changes. Sadly, it is inappropriate here, since this is NOT the reason.
- 12(514) : Why should this Figure be here?
- 12(518) : Wrong. As a scenario, this could be justified. You are confusing scenario (as you have defined it, which I have already criticized) with prediction/projection.
- 12(521) : See above.
- 12(525-527) : This is the Def. 1 vs Def. 2 issue. However, you have

the history and motivation wrong.

12(527-531) : Wrong. This issue has nothing to do with cold start vs warm start; it is to get over the drift problem (which it fails to do).

12(537) : Not 'especially'; mor appropriate may be 'but only'.

13(543) : 'were'; grammar!

13(543-545) : Not clear.

13(552-553) : Not clear.

13(579-581) : So what? Given your definition of scenario, this doesn't matter.

14(594) : Why use 'perceived'?

14(604) : This issue was first raised by Kim et al. (1987?).

It was first addressed in a credible manner by Wigley et al. (1990).

14(606) : 'appending' is a ridiculous word to use. Try 'adding'.

14(608) : 'often' to 'usually'.

14(613) : 'appended' to 'added'.

14(616) : 'appended' to 'added'.

14(617) : 'appended' to 'added'.

14(627,628) : Please cite the key initial papers by Kim et al. and Wigley et al.

15(635,636) : Clumsy sentence.

15(638) : Isn't the word 'physical' usually used? The process does not just involve dynamics.

15(642-648) : Mention of 1-way vs 2-way nesting needed here.

15(657-659) : You have failed to mention the most important reason for using LAMs, orography/topography.

16(667) : Please cite the key initial papers by Kim et al. and Wigley et al.

16(673) : 'predict and' to 'predictand'.

16(679-683) : Once again, you fail to mention the main advantage; viz. that statistical downscaling involve real-world data and so ensures that inter-variable relationships are realistic. Of course, these relationships may change; but LAMs don't even get the correct relationships for the present.

16(703)-17(716): These are VERY important results. They need far greater emphasis.

17(720) : In Australia? Or anywhere for that matter.

17(723-724) : See, e.g., Wigley (1999 - Pew report- and material cited therein).

17(725) : 'muiltple'?

17(730-732) : Not clear.

17(739-740) : This sentence sounds stupid. Rephrase.

- 17(744) : You cannot say 'most areas' and then cite only agriculture cases.
- 17(748) : The first clear exposition of this is in the oft-cited paper by Wigley (Nature, 1985). See also later paper in Climate Monitor.
- 17(755-756) : I disagree. Both methods have strengths and weaknesses.
- 18(770) : At last! A definition of 'weather generators'.
- 18(778-779) : Unclear.
- 18(798) : What means 'more definitive'?
- 18(803) : "Wilk's" to "Wilks".
- 18(805) : Hence, the work is irrelevant in the present context.
Delete irrelevant text.
- 19(to 821) : Most of the agriculture studies dealing with the effects of variability changes are flawed since they fail to separate the low-frequency effect of induced changes in winter soil moisture levels from the specific effect of within-growing-season variability changes.
- 19(826-839) : Since this should refer back to lines 823,824, this whole section amounts to a giant non sequitur.
- 20(880) : One could be much stronger than this. The use of high spatial resolution information is more than just 'warranted', it is absolutely essential. However, there is another approach that you have failed to mention at all. This is 'upscaling' of the impacts model. There is some relevant work on this in papers by Jarvis and McNaughton (and vice versa). Another related approach is the direct modelling of spatial patterns of agricultural yield (as in work by Wigley and Tu Qipu, which relates yield patterns to climate patterns). Presumably one could apply a similar approach to direct modelling of river flow. These approaches complement the rather boring direct approach of downscaling, and they may well circumvent some of its problems.
- 20(898) : Under this comes: model errors; sensitivity uncertainties; aerosol forcing uncertainties; lag uncertainties, regionalization versus global-mean uncertainties.
- 21(905) : lesser or greater than what??
- 21(916) : 'adequacy' is not the right word; hoe about 'appropriateness'?
- 21(928) : I disagree. Re-analysis data for precipitation are simply not good enough, and precipitation is the key variable in most impact areas. Also, in the regions where scenario data are most needed, real observational data are available. Re-analyses largely provide useful new data in regions where

data are not needed. The authors seem not to have thought this through.

- 21(to 931) : There are two papers by Wigley (conference proceedings, edited by Hanisch) which address the issue of the relative magnitudes of different sources of uncertainty in global-mean projections (emissions, aerosol forcing, carbon cycle, other trace gases, climate sensitivity). These papers are singularly relevant to this section.
- 21(939) : Actually, the range for total emissions is from 7.9 to 29.0GtC/yr. For fossil CO₂ emissions, the range is 6.5 to 28.8GtC/yr.
- 21(943) : Not just 'time-dependent evolution', but anything that has a specific time attached to it.
- 22(948) : The reference to Alcamo et al. here seems either perverse or ignorant. Recall that the topic is CLIMATE scenarios. In this context, MAGICC/SCENGEN is FAR better suited to exploring the consequences (right down the line) of emissions 'uncertainties'.
- 22(959-960) : MAGICC/SCENGEN already does this at the global-mean level. Furthermore, at least three O/AGCMs have fully embedded sulphur cycles already.
- 22(968) : 'specifications' is the wrong word. These things are NOT 'specified'.
- 22(970) : 'determine' to 'have'
- 22(972) : See also Wigley's Pew report (1999).
- 22(974-976) : Not straightforward? This really is utter garbage. In MAGICC/SCENGEN, this is extremely easy and straightforward.
- 22(985) : Ah ha! The 1-way/2-way nesting issue surfaces at last!
- 22(989-990) : See above.
- 23(999) : Actually, this issue was first raised in Santer et al. (1990). It has also been addressed in papers by Wigley and Palutikof (probably before anyone else).
- 23(1010-1011): The wording here is not quite right.
- 23(1022) : First done in Santer et al. (1990).
- 23(1030) : If one assumes stable patterns, which has been shown to be okay for the CO₂ component of change, then the SNR problem can be minimized by using changes over a long time interval.
- 23(1033) : This average response method was alluded to in Santer et al. (1990). It was first implemented in ESCAPE and later in MAGICC/SCENGEN. A good illustration of the method, including some relevant discussion of it, is given in the Wigley Pew report (1999). One of the critical aspects of this method (which is not even mentioned here!) is that the results

must be normalized by the global-mean temperature before averaging.

24(1040) : Is this the ACACIA program run out of NCAR? This program was established some years ago, and it would be extremely confusing if there were two programs with the same acronym.

24(1047) : Not 'a few', but many -- CMIP1.

24(1060) : 'rations' to 'ratios'.

24(1060-1062): Not clear.

24(1073) : What means 'non-standard forcing'? In my view, something like IS92a forcing would be 'standard', whereas 1% compound CO₂ is 'non-standard' (i.e., unrealistic and artificial).

24(1076-1078): Really? Why? I think this statement is wrong. There are a number of ways to determine SNR values from a single O/AGCM run. (Note the continuing confusing use of 'GCM', instead of O/AGCM.)

24(1085) : I don't think 'uncertainties' is quite the right word here. Input emissions scenarios, which are scenarios in the strict sense of the word, do not directly address uncertainty issues (although they can, with some trepidation and a not-inconsiderable amount of ingenuity, be used to define uncertainties). By the way, as far as I can see, the only scenario development method/software that does address the input and uncertainty issues is MAGICC/SCENGEN.

25(1090) : Again, these are not the most appropriate references. Key references are Santer et al. (1990), and papers on ESCAPE and MAGICC/SCENGEN.

25(1093) : What means 'annotation' here?

25(1102) : Actually, it was my idea.

25(1105,1106): No! The key assumption is actually linear superposition.

This is the way that SO₄ effects are handled. There are a number of papers that show that this assumption works well for temperature, and a paper by Ramaswamy and Chen in GRL that shows that it works also for precipitation. The tricky thing for this variable would be to prove statistically that it doesn't work. Given the SNR, it would be very difficult to reject the null hypothesis that $P(A)+P(B)=P(A+B)$, where A,B are the forcings and P(.) is the response pattern.

25(1108) : Plus numerous other papers.

25(1112,1113): This is very galling. The method may have been used in IMAGE, but they got it from ESCAPE, which goes back to Santer et al. (1990). MAGICC/SCENGEN pushes the idea as far as is possible. Schlesinger's COSMIC does things quite

similarly to MAGICC/SCENGEN. (Schlesinger was a co-author of the Santer et al. paper.)

25(1115) : Not clear.

25(1122) : All you can say here is 'may not hold', not 'probably does not hold'. Indeed, there are reasons to expect it to hold quite well.

25(1123) : Could begin new paragraph with 'Uncertainties'.

25(1123,1124): I think this statement is categorically wrong. MAGICC/SCENGEN incorporates SO₄ influences, as does COSMIC. There is no evidence at all that the uncertainties are thereby amplified. Indeed, there is evidence to the contrary (e.g. Penner et al., 1997). Idle and unsupported speculations like this do nobody any good.

25(1124,1125): I suspect your argument here would have to hinge on the possible spatial effects of a THC slowdown or shutdown. If so, say so. But, if this is the case, you must also note that the latest non-flux-corrected O/AGCMs do not show these major THC changes, and scaling approaches may well work out very well for these situations, even in stabilization cases. Please avoid jumping to unsubstantiated conclusions.

25(1125) : I refereed this paper, and I judged it to be an appalling display of ignorance. It should not be cited.

26(1134) : Why is this Figure here?

26(1138) : Ah ha! At last the normalization issue. This must come much earlier.

26(1144-1147): This is simply wrong. It is true that Ramaswamy and Chen dreamed up a case with big hemispheric-scale responses but little global-mean response, but this was totally unrealistic. In all cases that I have looked at, using the method employed by MAGICC/SCENGEN and COSMIC, this is simply NOT a problem.

26(1147,1148): Again, this is just WRONG!

26(1150+) : Again, this is my idea, and it was first implemented in MAGICC/SCENGEN. Please give credit where due.

26(1156-1159): Isn't this ALWAYS the case. In other words, the scaling method is almost universally applicable and useful.

26(1159-1162): I do not think this has been proven.

26(1164,1165): There are other methods, too.

26(1172) : Oh come on! Scaling handles MANY types of uncertainty (perhaps all), not just 'one type'.

27(1181) : 'documented' to 'quantified'?

27(to 1185) : etc., etc.

27(1193) : MAGICC/SCENGEN allows the user to consider this issue

by providing data on global precipitation pattern correlations.

Indeed, this software was the first to consider this issue (in spite of the Whetton and Pittock paper cited on line 1199).

27(1198-1201): Very clumsy text.

27(1203-1204): This is an issue we considered years ago in developing ESCAPE and MAGICC/SCENGEN. The trouble with judging a model on its regional performance is one of statistical significance.

It is much easier to get a good regional result by chance than to get results that are good globally.

27(1208-1211): Very clumsy text.

27(to 1214) : You have failed to mention a key issue. Is model skill in simulating present-day climate a reliable indicator of its skill in predicting future climate change? There is no evidence to support this idea, although it does sound a priori reasonable.

You must at least raise the issue.

28(1227) : Cite Morgan and Keith (1995) here.

28(1231) : This is a critical point. It needs more emphasis.

28(1235+) : What about inter-variable consistency? This needs to be discussed.

28(1236) : 'the manifold' to 'possible'.

28(1239) : Insert 'give' after 'chapters'.

28(1252) : Not clear.

28(1255) : So what? It is almost certainly irrelevant unless the CO₂ changes are bigger than anything anticipated, or unless there are nonlinear effects associated with THC changes (which looks increasingly unlikely).

28(1257) : 'mimics'? You must be joking! How about 'approximates'?

28(1262) : 'equal' (grammar).

28(1262,1263): How can smart people like this make such an elementary mistake!

29(1280,1281): This does not seem to be an appropriate reference.

29(1282) : 'albino' to 'albedo'.

29(1294) : This sea level consistency issue was first addressed by Wigley and Raper (Warrick et al. sea level book). It is, of course, avoided in MAGICC/SCENGEN.

29(1295) : 'dependable' to 'dependent'.

29(1295-1301): A giant red herring! Maybe some ignorant people produced inconsistent scenarios like this years ago, but the issue was also resolved years ago. All you need to say is that comprehensive software suites avoid these naive problems. Concentrate on the strengths of existing methods/software; don't reraise issues that were solved long ago.

29(1305-1308): Another misleading red herring, that fails to reflect

the current state of the science. Global-mean responses to aerosol forcing CAN be used to drive regional patterns. This is just what is done in MAGICC/SCENGEN and COSMIC.

29(1310,1311): Not clear.

29(1314) : Delete 'scenario'.

29(1318) : 'to daily' to 'in daily'.

30(1329,1330): 'stimulated new techniques' Oh yeah? The MAGICC/SCENGEN method has not changed in 7 years, and it still represents the state of the science.

30(1332,1333): True, but you have not explained them very well. Could you not have a summary Table that lists the strengths and weaknesses of the various methods, including the direct use of O/AGCM output. This would have helped you a lot in planning and structuring this chapter. It can still help in revising it; and be useful to readers.

30(1336-1339): Not clear.

30(1342) : You have mentioned this before, but you have failed to tell us what it is or given any example. A mention alone is valueless.

30(1344) : What means 'semi-formal'. I thought it was a dress protocol.

30(general) : A crucial need for scenarios (and for simple models) is to expand the range of cases covered by O/AGCMs.

END *****

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From: Mike Hulme <m.hulme@uea.ac.uk>
To: Jennifer F Crossley <J.Crossley@uea.ac.uk>
Subject: Re: masking of WWF maps
Date: Thu Jul 29 09:13:24 1999

Jenny,

Thanks for these.

After entering into debate with Barrie Pittock, I have decided to shift to using the 1 sigma level as a mask for all maps. This will not affect any of the temperature plots you have done until now, but means that the China and C.America precipitation maps will need re-drawing using 1 sigma. Please let me know when these are done.

Note also for Russia and that everything from now on for WWF (both T and P) should use 1 sigma as the mask.

Sorry about this and I realise this squeezes even more time away from the RCM.

Given what has happened and your role in producing these plots, you may be interested in the exchanges I have had with Barrie Pittock - it illustrates nicely the nuances of presenting climate scenarios in different Fora. Read these three emails in reverse order.

Mike

Dear Mike,

Thank you for your careful consideration of my "trenchant comments". I am now much happier with what you are doing, and indeed grateful for your hard work and enterprise in getting the new scenarios out so quickly for both IPCC and WWF. Shifting to a one standard deviation is certainly an improvement, along with some discussion of possible changes in extremes. I fully appreciate that analysis of daily output is a time-consuming future task, but meantime an appropriate caveat is needed. Maybe an additional upfront paragraph discussion of the very issues we have discussed re providing best estimates of changes, even if their statistical detectability can only be established after a long time period has elapsed, would be useful?

I should perhaps explain my delicate position in all this. As a retired CSIRO person I have somewhat more independence than before, and perhaps a reduced sense of vested interest in CSIRO, but I am still closely in touch and supportive of what CAR is doing. Also, I have a son who is now a leading staff member of WWF in Australia and who is naturally well informed on climate change issues. Moreover, Michael Rae, who is their local climate change staffer, is a member of the CSIRO sector advisory committee (along with some industry people as well) and well known to me. So I anticipated questions from WWF Australia, and from the media later when the scenarios are released, regarding the scenarios. I did

not want to be in the position of feeling the need to seriously question in public their presentation or interpretation. You have allayed my fears on that score, so that is great.

Roger may still follow up with some more detailed comments he is collating from people in CAR.

Best regards,

Barrie.

Barrie,

Thanks for your trenchant comments re. the scenario maps.

Let's get the bit about extremes out of the way because in what WWF have asked us to do (or what Tim Carter and I have done for WGII) we cannot produce new detailed analyses for all the 15 regions we are doing of GCM-based changes in daily or sub-daily events. Clearly for (some, many?) impacts such changes will be important and we (do and will) make comments to this effect in various places. [By the way, we do show some analyses of changes in the probability of extreme *seasons*, if not extreme days].

Your main point of contention, however, is about the portrayal of changes in mean seasonal T and P (and we are talking about 30-year climate averages here).

My reason for introducing the idea of only showing changes in T and P that *exceed* some level of 'natural' variability was a pedagogic one, rather than a formal statistical one (I concede that using '95% confidence' terminology in the WWF leaflet is misleading and will drop this). And the pedagogic role of this type of visual display is to bring home to people that (some, much or all of) GCM simulated changes in mean seasonal precip. for some regions do *not* amount to anything very large in relation to what may happen in the future to precip. anyway - a classic example is the African Sahel where *none* of the GCMs get precip. changes anything like as large as have been seen this century.

The reasons for this may be 1) because the GHG signal is poorly defined, i.e., a scatter of GCM P changes both above and below zero, and/or 2) because even with a tighter bunching of GCM predictions in one direction these may still not be large relative to 'natural' variations in 30-year mean precip. My approach of taking a pseudo-ensemble of GCMs, standardising and scaling and then plotting the Median *in relation to* natural variations is I think one of the more elegant ways of showing this. Of course, we could define natural variability to be the 1 sigma rather than the 2 sigma level, or simply the interquartile range of control climates or even just the 40-60 percentile range. What one chooses is a matter of judgement and probably for WWF I should use a less extreme threshold than 2 sigma.

The point behind all this is to emphasise that precip. changes are less well-defined than temp. changes *and* that we should be thinking of adaptation to *present* levels of precip. variability, rather than getting hung up on the problems of predicting future precip. levels. This pedagogic thinking is hard to communicate in a short WWF brochure.

Your concern about my message is well taken, however, and I intend to remove any reference to 95% confidence levels, to re-word the text to indicate that we are plotting precip. changes only 'where they are large relative to natural variability', and to reduce my threshold to the 1 sigma level of HadCM2 control variability (e.g. this has the effect of showing precip. changes for the majority of Australia even in the B1 scenario).

But I do not intend to abandon the concept. I think it important - even for Greenie groups - to present sober assessments of magnitudes of change. Thus making it clear that future changes in T are better defined than future changes in P, and also to point out that future emissions (and therefore climate change) may be as low as the B1 scenario (is B1 climate change negligible? I almost think so), whilst also being possibly as high as A2 is I think very important.

The alternative is to think that such a more subtle presentation is too sophisticated for WWF. But I think (hope) not.

Thanks again Barrie for forcing me to think through this again.

Mike

At 17:52 28/07/99 +1000, you wrote:

>Hello Mike,

>

>I am giving a preliminary response to your suggestion that Peter Whetton
>comment on your scenario material in case there is some urgency. Peter
>did write an email last Friday night before going on a week's holiday,
>but unfortunately the email system failed and it probably did not go and
>has been lost. He asked Roger Jones to respond on behalf of the group
>but Roger is snowed under at present.

>

>Peter and I did discuss it on Friday. Our main concern (although there
>are other more detailed ones) is your use of the 95% confidence limits
>of natural climatic variability as some sort of threshold for change.
>This is a reasonable thing to do if you are addressing the question of
>whether climatic change will be detectable at a "scientific level" of
>confidence, but that is certainly not the question I would expect WWF to
>>want answered, nor is it the one most relevant to giving policy advice.
>The relevant question is "What is the best estimate of climate change,
>given the assumption that increasing GH gases will cause change?". The
>contrast between these questions, the statistical criteria they require,
>and thus the answers, is what I was driving at in my comment on your
>paper in Nature. It is a very serious difference with serious
>consequences for how people will interpret your advice. The results as

>you present them suggest that many areas will have precipitation changes
>(particularly) which are small compared to natural variability, and
>therefore it does not matter. But if the change in mean is some
>appreciable fraction of natural variability, say, 50%, that is a very
>serious matter which ought to concern policy makers, because it will
>have cumulative impacts, especially in regard to large changes in the
>frequency and magnitude of extremes (floods and droughts). Surely you
>understand that! - refer to the standard diagrams of the impact on
>extremes of shifting a normal distribution by one standard deviation.

>

>What you are doing is using a strict Type I error criterion when others
>(WWF?) might think a Type II error criterion is more suitable (the
>Precautionary Principle), and reasonable people (like me of course!?)
>think a criterion in between which measures risk of serious impacts is
>what is needed for policymakers. The reference I gave in my comment in
>Nature may not be the best - but look at my argument in QJRM, 109,
>pp.46-48 (1983) for a clearer exposition on this point.

>

>The other related matter is that your scenarios for WWF, and for that
>matter for IPCC WG2, do not discuss the importance of changes in
>extremes, which are arguably the most important changes, however poorly
>understood they may be at present. This and the other caveats you are
>intending to include in the IPCC material, re scaling, sulfate aerosol
>effects, longer timescales, and change after stabilisation of
>concentrations, should be in the WWF material also, even if they
>complicate things a bit (I have not checked whether some of that is in
>your WWF stuff as yet).

>

>I would be very concerned if the material comes out under WWF auspices
>in a way that can be interpreted as saying that "even a
>greenie group like WWF" thinks large areas of the world will have
>negligible climate change. But that is where your 95% confidence limit
>leads.

>

>Sorry to be critical, but better now than later!

>

>Best regards,

>

>Barrie.

>

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>

>* As from 1 March 1999 I have become a CSIRO Post-Retirement Fellow.

>This means I do not have administrative responsibilities, and am

>working part-time, primarily on writing for the Intergovernmental Panel

>on Climate Change. Please refer any administrative matters or contract

>negotiations for the CIG to Dr. Peter Whetton, the new Group Leader, at

><peter.whetton@dar.csiro.au>, tel. +61 3 9239 4535.

>

>"Far better an approximate answer to the right question which is often
>vague, than an exact answer to the wrong question which can always be
>made precise." J.W. Tukey as cited by R. Lewin, Science 221,636-639.

>
>

From: Adam Markham <Adam.Markham@WWFUS.ORG>
To: m.hulme@uea.ac.uk, n.sheard@uea.ac.uk
Subject: WWF Australia
Date: Thu, 29 Jul 1999 09:43:09 -0400
Cc: mrae@wwf.org.au

Hi Mike,

I'm sure you will get some comments direct from Mike Rae in WWF Australia, but I wanted to pass on the gist of what they've said to me so far.

They are worried that this may present a slightly more conservative approach to the risks than they are hearing from CSIRO. In particular, they would like to see the section on variability and extreme events beefed up if possible. They regard an increased likelihood of even 50% of drought or extreme weather as a significant risk. Drought is also a particularly important issue for Australia, as are tropical storms.

I guess the bottom line is that if they are going to go with a big public splash on this they need something that will get good support from CSIRO scientists (who will certainly be asked to comment by the press). One paper they referred me to, which you probably know well is: "The Question of Significance" by Barrie in Nature Vol 397, 25 Feb 1999, p 657

Let me know what you think. Adam

From: mann@snow.geo.umass.edu

To: pedersen@eos.ubc.ca

Subject: No Subject

Date: Tue, 3 Aug 1999 17:41:02 -0400 (EDT)

Cc: calvert@unixg.ubc.ca, k.briffa@uea.ac.uk, rbradley@climate1.geo.umass.edu, weaver@ocean.seos.uvic.ca

Dear Tom,

Thanks for bringing that to our attention...

I checked out that page and, unfortunately what he has done is *so* ridden with problems that it isn't even worth confronting. Many of us (e.g., me, Phil Jones, Henry Pollack, Shao-Yang Huang, Rob Harris, and others) have been scratching our heads trying to find a statistically defensible way of combining the information in boreholes and "conventional" proxy indicators, and as yet it is not clear if it can be done, given in particular the loss of information due to geothermal diffusion, and the overriding important of land-usage changes and snowcover variations, on borehole temperature profiles. I don't think Hoyt has added anything scientifically productive in this regard. Looks more like he has wrecklessly convoluted borhole data with our reconstructions to get just the kind of result he wants to get...

Of course, there are issues with regard to secular trends in dendroclimatic reconstructions (which form an important, but not exclusive, role in our reconstructions) and nobody is better qualified to discuss these than Keith, or Malcolm Hughes, who have highlighted these issues in recent publications (there is a link to a nice recent "Nota Bene" Science piece by Keith and Tim Osborn on my webpage:

<http://eclogite.geo.umass.edu/climate/mike/mbh99.html>

With regard to "Co2 fertilization", it is ironic that Hoyt frames his analysis in these terms, when it precisely this effect (for better or for worse) we took great pains to account for in our recent millennial temperature reconstruction (see the above web page for more info). At least, we have done this in a reasonably statistically-defensible, if imperfect, manner, rather than an ad hoc attempt to get an answer, rather than follow

a scientifically meaningful process.

This thing wouldn't have a chance at passing peer-review (at least, not on this planet), so he posts it on the web--the downside of absolute freedom of dissemination I suppose. The material in question is the scientific equivalent of trash, plain and simple.

Like a lot of the "skeptics" out there, D.H. appears far less interested in honest scientific discourse, than in misleading as many unlucky soles as possible who wander into his den of disinformation (kind of like the "scientist" equivalent of an Ant Lion I suppose).

Every once and a while, I do choose to respond to this type of crap (e.g., with regard to Pat Michaels--my soon-to-be "neighbor"'s recent pieces in his "World Climate Report"). In D.H.'s case, I doubt even more that this would be at all productive. We just have to wait and see if he ever tries to get this kind of thing published in the peer-reviewed literature. For our part, I think the best approach is to, as Jonathan Overpeck has so effectively been doing, try whenever possible to educate the lay public about the essential distinction between peer-reviewed science and un-peer-reviewed...., well, whatever you want to call it.

Again, thanks for the head's up on this.

best regards,

mike mann

>X-Sender: tfp@pop.unixg.ubc.ca
>Date: Tue, 3 Aug 1999 13:36:36 -0700
>To: rbradley@climate1.geo.umass.edu
>From: Tom Pedersen <pedersen@eos.ubc.ca>
>Subject: Skeptics
>Cc: calvert@unixg.ubc.ca (Steve Calvert), k.briffa@uea.ac.uk,
> weaver@ocean.seos.uvic.ca
>
>Hi Ray:
>My colleague, Steve Calvert, has just brought to my attention a website of
>which I was unaware but you probably know well. It's at
><http://www.erols.com/dhoyt1>

>T.F. Pedersen
>Oceanography, Earth and Ocean Sciences, 6270 University Boulevard,
>University of British Columbia, Vancouver, B.C. Canada V6T 1Z4
>Telephone: 604-822-5984 Fax: 604-822-6091 Email: pedersen@eos.ubc.ca
><http://www.eos.ubc.ca/>
>
>

Michael E. Mann

Current | Starting Fall 1999

Adjunct Assistant Professor		Assistant Professor
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Phone: (413) 545-9573 FAX: (413) 545-1200

<http://www.geo.umass.edu/climate/mike>

From: "Karl E. Taylor" <taylor13@llnl.gov>
To: mmaccrac@usgcrp.gov
Subject: to mask or not
Date: Tue, 17 Aug 1999 16:30:58 -0700
Cc: taylor13@llnl.gov, santer1@llnl.gov, wigley@meeke.ucar.edu, p.jones@uea.ac.uk

Mike,

I thought maybe I could contribute a few comments to your concern over using a common coverage mask for surface and MSU temperatures. (Copy of your relevant paragraph copied below.)

Whether or not to mask depends on the question being addressed. If we wanted the best estimate of global mean MSU temperatures, then clearly we wouldn't want to mask. The issues we address, however, are largely based on an expectation (from models and observations) that over large portions of the globe strong vertical coupling tends to lead to large positive correlations between surface and lower tropospheric temperatures. There is a further (model-based) expectation that any warming trend at the surface should be slightly amplified higher up in the troposphere. These expectations seem to be contradicted by the MSU data (at least for global mean trends).

Masking makes most sense if there is in fact strong coupling between the surface and troposphere. Suppose the CO₂ warming signal were one with relatively strong warming over land areas and weaker warming over ocean. Suppose further that we only had surface temperature measurements over land, but had MSU retrievals over all the globe. Also assume a case of perfect coupling (1K rise in local upper air temperature for every 1K rise in local surface temperature).

In this case the unmasked global mean MSU temperature increase would be less than the "global" mean surface temperature increase, falsely indicating a damping with height of the CO₂ signal. If we masked the MSU temperature (sampling only over land), then the global means would be computed over the same area as the surface temperature and the MSU temperature change would equal the surface temperature change, indicating no damping of the response with height. This second conclusion would be the correct one. Note, however, that the true global mean temperature change (both at the surface and

aloft) would be best estimated using the MSU unmasked data (under the conditions of this hypothesized case).

Under different conditions, and again depending on what question is being addressed, it might be best not to mask the MSU data. In our paper we wanted to determine whether the apparent discrepancy between the MSU trend (very small) and the surface trend (positive, and larger) could be explained by coverage differences. This makes sense since models seem to indicate that the trends should be comparable. One explanation for the discrepancy is that in models true global means had been considered until now, whereas in the data the MSU mean was computed from global coverage, but the surface changes were computed from data covering about 70% of the globe. In our study both model data and observations were treated with the same mask so we rule out different sampling as a full explanation for the difference between surface and MSU temperature trends.

Hope this doesn't confuse things further.

cheers,
Karl

Mike wrote (in part):

I think one needs to be very careful about this coverage argument--basically because the atmosphere can move anomalies around compared to the surface. One would just not expect their spatial patterns to be the same, so taking a common spatial mask will not resolve this (even if it seems plausible). To illustrate, take an extreme example of there only being sfc msmts for the equatorial eastern Pacific (the El Nino region). There, the MSU and sfc temp go in opposite directions for quite plausible physical reasons. Doing a mask and comparing for that small region would make no sense and give negative correlations, etc. Now, in that sfc obs cover most of the globe, the problem will not be so severe, but it persists (it was for this reason that I was suggesting extrapolating to the global value for sfc temp based on changing coverage--not sure how to do that however). In any case, I believe that MSU and sfc should only be compared, if at all, for the globe as a whole.

From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Holocene paper
Date: Mon, 23 Aug 1999 13:56:46 +0500
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith,

I just come back from Yamal. We collected subfossil wood in Yuribey River basin (50-150 km northward of recent timberline) and have found about one hundred remains of trees.

Before departure for Yamal, on July 17, I have sent you draft outline of paper for Holocene. I asked Valery Mazepa to send it one more if any problems in connection. Now Valery is in Polar Ural and I don't know did you receive this outline.

Could you inform me about this.

Thank you.

Best regards,
Rashit M. Hantemirov

Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: rashit@ipae.uran.ru
Fax: +7 (3432) 29 41 61; phone: +7 (3432) 29 40 92

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Proposal to IARC
Date: Mon, 6 Sep 1999 17:18:44 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,

Some days ago we have got "JOINT ANNOUNCEMENT OF OPPORTUNITY" from International Arctic Research Center and Cooperative Institute for Arctic Research University of Alaska Fairbanks. The general theme is Global Change Research in the Arctic (full text with description is attached bellow). As we have read Research Themes from announcement they seem to be very congenial to our laboratory. What do you think about this? Is there point in submitting proposal to IARC and CIFAR at the University of Alaska Fairbanks? Research theme would be 5,000 year summer air temperature reconstruction from tree rings and impacts and consequences of global climate change on forest ecosystems in the Polar Ural and Yamal Peninsula (Subarctic regions of Russia).

We have no wide experience to submit proposal to any foreign administration. We need in some advice. Could you give us a piece of good advice how to do this well.

The questions are:

1. We are not sure whether this action and theme is contrary to our future cooperative work?
2. If not, how big our chance to get award?
3. Could we submit a proposal from our Institute only without U.S. partner? (Proposals from foreign institutions should preferably have a U.S. partner. See description bellow). If U.S. partner should be, who in your opinion would be?

Best regards.

Stepan.

From: ArcticInfo
<arcticInfo@mail.arcus.org>
To: arcticinfo@arcus.org
Subject: IARC Announcement of Opportunity

For more information on these research opportunities contact:

Professor Syun Akasofu, Director, IARC, Phone: 907/474-6012,
Fax: 907/474-5662, or E-mail: sakasofu@iarc.uaf.edu.

RESEARCH OPPORTUNITIES

JOINT ANNOUNCEMENT OF OPPORTUNITY

International Arctic Research Center and Cooperative Institute for
Arctic Research University of Alaska Fairbanks
Global Change Research in the Arctic

INTRODUCTION

Proposals are invited on topics of global change and its effects in the Arctic (detection; interactions and feedbacks; paleoclimates, arctic haze, ozone and UV; contaminants; impacts and consequences of change). The proposal deadline is 1 October 1999 and awards will be made in January 2000.

DESCRIPTION

The International Arctic Research Center (IARC) and the Cooperative Institute for Arctic Research (CIFAR) at the University of Alaska Fairbanks announce the availability of funding for global change research in the Arctic. The IARC is a new international research center at the University of Alaska Fairbanks, established jointly with Japan. The mission of the IARC is to provide an environment that will nurture multidisciplinary research by integrating and synthesizing past, present, and future studies in global change.

CIFAR is the NOAA-UAF Cooperative Institute for Arctic Research; it is combining the resources of its Arctic Research Initiative (ARI) with those of IARC under this announcement. The goal is to develop a focal point for a pan-Arctic synthesis of global change in which researchers from many different institutions throughout the United States and the rest of the world participate to combine their research results. Further details on IARC can be found on its web page at <http://www.iarc.uaf.edu/> and on CIFAR at <http://www.cifar.uaf.edu/>.

Proposals may be submitted from U.S. or foreign institutions that address studies on any of the following themes drawn from the IARC Science Plan and the CIFAR Arctic Research Initiative. Proposals from foreign institutions should preferably have a U.S. partner. The starting date for proposed work should be 1 January 2000, with a duration of up to 24 months. Funding for the second year will be contingent on the availability of additional funds, therefore each proposal should have a clear, achievable objective for the first year's work.

RESEARCH THEMES

1. Detection of contemporary climate change in the Arctic by ground observations, remote sensing and climate "fingerprinting".
2. Arctic paleoclimatic reconstructions from ice cores, tree rings, permafrost, lake and ocean sediments.
3. Atmosphere-ice-land-ocean interactions and feedbacks in the Arctic that affect change, including observations and modeling.
4. Arctic atmospheric chemistry, arctic haze, ozone and UV radiation and their effects.
5. Impacts and consequences of global climate change, including effects on biota and ecosystems in the Arctic.
6. Contaminant sources, transport pathways, and exposure to higher trophic levels and humans in the Arctic.

It is planned to fund several large projects and a number of medium (\$100K) or smaller projects. Proposals must include the full cost of logistics support required. A total of about \$ 4.5M is available in year 1 for this Announcement of Opportunity.

Proposals can request support for the following:

- * Research on any of the above six themes. Proposals that add value to ongoing research projects, or that share costs with other funded investigators, are encouraged.
- * Conducting workshops at the IARC to further define priorities or synthesize available information on any of the research themes listed above, or any theme from the IARC Science Plan.
- * Visiting scientists, for short- or longer-term visits, to the IARC in Fairbanks.
- * Development of generally useful curricula and courses in global change, or conducting global change outreach and educational activities.
- * U.S. participation in the work of the Arctic Council and its AMAP, CAFF, or PAME working groups.

All proposers should meet the following conditions:

- * PIs must attend an annual synthesis meeting of all IARC/CIFAR investigators in Fairbanks at which research results will be presented and working groups will synthesize results. Proposal budgets should include travel to Fairbanks.

- * All activities will be required to acknowledge the financial support from IARC and CIFAR in reports, papers, dissertations, etc.
- * Progress reports are due from all funded projects on 1 August 2000.
- * Copies of all publications resulting from funded projects are to be provided to IARC/CIFAR.

Proposals should not exceed 15 pages in text and illustrations, not counting CVs, budget page, and appendices. Further details on proposal preparation are attached below as an appendix.

Review criteria for research proposals are:

- * Does the proposal address the research themes listed above?
- * Does it propose high-quality research?
- * Does it advance the NOAA mission?
- * Is the PI (or are the PIs) well qualified to do the research?
- * Can the research be done in a timely manner?
- * Is it likely to lead to significant results?
- * Is it likely to contribute to a synthesis of research results on global change?

Proposals must be received by 1 October 1999. All proposals will be reviewed by a scientific peer review panel of prominent researchers that will advise a program management team drawn from NOAA, IARC, and CIFAR. Funds will be available in early 2000. Please submit proposals (originals and 10 copies) to the address below. Further information can also be obtained from the same office.

Professor Syun Akasofu, Director
International Arctic Research Center
University of Alaska Fairbanks
930 North Koyukuk Drive
P. O. Box 757340
Fairbanks, AK 99775-7340
Tel 907/474-6012
Fax 907/474-5662
e-mail: sakasofu@iarc.uaf.edu

Program Management Team:
Syun Akasofu, Director, IARC, University of Alaska, Fairbanks, AK

John Calder, Director, Arctic Research, NOAA-OAR, Silver Spring, MD
Gunter Weller, Director, CIFAR, University of Alaska, Fairbanks, AK

APPENDIX

INSTRUCTIONS FOR PROPOSAL PREPARATION FORMAT OF THE PROPOSAL

Proposals should be stapled in the upper left-hand corner, but otherwise be unbound, and have 2.5-cm margins at the top, bottom, and on each side. The type size must be clear and readily legible, in a standard font size of 10-12 point. The original signed copy should be clipped together (not stapled) and printed on one side of each sheet only. The 10 additional copies of the proposal may be printed on both sides.

When submitting collaborative proposals involving more than one institution, each institution should submit its own cover page with appropriate signatures and its own budget. The title of the proposal, the text, disclosures, vitae, etc., should be the same and a cover letter should indicate that the proposal is a collaborative one jointly submitted with another (or other) institution(s) which should be named.

SECTIONS OF THE PROPOSAL

1. Cover page. The cover page should include a title, the Principal Investigator's name(s) and affiliation(s), complete address, phone, fax, e-mail information, and budget summary broken out by year. It must be signed by an official authorized to legally bind the submitting organization.
2. Half-page abstract (on a separate page). This should list the nature of the proposed work (e.g., hypotheses to be tested, the relationship of the proposed studies to the research themes, the goals of any proposed workshops, relationship to the Arctic Council, etc.) and a summary of the key approach.
3. Project Description. This section should present the problem or opportunity to be addressed by the project, and state the questions, hypotheses, and project objectives, clearly relating them to the goals of this competition. Proposals should: summarize the approach that will be used to address the questions, hypotheses, and objectives; describe how the PIs and co-PIs would contribute to the overall study approach; describe the methods to be used; and present expected results.
4. Data Plan. The proposal should include a plan on

how the data generated by the proposed research will be made available to other scientists (e.g., web pages) and deposited in a recognized data archive.

5. References cited.

6. Milestone chart for the project.

7. Statement of the project responsibilities of each Principal Investigator and participant.

8. Budget. Pattern your budget after NSF

budget Form 1030. Budget categories include the following: salaries and wages, fringe benefits, equipment, travel, materials and supplies (expendable), publication costs, consultant services, computer services, sub-awards, tuition, other expenditures, and indirect costs (facilities and administration). The full cost of logistics should be included in the budget. Travel to an annual PI meeting in Fairbanks should be included. Travel expenses need to be broken down by airfare and per diem. Salaries for Government PIs will not be supported.

9. Biographical Sketch. This is limited to two pages for each Principal Investigator and should be focused on information directly relevant to undertaking the proposed research.

10. A short list of possible peer reviewers with whom you have no close working or personal relationship (optional).

11. Federal employees. Proposals are welcome from those Federal agencies whose legislated mission allows participation.

NONDISCRIMINATION The National Oceanic and Atmospheric Administration provides awards for research in the sciences. The awardee is wholly responsible for the conduct of such research and preparation of the results for publication. NOAA, therefore, does not assume responsibility for such findings or their interpretation. IARC and CIFAR welcome proposals on behalf of all qualified scientists and engineers, and strongly encourage women, minorities, and persons with disabilities to compete fully in any of the research and research-related programs described in this document. In accordance with Federal statutes and regulations, and NOAA policies, no person on the grounds of race, color, age, sex, national origin, or disability shall be excluded from participation in, denied the benefits of, or be subjected to discrimination under any program or activity receiving financial assistance from NOAA.

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would like to post to the mailing list send the message to
dan@arcus.org or arcus@arcus.org. You can search back issues of
ArcticInfo by content or date at
http://www.arcus.org/ArcticInfo/fr_Search.html If you have any
questions please contact the list administrator: dan@arcus.org
ARCUS
600 University Avenue, Suite 1 Fairbanks, AK 99709 907/474-1600
907/474-1604 fax

Lab. of Dendrochronology

Institute of Plant and Animal Ecology
8 Marta St.,
202 Ekaterinburg, 620144, Russia
e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
Subject: Re: Proposal to IARC
Date: Tue Sep 7 14:17:25 1999

Stepan

I have just returned from a week at a PAGES meeting in Switzerland. I presented the Yamal and Taimyr chronologies along with our recent analyses of the spatial patterns of variability in the northern network and the pressure and temperature interpretation of the patterns. All of this was well received.

As for your questions, it is very short notice to consider getting a well organised proposal together. My answers to your specific questions are
1. Such work would not necessarily be contrary to our current and future plans but there is undoubtedly a potential overlap and possible problem in distinguishing tasks and outputs. The next EC proposal must be clearly separate and I would be concerned if the potential referees asked what was the clear difference.

2. I have no experience (and presumably neither has anyone else as this is a new initiative) but I think the chances would depend on the degree of synthesis involved in the work and possibly how extensive the overall scope of the work is and also maybe who the U.S. collaborator is. I think your chance would be better as part of a large project , somewhat as we envisage for the next EC application. This is my opinion only and it may , of course, be wrong.

3. I see nothing preventing an application from your laboratory alone . If you do put in an application I would hope it made clear our ongoing collaboration. If you went for a collaborator in the U.S. the obvious person is Gordon Jacoby. I do not know if he is already submitting but I would think so. Please let me know what you decide . I will be phoning Gordon anyway to ask him about future collaboration on the EC front. I will keep you informed on that.

very best wishes

Keith

At 05:18 PM 9/6/99 +0500, you wrote:

>Dear Keith,

>Some days ago we have got "JOINT ANNOUNCEMENT OF
>OPPORTUNITY" from International Arctic Research Center and Cooperative
>Institute for Arctic Research University of Alaska Fairbanks. The
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>Stepan.

>

>From: ArcticInfo

><arcticInfo@mail.arcus.org>

>To: arcticinfo@arcus.org

>Subject: IARC Announcement of Opportunity

>For more information on these research

>opportunities contact:

>Professor Syun Akasofu, Director, IARC, Phone: 907/474-6012,

>Fax: 907/474-5662, or E-mail: sakasofu@iarc.uaf.edu.

>

>RESEARCH OPPORTUNITIES

>JOINT ANNOUNCEMENT OF OPPORTUNITY

>International Arctic Research Center and Cooperative Institute for

>Arctic Research University of Alaska Fairbanks

>Global Change Research in the Arctic

>

>INTRODUCTION

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>effects in the Arctic (detection; interactions and feedbacks;

>paleoclimates, arctic haze, ozone and UV; contaminants; impacts and

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>4. Arctic atmospheric chemistry, arctic haze, ozone and UV radiation and
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>5. Impacts and consequences of global climate change,

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>Proposals must be
>received by 1 October 1999. All proposals will be reviewed by a
>scientific peer review panel of prominent researchers that will advise
>a program management team drawn from NOAA, IARC, and CIFAR. Funds will
>be available in early 2000. Please submit proposals (originals and 10
>copies) to the address below. Further information can also be obtained
>from the same office.
>Professor Syun Akasofu, Director
>International Arctic Research Center
>University of Alaska Fairbanks
>930 North Koyukuk Drive
>P. O. Box 757340
>Fairbanks, AK 99775-7340
>Tel 907/474-6012
>Fax 907/474-5662
>e-mail: sakasofu@iarc.uaf.edu
>
>Program Management Team:
>Syun Akasofu, Director, IARC, University of Alaska, Fairbanks, AK
>John Calder, Director, Arctic Research, NOAA-OAR, Silver Spring, MD
>Gunter Weller, Director, CIFAR, University of Alaska, Fairbanks, AK
>*****

>
> APPENDIX

>INSTRUCTIONS FOR PROPOSAL PREPARATION
>FORMAT OF THE PROPOSAL
>Proposals should be stapled in the upper left-hand corner, but
>otherwise be unbound, and have 2.5-cm margins at the top, bottom, and
>on each side. The type size must be clear and readily legible, in
>a standard font size of 10-12 point. The original signed copy should

>be clipped together (not stapled) and printed on one side of each sheet
>only. The 10 additional copies of the proposal may be printed on both
>sides.

>

>When submitting collaborative proposals involving more than one
>institution, each institution should submit its own cover page with
>appropriate signatures and its own budget. The title of the proposal,
>the text, disclosures, vitae, etc., should be the same and a cover
>letter should indicate that the proposal is a collaborative one
>jointly submitted with another (or other) institution(s) which should
>be named.

>

>SECTIONS OF THE PROPOSAL

>1. Cover page. The cover page

>should include a title, the Principal Investigator's name(s) and
>affiliation(s), complete address, phone, fax, e-mail information, and
>budget summary broken out by year. It must be signed by an official
>authorized to legally bind the submitting organization.

>2. Half-page

>abstract (on a separate page). This should list the nature of the
>proposed work (e.g., hypotheses to be tested, the relationship of the
>proposed studies to the research themes, the goals of any proposed
>workshops, relationship to the Arctic Council, etc.) and a summary of
>the key approach.

>3. Project Description. This section should present

>the problem or opportunity to be addressed by the project, and state
>the questions, hypotheses, and project objectives, clearly relating
>them to the goals of this competition. Proposals should: summarize the
>approach that will be used to address the questions, hypotheses, and
>objectives; describe how the PIs and co-PIs would contribute to the
>overall study approach; describe the methods to be used; and present
>expected results.

>4. Data Plan. The proposal should include a plan on

>how the data generated by the proposed research will be made available
>to other scientists (e.g., web pages) and deposited in a recognized
>data archive.

>5. References cited.

>6. Milestone chart for the project.

>7. Statement of the project responsibilities of each Principal
>Investigator and participant.

>8. Budget. Pattern your budget after NSF

>budget Form 1030. Budget categories include the following: salaries
>and wages, fringe benefits, equipment, travel, materials and supplies
>(expendable), publication costs, consultant services, computer
>services, sub-awards, tuition, other expenditures, and indirect costs
>(facilities and administration). The full cost of logistics should be
>included in the budget. Travel to an annual PI meeting in Fairbanks
>should be included. Travel expenses need to be broken down by airfare
>and per diem. Salaries for Government PIs will not be supported.

>9. Biographical Sketch. This is limited to two pages for each Principal
>Investigator and should be focused on information directly relevant to
>undertaking the proposed research.

>10. A short list of possible peer

>reviewers with whom you have no close working or personal relationship

>(optional).
>11. Federal employees. Proposals are welcome from those
>Federal agencies whose legislated mission allows participation.
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>engineers, and strongly encourage women, minorities, and persons with
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>research-related programs described in this document. In accordance
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>shall be excluded from participation in, denied the benefits of, or be
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>-----

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>600 University Avenue, Suite 1 Fairbanks, AK 99709 907/474-1600
>907/474-1604 fax

>
>Lab. of Dendrochronology
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>Institute of Plant and Animal Ecology
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>202 Ekaterinburg, 620144, Russia
>e-mail: stepan@ipae.uran.ru
>Fax: +7 (3432) 29 41 61
>Phone: +7 (3432) 29 40 92
>
>
>
>

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Proposal to IARC
Date: Wed, 8 Sep 1999 16:44:52 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,

Thank you for answers to my questions. We decided do not participate in this project, as many problems are originated. And there is no time to write such proposal.

Last week I came back from the Polar Urals. The fieldwork was successful this summer. We remeasured all trees and seedlings along the transect, mapped forest-tundra ecosystems and tree-line over a large territory, made about 100 photos. I found very old living twigs of *Juniperus sibirica* (up to 700-800 years) and took samples from many dead twigs. We also collected many wood samples from living and dead larches of various ages. But we were bited by many thousands of mosquitos especially small ones.

Sincerely yours,

Dr. Stepan G. Shiyatov

Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

From: Trevor Davies <t.d.davies@uea.ac.uk>

To: c.flack@uea,c.bentham@uea,p.jones@uea,j.palutikof@uea,p.liss@uea, m.hulme@uea,r.k.turner@uea,a.watkinson@uea,k.brown@uea,j.darch@uea, parryml@aol.com

Subject: Discussion document for Tues/Wed

Date: Sun, 12 Sep 1999 12:21:08 +0100

Attached is a discussion document. It incorporates material provided by Simon Shackley (UMIST) & Mike Hulme. Jean has commented on it. It is intended to circulate this to consortium partners on Monday. if you have chance to read it & comment on it before it goes, that would be good; but I recognise that - in practice - time is too short. My apologies for that. (However, I do think there is a danger in presenting our partners with too 'final' a draft application at this stage. And we do need their bright ideas!).

CHRIS - please will you liaise with Jean to:

1. Get this document out to outside attendees.
2. Send out a list of attendees
3. Give outside people details of where to get the Research Councils' document 'Information for applicants to run the Centre' (web), if they don't already have it.
4. Send out an agenda (Jean is doing this)
5. Send out Kerry's diagram (Jean has)

CHRIS - will you also please fax copies of the ICER document (in your tray) to John Shepherd (Southampton 596258) and Nigel Arnell (I don't have fax number). [For info to others - we didn't send Soton a copy of the ICER bid earlier, because they were sitting on the fence].

Very many thanks.

Trevor

Attachment Converted: "c:\eudora\attach\Climate Change Centre.doc"

+++++

Professor Trevor D. Davies
Dean, School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

Tel. +44 1603 592836

Fax. +44 1603 507719

+++++From ???@??? Fri Sep 24 13:44:11 1999

Received: from [139.222.104.46] (helo=taff.cru.uea.ac.uk)

by mailserver1.uea.ac.uk with smtp (Exim 3.02 #1)

id 11UUP8-0001QM-00; Fri, 24 Sep 1999 13:24:46 +0100

Message-Id: <3.0.3.32.19990924132145.00a5ea6c@pop.uea.ac.uk>

X-Sender: e022@pop.uea.ac.uk

X-Mailer: QUALCOMM Windows Eudora Light Version 3.0.3 (32)

Date: Fri, 24 Sep 1999 13:21:45 +0100

To: n.adger@uea,j.alexander@uea,g.biggs@uea,k.briffa@uea,p.brimblecombe@uea,

s.dorling@uea,k.heywood@uea,t.jickells@uea,m.kelly@uea,b.maher@uea,

j.plane@uea,a.jordan@uea,m.penkett@uea,s.raper@uea,c.vincent@uea,

a.j.watson@uea

From: Trevor Davies <t.d.davies@uea.ac.uk>

Subject: Outline bid for new Climate Change Centre (CCC)

Cc: c.bentham@uea,p.jones@uea,j.palutikof@uea,p.liss@uea,m.hulme@uea,

r.k.turner@uea,a.watkinson@uea,k.brown@uea,j.darch@uea,parryml@aol.com

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Status:

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The research councils want a 5000 word outline bid by mid-October. The councils are putting up 2 million pounds per year for 5 years are NERC, EPSRC and ESRC. The research councils are putting the emphasis on "solutions" to climate change. They are clearly not looking for another version of CRU, the Hadley Centre, or any other existing centre in the UK. The focus is "downstream" of these existing centres.

Much of what they appear to want we anticipated in our JIF ICER (Institute for Connective Environmental Sciences) bid and, indeed, we made a provisional early strike for the CCC in that bid, although the research councils' intentions were not known at that point. Even if the JIF ICER bid is unsuccessful (& at this stage we are still optimistic), then we will still be able to take advantage of this "early" thinking in the final CCC bid.

We are aware of 3-4 competitors, which are mainly consortia of some form. Our consortium includes UMIST (number of departments), Southampton (number of departments), Cambridge (Dept of Econometrics), Sussex (Science Policy Research Unit), Cranfield (Ecotechnology Unit- Complex Systems Modelling), and Leeds (Institute for Transport Studies). There will also be a number of institutes associated with us, including Inst. Hydrology, BAS, Inst.

Terrestrial Ecology, Rutherford Appleton Laboratory, Building Research Establishment, John Innes Centre, and possibly other Institutes such as the Plymouth Marine Lab & the Proudman Oceanographic Lab. The hub of this consortium will be UEA. Visiting fellows etc will work in the Centre (& possibly also at 'secondary' centres like UMIST).

Business/industry links are important, as are links with relevant institutes abroad. We anticipate writing in some industrial/business partners.

Our philosophy is not to seek to maximise the input of resources to UEA, or to the consortium, in the short term, but to build a Centre which has the credibility and the authority to identify, initiate, orchestrate research programmes, and to include the best people available. We see this as the likeliest way to attract long-term funding & to ensure the long-term future of the CCC.

We have a fairly clear idea of the "science framework" of the CCC and, together with our partners, are now agreeing the "research challenges". At the moment the research challenges look something like this:

1. DEVELOPING THE TOOLKIT

Given that the Centre's starting point is to take advantage of the best research internationally (extant, on-going, and planned), it will be necessary to apply, refine, and develop methods of 'integration'. Much science and engineering research is focused on specific disciplinary issues. This has to be brought together with critical analyses of social and economic factors, to design more adaptive and effective policies, and more effective and appropriate engineering/technology. The best aspects of 'integrated assessments' will be applied with a UK focus. An important part of such assessments will be isolate emerging opportunities (for business/industry) afforded by climate change - in order to identify competitive opportunities it will be necessary to consider global pressure points. Existing models need to be linked. Reduced complexity modelling has a significant role.

The Toolkit can also be developed and tested via geographically-focused studies. For example, integrated coastal (incl. estuaries) management which will include: risk analysis; valuation of coastal environments; effects of adaptation (soft/hard engineering solutions) on coastline; ecological/economic models; etc.

Methods to characterise/measure vulnerability and adaptive capacity.

The Toolkit will also include some of the consultation/inclusion techniques outlined in UEA's JIF bid for ICER.

2. ABRUPT CHANGES AND EXTREMES

'Climate' research on abrupt/non-linear changes (in 'underlying' climate) and on changes in extreme event frequency (some of the Tools will need to be applied - or adapted for - this Challenge: for example, vulnerability/adaptation, risk analysis, reduced complexity modelling). Of particular importance is how the possibility of abrupt/non-linear change should be assimilated into decision-making frameworks (perception/risk analyses, etc.).

It will be necessary to consider the implications of non-climate 'shocks' - political and economic shocks; or combinations, for example, climate/weather extremes influencing perceptions (amongst business community and politicians) leading to sudden shifts of policy, investments, etc.

3 CARBON MANAGEMENT AND TECHNOLOGY

Adoption of clean technology (includes 'alternative' energy sources, and removal of C from emissions). In particular, clean technologies and solutions for developing countries link into identifying business opportunities. The impacts of clean technologies - landscape/lifestyle valuation. Incorporation (technological) into existing infrastructure/supply networks.

a. Carbon 'sequestration' - options, waste C recycling, use in building materials, long-term storage, etc. Oceans. Ambitious bio-engineering? (discussions with Norwich's John Innes Centre on-going).

b. Energy efficiency (technological), including control systems, especially when concentrated on one of the scale 'foci' (e.g. the household).

4. MANAGEMENT OF SOCIAL AND TECHNOLOGICAL CHANGE.

Factor 4 and beyond 1. We will need to go well beyond Factor 4 to stabilise the climate system. This programme would analyse and assess different emission trajectories, and look at how we would in practice achieve Factor

4+. It would include assessment of tools such as: C trading, domestic tradeable carbon quotas, regulation and taxation, voluntary agreements, opportunities for win-win scenarios through resource use minimisation, etc. Also, it would look at changes to a low-C economy at different scales: households, SMEs, large firms, MNCs; local to regional to national to global, etc., to sectoral: transport, energy supply, heavy & light manufacturing, services & finance, etc. Technology uptake. This includes reducing transport emissions and exploring low-consumption (water, energy, carbon) households. What about air traffic?

The research challenges above are not intended to be all-inclusive. We intend to use Research Challenges such as these 4, as "exemplars" of the sort of thing we will expand upon in the final bid.

The research councils have emphasised the importance of attracting a top-rate international scientist as Research Director. They also wish us to name the Executive Director at this point. We believe it should be someone with a reputation in climate research in their own right, good links etc with the "impacts" people and with funders, as well as being a good manager/organiser. We anticipate naming Mike Hulme. From what we have heard, that will give us an additional advantage over other bids.

At this point, we will welcome your comment, input, suggestions.

Trevor

++++
Professor Trevor D. Davies
Dean, School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

Tel. +44 1603 592836
Fax. +44 1603 507719

++++

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Additional material for final report and proposal
Date: Tue, 21 Sep 1999 18:22:36 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,
I am sending you an additional material which can be useful for
writing the final report and the next proposal.

Sincerely yours,

Dr. Stepan G. Shiyatov

Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92
Attachment Converted: "c:\eudora\attach\yamal-99.doc"

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>, 'Phil Jones' <p.jones@uea.ac.uk>
Subject: RE: IPCC revisions
Date: Wed, 22 Sep 1999 12:35:24 -0400
Cc: tkarl@ncdc.noaa.gov, mann@virginia.edu

Thanks for your response Keith,

For all:

Walked into this hornet's nest this morning! Keith and Phil have both raised some very good points. And I should point out that Chris, through no fault of his own, but probably through ME not conveying my thoughts very clearly to the others, definitely overstates any singular confidence I have in my own (Mann et al) series. I believe strongly that the strength in our discussion will be the fact that certain key features of past climate estimates are robust among a number of quasi-independent and truly independent estimates, each of which is not without its own limitations and potential biases. And I certainly don't want to abuse my lead authorship by advocating my own work.

I am perfectly amenable to keeping Keith's series in the plot, and can ask Ian Macadam (Chris?) to add it to the plot he has been preparing (nobody liked my own color/plotting conventions so I've given up doing this myself). The key thing is making sure the series are vertically aligned in a reasonable way. I had been using the entire 20th century, but in the case of Keith's, we need to align the first half of the 20th century w/ the corresponding mean values of the other series, due to the late 20th century decline.

So if Chris and Tom (?) are ok with this, I would be happy to add Keith's series. That having been said, it does raise a conundrum: We demonstrate (through comparing an extratropical averaging of our northern hemisphere patterns with Phil's more extratropical series) that the major discrepancies between Phil's and our series can be explained in terms of spatial sampling/latitudinal emphasis (seasonality seems to be secondary here, but probably explains much of the residual differences). But that explanation certainly can't rectify why Keith's series, which has similar seasonality *and* latitudinal emphasis to Phil's series, differs in large part in exactly the opposite direction that Phil's does from ours. This is the problem we all picked up on (everyone in the room at IPCC was in agreement that this was a problem and a potential distraction/detraction from the reasonably consensus viewpoint we'd like to show w/ the Jones et al and Mann et al series.

So, if we show Keith's series in this plot, we have to comment that

"something else" is responsible for the discrepancies in this case. Perhaps Keith can help us out a bit by explaining the processing that went into the series and the potential factors that might lead to it being "warmer" than the Jones et al and Mann et al series?? We would need to put in a few words in this regard. Otherwise, the skeptics have an field day casting doubt on our ability to understand the factors that influence these estimates and, thus, can undermine faith in the paleoestimates. I don't think that doubt is scientifically justified, and I'd hate to be the one to have to give it fodder!

The recent Crowley and Lowery multiproxy estimate is an important additional piece of information which I have indeed incorporated into the revised draft.

Tom actually estimates the same mean warming since the 17th century in his reconstruction, that we estimate in ours, so it is an added piece of information that Phil and I are probably in the ballpark (Tom has used a somewhat independent set of high and low-resolution proxy data and a very basic compositing methodology, similar to Bradley and Jones, so there is some independent new information in this estimate.

One other key result with respect to our own work is from a paper in the press in "Earth Interactions". An unofficial version is available here:

http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html

The key point we emphasize in this paper is that the low-frequency variability in our hemispheric temperature reconstruction is basically the same if we don't use any dendroclimatic indicators at all (though we certainly resolve less variance, can't get a skillful reconstruction as far back, and there are notable discrepancies at the decadal and interannual timescales). I believe I need to add a sentence to the current discussion on this point, since there is an unsubstantiated knee-jerk belief that our low-frequency variability is suppressed by the use of tree ring data.

We have shown that this is not the case: (see here:

http://www.ngdc.noaa.gov/paleo/ei/ei_datarev.html

and specifically, the plot and discussion here:

http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html

Ironically, you'll note that there is more low-frequency variability when the tree ring data *are* used, then when only other proxy and historical/instrumental data are used!

SO I think we're in the position to say/resolve somewhat more than, frankly, than Keith does, about the temperature history of the past millennium. And the issues I've spelled out all have to be dealt with in the chapter.

One last point: We will (like it or not) have SUBSTANTIAL opportunity/requirement to revise much of this discussion after review, so we don't have to resolve everything now. Just the big picture and the important details...

I'm sure we can can up with an arrangement that is amenable to all, and I'm looking forward to hearing back from Keith, Phil, and Chris in particular about the above, so we can quickly move towards finalizing a first draft.

Looking forward to hearing back w/ comments,

mike

At 04:19 PM 9/22/99 +0100, Keith Briffa wrote:

>

>Hi everyone

> Let me say that I don't mind what you put in the policy makers
>summary if there is a general concensus. However some general discussion
>would be valuable . First , like Phil , I think that the supposed
>separation of the tree-ring reconstruction from the others on the grounds
>that it is not a true "multi-proxy" series is hard to justify. What is true
>is that these particular tree-ring data best represent SUMMER temperatures
>mostly at the northern boreal forest regions. By virtue of this , they also
>definitely share significant variance with Northern Hemisphere land and
>land and marine ANNUAL temperatures - but at decadal and multidecadal
>timescales - simply by virtue of the fact that these series correlated with
>the former at these timescales. The multi proxy series (Mann et al . Jones
>et al) supposedly represent annual and summer seasons respectively, and
>both contain large proportions of tree-ring input. The latest tree-ring
>density curve (i.e. our data that have been processed to retain low
>frequency information) shows more similarity to the other two series- as do
>a number of other lower resolution data (Bradley et al, Peck et al ., and
>new Crowley series - see our recent Science piece) whether this represents
>'TRUTH' however is a difficult problem. I know Mike thinks his series is
>the 'best' and he might be right - but he may also be too dismissive of
>other data and possibly over confident in his (or should I say his use of
>other's). After all, the early (pre-instrumental) data are much less
>reliable as indicators of global temperature than is apparent in modern
>calibrations that include them and when we don't know the precise role of
>particular proxies in the earlier portions of reconstruction it remains
>problematic to assign genuine confidence limits at multidecadal and longer
>timescales. I still contend that multiple regression against the recent
>very trendy global mean series is potentially dangerous. You could
>calibrate the proxies to any number of seasons , regardless of their true
>optimum response . Not for a moment am I saying that the tree-ring , or any
>other proxy data, are better than Mike's series - indeed I am saying that
>the various reconstructions are not independent but that they likely
>contribute more information about reality together than they do alone. I do

>believe , that it should not be taken as read that Mike's series (or
>Jones et al. for that matter) is THE CORRECT ONE. I prefer a Figure that
>shows a multitude of reconstructions (e.g similar to that in my Science
>piece). Incidentally, arguing that any particular series is probably better
>on the basis of what we now about glaciers or solar output is flaky indeed.
>Glacier mass balance is driven by the difference mainly in winter
>accumulation and summer ablation , filtered in a complex non-linear way to
>give variously lagged tongue advance/retreat .Simple inference on the
>precedence of modern day snout positions does not translate easily into
>absolute (or relative) temperature levels now or in the past. Similarly, I
>don't see that we are able to substantiate the veracity of different
>temperature reconstructions through reference to Solar forcing theories
>without making assumptions on the effectiveness of (seasonally specific)
>long-term insolation changes in different parts of the globe and the
>contribution of solar forcing to the observed 20th century warming .
> There is still a potential problem with non-linear responses in the
>very recent period of some biological proxies (or perhaps a fertilisation
>through high CO2 or nitrate input) . I know there is pressure to present a
>nice tidy story as regards 'apparent unprecedented warming in a thousand
>years or more in the proxy data' but in reality the situation is not quite
>so simple. We don't have a lot of proxies that come right up to date and
>those that do (at least a significant number of tree proxies) some
>unexpected changes in response that do not match the recent warming. I do
>not think it wise that this issue be ignored in the chapter.
> For the record, I do believe that the proxy data do show unusually
>warm conditions in recent decades. I am not sure that this unusual warming
>is so clear in the summer responsive data. I believe that the recent warmth
>was probably matched about 1000 years ago. I do not believe that global
>mean annual temperatures have simply cooled progressively over thousands of
>years as Mike appears to and I contend that that there is strong evidence
>for major changes in climate over the Holocene (not Milankovich) that
>require explanation and that could represent part of the current or future
>background variability of our climate. I think the Venice meeting will be
>a good place to air these issues.
> Finally I appologise for this rather self-indulgent ramble, but I
>thought I may as well voice these points to you . I too would be happy to
>go through the recent draft of the chapter when it becomes available.

>
> cheers to all

> Keith

>
>At 01:07 PM 9/22/99 +0100, Folland, Chris wrote:

>>Dear All

>>
>>A proxy diagram of temperature change is a clear favourite for the Policy
>>Makers summary. But the current diagram with the tree ring only data
>>somewhat contradicts the multiproxy curve and dilutes the message rather
>>significantly. We want the truth. Mike thinks it lies nearer his result
>>(which seems in accord with what we know about worldwide mountain glaciers

>>and, less clearly, suspect about solar variations). The tree ring results
>>may still suffer from lack of multicentury time scale variance. This is
>>probably the most important issue to resolve in Chapter 2 at present.

>>
>>Chris

>>> -----Original Message-----
>>> From: Phil Jones [SMTP:p.jones@uea.ac.uk]
>>> Sent: 22 September 1999 12:58
>>> To: Michael E. Mann; k.briffa@uea.ac.uk
>>> Cc: ckfolland@meto.gov.uk; tkarl@ncdc.noaa.gov
>>> Subject: Re: IPCC revisions

>>>
>>>
>>> Mike,
>>> Been away in Japan the last week or so. Malcolm was there in a
>>> wheelchair
>>> because of his ruptured achilles. We both mentioned the lack of evidence
>>> for global scale change related to the MWE and LIA, but all the later
>>> Japanese speakers kept saying the same old things.

>>>
>>> As for the TAR Chap 2 it seems somewhat arbitrary divison to exclude
>>> the
>>> tree-ring only reconstructions. Keith's reconstruction is of a different
>>> character to other tree-ring work as it is as 'hemispheric in scale' as
>>> possible so is unlike any other tree-ring related work that is reported
>>> upon.
>>> If we go as is suggested then there would be two diagrams - one simpler
>>> one with just Mann et al and Jones et al and in another section Briffa et
>>> al. This might make it somewhat awkward for the reader trying to put them
>>> into context.

>>> The most important bit of the proxy section is the general discussion
>>> of
>>> 'Was there an MWE and a LIA' drawing all the strands together. Keith and
>>> I
>>> would be happy to look through any revisions of the section if there is
>>> time.

>>>
>>> One other thing, did you bring up the possibility of having a
>>> proxy-only
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>>> into dates soon for coming to see you.

>>>
>>> Cheers
>>> Phil

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>>> Prof. Phil Jones
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>>> School of Environmental Sciences Fax +44 (0) 1603 507784
>>> University of East Anglia
>>> Norwich Email p.jones@uea.ac.uk
>>> NR4 7TJ
>>> UK

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>--

>Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
>Norwich, NR4 7TJ, United Kingdom
>Phone: +44-1603-592090 Fax: +44-1603-507784

>

>

>

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<http://www.evsc.virginia.edu/faculty/people/mann.html>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>,k.briffa@uea.ac.uk
Subject: Re: IPCC revisions
Date: Wed, 22 Sep 1999 12:58:14 +0100
Cc: ckfolland@meto.gov.uk,tkarl@ncdc.noaa.gov

Mike,

Been away in Japan the last week or so. Malcolm was there in a wheelchair because of his ruptured achilles. We both mentioned the lack of evidence for global scale change related to the MWE and LIA, but all the later Japanese speakers kept saying the same old things.

As for the TAR Chap 2 it seems somewhat arbitrary divison to exclude the tree-ring only reconstructions. Keith's reconstruction is of a different character to other tree-ring work as it is as 'hemispheric in scale' as possible so is unlike any other tree-ring related work that is reported upon.

If we go as is suggested then there would be two diagrams - one simpler one with just Mann et al and Jones et al and in another section Briffa et al. This might make it somewhat awkward for the reader trying to put them into context.

The most important bit of the proxy section is the general discussion of 'Was there an MWE and a LIA' drawing all the strands together. Keith and I would be happy to look through any revisions of the section if there is time.

One other thing, did you bring up the possibility of having a proxy-only chapter (albeit short) for the next assessment ?

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Folland, Chris" <ckfolland@meto.gov.uk>, 'Phil Jones'
<p.jones@uea.ac.uk>, "Michael E. Mann"
<mann@multiproxy.evsc.virginia.edu>
Subject: RE: IPCC revisions
Date: Wed Sep 22 16:19:06 1999
Cc: tkarl@ncdc.noaa.gov

Hi everyone

Let me say that I don't mind what you put in the policy makers summary if there is a general concensus. However some general discussion would be valuable . First , like Phil , I think that the supposed separation of the tree-ring reconstruction from the others on the grounds that it is not a true "multi-proxy" series is hard to justify. What is true is that these particular tree-ring data best represent SUMMER temperatures mostly at the northern boreal forest regions. By virtue of this , they also definately share significant variance with Northern Hemisphere land and land and marine ANNUAL temperatures - but at decadal and multidecadal timescales - simply by virtue of the fact that these series correlated with the former at these timescales. The multi proxy series (Mann et al . Jones et al) supposedly represent annual and summer seasons respectively, and both contain large proportions of tree-ring input. The latest tree-ring density curve (i.e. our data that have been processed to retain low frequency information) shows more similarity to the other two series- as do a number of other lower resolution data (Bradley et al, Peck et al ., and new Crowley series - see our recent Science piece) whether this represents 'TRUTH' however is a difficult problem. I know Mike thinks his series is the 'best' and he might be right - but he may also be too dismissive of other data and possibly over confident in his (or should I say his use of other's). After all, the early (pre-instrumental) data are much less reliable as indicators of global temperature than is apparent in modern calibrations that include them and when we don't know the precise role of particular proxies in the earlier portions of reconstruction it remains problematic to assign genuine confidence limits at multidecadal and longer timescales. I still contend that multiple regression against the recent very trendy global mean series is potentially dangerous. You could calibrate the proxies to any number of seasons , regardless of their true optimum response . Not for a moment am I saying that the tree-ring , or any other proxy data, are better than Mike's series - indeed I am saying that the various reconstructions are not independent but that they likely contribute more information about reality together than they do alone. I do believe , that it should not be taken as read that Mike's series (or Jone's et al. for that matter) is THE CORRECT ONE. I prefer a Figure that shows a multitude of reconstructions (e.g similar to that in my Science piece). Incidentally, arguing that any particular series is probably better on the basis of what we now about glaciers or solar output is flaky indeed. Glacier mass balance is driven by the difference mainly in winter accumulation and summer ablation , filtered in a complex non-linear way to give variously lagged tongue advance/retreat .Simple inference on the precidence of modern day snout positions does not translate easily into absolute (or relative) temperature levels now or in the past. Similarly, I don't see that we are able to substantiate the veracity of different

temperature reconstructions through reference to Solar forcing theories without making assumptions on the effectiveness of (seasonally specific) long-term insolation changes in different parts of the globe and the contribution of solar forcing to the observed 20th century warming .

There is still a potential problem with non-linear responses in the very recent period of some biological proxies (or perhaps a fertilisation through high CO2 or nitrate input) . I know there is pressure to present a nice tidy story as regards 'apparent unprecedented warming in a thousand years or more in the proxy data' but in reality the situation is not quite so simple. We don't have a lot of proxies that come right up to date and those that do (at least a significant number of tree proxies) some unexpected changes in response that do not match the recent warming. I do not think it wise that this issue be ignored in the chapter.

For the record, I do believe that the proxy data do show unusually warm conditions in recent decades. I am not sure that this unusual warming is so clear in the summer responsive data. I believe that the recent warmth was probably matched about 1000 years ago. I do not believe that global mean annual temperatures have simply cooled progressively over thousands of years as Mike appears to and I contend that that there is strong evidence for major changes in climate over the Holocene (not Milankovich) that require explanation and that could represent part of the current or future background variability of our climate. I think the Venice meeting will be a good place to air these issues.

Finally I apologise for this rather self-indulgent ramble, but I thought I may as well voice these points to you . I too would be happy to go through the recent draft of the chapter when it becomes available.

cheers to all
Keith

At 01:07 PM 9/22/99 +0100, Folland, Chris wrote:

>Dear All

>

>A proxy diagram of temperature change is a clear favourite for the Policy

>Makers summary. But the current diagram with the tree ring only data

>somewhat contradicts the multiproxy curve and dilutes the message rather

>significantly. We want the truth. Mike thinks it lies nearer his result

>(which seems in accord with what we know about worldwide mountain glaciers

>and, less clearly, suspect about solar variations). The tree ring results

>may still suffer from lack of multicentury time scale variance. This is

>probably the most important issue to resolve in Chapter 2 at present.

>

>Chris

>

>> -----Original Message-----

>> From: Phil Jones [SMTP:p.jones@uea.ac.uk]

>> Sent: 22 September 1999 12:58

>> To: Michael E. Mann; k.briffa@uea.ac.uk

>> Cc: ckfolland@meto.gov.uk; tkarl@ncdc.noaa.gov

>> Subject: Re: IPCC revisions

>>
>>
>> Mike,
>> Been away in Japan the last week or so. Malcolm was there in a
>> wheelchair
>> because of his ruptured achilles. We both mentioned the lack of
evidence
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>> Japanese speakers kept saying the same old things.
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>> Cheers
>> Phil
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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>, Keith Briffa <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>
Subject: RE: IPCC revisions
Date: Thu, 23 Sep 1999 13:34:14 -0400
Cc: tkarl@ncdc.noaa.gov, mann@virginia.edu

Thanks for your comments Phil,

They look quite reasonable, and I will seek to incorporate them. I'll need Keith's comments by tomorrow morning (my time) at the very latest if I am to have time to assess them and incorporate them.

Some important specifics:

1) I am definitely using the version of the Briffa et al series you sent in which Keith had restandardized to retain **more** low-frequency variability relative to the one shown by Briffa et al (1998). So already, the reconstruction I'm using is one-step removed from the published series (as far as I know!) and that makes our use of even this series a bit tenuous in my mind, but I'm happy to do it and let the reviewers tell us if they see any problem. If I understand you correctly, there is yet a new version of this series that is two steps removed from Briffa et al (1998)? Frankly, at this stage I think we have to go w/ what we have (please see Ian Macadam's plot when it is available--I think the story it tells isn't all that bad, actually) for the time being. Things as you say will change following review anyways.

2) One other thingp--I'm actually averse to shortening the section on sediments. Even if they haven't contributed to some of the multiproxy studies (they certainly **did** contribute to Overpeck et al!) there are some important results there irrespective of the role of the proxies in multiproxy studies. Lets, again, wait for reviews before shortening this...

3) We could eliminate the map of the boreholes, although I actually think it is essential to see what the contributing spatial sampling (and, accordingly, the potential bias of that sampling in determining "global mean temperature") actually is. So I vote for keeping it for the time being. Again, it's an extremity that we can afford to lose if necessary in the end..

4) One important note on references: We don't have time at this late stage to dig up incomplete citations, so you'll need to give me full citations for any suggested added references (e.g. the Villalba paper). FYI, the Crowley and Lowery paper is Tom's Ambio paper. He observes a mean warming of about 0.5 C since the 17th century giving us yet another datapoint in the scatter of estimates...

5) I agree, the ranking of centuries is more specific than it needs to be. I don't know what I was thinking. You sure that didn't come from the text you originally contributed?? In any case, we can eliminate much of it in my opinion too...

On the whole, I have never been under the assumption that you and I would have independently assessed the evidence quite the same way. I would hope we would have come up w/ the same key points, and so your comments in that regard are reassuring. I feel confident in my ability to defend the science that is presented here, so let the reviews fall where they may. I'm sure we will be forced to admit some changes, as well as "minority viewpoints" and alternative interpretations along the way. That's what will make this all interesting...

mike

At 05:20 PM 9/23/99 +0100, Phil Jones wrote:

>
> Mike,
> Here are my thoughts on the text you sent. Keith will be sending some
> as well hopefully later today. One important aspect Keith will address is
> whether you're using the latest Briffa et al curve. We know you're not but
> the
> one with the greater low frequency and therefore much better chance of
> looking much better with the other two series, isn't yet published. We know
> it looks better in plots we have here.
>
> Specifics :
>
> p1 line 10 - say mid-19th century rather than the 20th century
>
> lines 18-20 - seems a bit too much here with three refs on laminated
> sediments.

>
> line 46 Add Briffa et al (1998b) to Cook(1995).
>
> p2 line 59 - I would suggest changing 'a particularly' to 'the most' .
>
> line 64 - I would add a reference here to the paper by Crowley and
> Kim (1999) in GRL (July) where this aspect is also discussed.
>
> p3 line 101 - I would add Argentina as well as Chile adding a ref to
> Villalba (1990) in QR.
>
> line 108 change 'key' to 'vital'
>
> line 119 'have providing' to 'provide' . There are several instances
> where the text doesn't read that well. I suspect as there are several
> iterations to go it is not that important yet !
>
> The coral section is just about the right size now and is justly
> devoid of references !
>
> p4 line 151 I would add a reference here to Morgan and van Ommen (1997)
> 'Seasonality in late-Holocene climate from ice core records',
> The Holocene 7, 351-4. This is the Law Dome core which is the best
> available with regards to dating in either hemisphere. It should be
> there.
>
> As with the coral section the ice core section expresses some
> cautionary notes with regard to dating etc which I think are justified.
> I suspect teh contrast with the tree-ring section will draw some
> criticism. Just a warning !
>
> As none of the multiproxy reconstructions use any sediment information
> this section seems overlarge and could be reduced.
>
> p189 century-scale add in the 'y'
>
> p5 The borehole section is also a bit overlong. I don't know whether the
> map really adds something. Not that vehement on this.
>
> With respect to comapring high and low frequency aspects the diagram
> comparing CET with the UK boreholes is now out. I've sent a copy to
> Chris. It is in :

- >
- > Jones PD, 1999 : Classics in physical geography revisited - Manley's
- > CET series. Progress in Physical Geography 23, 425-428.
- >
- > line 245 the 'is' is not needed.
- >
- > p6 I still think that a reference to Raper et al (1996) would be good
- > here. This models a glacier in northern Sweden using the northern
- > Fennoscandian temperature reconstructions since AD 500. Again it shows
- > how a low frequency estimate (the glacial snout position) can be compared
- > with a high-frequency temperature reconstruction from trees.
- >
- > Raper, SCB, Briffa KR and Wigley TML, 1996: Glacial change in northern
- > Sweden from AD 500: a simple geometric model of Storglaciaren. Journal
- > of Glaciology 42, 341-351.
- >
- > line 268 IPCC(1996) earlier - is it 95 or 96
- >
- > p 7 line 295 I would like to add my paper in Reviews of Geophysics in 1999
- > as that also says that 1998 was likely to be the warmest year of the
- > millennium.
- >
- > line 334 I would like to see Bradley (1999). I must get a copy from
- > Ray in Venice.
- >
- > p7-9 All need a careful read through for English and the arguments.
- >
- > At the bottom of p8 I think you make too much of the differences in the
- > ranking of the centuries. The boreholes would agree with my series with
- > the 17th being colder than the 19th, although they may not be able to
- > resolve the timescales then.
- >
- > Is the Crowley and Lowery (1999) the paper Tom's submitted to Ambio ?
- >
- > I've not commented much on this final section as again I suspect there
- > are many things you will have to justify in the next two sets of reviews.
- > On the whole I think most is OK and I support the final paragraph. I
- > don't believe the astronomical argument as an explanation over the
- > last 1000 years but we can differ on that.
- >
- > I know I would have written this final section 2.3.3 somewhat differently
- > with different emphases and slants but the basic final conclusion would

> have been the same.

>

> Cheers

> Phil

>

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>, 'Phil Jones' <p.jones@uea.ac.uk>
Subject: RE: IPCC revisions
Date: Thu, 23 Sep 1999 13:47:22 -0400
Cc: tkarl@ncdc.noaa.gov, mann@virginia.edu

Thanks alot Keith,

Your comments and suggestions sound good on all counts.

Clearly there is one overiding thing to make sure of here: that we have the right version of your series. I **think** that we do, and you might have been looking at an old version of the comparison Figure??

Please check out the data here ASAP:

<ftp://eclogite.geo.umass.edu/pub/mann/IPCC/MILLENNIUM/>

This directory has all the series, aligned as I described to have a 1961-90 base climatology (or in the case of your series, a pseudo 1961-90 base climatology achieved by actually matching the mean of your series and the instrumental record over the interval 1931-60 interval). These are the data that Ian Macadam is hopefully presently plotting up, and I don't think the discrepancies between the different series are as bad as we percieved earlier (other than the late 19th century where you are somewhat on the warm side relative to the rest). Please confirm ASAP that we have the right version of the series (note, these have all been 40 year lowpassed)...

One other thing, I think you misinterpreted my statement:

>
>SO I think we're in the position to say/resolve somewhat more than, frankly,
>than Keith does, about the temperature history of the past millennium.
>And the issues I've spelled out all have to be dealt with in the chapter.
>

I wasn't talking about the comparison of our two series! I was talking about our two different opinions on how confident we are about our ability, as a community, to assess the actual climate changes over this timeframe. And perhaps we're closer here than I assumed anyways. Sorry about the misunderstanding. With your interpretation, my comment must I have sounded really obnoxious!

At 06:29 PM 9/23/99 +0100, Keith Briffa wrote:

>

>Dear Mike (and all)

>

>Some remarks in response to your recent message

>

>I believe strongly that the strength in our discussion

>>will be the fact that certain key features of past climate estimates are

>>robust among a number of quasi-independent and truly independent estimates,

>>each

>>of which is not without its own limitations and potential biases

>

>Mike , I agree very much with the above sentiment. My concern was motivated

>by the possibility of expressing an impression of more consensus than might

>actually exist . I suppose the earlier talk implying that we should not

>'muddy the waters' by including contradictory evidence worried me . IPCC is

>supposed to represent consensus but also areas of uncertainty in the

>evidence. Of course where there are good reasons for the differences in

>series (such as different seasonal responses or geographic bias) it is

>equally important not to overstress the discrepancies or suggest

>contradiction where it does not exist.

>

>

> And I

>>certainly don't want to abuse my lead authorship by advocating my own work.

>>

>

>I sincerely hope this was not implied in anything I wrote - It was not

>intended

>

>>I am perfectly amenable to keeping Keith's series in the plot, and can ask

>>Ian Macadam (Chris?) to add it to the plot he has been preparing (nobody

>>liked my own color/plotting conventions so I've given up doing this myself).

>>The key thing is making sure the series are vertically aligned in a

>reasonable

>>way. I had been using the entire 20th century, but in the case of Keith's,

>>we need to align the first half of the 20th century w/ the corresponding

mean

>>values of the other series, due to the late 20th century decline.

>>

>

>Again I agree. Also , I am not sure which version of the curve you are now

>referring to. The original draft did show our higher frequency curve i.e.

>the version with background changes effectively filtered out (intended to

>emphasise the extreme interannual density excursions and their coincidence

>with volcanic eruptions) . The relevant one here is a smoothed version in

>which low-frequency changes are preserved. I can supply this and it will be

>in press by the time of the next reworking of the text.

>

>Your above point on correct scaling is relevant also to Phil's curve which

>was not originally calibrated (in a formal regression sense) with the
>summer temperature data - it was just given the same mean and standard
>deviation over a specific period. Hence the issue of equivalent scaling of
>all series is vital if we are to discuss specific period temperature
>anomalies in different series or compare temperature trends in absolute
>degrees.

>
>>So if Chris and Tom (?) are ok with this, I would be happy to add Keith's
>>series. That having been said, it does raise a conundrum: We demonstrate
>>(through comparing an extratropical averaging of our northern hemisphere
>>patterns with Phil's more extratropical series) that the major
>>discrepancies between Phil's and our series can be explained in terms of
>>spatial sampling/latitudinal emphasis (seasonality seems to be secondary
>>here, but probably explains much of the residual differences). But that
>>explanation certainly can't rectify why Keith's series, which has similar
>>seasonality
>>*and* latitudinal emphasis to Phil's series, differs in large part in
>>exactly the opposite direction that Phil's does from ours. This is the
>>problem we
>>all picked up on (everyone in the room at IPCC was in agreement that this
>>was a problem and a potential distraction/detraction from the reasonably
>>consensus viewpoint we'd like to show w/ the Jones et al and Mann et al
>>series.

>>

>

>I am not sure this is true if the relevant series of ours is used. We need
>to reexamine the curves and perhaps look at the different regional and
>seasonal data in the instrumental record and over common regions in the
>different reconstructed series. We would be happy to work with you on this.
>Also remember that our (density) series does not claim hemispheric or
>annual coverage.

>

>

>>So, if we show Keith's series in this plot, we have to comment that
>>"something else" is responsible for the discrepancies in this case. Perhaps
>>Keith can
>>help us out a bit by explaining the processing that went into the series
>>and the potential factors that might lead to it being "warmer" than the
Jones
>>et al and Mann et al series?? We would need to put in a few words in this
>>regard. Otherwise, the skeptics have an field day casting
>>doubt on our ability to understand the factors that influence these
>estimates
>>and, thus, can undermine faith in the paleoestimates.

>

>The best approach here is for us to circulate a paper addressing all the
>above points. I'll do this as soon as possible.

>

> I don't think that

>>doubt is scientifically justified, and I'd hate to be the one to have
>>to give it fodder!

>>
>>

>>The recent Crowley and Lowery multiproxy estimate is an important
>>additional piece of information which I have indeed incorporated into the
>>revised draft.

>>Tom actually estimates the same mean warming since the 17th century in his
>>reconstruction, that we estimate in ours, so it is an added piece of
>>information that Phil and I are probably in the ballpark (Tom has used
>>a somewhat independent set of high and low-resolution proxy data and a very
>>basic compositing methodology, similar to Bradley and Jones, so there is
>>some independent new information in this estimate.

>>
>

>fair enough - but I repeat that the magnitude of the observed warming in
>the 20th century is different in summer and annual data

>
>

>>One other key result with respect to our own work is from a paper in the
>>press in "Earth Interactions". An unofficial version is available here:

>>
>>

>>http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html

>>

>>The key point we emphasize in this paper is that the low-frequency
>>variability in our hemispheric temperature reconstruction is basically the
>>same if we don't use any dendroclimatic indicators at all (though we
>>certainly resolve less variance, can't get a skillful reconstruction as far
>>back, and there are notable discrepancies at the decadal and interannual
>>timescales). I believe I need to add a sentence to the current discussion
>>on this point,

>>since there is an unsubstantiated knee-jerk belief that our low-frequency
>>variability is suppressed by the use of tree ring data.

>>

>>We have shown that this is not the case: (see here:

>>http://www.ngdc.noaa.gov/paleo/ei/ei_datarev.html

>>and specifically, the plot and discussion here:

>>http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html

>>Ironically, you'll note that there is more low-frequency variability when
>>the tree ring data *are* used, then when only other proxy and
>>historical/instrumental data are used!

>>
>
>

>This is certainly relevant and sounds really interesting. I need to look at
>this in detail. The effect of the including tree-ring data or not, is
>moderated by the importance of the particular series in the various
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>prospect of affecting (reducing) the apparent magnitude of the 20th century

>warming by loading on high-pass filtered chronologies , but equally a
>danger of exaggerating it if the series used or emphasised in th calibration
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>and I) would love to collaborate with you on exploring this issue (and the
>role of instrumental predictors) in the various approaches.
>The key here is knowing much more about the role of specific predictors
>through time and their associated strengths and weaknesses.

>
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>>SO I think we're in the position to say/resolve somewhat more than, frankly,
>>than Keith does, about the temperature history of the past millennium.
>>And the issues I've spelled out all have to be dealt with in the chapter.

>>

>

>I certainly do not disagree with you - the scale of your input data
>undoubtedly must contain more information than our set . I have never
>implied anything to the contrary. I do not believe that our data are likely
>to tell us more than summer variability at northern latitudes . The
>discussion is only about how close our and your data likely represent what
>they are calibrated against , back in time. Let's not imagine a
>disagreement where there is none.

>
>
>

>>One last point: We will (like it or not) have SUBSTANTIAL
>>opportunity/requirement to revise much of this discussion after review, so
>>we don't have to resolve everything now. Just the big picture and the
>>important details...

>>

>>I'm sure we can can up with an arrangement that is amenable to all, and I'm
>>looking forward to hearing back from Keith, Phil, and Chris in particular
>>about the above, so we can quickly move towards finalizing a first draft.

>>
>>

>

>Yes indeed. The reviewing will lead to much comment and likely disagreement
>by the masses. This is the way of these things. It is always a thankless
>task undertaking these drafting jobs and I think you are doing a good job.
>Tommorrow I'll send some very minor comments on typos and the like if you
>>want them - or have you picked many of them up? Anyway , keep up the good
>work .

>

> best wishes
> Keith

>

>--

>Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
>Norwich, NR4 7TJ, United Kingdom
>Phone: +44-1603-592090 Fax: +44-1603-507784

>
>
>

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<http://www.evsc.virginia.edu/faculty/people/mann.html>

From: Phil Jones <p.jones@uea.ac.uk>

To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, "Folland, Chris" <ckfolland@meto.gov.uk>, Keith Briffa <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>

Subject: RE: IPCC revisions

Date: Thu, 23 Sep 1999 17:20:56 +0100

Cc: tkarl@ncdc.noaa.gov

Mike,

Here are my thoughts on the text you sent. Keith will be sending some as well hopefully later today. One important aspect Keith will address is whether you're using the latest Briffa et al curve. We know you're not but the one with the greater low frequency and therefore much better chance of looking much better with the other two series, isn't yet published. We know it looks better in plots we have here.

Specifics :

p1 line 10 - say mid-19th century rather than the 20th century

lines 18-20 - seems a bit too much here with three refs on laminated sediments.

line 46 Add Briffa et al (1998b) to Cook(1995).

p2 line 59 - I would suggest changing 'a particularly' to 'the most' .

line 64 - I would add a reference here to the paper by Crowley and Kim (1999) in GRL (July) where this aspect is also discussed.

p3 line 101 - I would add Argentina as well as Chile adding a ref to Villalba (1990) in QR.

line 108 change 'key' to 'vital'

line 119 'have providing' to 'provide' . There are several instances where the text doesn't read that well. I suspect as there are several iterations to go it is not that important yet !

The coral section is just about the right size now and is justly devoid of references !

p4 line 151 I would add a reference here to Morgan and van Ommen (1997) 'Seasonality in late-Holocene climate from ice core records', The Holocene 7, 351-4. This is the Law Dome core which is the best available with regards to dating in either hemisphere. It should be there.

As with the coral section the ice core section expresses some cautionary notes with regard to dating etc which I think are justified. I suspect the contrast with the tree-ring section will draw some criticism. Just a warning !

As none of the multiproxy reconstructions use any sediment information this section seems overlarge and could be reduced.

p189 century-scale add in the 'y'

p5 The borehole section is also a bit overlong. I don't know whether the map really adds something. Not that vehement on this.

With respect to comparing high and low frequency aspects the diagram comparing CET with the UK boreholes is now out. I've sent a copy to Chris. It is in :

Jones PD, 1999 : Classics in physical geography revisited - Manley's CET series. Progress in Physical Geography 23, 425-428.

line 245 the 'is' is not needed.

p6 I still think that a reference to Raper et al (1996) would be good here. This models a glacier in northern Sweden using the northern Fennoscandian temperature reconstructions since AD 500. Again it shows how a low frequency estimate (the glacial snout position) can be compared with a high-frequency temperature reconstruction from trees.

Raper, SCB, Briffa KR and Wigley TML, 1996: Glacial change in northern Sweden from AD 500: a simple geometric model of Storglaciaren. Journal of Glaciology 42, 341-351.

line 268 IPCC(1996) earlier - is it 95 or 96

p 7 line 295 I would like to add my paper in Reviews of Geophysics in 1999 as that also says that 1998 was likely to be the warmest year of the millennium.

line 334 I would like to see Bradley (1999). I must get a copy from Ray in Venice.

p7-9 All need a careful read through for English and the arguments.

At the bottom of p8 I think you make too much of the differences in the ranking of the centuries. The boreholes would agree with my series with the 17th being colder than the 19th, although they may not be able to resolve the timescales then.

Is the Crowley and Lowery (1999) the paper Tom's submitted to Ambio ?

I've not commented much on this final section as again I suspect there are many things you will have to justify in the next two sets of reviews. On the whole I think most is OK and I support the final paragraph. I don't believe the astronomical argument as an explanation over the last 1000 years but we can differ on that.

I know I would have written this final section 2.3.3 somewhat differently with different emphases and slants but the basic final conclusion would have been the same.

Cheers
Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, "Folland,
Chris" <ckfolland@meto.gov.uk>, 'Phil Jones' <p.jones@uea.ac.uk>
Subject: RE: IPCC revisions
Date: Thu Sep 23 18:29:05 1999
Cc: tkarl@ncdc.noaa.gov, mann@virginia.edu

Dear Mike (and all)

Some remarks in response to your recent message

I believe strongly that the strength in our discussion
>will be the fact that certain key features of past climate estimates are
>robust among a number of quasi-independent and truly independent
estimates,
>each
>of which is not without its own limitations and potential biases

Mike , I agree very much with the above sentiment. My concern was
motivated by the possibility of expressing an impression of more
consensus than might actually exist . I suppose the earlier talk implying
that we should not 'muddy the waters' by including contradictory evidence
worried me . IPCC is supposed to represent consensus but also areas of
uncertainty in the evidence. Of course where there are good reasons for
the differences in series (such as different seasonal responses or
geographic bias) it is equally important not to overstress the
discrepancies or suggest contradiction where it does not exist.

And I
>certainly don't want to abuse my lead authorship by advocating my own
work.
>

I sincerely hope this was not implied in anything I wrote - It was not
intended

>I am perfectly amenable to keeping Keith's series in the plot, and can
ask
>Ian Macadam (Chris?) to add it to the plot he has been preparing
(nobody
>liked my own color/plotting conventions so I've given up doing this
myself).
>The key thing is making sure the series are vertically aligned in a
reasonable
>way. I had been using the entire 20th century, but in the case of
Keith's,
>we need to align the first half of the 20th century w/ the corresponding
mean
>values of the other series, due to the late 20th century decline.
>

Again I agree. Also , I am not sure which version of the curve you are now referring to. The original draft did show our higher frequency curve i.e. the version with background changes effectively filtered out (intended to emphasise the extreme interannual density excursions and their coincidence with volcanic eruptions) . The relevant one here is a smoothed version in which low-frequency changes are preserved. I can supply this and it will be in press by the time of the next reworking of the text.

Your above point on correct scaling is relevant also to Phil's curve which was not originally calibrated (in a formal regression sense) with the summer temperature data - it was just given the same mean and standard deviation over a specific period. Hence the issue of equivalent scaling of all series is vital if we are to discuss specific period temperature anomalies in different series or compare temperature trends in absolute degrees.

>So if Chris and Tom (?) are ok with this, I would be happy to add Keith's series. That having been said, it does raise a conundrum: We demonstrate (through comparing an extratropical averaging of our northern hemisphere patterns with Phil's more extratropical series) that the major discrepancies between Phil's and our series can be explained in terms of spatial sampling/latitudinal emphasis (seasonality seems to be secondary here, but probably explains much of the residual differences). But that explanation certainly can't rectify why Keith's series, which has similar seasonality and latitudinal emphasis to Phil's series, differs in large part in exactly the opposite direction that Phil's does from ours. This is the problem we all picked up on (everyone in the room at IPCC was in agreement that this was a problem and a potential distraction/detraction from the reasonably consensus viewpoint we'd like to show w/ the Jones et al and Mann et al series.

>

I am not sure this is true if the relevant series of ours is used. We need to reexamine the curves and perhaps look at the different regional and seasonal data in the instrumental record and over common regions in the different reconstructed series. We would be happy to work with you on this. Also remember that our (density)series does not claim hemispheric or annual coverage.

>So, if we show Keith's series in this plot, we have to comment that >"something else" is responsible for the discrepancies in this case. Perhaps Keith can help us out a bit by explaining the processing that went into the series and the potential factors that might lead to it being "warmer" than the Jones

>et al and Mann et al series?? We would need to put in a few words in
>this
>regard. Otherwise, the skeptics have an field day casting
>doubt on our ability to understand the factors that influence these
>estimates
>and, thus, can undermine faith in the paleoestimates.

The best approach here is for us to circulate a paper addressing all the
above points. I'll do this as soon as possible.

I don't think that
>doubt is scientifically justified, and I'd hate to be the one to have
>to give it fodder!
>
>
>The recent Crowley and Lowery multiproxy estimate is an important
>additional piece of information which I have indeed incorporated into
>the
>revised draft.
>Tom actually estimates the same mean warming since the 17th century in
>his
>reconstruction, that we estimate in ours, so it is an added piece of
>information that Phil and I are probably in the ballpark (Tom has used
>a somewhat independent set of high and low-resolution proxy data and a
>very
>basic compositing methodology, similar to Bradley and Jones, so there is
>some independent new information in this estimate.
>

fair enough - but I repeat that the magnitude of the observed warming in
the 20th century is different in summer and annual data

>One other key result with respect to our own work is from a paper in the
>press in "Earth Interactions". An unofficial version is available here:
>
>http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html
>
>The key point we emphasize in this paper is that the low-frequency
>variability in our hemispheric temperature reconstruction is basically
>the
>same if we don't use any dendroclimatic indicators at all (though we
>certainly resolve less variance, can't get a skillful reconstruction as
>far
>back, and there are notable discrepancies at the decadal and interannual
>timescales). I believe I need to add a sentence to the current
>discussion
>on this point,
>since there is an unsubstantiated knee-jerk belief that our low-
>frequency
>variability is suppressed by the use of tree ring data.
>
>We have shown that this is not the case: (see here:
>http://www.ngdc.noaa.gov/paleo/ei/ei_datarev.html

>and specifically, the plot and discussion here:
>http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html
>Ironically, you'll note that there is more low-frequency variability
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>the tree ring data *are* used, then when only other proxy and
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This is certainly relevant and sounds really interesting. I need to look at this in detail. The effect of the including tree-ring data or not, is moderated by the importance of the particular series in the various reconstructions (relative coefficient magnitudes). There is certainly some prospect of affecting (reducing) the apparent magnitude of the 20th century warming by loading on high-pass filtered chronologies , but equally a danger of exaggerating it if the series used or emphasised in th calibration have been fertilized by CO2 or something else. As you know we (Tim, Phil and I) would love to collaborate with you on exploring this issue (and the role of instrumental predictors) in the various approaches.

The key here is knowing much more about the role of specific predictors through time and their associated strengths and weaknesses.

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>than Keith does, about the temperature history of the past millennium.
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I certainly do not disagree with you - the scale of your input data undoubtedly must contain more information than our set . I have never implied anything to the contrary. I do not believe that our data are likely to tell us more than summer variability at northern latitudes . The discussion is only about how close our and your data likely represent what they are calibrated against , back in time. Let's not imagine a disagreement where there is none.

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Yes indeed. The reviewing will lead to much comment and likely disagreement by the masses. This is the way of these things. It is always a thankless task undertaking these drafting jobs and I think you are doing a good job. Tommorrow I'll send some very minor comments on typos and the like if you want them - or have you picked many of them up? Anyway , keep up the good work .

best wishes

Keith

From: Jim Fairchild-Parks <jparks@LTRR.ARIZONA.EDU>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: crossdating difficult tree-ring series
Date: Thu, 30 Sep 1999 13:21:13 -0700
Reply-to: grissino@VALDOSTA.EDU

Forumites,

Ouch, my hackles are rising so high, it hurts. (Just what exactly are hackles, anyway?).

Yes, computer crossdating ring series with special problems is always dangerous. But this is where good old skeleton-plot dating with intensive and thorough visual examination of the WOOD becomes the way to go.

I don't know about Thuja, but with the Juniperus species in the U.S. I've worked with, rings piching in and out can be a problem. You can lose 50-100 rings that way, sometimes. However, a different radius of the sample may possess all those absent rings. It's nice to have a cross-section of the subject tree, though I know this isn't always possible.

I don't understand physiologically what's going on with the Canadian cedars, but dendrochronologically speaking, absent rings are absent rings, no matter what the reason for the rings not forming on any given portion of the tree. I'll leave the reasons to scientists like Frank Telewski.

I do know that with some dying trees -- like the pinyons from New Mexico that died in the Great 1950s Drought -- the ring series on the outside became so suppressed that individual rings were indiscernable. Fortunately, other trees growing in more favorable spots had distinguishable -- though still suppressed -- rings. Traditional skeleton-plot croosdating -- along with its concomitant intensive visual analysis -- made it possible to sort though these problems.

I am not, however, an America-centrist skeleton-plot-dating bigot! I have a true appreciation for computer crossdating where it is appropriate and indeed necessary. I myself was recently involved dating high-elevation bristlecone pine from northern Arizona, U.S.A.

The multi-millennial length of the chronology -- as well as the freedom from absent rings and the presence of frost-year marker rings -- made computer crossdating advisable. Of course every significant computer dating correlation was thoroughly checked out on the WOOD, and if the visual characteristics of the tree rings themselves did not support the computer dating, we threw out the date -- right out the window. Discarded computer dates collected on the parking lot beneath our offices and needed to be hauled off to the dump everyday.

I apologize for the aggressive (though sincere) tone of this message, but every few years I feel the need to rant and rave about the importance of WOOD and "pure" forms of crossdating.

Best Regards,

Jim Parks
Laboratory of Tree-Ring Research
jparks@ltrr.arizona.edu

From: "Mike Hulme" <m.hulme@uea.ac.uk>
To: <t.d.davies@uea.ac.uk>, <c.bentham@uea.ac.uk>, <p.jones@uea.ac.uk>, <j.palutikof@uea.ac.uk>, <p.liss@uea.ac.uk>, <r.k.turner@uea.ac.uk>, <j.darch@uea.ac.uk>, <a.watkinson@uea.ac.uk>, <k.brown@uea.ac.uk>, <parryml@aol.com>
Subject: national climate change centre meeting - documents
Date: Sun, 3 Oct 1999 22:19:48 +0100
Cc: <m.hulme@uea.ac.uk>

Dear All,

Here are some notes and suggestions for our national climate centre meeting on Monday morning (1000hrs). A suggested agenda of the main points we need to cover is in this email. The attached document has three components (also appended as text to the email):

A suggested Outline Bid structure with some comments/questions
A draft of a possible 600-word opening statement
A draft of the six (from original four) research challenges (ca. 2,400 words)

We really need to aim to get a first full draft of the bid out to our Partners by late Wednesday this week, thus allowing 7 days for iterations.

Mike

NCCC: UEA Working Group Meeting, 4 October

Suggested Agenda

1. The research challenges (draft attached)
2. RD and Schneider (?)
3. The Assessment Panel; key issues for Schellnhuber
4. The structure of the outline proposal (see attached suggestion)
5. The name of the Centre
6. Timetable for submission (8 working days left)

**

Outline Proposal
Suggested Contents – cf. invitation to bid

Opening Statement (500 words)

Who are the co-applicants?
Hulme, Davies, Jones, Liss, Palutikof, Parry, Turner, Watkinson, Brown?
Allen, Arnell, Berkhout, Bristow, Cannell, Choularton, Halliday, Jenkins,
Kohler, Launder, Markvard, Reynard, Shepherd, Shackley? – is this too many?

The strengths of the UEA-led Team (1000 words)
- being drafted by UMIST

Research Director 100 words

Management team, structure, strategy (500 words)

Advisory Board - Hasselmann, Rotmans, McQuaid, Mary Archer (Chair of National Energy Foundation), Basil Butler (RAE), Wigley, and named others? Management Team, Programme Leaders, What building do we use? – and a suggested physical presence at Southampton and UMIST

Initial research plan/agenda - the Challenges (2000 words)
0-order draft (attached)

How will we achieve - integration, collaboration, exploit results, attract funding? (500 words) (this might be folded into the discussion of the strengths of the UEA Team)

integrated research

formal or informal integration; IAMs are one way, but I'm not so keen on them. Some research themes may develop their own limited IAMs, e.g. optimal policy. Overall informal integration may be achieved through a

common scenario approach/framework

collaboration in UK and abroad

establish MoUs with parallel centres abroad – RIVM (Neth.), PIK (Germany), ICIS (Neth.), MIT (US), Batelle (US), TERI (India), CICERO (Norway), etc. Host an international conference early on to 'position' the UK NCCC in the wider field.

relevant and strategic research results and knowledge-transfer

establish regular policy briefings, both written and verbal, targeted at the business community; CBI link; UKCIP. Have a strong media presence, with a p-t communications person.

attract additional funding

may be not so easy, cf. UKCIP on impacts research have only been able to mobilise small amounts of money. Need some big corporate sponsors – what do we say about this in the outline bid?. Appoint a p-t 'marketing' person (maybe the other half of communications).

Training strategy (250 words)

Ring-fence money for training/workshops/fellowships - how much?

Training not just for researchers, but also for managers in public/private sectors. These could be 1-day sessions, as well as longer 1-week courses (cf. the Harvard course), and also longer-term secondments.

Should also maximise our links with the B.Council and DfID to bring international scientists and policy-advisors into the loop. These people can act to facilitate the two-way flow and testing of ideas between UK and developing countries. Some of our research themes would have global dimensions – optimal policy, C sequestration,

UNESCO Southampton

Financial plan - salaries, equipment, sub-contracted research, collaboration expenses
- estimates from Trevor

Operations timetable - phases, etc.
- what ideas do we have for this?

Other contacts

institutions involved, but outside the bid
BRE, BAS, NRI, POL, LSHTM, AEA, Hadley Centre, UKCIP, etc.

other academic/user bodies who are relevant
RIVM, ICIS, TERI, RDBs, BP, Fuji, PowerGen, BP Solarex, ETSU/DTI
photovoltaic test facility, Severn-Trent,

Appendix

1 page CVs for co-applicants
signed statement from institution(s)

[extraction of purpose from the RC's document the integration of scientific research that will shape and underpin sustainable solutions to the climate change challenge].

Possible Opening Statement

The prospect of human-induced global climate change initially emerged as a research challenge for the natural sciences. Since the causes of climate change are profoundly rooted in society and the consequences of climate change for society can only be understood through social and cultural insight, the social sciences have become increasingly engaged in the research effort. With attention now turning to 'solutions' to climate change, new climate change management strategies need identifying and promoting, need to be targeted at both mitigation and adaptation objectives, and need to embrace a full array of emerging policy instruments and engineering technologies. The participation of the engineering and technological sciences, alongside the environmental and social sciences, has therefore become critical to meet this rapidly evolving research agenda.

But climate change is not just intellectually embracing challenge. It is also an experiential one. Climate change is unique in that it poses questions on space and time scales over which individual humans (especially space) and governments (especially time) are not used to thinking or do not find it easy to think. In this sense climate change is a problem of ultimate penetration and of ultimate connectivity; penetration, because we will all experience and react to climate change in some way, and connectivity, because emissions are driven by a global economy, because the response of the physical system is planetary, and because these social and natural systems are intimately co-evolving.

The intellectual and experiential challenges of climate change create a new and distinctive lens through which we can envision the future. These insights into the future - often termed scenarios - suggest to us various tools and instruments that may allow us to fashion and shape the future. This sets us out on a course of climate change management, an active and considered pursuit of desirable long-term objectives. Establishing such objectives is essential in order to adequately define the 'problem' of climate change, and even more essential if 'solutions' to this problem are going to be designed. The prospect of climate change, at the very least therefore, forces us to think about what sort of future we regard as desirable.

The UEA-led Consortium sees the new national climate change centre as an exciting opportunity to build connected research structures and outputs that exploit partnerships between science and business, between the household and government, and between the UK and emerging parallel initiatives around the world. With a strong foundation of inter-disciplinary research, and through the engaging of both public and private organisations and of both governments and individuals, there is a real prospect that we can implement emerging 'solutions' to climate change and create new ones. These 'solutions' need to engage with both mitigation and adaptation objectives and, most importantly, need to recognise and function on a hierarchy of scales ranging from the household to the global. The UK climate change centre will be built around three key principles:

The deployment of practised, inter-disciplinary research teams, who have already pioneered new insights and approaches into the questions raised by climate change, but releasing them to explore novel approaches for thinking laterally across natural, social and engineering sciences.

The practising of an inclusionary process of research in which we explore - with their developers - ways of mobilising many of the new technologies, lifestyles, regulatory mechanisms that are emerging from our technological, social and political cultures to allow us to manage climate change in the twenty-first century.

The establishment of a focal point in the UK and abroad for the open and constructive exchange of insights concerning climate change solutions across cultural divides - public-private, households-corporations, North-South.

These three key characteristics - a research programme, an engagement with stakeholders, and an educational/opinion-shaping role - are the three central elements of the new centre as proposed by the UEA-led Consortium. [Given the essential need for integration in all three of these elements, we propose the centre be called the "UK Centre for Integrated Climate Change Studies" (UK CICCSS)]. The rest of this outline proposal will demonstrate, in an indicative rather than an exhaustive way, how we would operationalise these principles in terms of both management and research ideas. [refer to our conceptual schematic here or later?]

Proposed Challenges to be included in the Outline Bid

Draft, Mike Hulme, 2 October

[It may be worth including some examples of key stakeholder/client interests under each of these. These six research challenges are exemplars, for the outline proposal, of the thinking behind our bid. Each of them may potentially involve all of the Centre's Partners - and numerous organisations beyond - and each of them are therefore integrating activities. Each of these Challenges, if developed into Research Programmes, would have a Programme Leader, appointed from within the Consortium, and accountable to the Centre's Management Team. Each of the Challenges should be able to be contextualised by our (revised) conceptual schematic of the process of integration - if we are still going to show this.]

Challenge 1: Carbon Management

Carbon management poses two fundamental questions. Given a continuing pre-dominance of fossil carbon fuels how can we combust less (the energy efficiency question) and given that a proportion of this combusted carbon will enter the atmosphere how can we sequester larger volumes within the biosphere and oceans (the carbon sequestration question)? In thinking about improving our management of carbon, the Centre will address both these questions.

Combined heat and power plants and decentralised energy generation for energy intensive industries are areas where technology can make a considerable contribution to emissions reduction. Locations and markets where investment in these technologies is both politically and economically feasible need to be identified. For LDCs, the provisions of the Kyoto Protocol for Joint Implementation are relevant here. Supplementary engineering challenges in this area include energy storage systems, fuel cell and novel transportation technologies.

Research should also be directed to the identification of business opportunities in the mitigation of climate change. This would involve a process of identifying 'climate change markets' where UK products and technologies could be supplied. One potential growth area is that of the use of modern, cheap control technology to optimise the performance of household energy management systems. Where growth markets are identified, suitable technology and service products can be developed. Business could be approached for ideas through the DTI-funded liaison officer. This work would also inform development and aid policy within the UK government. We would also draw upon the extensive experiences of UK agencies involved in delivering 'win-win' energy and waste minimisation programmes (such as Energy Efficiency and Environmental Technology Best Practice Programmes, Ground Work Trust, Business Links, and so on). Other country experiences would also be useful input, for example the highly effective programmes of boosting company productivity by reducing greenhouse gas emissions developed in the USA.

The introduction of the climate change levy in March 2001 will be analysed by the Centre in terms of its effectiveness at delivering emission reductions and its costs/benefits to a range of units (firms, sectors, regions, nationally). In addition, the introduction of voluntary agreements for some companies in return for a reduction in the levy charged will be analysed along similar lines. The DTI-ACBE led initiative on voluntary use of tradeable emission schemes will provide important empirical evidence on the relative costs of achieving given emission reductions by a taxation scheme compared to emissions trading.

While conventional carbon sequestration technologies are not considered a long-term solution to climate change, there is nevertheless a need to research the most efficient ways of implementing such technologies and also a need to research new, longer-term sequestration technologies through bio-engineering and deep ocean sinks. The Centre will explore the feasibility of both these latter two technologies, in collaboration with the John Innes Centre for the bio-engineering. [We may only have 30 years to get some 'emergency' carbon sequestration techniques sorted out under the scenario that we don't manage to get enough CO₂ emissions reduction.] A mixture of methods and tools will be required to evaluate sequestration options - life-cycle costing and LCA, environmental impact analysis, technological assessment, public acceptance, etc. Some work on biomass sequestration may also be needed to feed into the global

assessments/evaluation of this option. Given the sensitivity of this issue under the terms of the Kyoto Protocol, the UK government needs excellent advice on methods, assumptions, pitfalls, etc.

[Links outside the Centre to: JIC, many others

Challenge 2: The Renewables Challenge

A parallel challenge to that of carbon management is how to stimulate and release the full potential for zero- or low-carbon renewable energies? This therefore is the third strand of the strategy to meet and surpass the carbon emissions reduction obligations placed on developed nations by Kyoto. There are a number of research questions related to this Challenge that again require engagement by the engineering, environmental and social science communities within an integrated framework. Too much work to date has compartmentalised the three perspectives.

The EU has a target of 12 per cent of primary energy to be met from renewable energy by the year 2010. Meeting such a target, let alone moving beyond it, has major implications for the electricity delivery systems in the UK. How to get this much renewable energy - from intermittent sources - linked, delivered and purchased by customers? Engineers and economists need an opportunity to explore the long-term implications of such policy objectives. Related questions concern the landscape and infrastructural implications of an expanded uptake of biofuels in the UK.

Many renewable technologies appear in various EPSRC research programmes, but they need to be brought together to produce scenarios whose emissions and life-cycle costs can be assessed in a common framework, thus enabling more practical advice and comment on energy policy debates. Some of these scenarios could be taken further in the form of pilot-demonstration projects.

There needs to be mechanisms established for the better integration of architectural design with renewable energies, e.g. solar and wind. The design of these new technologies needs explicitly to consider the architectural consequences for domestic, commercial and industrial structures. Partners who are directly involved in delivering design solutions in this area will be invited by the Centre to establish 'demonstration' projects to explore how successful such solutions are in practice. [can we give some specific examples of Partners and projects here?]

One of the obstacles to the more rapid exploitation of wind energy in the UK relates to landscape value and aesthetics. This is an issue that needs the interaction of design technologists and social scientists - including psychologists - to explore cultural and behavioural limits to new renewable technology uptake. We propose that the visualisation facility of the Centre be exploited to research these issues through involving the wider community.

[Links outside the Centre to:

Challenge 3: Singularities, Non-Linear Changes and Extreme Events

The climate system is generally assumed to be 'well-behaved'. Certainly, much of the scenario and impacts work assessed by the IPCC (and that has

therefore fed through into climate policy) has assumed conditions of relative regularity in future climate. However, not only does the climate system possess the potential for rapid, singular changes (i.e., a complex, non-linear system being rapidly forced), but recent research has shown using theoretical models and palaeo-evidence that such potential changes can be and have been realised. Elsewhere, thresholds and sensitivities of natural/social systems to changing frequencies of extreme weather events induce additional non-linearities in the environmental responses to climate change. There are also singularities and non-linear processes operating in the social/political drivers of climate change - for example, political or economic 'shocks' that may fundamentally and rapidly re-direct our technological/economic futures away from 'conventional' pathways.

A particular Challenge to be addressed by the Centre will therefore be how such potential for non-linear behaviour - in both climate and non-climate systems - can be both modelled and introduced into scenario exercises. Recent work with reduced-complexity models has shown the potential to model such non-linear behaviour in a quasi-stochastic manner and such modelling work will be developed by the Centre. A corollary of this is to better understand how such abrupt changes should be assimilated into decision-making frameworks and policy analysis. This requires the involvement of risk theoreticians and risk analytic tools. The possible interactions between these two complex non-linear systems - the climate and the social - is of particular importance. For example, an abrupt climate change or a string of short-term weather extremes can radically influence perceptions amongst the business community and politicians and lead to sudden shifts in policy, investment flows, etc. The implications of such singular behaviour for vulnerability and adaptation strategies have not been well explored. This kind of analysis would be important to many commercial sectors, which are highly concerned about the unexpected and about extremes. This is an inter-disciplinary Challenge the Centre will be uniquely well-placed to address.

[Links outside the Centre to: POL, Hadley Centre, PIK,

Challenge 4: Managing the Coastal Zone

There are many geographic domains where climate change poses particular problems for the management of natural and social assets - coasts, uplands, cities, river basins, etc. We propose that the Centre should pay particular attention to one such domain, since these provide physical entities within which many of the issues of climate change vulnerability and adaptation play out in a given context of local/regional governance. We suggest that the coastal zone best epitomises this challenge of integrating our social, environmental and engineering knowledge. A unique feature of the interaction between climate change and the coastal zone is the very long time-scales over which sea-level rise impacts will materialise - of all the impacts of climate change these are least amenable to mitigation and therefore where appropriately designed adaptation strategies are most needed.

Research is first needed to improve our understanding of the threats posed by climate change, most notably changing storm-surge frequencies along the UK coast and changes in estuarine hydrology and ecology. This will involve coupled high resolution ocean-atmosphere modelling, estuarine economic/ecological modelling, and the assimilation of such modelling results into a risk analysis framework.

Designing an array of possible management options for the coastal zone needs to involve economists, ecologists, marine scientists, and coastal engineers. A range of options from 'hard' engineering solutions to managed retreat need identifying. The desirability of any one or combination of these management options for the coastal zone can then only be evaluated following an understanding of the value of the coastal environment and the services it delivers. Such valuation needs to be a fully participatory process involving local communities, local government, landowners, NGOs, and national regulatory bodies. We propose the Centre plays an active role in bringing together insights from integrated modelling exercises and from stakeholder participatory exercises, thus enabling better public participation in the policy-forming process (see integration methodologies - Challenge 6). This role would involve novel visualisation techniques of coastal environments to exploit both modelling results and individual perceptions of coastal landscape value.

[Links outside the Centre to: EA/MAFF, NGOs/Conservation, LAs, Railtrack, construction companies,

Challenge 5: Beyond Factor 4

There is a growing body of opinion that in order to mitigate climate change, or even to adapt to it, significant changes in current patterns of consumption, and therefore lifestyle, are necessary. This raises the question of how to direct consumption of goods and services towards more sustainable paths. The scale of the Challenge here suggests that we need to go well beyond Factor 4 - doubling wealth, halving resource use.

One unsolved dilemma is that of expanding car use for personal transportation. The psychology, behavioural sociology and economics of people's use of cars is reasonably well understood. What needs to be researched are methods to manage the ever-increasing demand for travel, especially car and air travel, that ranges from taking the children to school, to car-based salespeople, to international business and holiday travel. Research will also be needed into managing the overturning of the vehicle stock and transport infrastructure under conditions of novel transportation technologies - infrastructural inertia is an obstacle to new technology uptake.

The concept of a low consumption household is a further desirable objective which is easy to state and not straightforward to achieve. This way of analysing human activity is inherently interdisciplinary and looks at the activities of a household - housing, domestic appliances and services, transport needs, consumption, work and leisure time use, waste generation and recycling - in terms of the interactions between them. For example, housing choice is partly determined by the work/leisure split, which then determines the demand for transport; consumption generates waste and also contributes to energy demand. Another important example is that of home insulation. The UK has a poorly insulated housing stock and even new housing could be built to much higher standards of energy efficiency. Research, in conjunction with the construction industry, is needed into the adoption of new building standards and (politically acceptable) economic incentives for low-energy housing is needed. This is especially relevant given the current debate about the millions of new households predicted for the UK in the next 20-30 years and the greenfield/brownfield land use argument.

Partners who are directly involved in delivering sustainable solutions will be involved in setting up 'demonstration' projects to explore how

successful such methodologies are in practice. For example, the Centre will explore whether 'climate-friendly' households can be demonstrated in practice. Partners could include Going for Green, National Centre for Business and Ecology, Forum for the Future, Sustainability Northwest, United Utilities, Eastern Group, Anglian Water and other water companies, etc.

[Links outside the Centre to:

Challenge 6: Integration Methodologies

An important methodology which provides insights into the dynamics of climate and social change, but which has not yet been fully developed for the UK is that of integrated assessment. Integrated assessment encompasses formal modelling approaches and more participatory and qualitative explorations of the future. Integrated modelling includes both reduced-form models and complex systems models. All integrated assessment is built around the concept of scenarios, used either in the more traditional role of 'what-if' or in a 'back-casting' role. While integrated assessments of climate change have developed substantially over the last decade, few have embraced the engineering community to explore the feasibility of pathways with rapid uptake of new technologies. The Challenge for the Centre will be to develop further existing modelling and participatory approaches for integrated assessment and apply them to the five research Challenges identified above.

The integrated modelling framework that is required to address these concerns is obviously extremely difficult to imagine. Recent advances based on complex systems modelling do, however, suggest how such frameworks may be achieved (e.g. NEXSUS, ESRC Priority Network). These are constituted of a spatial hierarchy of nested models representing the possible behaviours of complex social, economic, ecological, and technological systems at different spatial and temporal scales of resolution. They can explore the possibility of emergent behaviour at larger scales, as well as the effects of micro-responses and adaptations at smaller ones. In order to address the issues raised by climate change and its associated impacts and responses, considerable development of this framework would be necessary. However, without it, there seems little prospect of providing a rational basis for the assessment of possible climate policies or actions.

The Centre will also develop parallel research into participatory approaches for the development of integrated scenarios of the future. This will include the public perception of environmental risks caused by climate change; peoples actions in response to these perceptions is also important. Identification of suitable scenarios for presentation in participative experiments on public/corporate response would involve the physical sciences in co-operation with engineers, political scientists, psychologists and economists. Methods include surveys, focus groups, citizens juries and stakeholder workshops. [CSERGE/UMIST developing these ideas; use the ICER Visualisation Laboratory]. More in-depth empirical research could be undertaken to understand better individual and organisational decision-making on climate change related issues, such as energy consumption, transport choices, and so on. This activity would have the objective of developing methodologies for assessing the public response to the particular problems identified in the Carbon Management, Renewables and Factor 4+ Challenges above. Through interactions with business it may also open up the possibility of 'design-oriented scenarios', i.e., in which the scenario identifies a need for a new kind of product/process design in response to a prospective future socio-political change.

[Links outside the Centre to: other process modelling centres, ULYSSES,

.....

Attachment Converted: "c:\eudora\attach\outline.bid.doc"

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, imacadam@meto.gov.uk
Subject: Re: Briffa et al. series for IPCC figure
Date: Tue, 05 Oct 1999 12:31:56 -0400
Cc: k.briffa@uea, p.jones@uea, ckfolland@meto.gov.uk, tkarl@ncdc.noaa.gov

Dear Tim,

Thanks for the information. I don't want to speak for Tom Karl, but I think it may be a bit too late (past the Oct 1 deadline) to make further revisions in the draft 1.0. It would be a bit of an imposition on Tom at this point given what he's been through in finalizing the draft. However, I see no reason that we can't make that revision when the paper comes back from expert review in a couple months. We'll have the further advantage that the supporting manuscript you describe should be available at that point (a requirement in the IPCC peer-review process). I think we'll all be looking forward to updating the plot w/ the latest series you describe...

As for decisions about the most appropriate baseline period to use for the series, that is as you point out an important issue and one we have to consider with some circumspection, especially if a "modern" calibration (e.g., 1931-1960) to the instrumental record gives a substantially different alignment from the more 19th century-oriented calibration you describe. The tradeoff of course is that the instrumental series itself is considerably less certain prior to the 20th century while, as you point out, the non-climatic influence on tree growth may be setting in by the mid 20th century. Something I think we can iron out satisfactorily at the next juncture.

I hope the above sounds ok to you guys. Let me know. Thanks,

mike

At 04:18 PM 10/5/99 +0100, Tim Osborn wrote:

>Dear Mike and Ian

>

>Keith has asked me to send you a timeseries for the IPCC multi-proxy
>reconstruction figure, to replace the one you currently have. The data are
>attached to this e-mail. They go from 1402 to 1995, although we usually
>stop the series in 1960 because of the recent non-temperature signal that
>is superimposed on the tree-ring data that we use. I haven't put a 40-yr

>smoothing through them - I thought it best if you were to do this to ensure
>the same filter was used for all curves.
>
>The raw data are the same as used in Briffa et al. (1998), the Nature paper
>that I think you have the reference for already. They are analysed in a
>different way, to retain the low-frequency variations. In this sense, it
>is one-step removed from Briffa et al. (1998). It is not two-steps removed
>from Briffa et al. (1998), since the new series is simply a *replacement*
>for the one that you have been using, rather than being one-step further.
>
>A new manuscript is in preparation describing this alternative analysis
>method, the calibration of the resulting series, and their comparison with
>other reconstructions. We are considering submitting this manuscript to J.
>Geophys. Res. when it is ready, but for now it is best cited as:
>Briffa KR, Osborn TJ, Schweingruber FH, Harris IC and Jones PD (1999)
>Extracting low-frequency temperature variations from a northern tree-ring
>density network. In preparation.
>Keith will be sending you a copy of the manuscript when it is nearer to
>completion.
>
>I have also attached a PS file showing the original Briffa et al. (1998)
>curve, with annotation of cold years associated with known volcanic
>eruptions. Overlain on this, you will see a green curve. This is the new
>series with a 40-yr filter through it. This is just so that you can see
>what it should look like (**ignore the temperature scale on this
>figure**, since the baseline is non-standard).
>
>With regard to the baseline, the data I've sent are calibrated over the
>period 1881-1960 against the instrumental Apr-Sep temperatures averaged over
>all land grid boxes with observed data that are north of 20N. As such, the
>mean of our reconstruction over 1881-1960 matches the mean of the observed
>target series over the same period. Since the observed series consists of
>degrees C anomalies wrt to 1961-90, we say that the reconstructed series
>also represents degrees C anomalies wrt to 1961-90. One could, of course,
>shift the mean of our reconstruction so that it matched the observed series
>over a different period - say 1931-60 - but I don't see that this improves
>things. Indeed, if the non-temperature signal that causes the decline in
>tree-ring density begins before 1960, then a short 1931-60 period might
>yield a more biased result than using a longer 1881-1960 period.
>
>If you have any queries regarding this replacement data, then please e-mail
>me and/or Keith.
>

>Best regards

>

>Tim

>

>Calibrated against observed Apr-Sep temperature over 1881-1960

>averaged over all land grid boxes north of 20N

>

>

>Year Reconstructed temperature anomaly (degrees C wrt 1961-90)

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>
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>Norwich NR4 7TJ | sunclock:
>UK | <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
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From: Tim Osborn <t.osborn@uea.ac.uk>
To: mann@virginia.edu,imacadam@meto.gov.uk
Subject: Briffa et al. series for IPCC figure
Date: Tue, 05 Oct 1999 16:18:29 +0100
Cc: k.briffa@uea,p.jones@uea

Dear Mike and Ian

Keith has asked me to send you a timeseries for the IPCC multi-proxy reconstruction figure, to replace the one you currently have. The data are attached to this e-mail. They go from 1402 to 1995, although we usually stop the series in 1960 because of the recent non-temperature signal that is superimposed on the tree-ring data that we use. I haven't put a 40-yr smoothing through them - I thought it best if you were to do this to ensure the same filter was used for all curves.

The raw data are the same as used in Briffa et al. (1998), the Nature paper that I think you have the reference for already. They are analysed in a different way, to retain the low-frequency variations. In this sense, it is one-step removed from Briffa et al. (1998). It is not two-steps removed from Briffa et al. (1998), since the new series is simply a *replacement* for the one that you have been using, rather than being one-step further.

A new manuscript is in preparation describing this alternative analysis method, the calibration of the resulting series, and their comparison with other reconstructions. We are considering submitting this manuscript to J. Geophys. Res. when it is ready, but for now it is best cited as:
Briffa KR, Osborn TJ, Schweingruber FH, Harris IC and Jones PD (1999) Extracting low-frequency temperature variations from a northern tree-ring density network. In preparation.
Keith will be sending you a copy of the manuscript when it is nearer to completion.

I have also attached a PS file showing the original Briffa et al. (1998) curve, with annotation of cold years associated with known volcanic eruptions. Overlain on this, you will see a green curve. This is the new series with a 40-yr filter through it. This is just so that you can see what it should look like (**ignore the temperature scale on this figure**, since the baseline is non-standard).

With regard to the baseline, the data I've sent are calibrated over the period 1881-1960 against the instrumental Apr-Sep temperatures averaged over

all land grid boxes with observed data that are north of 20N. As such, the mean of our reconstruction over 1881-1960 matches the mean of the observed target series over the same period. Since the observed series consists of degrees C anomalies wrt to 1961-90, we say that the reconstructed series also represents degrees C anomalies wrt to 1961-90. One could, of course, shift the mean of our reconstruction so that it matched the observed series over a different period - say 1931-60 - but I don't see that this improves things. Indeed, if the non-temperature signal that causes the decline in tree-ring density begins before 1960, then a short 1931-60 period might yield a more biased result than using a longer 1881-1960 period.

If you have any queries regarding this replacement data, then please e-mail me and/or Keith.

Best regards

Tim

Calibrated against observed Apr-Sep temperature over 1881-1960
averaged over all land grid boxes north of 20N

Year Reconstructed temperature anomaly (degrees C wrt 1961-90)

1402	-0.283
1403	-0.334
1404	-0.286
1405	-0.350
1406	-0.152
1407	-0.124
1408	-0.220
1409	-0.175
1410	-0.100
1411	-0.129
1412	-0.226
1413	-0.115
1414	-0.386
1415	-0.319
1416	-0.277
1417	-0.136
1418	-0.172
1419	-0.294
1420	-0.280
1421	-0.335

1422 -0.406
1423 -0.312
1424 -0.207
1425 -0.136
1426 -0.354
1427 -0.222
1428 -0.305
1429 -0.322
1430 -0.282
1431 -0.143
1432 -0.212
1433 -0.234
1434 -0.076
1435 -0.309
1436 -0.411
1437 -0.122
1438 -0.272
1439 -0.159
1440 -0.330
1441 -0.160
1442 -0.105
1443 -0.080
1444 -0.308
1445 -0.138
1446 -0.317
1447 -0.270
1448 -0.301
1449 -0.357
1450 -0.137
1451 -0.183
1452 -0.207
1453 -0.485
1454 -0.265
1455 -0.358
1456 -0.241
1457 -0.199
1458 -0.366
1459 -0.397
1460 -0.252
1461 -0.230
1462 -0.252
1463 -0.209
1464 -0.174

1465 -0.174
1466 -0.280
1467 -0.256
1468 -0.256
1469 -0.222
1470 -0.237
1471 -0.094
1472 -0.122
1473 -0.056
1474 -0.320
1475 -0.376
1476 -0.133
1477 -0.075
1478 0.037
1479 -0.161
1480 -0.379
1481 -0.513
1482 -0.286
1483 -0.354
1484 -0.327
1485 -0.208
1486 -0.125
1487 -0.380
1488 -0.193
1489 -0.245
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1491 -0.244
1492 -0.146
1493 -0.278
1494 -0.394
1495 -0.526
1496 -0.275
1497 -0.264
1498 -0.233
1499 -0.169
1500 -0.128
1501 -0.415
1502 -0.306
1503 0.011
1504 -0.013
1505 -0.378
1506 -0.226
1507 -0.428

1508 -0.192
1509 -0.312
1510 -0.157
1511 -0.162
1512 -0.188
1513 -0.135
1514 -0.418
1515 -0.258
1516 -0.381
1517 -0.134
1518 -0.180
1519 -0.166
1520 -0.035
1521 -0.384
1522 -0.302
1523 -0.541
1524 -0.371
1525 -0.183
1526 -0.289
1527 -0.224
1528 -0.247
1529 -0.432
1530 -0.291
1531 -0.467
1532 -0.343
1533 -0.586
1534 -0.183
1535 -0.417
1536 -0.350
1537 -0.257
1538 -0.451
1539 -0.398
1540 -0.497
1541 -0.406
1542 -0.584
1543 -0.448
1544 -0.317
1545 -0.312
1546 -0.289
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1551 -0.074
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1620 -0.706
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Norwich NR4 7TJ | sunclock:
UK | <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

From: "Sujata Gupta" < Sujatag@teri.res.in >
To: <m.hulme@uea.ac.uk>
Subject: Re: UK National Climate Change Centre
Date: Tue, 05 Oct 1999 19:16:32 +0530
Cc: <t.d.davies@uea.ac.uk>

Dear Mike,

I was on travel and hence the delay in responding to your email. TERI will be interested in being one of the International Supporting Institutes for the Centre. I will fax a letter to you tomorrow and send the original by post.

I have not heard on the DETR proposal as yet.

Best wishes

Sujata

Sujata Gupta, Ph.D.
Fellow and Dean
Policy Analysis Division
TERI

>>> Mike Hulme <m.hulme@uea.ac.uk> 09/28/99 02:34AM >>>

Dear Sujata,

This may well not be news to you, but the UK government has recently requested bids from UK universities to house a new 'National Climate Change Centre'. The Centre would receive funds of 2 million pounds sterling per year for (at least initially) five years. The role of the Centre would be to compliment existing work on climate modelling and data analysis (IPCC WGI areas) by focussing on 'solutions' (mitigation and adaptation options and their implementation), specifically for the UK government and business community, but within a global context. The emphasis appears to be on IPCC WG3 area with a strong commitment to integrated research, but with some overlap with WG2. The Centre would carry out independent research, but would also be expected to make use of, and to integrate, existing UK research and expertise. It would be expected to contribute to and to foster interdisciplinary research that underpins sustainable solutions to the climate change problem.

UEA is making a bid for this Centre. Applications are due by mid-October. UEA is well-known for CRU, but it also has strengths in data distribution to the climate impacts community, in impacts research, and in environmental economics (CSERGE). While these areas are fundamental foundation stones for the science that the Centre is expected to develop, the Centre would need to expand significantly beyond these areas. We have a Consortium in place as follows

- 6-7 Senior Partners - (UEA, UMIST, U.Southampton, Dept. Economics at U.Cambridge, Cranfield, Leeds Institute of Transport Studies, IH and ITE)
- Affiliated UK Organisations - (we have 6-8 of these)
- Supporting Business Links
- Supporting International Organisations

If UEA were to succeed in its bid for the Centre, then it would seek to develop strong links with other institutions abroad in order to strengthen

its own intellectual base and, through such links, to contribute to the development and implementation of the science. We would see TERI as one of these Supporting International Organisations.

To this end, we would like a short letter of support from yourself - on behalf of the Policy Analysis Division, or a wider TERI grouping if you feel able to represent them - indicating that you fully support the UEA bid and would exclusively lend your backing to this Consortium and be keen to interact closely with us at a research level were the Centre to come to UEA. This interaction may take the form of exchanging scientists, testing out new methodologies, developing/advising on workshops, providing entry-points into international policy initiatives, etc., etc.

Nothing too formal or lengthy at this stage, but we would like to provide the Council's with a flavour of the breadth of our existing and future collaboration in the field and our ability to mobilise support in our favour.

Many thanks. Please send to Prof. Trevor Davies, Dean, Environmental Sciences, UEA, Norwich, NR4 7TJ, before the 12th October.

Feel free to ask me for more details, etc. Our written text is beginning to take shape and we will circulate a draft of this to you before the bid goes in.

Regards,

Mike

p.s. I have not yet heard anything about the DETR India Programme. Have you?

Dr Mike Hulme
Reader in Climatology tel: +44 1603 593162
Climatic Research Unit fax: +44 1603 507784
School of Environmental Science email: m.hulme@uea.ac.uk
University of East Anglia web site: http://www.cru.uea.ac.uk/~mikeh/
Norwich NR4 7TJ

Annual mean temperature in Central England for 1999
is currently about +1.4 deg C above the 1961-90 average

The global-mean surface air temperature anomaly for 1998
was +0.57 deg C above the 1961-90 average, the warmest year yet recorded

From: Tom Wigley <wigley@meeke.ucar.edu>

To: Mike Hulme <m.hulme@uea.ac.uk>

Subject: Re: outline bid for Centre

Date: Wed, 6 Oct 1999 14:51:37 -0600 (MDT)

Cc: j.Rotmans@icis.unimaas.nl, hasslemann@uea.ac.uk, "Stephen H. Schneider" <shs@leland.stanford.edu>

Dear Mike,

I've not yet looked at your Tyndall biography, but I see your logic in suggesting his name. His 1861 papers in Phil. Mag. Ser. 4, 22, 169-194 and 273-285 were arguable the first reasonable descriptions of the CO₂ (or, in his words, "carbonic acid") greenhouse effect. However, it is generally believed that Fourier, in 1827, was the first person to allude to a greenhouse effect and to suggest that human activities might affect the climate (see, e.g., Ramanathan, Science 240, 293-299, 1988).

In my view, however, neither Tyndall nor Fourier would be appropriate for naming a climate centre devoted to human-induced change. Tyndall is not appropriate because he did not consider (or even dream of) the human influence; while Fourier is not appropriate because it would not be P.C. to name a UK centre after a Frenchman. Furthermore, both Tyndall and Fourier are well-known and well-recognized for their contributions in *other* areas.

The person who really deserves the credit is Callendar who, in 1938, not only suggested that human influences were causing CO₂ to increase, but also that this was causing global warming. Furthermore, he did an amazing job documenting both the CO₂ build up *and* the warming. Essentially, it was Callendar who, more than 60 years ago, really exposed the problem that is our current concern. His work was a quantum leap above anything done previously; and, one could argue, was not really improved upon until Manabe and Wetherald's seminal 1967 (JAS 24, 241-259) paper. I doubt whether there is an intellectual milestone in *any* field that compares with this.

Best wishes,

Tom

On Tue, 5 Oct 1999, Mike Hulme wrote:

> Dear 'Advisory Board member',

>
> As tentative nominees for the 'Advisory Board' for the UEA-led bid for the
> new UK National Climate Change Centre, I am sending you a first full draft
> of our outline bid. This is due with the Council's on the 15th October.
> Needless to say, please regard this document as confidential and do *not*
> circulate it to third parties.
>
> I would like to invite your comments in the next few days on the draft. I
> can accept comments until Tuesday 12th October, but earlier comments will
> prove most useful. Appended below is the communication sent out to our
> co-applicants with this draft. Please bear in mind that this is the first
> full draft we have put together and it is very rough and ready.
>
> You may find it easier to download from the named web site.
>
> Thank you for your time. Please direct any comments to the Consortium via me.
>
> Regards,
>
> Mike

```
*****  
*Tom M.L. Wigley                *  
*Senior Scientist                *  
*National Center for Atmospheric Research      *  
*P.O. Box 3000                   *  
*Boulder, CO 80307-3000          *  
*USA                             *  
*Phone: 303-497-2690             *  
*Fax: 303-497-2699              *  
*E-mail: wigley@ucar.edu         *  
*****
```

From: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: apologies
Date: Fri, 8 Oct 1999 22:57:48 +0200
Reply-to: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>

Dear Mike,

I can understand you very well. I would have been more nervous about this, hadn't the preparations AND registrations been going as well as they have done: just now, I feel pretty comfortable about the meeting. Sure, it's a pity not having you around, but I guess you are taking the appropriate decision under your particular circumstances.

Perhaps I shouldn't be doing this, but let me add a VERY CONFIDENTIAL piece of information for you. It won't make your life less stressful during the next few days, and I really MUST ask you to keep this confidential at your end (since I am effectively breaking a confidentiality here, and I wouldn't want Edinburgh to know that), but I received the following e-mail on October 6:

Dear Dr Cramer,

I am contacting you on behalf of Prof Paul Jarvis to check whether you are willing to have your name mentioned in association with a project he is hoping to undertake. The project is part of a much larger package of projects which forms the nucleus of a bid being made by the University of Edinburgh and other partners to host a new Climate Change Centre, to be funded by the UK Research Councils at 10 million GBP over 5 years (for further details of this opportunity see: <http://www.nerc.ac.uk/press/aoclim.html>). I work in a small unit of the University of Edinburgh that has responsibility for co-ordinating multi-disciplinary environmental research bids. Currently we are preparing the Outline Bid (deadline 15 October), so nothing should be regarded as firm, and details will be open to modification in the Full Bid, which we will prepare if the Outline Bid is successful.

Below I reproduce the text we are proposing to include in the Outline Bid. Please confirm whether or not you are willing to have your name included.

Please treat this email as confidential.

Best regards,

Simon Allen.

=====
Dr S J Allen, Research Co-ordinator
Centre for the study of Environmental Change and Sustainability (CECS)
University of Edinburgh
John Muir Building, King's Buildings, Mayfield Road, Edinburgh EH9 3JK

Tel: 0131 650 7215 Email: simon.allen@ed.ac.uk
Fax: 0131 650 7214 <http://www.cecs.ed.ac.uk>
=====

Issue: Will terrestrial carbon sinks saturate?

It has been proposed that the assimilation of CO₂ by vegetation will reach saturation within the foreseeable future as atmospheric CO₂ concentrations continue to rise and that, conversely, increase in temperature will lead to open-ended increase in respiration by soil heterotrophs, so that at some point in the not too distant future, CO₂ efflux will come to exceed CO₂ influx.

This far-reaching assumption derives from global models that lack a consideration of acclimation, feed backs and biological constraints acting on these processes. This proposition will be critically evaluated using Dynamic Global Vegetation Models (DGVM's) that include appropriate feed backs derived from new data that are becoming available from on-going experiments in the UK and elsewhere. This core project will be executed over two years by a research fellow at the University of Edinburgh, under the supervision of Professor Paul Jarvis, FRS. The project will involve close collaboration with: the Max Planck Institut fur Biogeochemie (Prof I Colin Prentice) and the Potsdam Institute for Climate Impacts Research (Dr Wolfgang Cramer) where fully operational DGVMs are in use; the Dept of Production Ecology, University of Uppsala (Prof Sune Linder), currently conducting soil warming experiments in northern Sweden.

Costs (GBP):	Yr 1	Yr 2
Research fellow	50 k	52 k

Travel/interaction 4 k 4 k

Total project cost: 54 k 56 k

-----end of Edinburgh mail-----

To me, this comes at a very strange moment, since I am, with Bert Bolin, in a very strange situation with the completion of our second draft of the IPCC Special Report on Sinks due Land Use and Forestry. The very issue they propose to collaborate with Colin and myself about was the most contentious one of all, and Paul on one side, and several others including myself on the other side, had diametrically opposing opinions. In fact, I simply believe Jarvis either wasn't able or not wasn't willing to understand what the real issue was.

Anyway, I don't know whether, and if, in which way, this may or may not affect your completion of the UEA bid, but I thought I'd better let you know. Obviously I discussed this with Colin, and his response is that he a) would place his bet on your rather than the Edinburgh bid in terms of potential success, and b) that he nevertheless thinks Edinburgh is proposing the appropriate thing to do here, and that he therefore will reply positive to their request for collaboration. Unless you see a strong reason for recommending me to NOT do the same (we can talk about this in Brussels of course), I shall probably reply in the same positive way.

Take care,

Wolfgang

PS: I am really uncertain whether I do something terribly bad in sending this to you, after the explicit request for confidentiality - so please keep this among the two of us...

On Freitag, 8. Oktober 1999, you wrote:

> Wolfgang,

> I shall have to apologise, but I will not be able to make the ECLAT meeting
> at all. The pressures of getting our UK National Climate Change Centre
> outline bid together for the 15th October are now such that I have to be
> here on the 13th and 14th (being in Brussels in the 12th is not very
> helpful either, but I can at least get back to UEA for Wednesday/Thursday

> to wrap up the bid). I have the lead responsibility now at UEA for
> co-ordinating our proposal - 8 institutions, 24-co-applicants, so you can
> imagine the headaches involved. But we want to make sure Hans-Joachim has
> a good proposal tabled from UEA when he meets with the Assessment Panel
> later in November!

> I really regret not being there - you have done a great job in pulling the
> programme and people together amidst IPCC activities. I have asked Tim
> Carter to present the IPCC/ACACIA speech and I am sure he will!

> Tim Carter and David Viner will co-ordinate over what needs doing for the
> proceedings which I insist will be a Cramer et al. (ed) (1999/2000)
> publication. David and Ruth will bring several dozen copies of the
> Helsinki book for distribution. It is important to get the breakout groups
> to get text together on their deliberations while at the meeting. You will
> see what we have done to the Helsinki material. For the Green Workshop we
> should not exceed 100pp. (cf. 128pp. for Helsinki) and colour should be
> avoided where possible. CRU will take over the sub-editing and desk-top
> publishing role again.

> I guess I will see you in Brussels anyway.

> Gabi please cancel my hotel reservation and travel pick-up.
> Thank you for your efficiency in organising all this.

> Best regards,

> Mike

mailto:Wolfgang.Cramer@pik-potsdam.de

From: "R K Pachauri" <pachauri@teri.res.in>
To: <m.hulme@uea.ac.uk>
Subject: Workshop on "North-South Strategies for Sustainable Development", November 1, 1999
Date: Wed, 13 Oct 1999 15:57:37 +0530

Workshop on "North-South Strategies for Sustainable Development",
November 1, 1999

Dear Dr Hulme,

TERI is hosting an event at the Fifth Conference of the Parties on "North-South Strategies for Sustainable Development". At this event we intend to generate a discussion on the impetus for furthering the objectives of the UN Framework Convention on Climate Change. Not only is there a need to review the provisions in the Kyoto Protocol but also to develop a framework for operationalizing it. In particular, the workshop will focus on the Clean Development Mechanism. The workshop also aims to identify drivers that could maintain the momentum, which was achieved at Kyoto, ratification of the Protocol notwithstanding.

Hoping you were already at Bonn, I would like to invite you to provide your valuable viewpoint as a discussant at our event scheduled for November 1, 1999 at Hotel Maritim from 1800 - 1930 hours. A brief background note highlighting the issues intended for discussions during the Workshop as well as the Workshop agenda is attached herewith for your perusal. In case you have not planned for Bonn, I would deeply appreciate it if you could forward this mail to prospective participants to COP 5.

Thanking you and looking forward to meeting you at Bonn.

With warm regards,

R K Pachauri

Attachment Converted: "c:\eudora\attach\Wkshp-bkgground1.doc"

From: Eric Steig <steig@igl.geol.upenn.edu>
To: domraynaud@glaciog.ujf-grenoble.fr
Subject: No Subject
Date: Thu, 14 Oct 1999 13:44:49 -0400 (EDT)
Cc: jto@ngdc.noaa.gov, k.briffa@uea.ac.uk, icdc@igl.geol.upenn.edu

Dear Dominique,

Jonathon Overpeck forwarded your email to me some time ago, regarding Holocene ice core data. I apologize for the delay in responding.

Frist, regarding US contacts for ice core data. I am happy to work on this as you suggest, and it certainly makes sense to have me involved since I have been working on ice core data management for some time. I can probably do a good job representing the US Arctic/Antarctic community, but Lonnie Thompson should also be contacted, since there is so much data from tropical glaciers that is not yet publicly available. In any case, I look forward to working with you on this.

Second, regarding ice core relevant for Holocene studies:

It would be ideal to include all of the Antarctic cores drilled so far: Dome B, Dome C, Vostok, Komsomolskaya, Byrd, etc. Much of the stable isotope data for these cores is already available at our "Ice Core Data Cooperative" web site. Valerie Masson, Jean Jouzel, myself and others recently submitted a paper comparing isotope data from all of these cores, and I should be able to get the data from her. Also at the Data Co-op site are data from the Canadian ice caps (we do not yet have Penny Ice Cap, but I can talk with David Fisher about this), Mount Logan, and from some temperate ice cores including Fremont Glacier. These data are better than commonly believed and may be useful.

I think that any Holocene climate compilation really needs chemistry and gas data as well as isotope data. Although chemical concentrations have not been measured on many of the cores, a very important data set that is missing from our current archive is the chemistry data from the Antarctic cores. All of the Taylor Dome chemistry data is available at www.sas.upenn.edu/~esteig/taylor.html but as far as I am aware there is no other chemistry data out there. It would be wonderful if you could convince Michel Legrand and colleagues to send these data to me, for inclusion on the Ice Core Data site, for both the Holocene the glacial periods.

All of the data that I currently have are available via the NOAA web server "International Ice Core Data Cooperative". The site also lists cores which exist but for which data are still needed. The direct link is:

<http://www.ngdc.noaa.gov/paleo/icecore/iicdc.html>

I apologize that the pages are not in very good order; most of my time when I had hoped to be working on this was devoted to the production of the GISP2/GRIP CDROM, which took considerably more effort than expected. I plan to begin improving those pages soon. Let me know if you have additional questions.

Warm regards,

Eric Steig

From: Tom Wigley <wigley@meeker.ucar.edu>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: CONFIDENTIAL: CRU scenarios
Date: Mon, 1 Nov 1999 14:15:36 -0700 (MST)
Cc: rwatson@worldbank.org

Dear Mike,

Thanks for your detailed response about your use of the SRES scenarios. I'm sure it will be useful to Bob Watson. I wish I could explain better what Bob's problem entails -- it is intensely political. My judgement is that, if I tell you more, then this will indirectly help Bob in answering the questions posed of him by Sensenbrenner; particularly should Bob need to get back to you. Please note that this is confidential information. Please note, too, that I am making my own judgement on this in the interest of clarifying a complex issue. I have not been authorized by Bob, or anyone associated with IPCC, to divulge this information.

The stated concern of Sensenbrenner is that the use of the SRES scenarios prior to their ratification might, in some way, jeopardize IPCC's "independence and objectivity". Sensenbrenner apparently uses as guidelines in making his judgement "IPCC's 'Principles' (as) approved in Vienna, Austria in October 1998" together with "June 11 and 28, 1999 letters" giving "Appendix A to the Principles, which is entitled 'Procedures for the Preparation, Review, Acceptance, Approval and Publication of IPCC Reports' (which was) approved ... in April 1999". Sensenbrenner implies that these documents "raise concerns about the use of preliminary IPCC material by Dr. Wigley and the Pew Center on Global Climate Change for non-IPCC purposes, apparently without IPCC sanction". He considers that "these issues (are) significant because they relate directly to the integrity of the IPCC process".

In my case, I bypassed the "IPCC process" by obtaining permission, in writing, from the 4 groups who produced the marker scenarios. I did not acknowledge the CIESIN web site. In your case, apparently, you did. The problem here is that this site stated very clearly that the data were "not for citation or quotation". Did you take notice of this?

My view is, and has always been, that contributors to such data sets or distribution sites do not give up the intellectual property rights to their own data. They could do so, of course, by signing appropriate legal/copyright documents; but I have never done this, nor, as far as I

know, has anyone who contributed to the CIESIN site. This is why I went to the individual authors in order to obtain permission to use their data in my Pew report. I hope you can see that there is an important difference between what you did and what I did. At face value, it would appear that you have ignored the clearly-stated message that the CIESIN site data were "not for citation or quotation". (More on this point below.)

You refer back to the July 1998 Bureau meeting agreeing that the preliminary SRES scenarios (in your words) "could, and should, be used by scientists". From my reading of the background material, this is subtly wrong -- the Bureau only agreed that the data could be used by "the GCM modeling community". As it happens, I am part of that community, and I acted as the interface between the scenarios and the rest of the NCAR GCM team, providing SRES data to them in a form that could be used for our GCM runs. I do not think you can claim to have filled this particular and quite specific role in your work.

However, there are some interesting subtleties here that, I think, vindicate your position. The issue is what is meant by the "GCM modeling community". In my view, anyone who uses GCM data either to provide data sets to the impacts community or to carry out diagnostic studies directly to improve GCMs is part of this community. (Note that this does **not** allow one to include the impacts modelers as part of the GCM community.) The two stated aspects are precisely what you do. Furthermore, SCENGEN (which I presume you have used in your work) makes direct use of GCMs in order to produce spatially-specific climate results based on any given emissions scenarios (including the SRES scenarios). The SCENGEN method is simply an alternative way of translating emissions scenarios into GCM-based and GCM-type output. In my view, anyone using the SRES scenarios in the development of SCENGEN, or applying SCENGEN to produce spatially-specific climate results for dissemination to others, must be included as part of the "GCM modeling community" referred to in the Bureau's agreement regarding use of the SRES scenarios. You may have interpreted the Bureau's statements even more broadly than this -- but this is of no consequence, since what you have done also falls squarely within the more restricted interpretation that I have given above.

Nevertheless, I think it would have been wiser for you to have done things the way I did, rather than to have acknowledged the CIESIN site as your source.

The next issue, raised in your email, concerns the DDC. I have not looked

at this site, but I presume it duplicates what was on the CIESIN site. If so, then its use (and the use of the preliminary SRES data) must be controlled by the rules under which the DDC was set up and operates. The key questions, therefore, are:

- (1) Do these rules allow the use of these data by anyone?
- (2) Do the SRES data, as it appears on this site, include the statement "not for citation or quotation"?
- (3) Does this make moot the whole issue of the use of the SRES scenarios?

In other words, if these data are available to all and sundry, with no restrictions, through DDC, then no one can complain about their use. (Although, in your case, since you acknowledged CIESIN rather than DDC, you may still be subject to criticism.)

What this could amount to is a loophole in the IPCC rules of procedure. Sensenbrenner might then argue that this loophole should be closed by clarifying and tightening the rules for the DDC.

The bottom line is that I think you have done things in a perfectly legitimate way. Even acknowledging the CIESIN site is legitimate, since your primary application was in the production of climate change scenarios as a member of the "GCM modeling community" as I believe this community should be defined. You have then distributed these results to the global climate impacts community who, in turn, will be feeding their results back into the IPCC process through WGII. Your chosen method of distribution (especially the WWF pathway) might be judged as less than ideal; but I cannot see anything that you have done that goes explicitly or implicitly against IPCC regulations.

Below the bottom line is the concern expressed by Sensenbrenner that these actions (yours and mine) might, in some way, have undermined the "integrity of the IPCC process". It would be interesting to hear from Sensenbrenner just how he thinks that might have happened. All we have done is distribute credible and defensible scientific information. If this information were to be in conflict with the currently best-available science, this might be an issue of concern -- but it is not. The more such credible scientific information is distributed to the community, particularly when it is presented in an easily-read, non-technical yet authoritative way, the better. I can see no way that this can distort the IPCC process. Some people, however, appear to think that it might. (A less kind interpretation might be that they are just trying to slow down the process by tying it up in legal and procedural knots -- but I have no

evidence that this is what they are trying to do.)

I hope you can see from the above quotes and somewhat convoluted arguments what a legal and political minefield this is. These sorts of issues do not seem to arise outside of the USA; but here they take on an enormous importance. One must tread very cautiously.

Cheers,

Tom

On Sat, 30 Oct 1999, Mike Hulme wrote:

- > Bob,
- >
- > You will have seen Tom Wigley's email asking me about the climate scenarios
- > I prepared for WWF and which were distributed 2 weeks ago. I have just got
- > back from a trip away and am concerned that *you* are concerned, hence my
- > immediate reply.
- >
- > These CRU/WWF regional/national scenarios *do* use the preliminary SRES98
- > emissions scenarios that are posted on the CIESIN and IPCC DDC web sites.
- > The CRU/WWF reports state that preliminary emissions scenarios sre used,
- > they acknowledge the CIESIN source of these emissions, and they make it
- > clear that the derived climate scenarios are the work and responsibility of
- > the authors alone.
- >
- > Maybe some background would help explain why I do not think that from my
- > perspective there is cause for concern (although I am aware of the
- > criticism the SRES report has increasingly been receiving and that the
- > issues are bigger than I may realise):
- >
- > _____
- >
- > July '98: IPCC Bureau meeting agreed that the preliminary SRES emissions
- > scenarios could, and should, be used by scientists in their unapproved
- > form.

>
> Dec '98: the above was reiterated to WGI scientists at the Paris LA
> meeting. In particular, it was recognised that SAR science would have to
> be used in the interim (i.e., next 12-18 months) to generate the climatic
> (and consequently impact) implications of the SRES emissions.
>
> Jan '99: the SRES Open Process ended. The IPCC DDC placed the preliminary
> SRES98 emissions scenarios on the open DDC web site as requested by the
> IPCC Task Group on Climate Scenarios for Impact Assessment (Chair Martin
> Parry). The objective of the DDC right from its original 1997 commission
> was to provide timely access to emissions scenarios, observed climate
> datasets and new GCM experiments (all of which would be assessed in the
> IPCC TAR), thus enabling impact scientists worldwide to construct and apply
> consistent climate scenarios (this information has already been used by
> several 100 scientists, including many in developing countries). Only in
> this way would it be at all possible for WGII to have access to
> impact/adaptation science that was in any way consistent with the WGIII
> (SRES emissions) and WGI (climate modelling) material. The placing of the
> SRES98 emissions on the DDC web site was widely discussed in the TG CIA and
> was publicised at the time to the research community using the DDC,
> including through the A4-flier advertising the DDC that was sent to the WGI
> (and WGII?) mailing list.
>
> Feb '99: Hulme&Carter used the preliminary SRES98 emissions (and other DDC
> products) to develop climate scenarios
> for the European Union as part of the EU-funded ACACIA assessment
> (unrelated to Tom's US-based ACACIA). The approach
> I took in using the SRES98 emissions for the ACACIA climate scenarios was
> *my* decision and was not part of any IPCC activity. The ACACIA climate
> scenarios, and indeed entire EU ACACIA impacts assessment, have been widely
> reviewed within Europe, and are part of the draft report presented to
> Brussels last month. They will published in their final form in June 2000.
> This EU-ACACIA activity has done in my view *exactly* what the DDC was
> intended to do, namely allow impact scientists to generate results using
> consistent scenarios and assumptions; these results provide the raw
> material for IPCC LAs to assess in their TAR chapters!
>
> My approach for converting the preliminary SRES98 emissions into climate
> scenarios is also being used in many other EU and UK-funded impact research
> programmes and is generating a variety of scientific reports and papers -
> several of the latter are under peer-review at the moment and may be
> citeable in time for the 2nd-order WGII drafts.
>

> ***Is an apology needed for this activity? If so, then I and others on the
> IPCC TGCIA totally misunderstood the brief of the DDC and the intent of the
> July 98 and Dec. 98 IPCC decisions.***
>
> May '99: WWF commissioned me to prepare a set of national/regional climate
> scenarios for them to launch in October 1999. It seemed entirely
> appropriate and legitimate to me to use the same method I had adopted for
> EU-ACACIA to generate these WWF scenarios.
>
> June '99: Tom's Pew Report was published using SRES98 emissions in a not
> dissimilar way to me (i.e., using them to drive a simple climate model
> based on SAR science).
>
> July '99: following some controversy over the Pew Report, there was an
> email circular from WGI TSU (Griggs) reminding LAs that there was 'active
> encouragement' from IPCC for scientists to use the preliminary SRES98
> emissions in modelling work. The conditions were that it should be stated
> that they were unapproved by IPCC (i.e, preliminary) and that work using
> them should ideally be peer-reviewed and published. Tom Wigley followed-up
> on this circular by explaining *his* use of SRES98 in the Pew Report, the
> conditions he met and his justification for using them. I noted this
> correspondence at the time and did not feel that my use of SRES98 emissions
> in my WWF work was out of order.
>
> Oct '99: the 15 sets of CRU/WWF regional/national scenarios were published
> and widely distributed by WWF. These leaflets state that 'preliminary IPCC
> emissions scenarios' are used, acknowledge the source of these emissions as
> the CIESIN site, and make clear that the climate scenarios are the work of
> the authors alone and no other organisation. Furthermore, the approach I
> have taken (which I originally designed back in December 1998) has been
> subject to a diversity of peer-review activities, and will shortly be
> published.
> _____
>
> Sorry for making this a lengthy reply, but it seems best to spell out the
> history and my thinking to avoid any room for misunderstanding. In
> summary, the only two grounds on which I think I could be criticised for
> using the SRES98 emissions in my CRU/WWF climate scenarios are if:
>
> 1) the IPCC DDC was wrong to put the SRES98 emissions on its web site back
> in January 1999 and to publicise its purpose in doing so. If we *were*
> wrong, then this error goes back to January 1999 and the TGCIA
> fundamentally misunderstood its brief.

> 2) the pronouncements of the IPCC in July 1998 and December 1998 were
> intended to apply *only* to scientists who had a formal role in the IPCC
> and that the SRES98 emissions could only be used for 'official' IPCC
> scientific activities whatever these may be. This would draw a very
> dubious line between science done for IPCC and science done 'not for IPCC'.
> IPCC's brief is to assess *all*, done by no matter whom or for what
> purpose.

>
> Best wishes,

>
> Mike

>
> Dr Mike Hulme
> Reader, Climatic Research Unit
> School of Environmental Sciences
> University of East Anglia
> Norwich NR4 7TJ
> (tel: +44 1603 593162; fax: +44 1603 507784)
> (email: m.hulme@uea.ac.uk)
> (web: <http://www.cru.uea.ac.uk/~mikeh>)

> -----

>> From: Tom Wigley <wigley@meeker.ucar.edu>
>> To: Mike Hulme <m.hulme@uea.ac.uk>
>> Cc: Robert Watson <rwatson@worldbank.org>
>> Subject: CONFIDENTIAL: CRU scenarios
>> Date: 27 October 1999 19:02

>>
>> ****In strictest confidence****

>>
>> Dear Mike,

>>
>> Bob Watson contacted me last week asking about some climate results that
>> he apparently saw on the CRU and/or WWF web pages. The CRU web site
>> states that you have produced (and already distributed) a set of regional
>> scenario leaflets based on "new ghg emissions scenarios", which I think
>> is
>> what Bob may be concerned about.

>>
>> I hope that "new" does not refer to the SRES scenarios. You may recall
>> that, when I was in CRU, I showed you, in confidence, a letter from F.
>> James Sensenbrenner, chairman of the U.S. House of Representatives
>> Committee on Science, criticizing IPCC for "allowing" me to use these

>> scenarios in my Pew Report.
 >>
 >> Unfortunately, this issue is not going away, and any further perceived
 >> "misuse" of the SRES scenarios prior to their IPCC ratification would
 >> exacerbate the problem considerably.
 >>
 >> I do hope, therefore, that you have *not* used the SRES scenarios. I
 >> expect not, since I explained the potential problems to you in July.
 >> Please reassure me -- and Bob.
 >>
 >> If, by chance, you *have* used the SRES scenarios, but not yet
 > distributed
 >> the WWF leaflets, I urge you to hold fire until you have contacted Bob.
 >>
 >> Best wishes,
 >>
 >>
 >> Tom

>> *****
 >> *Tom M.L. Wigley *
 >> *Senior Scientist *
 >> *National Center for Atmospheric Research *
 >> *P.O. Box 3000 *
 >> *Boulder, CO 80307-3000 *
 >> *USA *
 >> *Phone: 303-497-2690 *
 >> *Fax: 303-497-2699 *
 >> *E-mail: wigley@ucar.edu *
 >> *****

Tom M.L. Wigley
 Senior Scientist
 National Center for Atmospheric Research
 P.O. Box 3000

Boulder, CO 80307-3000

USA

Phone: 303-497-2690

Fax: 303-497-2699

E-mail: wigley@ucar.edu

Web: <http://www.acacia.ucar.edu>

From: Mike Hulme <m.hulme@uea.ac.uk>
To: wigley
Subject: MAGICC/SCENGEN
Date: Fri Nov 12 18:19:52 1999
Cc: s.raper,m.salmon,m.hulme,barrow

Tom,

Sorry I couldn't say goodbye - I was actually on the phone to Bo Lim at the time. I also wanted to ask you about your views on the UK national climate change centre, but this can wait until later.

Anyway, about MAGICC/SCENGEN Workbook I think we agreed the following things for this UNDP version

- a select number of emissions scenarios, IS92, SRES98, 550 and 750 stabilisation cases, some Kyoto variants (perhaps from IS92a,e,d reference), and 1-2 others you may recommend. I would be keen to use your *.gas files if that's OK, even though I have some of my own. You may have done the SO2 into regions, which I haven't. Could you send me a selection?
- you would think about how to handle the CH4 adjustment to ensure SAR replicability across the emissions scenarios. This may require a tweak in the MAGICC code which Mike will have to recompile.
- we should aim to reproduce the SAR results as closely as possible in this version, e.g. use 6.37Wm⁻² rather than 5.5, and not use Prather's methane concentrations (an Annex in the Workbook will explain this).
- the variable upwelling rate will be hard-wired. Choices will remain for the Dn80s, climate sensitivity and aerosol forcing.
- SCENGEN will have the new DDC patterns included and we will switch off the buttons for the older 2xCO2 patterns.
- SCENGEN will output values over land and ocean.
- the Help Screens will need updating. I will attempt this and then check them all with you to make sure you agree.

The only problem I can foresee is that the 2.32 version that Mike and you produced in the summer corrected the aerosol calculations and also used Prather's methane concentrations. If we now want a version with correct aerosol concentrations and IPCC SAR Chapter 6 CH4 concentrations, *plus* a CH4 tweak to handle the ad hoc adjustment, then Mike Salmon will need a new and unique FORTRAN version of MAGICC. Am I right?

I have agreed with Bo Lim to get a first draft of the Workbook by 17 December, but the final version and all the CDs will not be agreed until February 2000.

Have I missed anything?

Regards,

Mike

From: Phil Jones <p.jones@uea.ac.uk>
To: ray bradley <rbradley@geo.umass.edu>,mann@virginia.edu, mhughes@ltrr.arizona.edu
Subject: Diagram for WMO Statement
Date: Tue, 16 Nov 1999 13:31:15 +0000
Cc: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk

Dear Ray, Mike and Malcolm,

Once Tim's got a diagram here we'll send that either later today or first thing tomorrow.

I've just completed Mike's Nature trick of adding in the real temps to each series for the last 20 years (ie from 1981 onwards) amd from 1961 for Keith's to hide the decline. Mike's series got the annual land and marine values while the other two got April-Sept for NH land N of 20N. The latter two are real for 1999, while the estimate for 1999 for NH combined is +0.44C wrt 61-90. The Global estimate for 1999 with data through Oct is +0.35C cf. 0.57 for 1998.

Thanks for the comments, Ray.

Cheers
Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>
To: "F. Ian Woodward" <F.I.Woodward@Sheffield.ac.uk>, "Nigel W. Arnell"
<N.W.Arnell@soton.ac.uk>, Alberte Bondeau <Alberte.Bondeau@pik-
potsdam.de>, Ben Smith <Ben.Smith@planteco.lu.se>, Colin Prentice
<Colin.Prentice@bgc-jena.mpg.de>, Harald Bugmann <bugmann@waho.ethz.ch>,
Jos@ Manuel Moreno9yZW5v <jmmoreno@greco.cc-to.uclm.es>, Mark Rounsevell
<rrousevell@geog.ucl.ac.be>, Martin Sykes
<vxt_masy@luecology.ecol.lu.se>, Mike Hulme <m.hulme@uea.ac.uk>, Pete
Smith <pete.smith@bbsrc.ac.uk>, Pierre Friedlingstein
<pierre@lsce.saclay.cea.fr>, Riccardo Valentini <rik@unitus.it>, Rik
Leemans <Rik.Leemans@rivm.nl>, Sandra Lavorel <lavorel@cefe.cnrs-mop.fr>,
Sergey Venevski <Sergey.Venevski@pik-potsdam.de>, Stephen Sitch
<Stephen.Sitch@pik-potsdam.de>, Torben Christensen
<torben.christensen@planteco.lu.se>, Wolfgang Knorr <knorr@dkrz.de>,
Wolfgang Lucht <Wolfgang.Lucht@pik-potsdam.de>
Subject: A-TEAM Call is out
Date: Thu, 18 Nov 1999 14:33:21 +0100
Reply-to: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>

Dear colleagues,

you may already know it: the EU FP5 second call for proposals is out since today (<http://www.cordis.lu/eesd/calls/calls.htm>), as expected, and the deadline for submission is Feb 15.

The new call does indeed answer a question I have been wondering about when I heard from many first-call projects that they were asked to re-submit. The present call is only for the slots that were, last year, declared to be opened at this stage (not for the previous slots). Probably the re-invited proposals then still bid for the old money (or at least, I hope so).

There is however one important exception: "2.3.1 Mitigation and adaptation to global change". About this, the official document says "re-open ... because of the quality of proposal received in reply to the call of 20 March 1999". Further down, they point out that Kyoto really is tremendously important for the commission ("primary objective"), and then comes the following far-reaching sentence: "If one takes into account the time lag between the research results, the political decisions and the actual emission reduction it is evident that the year 2000 is the last opportunity for research to cover the remaining analytical gaps of priority." (da_pg2_en_199902.pdf, page 6). Tough!

This mail goes to all people I have currently listed as "likely participants in A-TEAM", although the group may either grow or shrink as the remaining time passes by (depending, among other things, on your inputs!).

The present state of development is that I have recently circulated another draft of our basic document among just a few of you, hoping for input to it REALLY SOON. On the basis of this, I intend to

- a) develop a better draft that will then be circulated to all of you,
- b) organize a small brainstorming meeting, hopefully before christmas, but again only with a small core group,
- c) by christmas, provide you with a roadmap for the remaining things to be done.

Best regards,

Wolfgang

Wolfgang Cramer
Department of Global Change and Natural Systems
Potsdam Institute for Climate Impact Research
PO Box 60 12 03, D-14412 Potsdam, Germany
Tel.: +49-331-288-2521, Fax: +49-331-288-2600
<mailto:Wolfgang.Cramer@pik-potsdam.de>

NOTE: IF YOU NEED TO SEND ATTACHMENTS TO ME, PLEASE:

- 1) avoid sending MS-Word *.doc files (send rtf instead)
 - 2) if the attachments exceed 500kB, contact me before sending anything
-

From: Mike Hulme <m.hulme@uea.ac.uk>
To: Simon.Shackley@umist.ac.uk
Subject: Re: industrial and commercial contacts
Date: Mon Jan 10 17:01:32 2000

Simon,

I have talked with Tim O'Riordan and others here today and Tim has a wealth of contacts he is prepared to help with. Four specific ones from Tim are:

- Charlotte Grezo, BP Fuel Options (possibly on the Assessment Panel. She is also on the ESRC Research Priorities Board), but someone Tim can easily talk with. There are others in BP Tim knows too.
- Richard Sykes, Head of Environment Division at Shell International
- Chris Laing, Managing Director, Laing Construction (also maybe someone at Bovis)
- ??, someone high-up in Unilever whose name escapes me.

And then Simon Gerrard here in our Risk Unit suggested the following personal contacts:

- ??, someone senior at AMEC Engineering in Yarmouth (involved with North Sea industry and wind energy)
- Richard Powell, Director of the East of England Development Board

You can add these to your list and I can ensure that Tim and Simon feed the right material through once finalised.

I will phone tomorrow re. the texts.

Cheers,

Mike

At 20:30 07/01/00 BST, you wrote:

>dear colleagues

>

>re: List of Industrial and Commercial Contacts to Elicit Support

>from for the Tyndall Centre

>

>This is the list so far. Our contact person is given in brackets

>afterwards. There is some discussion on whether we

>should restrict ourselves to board level contacts - hence Dlugolecki

>is not board level but highly knowledgeable about climate change.

>I think people such as that, who are well known for their climate

>change interests, are worth writing to for support. There may be

>less value in writing to lesser known personnel at a non-board level.

>

>SPRU has offered to elicit support from their energy programme

>sponsors which will help beef things up. (Frans: is the Alstom

>contact the same as Nick Jenkin's below? Also, do you have a BP

>Amoco contact? The name I've come up with is Paul Rutter, chief

>engineer, but he is not a personal contact]

>

>We could probably do with some more names from the financial sector.

>Does anyone know any investment bankers?

>

>Please send additional names as quickly as possible so we can

>finalise the list.

>

>I am sending a draft of the generic version of the letter eliciting

>support and the 2 page summary to Mike to look over. Then this can be

>used as a basis for letter writing by the Tyndall contact (the person

>in brackets).

>

>Mr Alan Wood CEO Siemens plc [Nick Jenkins]

>Mr Mike Hughes CE Midlands Electricity (Visiting Prof at UMIST) [Nick

>Jenkins]

>Mr Keith Taylor, Chairman and CEO of Esso UK (John

>Shepherd]

>Mr Brian Duckworth, Managing Director, Severn-Trent Water

>[Mike Hulme]

>Dr Jeremy Leggett, Director, Solar Century [Mike Hulme]

>Mr Brian Ford, Director of Quality, United Utilities plc [Simon

>Shackley]

>Dr Andrew Dlugolecki, CGU [Jean Palutikof]

>Dr Ted Ellis, VP Building Products, Pilkington plc [Simon Shackley]

>Mr Mervyn Pedalty, CEO, Cooperative Bank plc [Simon Shackley]

>

>

>Possibles:

>Mr John Loughhead, Technology Director ALSTOM [Nick Jenkins]

>Mr Edward Hyams, Managing Director Eastern Generation [Nick

>Jenkins]

>Dr David Parry, Director Power Technology Centre, Powergen

>[Nick Jenkins]

>Mike Townsend, Director, The Woodland Trust [Melvin

>Cannell]

>Mr Paul Rutter, BP Amoco [via Terry Lazenby, UMIST]

>

>With kind regards

>

>Simon Shackley

>

>

>

>

>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: stepan@ipae.uran.ru, ifor@krsk.infotel.ru, fritz.schweingruber@wsl.ch
Subject: EC contract proposal
Date: Thu Jan 13 17:31:47 2000
Cc: t.osborn@uea.ac.uk

Hi Stepan and Eugene (Eugene are you getting these messages?)

You will have the first idea of things now and soon the first forms will come which must be filled in and signed and stamped and returned here by FAX and as soon as possible by REAL mail. The original forms must be submitted from here in February. This message is to reiterate that the reviewing process this time is going to look very carefully at the breakdown of costs in relation to precise tasks. There is even a section of the form that asks for proportional costs associated with individual deliverables. Therefore it is important to specify (at least for the sake of the plan) precisely what work can be done and the person hour costs, materials, travel, fieldwork, equipment (corers, durable equipment like computer ,GPS, etc: consumable costs like xray film etc.etc.) . I need you to think in terms of intensive sampling of modern and sub-fossil wood with the emphasis on major contributions to extending the network in Russia both ringwidth (in Ekaterinburg) and a major part of the densitometry , perhaps of Russian and non-Russian samples(?) (in Ekaterinburg). THIS IS NOT TO SAY I AM ASSUMING YOU ARE ONLY DATA PROVIDERS . I do not look on you in this way. It is simply that I have to make a strong @SPECIAL CASE@ for your both being partners and the relatively large funds that I have suggested must be convincingly justified. Your involvement is crucial on the scientific side and I will emphasise this strongly. But it is also important to display to referees what the money will go on. Hence you need to suggest various options to me in terms of possible sampling work, laboratory work and analysis and cost out these different options to cover different possible plans. We will then sort out an optimum one . You must budget realistically for travel, fieldwork travel and equipment - which I believe are expensive. Also note our earlier message as regards travel to Europe. I would very much appreciate help with up to date information on state of the art of the Russian data for background, potential of new areas or your ideas of where best to concentrate updating work.

In both Yamal and Taimyr , the continued work on the long chronologies to greatly increase sample numbers is still very high on my list of priorities and the work Stepan (and Rashit) are doing to reconstruct tree-line changes on a detailed resolution is very very important. So please try to think about the details of new sampling sites(need bigger sample numbers with different age trees at each to look at age-dependent growth changes); best areas needing updating; subfossil continuation; real numbers for different cost options and start to interact with me and Tim (and Fritz) re the possible distribution of densitometry work. Finally, Eugene, I think your comments on the ring structure and using input from simulations and model (GCM) data are important. Can we factor in some exploratory work on this or is it better to do it as part of a separate proposal - I have two more in mind in the coming months (one to NERC in UK and one to the Leverhulme Foundation - more about these later).

for now that better be all

best wishes

Keith (p.s please copy all replies to Tim)

From: "Sujata Gupta" <suajatag@teri.res.in>
To: <m.hulme@uea.ac.uk>
Subject: Re: Tyndall Centre bid
Date: Wed, 16 Feb 2000 09:54:12 +0530
Cc: <ritu.kumar@commonwealth.int >, "R K Pachauri" <pachauri@teri.res.in>

Dear Mike

Thank you for sending the outline bid submitted last October. After reviewing the document, my colleagues and I were of the view that TERI should go non-exclusive. Our primary interest is to be part of the project and given that we (TERI) would have the role of an affiliate in both the bids, it was decided that we go non-exclusive.

We understand that the outline bid is confidential and I can assure you that it will not be shared with anyone outside the concerned colleagues at TERI. Also, I assure you of all possible support TERI can provide in developing the final bid. We look forward to a fruitful association with you on the project.

Wishing you all the best in securing the bid.

Kind regards

Sujata

Sujata Gupta Ph.D.

Fellow and Dean

Policy Analysis Division

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>>> Mike Hulme <m.hulme@uea.ac.uk> 02/12/00 11:56PM >>>

Dear Sujata,

I attach a copy of our outline bid from last October - it is now evolving rapidly of course in preparation for final submission. This gives you a quick idea about our Consortium and plans. You will also see the names and

institutes of our partners. May I re-iterate that this document is

confidential and must not be disclosed to anyone outside your immediate colleagues in TERI.

TERI was *not* listed as a formal co-applicant (non-UK institutions are not eligible to be formal co-applicants), but was listed as an 'affiliated organisation' along with about 10 others here in the UK. We would propose to do the same in the final bid, but say a little bit more about where and how TERI would interact with us were we to win the Centre.

If you decide to remain exclusively with our bid, then I will send you the first draft of our final submission during the next week - this will indicate more details about our research programmes and where TERI may be seen to interact with us as a key overseas collaborator.

However, if you decide to join with both bids - Imperial and UEA - then we will simply continue to list you as a collaborator, but we could not then agree to any further interaction over the next 2 weeks.

Best Regards,

Mike

At 10:45 10/02/00 +0530, you wrote:

>Dear Mike

>

>Thank you for your email. I appreciate your understanding of our position. TERI is essentially interested in working on the project. I can assure you that we will not disclose any information provided by you to the other finalist or anyone else for that matter and maintain strict confidentiality.

>

>However, I did not receive the original bid document or an outline of the proposal. We are not clear if TERI has been listed as a partner up-front or has been mentioned as an associate. I would greatly appreciate it if you could let me know TERI's status in the original document. This will help in our taking a decision on the exclusivity front, as yet we are still debating on the matter and have not reverted to the Imperial team. Also, who are the other members of the team headed by you.

>

>We look forward to working with you and hope we are able to reach a decision which is mutually beneficial.

>

>Best wishes

>

>Sujata
>
>
>
>Sujata Gupta Ph.D.
>Fellow and Dean
>Policy Analysis Division
>*****
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>Web www.teriin.org
>
>>> Mike Hulme <m.hulme@uea.ac.uk> 02/08/00 01:49AM >>>
>Dear Sujata,
>
>I have consulted with colleagues in our Consortium and we consider the
>following to be the position

>
>- we clearly would prefer TERI to affiliate to only one of the two
>finalists, and obviously we prefer that one to be our bid. This is
>especially the case since we made our initial approach to you last
>September when there were still seven bids in the making; no-one else
>approached you at that stage and therefore we feel we have some
preference
>through prior approach.
>
>- we recognise that *you* may now consider it in your interest to
affiliate
>to both finalists to cover yourselves either way (although *we* consider
>there are strong grounds for you not to do so). This is your choice of
>course, although were you to do this then I must point out the following
>two consequences:
>
>a) since I believe I sent you last October/November a copy of our
outline
>bid for the Centre I would need to insist that you do not divulge the
>contents of this outline to Imperial College. This is clearly a case of
>professional integrity which we are sure you understand.
>
>b) if you indicate that you are also joining with Imperial then this
>effectively precludes any further dialogue between us over the remaining
3
>weeks before submission. All that we would be able to do would be to
name

>you and your expertise in our submission rather than engage you
>interactively in shaping 1-2 of our ideas (which was my original
intention
>as our final bid shapes up).
>
>Please let me know how you wish to proceed - either way, I look forward
to
>a fruitful association between us in the event of our bid succeeding
with
>the UK Research Councils.
>
>Best regards,
>
>Mike
>
>At 16:00 01/02/00 +0530, you wrote:
>>Dear Mike,
>>
>>TERI has a presence in London as of 25 January. My colleague Dr Ritu
Kumar
>there has been approached by the consortia led by Imperial College
>>for TERI to join them. I am writing to explore the possibility of TERI
>joining both consortia on a non-exclusive basis. This would of course
imply
>that we do not share/participate in the preparation of the bid. Any
inputs
>provided by TERI would be common to both consortia, unless it was in
>response to a specific request by a particular partner.
>>
>>As we have committed to you first, we will revert to Imperial College
for
>a non-exclusive tie-up, only after discussing the matter with you.
>>
>>I am copying this email to my colleague Dr Kumar.
>>
>>Looking forward to hearing from you.
>>
>>Regards
>>
>>Sujata
>>
>>
>>
>>
>>
>>
>>Sujata Gupta Ph.D.
>>Fellow and Dean
>>Policy Analysis Division
>>*****
*
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>>Web www.teriin.org
>>
>>>> Mike Hulme <m.hulme@uea.ac.uk> 01/19/00 02:52PM >>>
>>Thank you Sujata I will keep you informed about our needs for
>>bidding for the UK Climate Change Centre.
>>
>>And it *was* me that you had a conversation with in Canberra about
>>reviewers for Chapter 3 on scenarios. I will forward your suggestion
on to
>>the TSU II.
>>
>>Regards,
>>
>>Mike
>>
>>
>>At 11:56 19/01/00 +0530, you wrote:
>>>Dear Dr Hulme
>>>
>>>TERI will be happy to provide sole support to the consortium led by
you
>>and UEA. I was on travel and hence could not respond earlier. Please
let
>>me know if we can assist in any way in the preparation of the bid.
>>>
>>>If I recollect we had a discussion on a possible reviewer for the
>>scenarios chapter from India who was thus far not involved with the
IPCC
>>process. I can suggest the name of Dr Shreekant Gupta at the Delhi
School
>>of Economics, New Delhi. It is quite possible that I had this
discussion
>>with Tom Downing. Please let me know if I am communicating to the wrong
>>person on this matter.
>>>
>>>Best wishes for the new year
>>>
>>>Sujata
>>>
>>>
>>>
>>>Sujata Gupta Ph.D.
>>>Fellow and Dean
>>>Policy Analysis Division

>>>*****
**

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>>>*****
**

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>>>

>>>>> Mike Hulme <m.hulme@uea.ac.uk> 01/05/00 06:54PM >>>

>>>Dear Colleague,

>>>

>>>Thank you very much for your support for our bid to run the new UK Climate

>>>Change Centre being established by three of our national research councils.

>>> We have heard that just two of the seven outline bids have been invited to

>>>submit detailed proposals and that the Consortium led by UEA is one of

>>>these two. Final bids are required by 29th February. The UEA-led bid

>>>proposes the new Centre to be called the Tyndall Centre for Climate Change

>>>Research (named after the 19th century British physicist who experimented

>>>with the radiative properties of greenhouse gases, John Tyndall).

>>>

>>>Assuming you are happy to continue sole support for our initiative, and on

>>>the undertaking that you do not disclose our outline bid to other parties

>>>who may be aligned with the other finalist (a Consortium led by Imperial

>>>College and involving the Environmental Change Institute at Oxford and the

>>>U. Edinburgh), then I will send you a copy of our outline proposal.

>>>

>>>There are a number of aspects of this outline bid that we will change and

>>>develop before 29th Feb. and it may be that I am back in contact with you

>>>to ask for some additional text of support about some concrete ways the UK

>>>Tyndall Centre could collaborate with your organisation.

>>>

>>>We would also, of course, welcome any suggestions you may have about such

>>>future collaboration.
>>>
>>>Best wishes for the New Year,
>>>
>>>Mike
>>>
>>>
>>>*****

>>>****
>>>Dr Mike Hulme
>>>Reader in Climatology tel: +44 1603 593162
>>>Climatic Research Unit fax: +44 1603 507784
>>>School of Environmental Science email: m.hulme@uea.ac.uk
>>>University of East Anglia web site:
>http://www.cru.uea.ac.uk/~mikeh/
>>>Norwich NR4 7TJ
>>>*****

>>>****
>>> The estimated annual mean temperature in Central England for 1999 is
>+1.16
>>>degC above the 1961-90 average, the warmest year recorded in
>341
>>>years
>>>

>>> The estimated global-mean surface air temperature anomaly for
>1999 is
>>> +0.33 deg C above the 1961-90 average, the 5th warmest year yet
>recorded
>>>*****

>>>****
>>> Neither of these estimates have yet been confirmed
>>> *****
>>>
>>>
>>>*****

>>Dr Mike Hulme
>>Reader in Climatology tel: +44 1603 593162
>>Climatic Research Unit fax: +44 1603 507784
>>School of Environmental Science email: m.hulme@uea.ac.uk
>>University of East Anglia web site:
>http://www.cru.uea.ac.uk/~mikeh/
>>Norwich NR4 7TJ
>>*****

>>The unconfirmed annual mean temperature in Central England for 1999 was
>+1.16

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>> *****
>> The unconfirmed global-mean surface air temperature anomaly for 1999 was
>> +0.33 deg C above the 1961-90 average, the 5th warmest year yet recorded
>> *****

>>
>>
> *****

>Dr Mike Hulme
>Reader in Climatology tel: +44 1603 593162
>Climatic Research Unit fax: +44 1603 507784
>School of Environmental Science email: m.hulme@uea.ac.uk
>University of East Anglia web site:
<http://www.cru.uea.ac.uk/~mikeh/>
>Norwich NR4 7TJ
> *****

>The unconfirmed annual mean temperature in Central England for 1999 was +1.16
> degC above the 1961-90 average, the warmest year recorded in 341 years
> *****
> The unconfirmed global-mean surface air temperature anomaly for 1999 was
> +0.33 deg C above the 1961-90 average, the 5th warmest year yet recorded
> *****

>
>

From: John Shepherd <John.G.Shepherd@soc.soton.ac.uk>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: BGS, Esso, & CV for Tyndall bid
Date: Thu, 24 Feb 2000 17:37:30 +0000

Mike

BGS are now on board, so please leave them in the text : I have drafted a letter for David Falvey to sign and sent it. I hope we shall get it back in time...

The Esso (Exxon-Mobil) situation is still promising, but they're having to get clearance from HQ in the USA (my best contact retired (with cancer) just a few weeks ago, so we've had to work around the new CE, to whom all this is news...). They know the deadline and will do their best for us.

Finally, my short informal CV is attached, as requested.

Hope the drafting is coming together well.

John

Attachment Converted: "c:\eudora\attach\JGS_CV_informal.doc"

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
Subject: Re: newest reconstruction
Date: Mon Feb 28 13:50:17 2000
Cc: k.briffa@uea, t.osborn@uea

At 11:56 25/02/00 -0500, you wrote:

>I need your newest northern hemisphere density-based tree-ring
reconstruction
>and appropriate reference for updating IPCC. Please send in ASCII format
as
>soon as possible so we can incorporate. I hope all is well. Thanks,

Hi Mike

Keith asked me to get back to you on this. The reconstruction is the same as the one I sent on the 5th October 1999, but I'm sending it again in case that e-mail isn't handy. The reconstruction has now been published, in the following paper:

Briffa K.R. (2000) Annual climate variability in the Holocene: interpreting the message of ancient trees. Quaternary Science Reviews 19, 87-105.

This paper does not, however, give full details about how the reconstruction was obtained. The details are not yet published, but will soon be submitted:

Briffa KR, Osborn TJ, Schweingruber FH, Harris IC, Jones PD, Shiyatov SG and Vaganov EA (2000) Low-frequency temperature variations from a northern tree-ring density network. In preparation (to be submitted to Journal of Geophysical Research).

Details about the file I'm sending you (repeated from 5th Oct 99):

The data are attached to this e-mail. They go from 1402 to 1994, although we usually stop the series in 1960 because of the recent non-temperature signal that is superimposed on the tree-ring data that we use. I haven't put a 40-yr smoothing through them - I thought it best if you were to do this to ensure the same filter was used for all curves. The data I've sent are calibrated over the period 1881-1960 against the instrumental Apr-Sep temperatures averaged over all land grid boxes (that have observed data) that are north of 20N. As such, the mean of our reconstruction over 1881-1960 matches the mean of the observed target series over the same period. Since the observed series consists of degrees C anomalies wrt to 1961-90, we say that the reconstructed series also represents degrees C anomalies wrt to 1961-90.

(I've already truncated the series at 1960 because of the problems with the recent period.)

Best regards

Tim

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Frank Oldfield <frank.oldfield@pages.unibe.ch>
Subject: Re: PAGES QSR volume
Date: Thu Mar 2 01:12:02 2000
Cc: matti.saarnisto@gsf.fi, brigham-grette@geo.umass.edu,
D.Jewson@ulst.ac.uk, keith.alverson@pages.unibe.ch,
fritz.schweingruber@wsl.ch

Hi Frank

I have two names - one of which you know well. First , I strongly urge that one copy be sent to

Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia

This is the home of the Laboratory of Dendrochronology , headed by Dr. Stepan G. Shiyatov and I would suggest you consign the book to him, or through him , to a genearl library if one exists.

e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

I know they have very limited resources but they will make real use of the volume . They are genuinely active and the work they do is truly 'world class'. You will remember also that one of their younger scientists (Rashit Hantemirov) won a prize in London at the Open Science meeting for his poster on the long Yamal chronology. This group gets my first and strongest vote.

My other suggestion is to send one to Eugene Vaganov's Institute of Forest.

They are not so strapped for resources as the Ekaterinburg lab. but they are large and have many active areas of research and the book would get a wide audience.

Eugene's email is
ifor@krsk.infotel.ru

Then there is the question of getting them there . The post is not reliable. You might send them to Fritz Schweingruber's laboratory from where they could be picked up or carried to Russia ?

Hope this helps
best wishes

Keith

e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

At 12:58 PM 3/2/00 +0100, Frank Oldfield wrote:

>Dear Keith, Julie, Matti and David,

>

>We are compiling a list of people and/or institutions in the former USSR
>to

>whom we should send FREE copies of the PAGES Open Science Meeting
>Special

>issue of Quaternary Science Reviews. For this, we need some help and
>advice

>in the way of key addresses and contacts. Where it seems best to send
>the

>book to a library we'd quite like to inform at least one key academic in
>the Institution that we are doing this. Where we are sending to an
>individual, we need to be able to trust in a degree of collegiality and
>we

>shall indicate that we want to be sure the book will be made as widely
>available as possible. We do not anticipate being able to send more than
>10

>or so copies for free; others may be available at a reduced rate at the
>end

>of the year. This means a selective and carefully compiled 'hit list' is
>required.

>

>Over to you - we need your help.

>

>Many thanks,

>

>Frank

>

>

>_____

>Frank Oldfield

>

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><http://www.pages.unibe.ch/pages.html>

>

>

>

From: Phil Jones <p.jones@uea.ac.uk>

To: Shaopeng Huang <shaopeng@geo.lsa.umich.edu>, hpollack@geo.lsa.umich.edu

Subject: Nature paper and beyond

Date: Fri, 03 Mar 2000 13:04:24 +0000

Cc: mann@multiproxy.evsc.virginia.edu, tom@ocean.tamu.edu, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

Dear Shaopeng and Henry,

First, congratulations on the Nature paper. Can you send me some reprints when you get them ?

I was at a meeting this week with Tom Crowley and we were discussing ways to reconcile the high-freq proxies with your borehole data. Here are a couple of our thoughts. Involving Mike Mann and others here in CRU, as they all have an input.

1. I've shown that the borehole data in Europe agree well with the long instrumental data in both the UK and Europe. The biggest differences/problems seem to come with the North American borehole data, which show the 16/17/18th data much cooler than the European/Asian/African data in the 16/17th century. I'm still reminded by the potential effects of land-use changes, principally in the eastern US, which could be making your North American series too cool. I realise you've taken great care with the selection, but this is a nagging doubt and will be picked up by the few skeptics trying to divide us all about the course of change over the last millennium. Is it possible to subdivide the North American borehole data into regions where we can be confident of no land-use changes (possibly and thinking aloud say Canada and the western US and Alaska) ? The aim of this (possibly joint work) is to try and reconcile the low- and high-freq proxies. Tom Crowley has a series for the NH where he's combined about 20 series (a few of which are in Mike's and the series we've produced here but he has over half the series from less-well resolved proxies - shallow marine and lake sediments) and he gets something very similar to Mike and CRU.

2. As all our (Mike, Tom and CRU) all show that the first few centuries of the millennium were cooler than the 20th century, we will come in for some flak from the skeptics saying we're wrong because everyone knows it was warmer in the Medieval period. We can show why we believe we are correct with independent data from glacial advances and even slower responding proxies, however, what are the chances of putting together a group of a very few borhole series that are deep enough to get the last 1000 years. Basically trying to head off criticisms of the IPCC chapter, but good

science in that we will be rewriting people's perceived wisdom about the course of temperature change over the past millennium. It is important as studies of the millennium will help to show that the levels of natural variability from models are reasonable. Tom has run his EBM with current best estimates of past forcing (Be-10 as a proxy for solar output and Alan Robock's ice core volcanic index) and this produces a series similar to all series of the last 1000 years.

The above is just ideas of how we, as a group, could/should try and reduce criticisms etc over the next year or so. Nothing is sacred. Your North American borehole series could be correct as it is annual and most of the high-freq proxy series respond mainly to summer variations. Is yours really annual when there is a marked seasonal snow cover season ?

Cheers
Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: "ifor" <ifor@krsk.infotel.ru>
To: "Briffa Keith" <K.Briffa@uea.ac.uk>
Subject: No Subject
Date: Thu, 9 Mar 2000 11:33:37 +0700

Dear Keith,

we Mukhtar and me are definitely out from Abisko workshop,

so you are free to present any material suitable.

Make the same in France, no problem with permission.

Best wishes, Gene.

From ???@??? Wed Mar 08 20:29:20 2000 Received: from [139.222.230.3]
(helo=mailgate3.uea.ac.uk) by mailserver1.uea.ac.uk with smtp (Exim 3.02 #1) id
12SxCi-0001SB-00 for f023@smtp.uea.ac.uk; Thu, 09 Mar 2000 07:17:52 +0000 Received: from
DarkOne.ural.net [195.64.192.49] by mailgate3.uea.ac.uk with esmtp (Exim 1.73 #1) id
12Sx7z-00020G-00; Thu, 9 Mar 2000 07:12:59 +0000 Received: from relay.uran.ru
(atreyu.ural.net [195.19.137.69]) by DarkOne.ural.net (8.10.0/eTn) with ESMTP id
e297CwJ06512 for ; Thu, 9 Mar 2000 12:12:58 +0500 (ES) Received: from ipae.uran.ru
([195.19.128.15]) by relay.uran.ru (8.9.3/eTn) with SMTP id MAA56670 for ; Thu, 9 Mar 2000
12:12:49 +0500 (ES) Received: from mail.ipae.uran.ru (rashit.ipae.uran.ru [195.19.135.143]
) by ipae.uran.ru (Hethmon Brothers Smtpd) ; Thu, 9 Mar 2000 12:16:06 +0500 Date: Thu, 9
Mar 2000 12:15:07 +0500 From: Rashit Hantemirov X-Mailer: The Bat! (v1.00 Build 1311)
Registered to Andy Malyshev Reply-To: Rashit Hantemirov Organization: IPAE Priority: Normal
Message-ID: <3511.000309@ipae.uran.ru> To: Keith Briffa Subject: Re: meeting in Sweden
References: <3.0.1.32.20000308021839.00746228@pop.uea.ac.uk> Mime-Version: 1.0
Content-Type: text/plain; charset=us-ascii Content-Transfer-Encoding: 7bit Status: Dear
Keith, I'm glad that chance to see you in Sweden has arisen, because I will hardly come to
Mendoza. I was invited to Abisko under curious circumstances and was pleasantly surprised
seeing you among participants. I apologize if my participating give you trouble with
preparing your paper. I'm going to present results of tree line reconstruction in Yamal,
based on about 50 radiocarbon data (from 9500 BP) and about 500 samples dated using Yamal
chronology (from 7000 BP). May be some short-scale falls in summer temperature will be
examined as a potential cause of tree line recession. Organizers will pay for my travel,
accommodation and food (otherwise I could not come to Sweden). I don't know about other
participants. Best regards, Rashit M. Hantemirov Lab. of Dendrochronology Institute of
Plant and Animal Ecology 8 Marta St., 202 Ekaterinburg, 620144, Russia e-mail:
rashit@ipae.uran.ru Fax: +7 (3432) 29 41 61; phone: +7 (3432) 29 40 92

From: Trevor Davies <t.d.davies@uea.ac.uk>

To: r.k.turner@uea,g.bentham@uea,t.oriordan@uea,n.pidgeon@uea,p.jones@uea, j.palutikof@uea,n.adger@uea,i.bateman@uea,m.hulme@uea,a.lovet@uea

Subject: JIF news

Date: Tue, 28 Mar 2000 13:38:11 +0100

We have heard from ESRC that the ICER bid has been successful. We are to be funded at a "reduced level", although we don't know what that is yet. Our guess is that it will be close to the 10 million we were asked to approach (the revised bid was about 12.5 million).

Well done everyone.

The letter asks us not to make any public announcement, publicity or press releases until 4 April, when there will be a JIF press conference (altho we are encouraged to prepare the press as soon as possible). Please, therefore, continue to regard this information as confidential as far as the outside world is concerned - I shall ask the Press Office to do the necessary.

I will send a note out to all faculty later this afternoon.

Trevor

++++
Professor Trevor D. Davies
Dean, School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

Tel. +44 1603 592836
Fax. +44 1603 507719

++++

From: Keith Briffa <k.briffa@uea.ac.uk>
To: stepan@ipae.uran.ru, ifor@krsk.infotel.ru
Subject: Mendoza, intas
Date: Fri Apr 14 04:05:14 2000

Dear Stepan and Eugene

I was very much looking forward to seeing you both and talking over progress and future plans. I am very sorry that you were not able to attend the Mondoza meeting. I used my introductory talk for the long chronology session to illustrate the great progress and important potential of the Yamal and Taimyr work - and gave a clear indication of the quality and world significance of the continuing research at Ekaterinburg and Krasnoyarsk , and the work of Rashit and Muchtar. Please also let me apologise that Fritz may have been over zealous in requesting receipts for the small amount of money he is to forward to you. I have received these but it was not my intention that he should keep this money until the receipts were to hand. I hope no offence was taken and I am sorry that this money has not been forwarded earlier. I have asked him to send it straight away. Also I hope Stepan that you are now well. I am now back as you see and my first job is to write and send the INTAS report . I will forward copies as soon as it is complete. I have heard nothing about our proposal to the European Commission but I am not confident.

I will be sending your manuscripts back with comments in the near future for the Holocene issue.

It is my greatest hope that collaboration is continued between us even if our latest application fails and I will do my very best to find other sources of support in the future. I really want to understand more about the cell growth model and the link between long term changes in treelines and the lack of very long term evidence of climate change in our ring width and density chronologies. Please let us stay more closely in touch in the future.

my very best wishes

Keith

From: "Michael E. Mann" <mann@virginia.edu>
To: Christoph Schmutz <schmutz@giub.unibe.ch>
Subject: Re: Your recent GRL paper (fwd)
Date: Wed, 19 Apr 2000 12:24:42 -0400
Cc: drdendro@ldeo.columbia.edu, Juerg Luterbacher <juerg@giub.unibe.ch>, Elena Xoplaki <xoplaki@giub.unibe.ch>, Heinz Wanner <wanner@giub.unibe.ch>, Dimitrios Gyalistras <gyalistr@giub.unibe.ch>, mann@multiproxy.evsc.virginia.edu, cullen@ldeo.columbia.edu, druidrd@ldeo.columbia.edu, p.jones@uea.ac.uk, k.briffa@uea.ac.uk, christian.pfister@hist.unibe.ch

Christoph,

I have time for just a few brief comments. I'll leave Ed and the others to follow up if they wish...

mike mann

At 05:13 PM 4/19/00 +0200, you wrote:

>
>Dear Prof. Cook
>
>I have received your comments and the comments of Prof. Mann (Juerg >kindly forwarded me the messages).
>
>First I would like to point out that our paper clearly has the intention >to contribute in a constructive way to the discussion of proxy-based >climate reconstructions. This was the reason for fitting available >proxy-based indices onto J, in order to assess the potential of the >complementary information in the proxy data. In fact, we need proxy-data >to go further back. But it is essential to know the limitations and there >ARE obviously major limitations.
>
>As you mentioned, there might be some non-stationarities in the NAO.
>

Hmmm. I *think* what Ed actually meant is that if one samples e.g. only a subset of the quadrupole set of temperature "lobes" of the NAO (especially, if one samples only, say, one of them--the European one), then one will necessarily be seeing a combination of the NAO, and any other climate patterns that have a distinct regional overprint in that region. In the case of Europe, there are several. So the "nonstationarity" isn't in the *true* NAO, it is an attempt to *define* the NAO in terms of an insufficient subsample of regions influence by it.

>However, the signature of the NAO shows to be quite robust for most of the
>20.th century. As you said, we do not know if there is in fact a probably
>strongly biased signal towards the European continent back in time.
>

>I have downloaded the preprint paper by Cullen et al. In a first overview
>it seems to me that one of my main conclusions, which states that it is
>important to use the complementary information in the data is confirmed by
>their work. In fact this was already one of the conclusions in the
>Luterbacher et al. 1999 paper (number of used predictors are an important
>factor for the obtained skill).
>
>It would have been nice to find the Luterbacher et al. 1999 index in the
>analyses of the mentioned Cullen et al. paper (e.g. in the Tables 1 to 3).

In fact, the Cullen et al paper was originally written and submitted well before the paper you cite (GRL has an extremely fast turnaround time relative to Paleoceanography), and it wouldn't have been appropriate for Heidi Cullen to redo all the analyses using this additional index, at the time the paper was already in review/in press.

>
>The loss of skill (1840-1873) found in table 3 of the mentioned Cullen et
>al. paper implies again that proxy-based index reconstructions have to be
>verified rigorously in the pre-1850 period. The Luterbacher et al. 1999
>index might give some help for the validation of proxy-based
>reconstruction attempts. This index will be open to the public after the
>EGS2000 conference. (<http://www.giub.unibe.ch/klimet>)
>

>Since I'm not a specialist in tree-ring proxy-data you could probably
>better explain the following questions that I (honestly) can not explain:

>
>Why are the different proxy-indices not significantly correlated back in
>time (if one considers a serious significance testing procedure) on the
>interannual and decadal time-scale?

Hmmm. I'm not sure how you come to this conclusion from the results we show.

Several proxy indices are in fact quite significantly correlated (the Appenzeller index is the only one that doesn't show close correlation with the others).

>How is it possible (from a biological and physical point of view) to
>relate the mid- and high latitude tree-ring density and width to the

>main winter circulation pattern in Europe?

>

I'm sure Ed and Keith can point you to the relevant wealth of literature on this.

>

>

>Sincerely yours, Christoph Schmutz

>

>

>> From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

>> To: Ed Cook <drdendro@ldeo.columbia.edu> ,

>> Juerg Luterbacher <juerg@giub.unibe.ch>

>> Cc: cullen@ldeo.columbia.edu, druidrd@ldeo.columbia.edu,

p.jones@uea.ac.uk,

>> k.briffa@uea.ac.uk

>> Subject: Re: Your recent GRL paper

>>

>> Thanks for your comments Ed,

>>

>> I agree with them, and think this needs to be looked into further. I would

>> encourage those who haven't yet, to take a look at the Cullen et al

>> manuscript which covers the same territory and comes to somewhat different

>> conclusions. The manuscript is now in-press in Paleoclimatology, and is

>> available in

>> preprint form here (both as postscript and pdf file):

>>

>> <http://rainbow.ldeo.columbia.edu/climategroup/papers/>

>>

>> Would be interested in peoples thoughts.

>>

>> regards,

>>

>> mike

>> At 04:34 PM 4/18/00 -0400, Ed Cook wrote:

>> >Dear Juerg,

>> >

>> >I have just completed reading your most recent GRL paper (Schmutz et al.,

>> >2000) on NAO reconstructions in which you show that proxy-based NAO

>> >reconstructions are probably wanting. It is not possible to strongly defend

>> >my reconstruction at this time (indeed I was extremely cautious in my

>> >description of it with regards to over-fitting problems, etc.).

However, I

>> >do think that there are some issues that have not been fully explored,

>> >which could help explain some of the non-stationarity in the relationships

>> >found between your index and mine (at least) based on proxy data alone.

>> >First, my NAO reconstruction is based on 6 North American and 4 European tree-ring chronologies. Because the putative NAO information in these records spans the North Atlantic and nicely brackets the NAO centers of action as we know them now, they potentially contain past information that is missing from a purely European-based estimate of NAO. This could occur if the NAO did not affect climate on both sides of the North Atlantic in the same roughly symmetric way back in time as it does now. If this were the case (and we have no way of knowing that now as far as I know), then it is conceivable that your L index is excessively biased towards Europe, as would be the extended Jones SLP index. If so, any comparisons between your L index and my proxy index with the Jones index would be hopelessly biased in your favor. This is not to say that my reconstruction is as good as yours, but it might not be as bad as your results indicate either.

>> >

>> >Indeed, I did make some effort to "verify" my reconstruction against early instrumental records, with somewhat contradictory and potentially interesting results. Over the 1841-1873 period, my record correlates significantly with Stykkisholmer SLP (-0.456) and Oslo temperatures (0.323), but not Bermuda SLP (0.156) and Central England temperatures (0.211). The "appearance" of significant verification with only the more northerly instrumental records may be telling us something about differences in circulation and SSTs over the North Atlantic from what is now the case. This could affect the way in which the NAO affects climate jointly over North America and Europe. Of course, when I added some earlier observations (same stations) to the verification tests (Table 4 of my paper), the results weakened considerably. So, maybe this means that my NAO reconstruction is indeed poor. However, I must admit to having doubts about the quality of the early instrumental records despite the great efforts made to homogenize and correct them. This is especially the case with regards to low-frequency variability, but can also extend to individual

>

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
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Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (804) 924-7770 FAX: (804) 982-2137
<http://www.evsc.virginia.edu/faculty/people/mann.html>

From: Mike Hulme <m.hulme@uea.ac.uk>
To: t.d.davies
Subject: ESSO
Date: Fri May 5 10:24:25 2000

>Date: Fri, 05 May 2000 10:04:21 +0100
>To: shepherd
>From: Mike Hulme <m.hulme@uea.ac.uk>
>Subject: ESSO

>

>John,

>

>I can make a London lunch on either 19 or 20, but with a strong preference for 20th. Trevor could also make both days if necessary. By then we will have got further with the Tyndall contract so it would be useful to talk with Esso (do you have a copy of the Exxonmobil booklet referred to?).

>

>Let me know how this proceeds,

>

>Mike

>

>

From: John Shepherd <j.g.shepherd@soc.soton.ac.uk>
To: t.d.davies@uea.ac.uk
Subject: Re: ESSO
Date: Wed, 24 May 2000 13:00:43 +0100
Cc: Mike Hulme <m.hulme@uea.ac.uk>

Trevor

I gather you're going to collect the free lunch(?) with Esso ! I agree with Mike's analysis : i.e. there's room for some constructive dialogue...

See you on the 1014 from Ipswich (0940 from Norwich), for a kick-off at 12 noon ??

John

At 14:07 19/05/00 +0100, Mike Hulme wrote:

>John,

>

>It will be Trevor on the 19th for ESSO - too tricky for my schedule. I
>will pass the Esso booklet onto Trevor.

>

>Esso have selectively quoted to (over)-emphasise the uncertainties re.
>climate change, but at least they have moved beyond denial and recognise
>that potential unknown long-term risks may require tangible short-term
>actions. Seems to be some room for negotiation over what research needs
>doing. I would think Tyndall should have an open mind about this and try
>to find the slants that would appeal to Esso. Uncertainty and risk
>analysis and C sequestration may be the sort of things that appeal.

>

>See you Wednesday,

>

>Mike

>

>At 16:23 10/05/00 +0100, you wrote:

>>Mike

>> Despite my efforts Esso have gone firm on 19th (to fit the schedule of
>>their man from the USA). Can you decide between you who should come (I
>>suggest one is enough) : it'll be lunchtime somewhere in London. I shall
>>be travelling from Ipswich (it's my week for the Aldeburgh Festival) so we
>>could possibly meet on the train there ??

>>

>> Copies of the Esso booklet arrived yesterday and are now on their way to

>>you... I read it last night and wrote "misleading" and "wrong" in the
>>margins in quite a few places !

>>

>> John

>>

>>At 10:04 05/05/00 +0100, you wrote:

>>>John,

>>>

>>>I can make a London lunch on either 19 or 20, but with a strong preference
>>>for 20th. Trevor could also make both days if necessary. By then we will
>>>have got further with the Tyndall contract so it would useful to talk with
>>>Esso (do you have a copy of the Exxonmobil booklet referred to?).

>>>

>>>Let me know how this proceeds,

>>>

>>>Mike

>>>

>>>

>>>

>>

>

From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Noguer, Maria" <mnoguer@meto.gov.uk>, 'tar10 ' <tar10@egs.uct.ac.za>
Subject: Re: Precipitation map for the Box
Date: Fri, 30 Jun 2000 08:08:12 +0100

<x-flowed>

Dear Chapter 10,

Sorry I missed out on the meeting.

In general I like the proposed Figure and suggested Box contents (and I particularly agree that the diversity of downscaling methods and results precludes using them as a basis of consolidated regional conclusions). I also agree with others that it looks better with the +/- signs included. However, there are 2-3 points that concern me, mostly from the perspective of climate scenarios (Chapter 13 - and also Chapter 9).

- it needs to be made very clear if any numbers are cited in the Box (e.g. 2-6degC for continental warming) that these refer to only **one** forcing scenario, namely 1% p.a.

- rather than talk about GHG and SUL I would suggest the more conventional nomenclature of GG and GS (the SUL runs are not just SUL forcing of course, which might give that impression).

- another very important caveat concerns the GS (SUL) results - these all stem from IS92a type aerosol forcing a la IPCC SAR. Most of the new SRES forcings used in TAR and Chapter 9 for example have much smaller or even positive SO4 forcing relative to 1990. In principle this could actually switch the sign of the precip. changes in some regions. There is the danger of inconsistency here between Chapter 9 (TAR aerosol effects) and Chapter 10 Box (SAR aerosol effects) if this is not carefully explained. For example, in CAM and JJA it appears that aerosols switch the P change from 'strongly negative' to being 'uncertain' - but this is only for IS92a aerosol forcing: it is not a conclusion that would be valid for SRES aerosol forcing!

- as Filippo says, another key uncertainty not represented in the Box is forcing uncertainty - again, Chapter 9 present a wide range of Tglobal results, part of which relates to prior assumptions about which SRES forcing materialises. We do a disservice if we give the impression in Chapter 10 Box that these regional responses are independent of what future

forcing materialises. For example, under the lowest SRES forcing (B1) the precip. response in some regions would revert back to being very small and therefore indistinguishable from noise.

- with regard to temperature and Filippo's comment, Chapter 9 has global maps of T change, averaged across the standard set of AOGCM experiments (ranges are also shown). This is in effect the information being sought-for by readers of Chapter 10 is it not. I would have thought that back-references in the Box to Figure 9.9 would be sensible.

See you all in Victoria,

Mike

At 14:35 27/06/00 +0100, Noguier, Maria wrote:

>Dear all,

>

>Here are two examples that Paul has put together regarding the map of
>changes in precipitation drawn from Figure 10.5

>Do you think it works? Please send me any suggestions that you may.

>

> <<Fig01a.pdf>> <<Fig01b.pdf>>

>

>Regards,

>

>Maria

>

>*****

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>*****

>
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>

</x-flowed>

From: stepan <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Manuskript of papes
Date: Tue, 4 Jul 2000 11:30:39 +0600
Reply-to: stepan <stepan@ipae.uran.ru>
Cc: t.osborn@uea.ac.uk

Dear Keith and Tim,

Thank you for the papers which I have received some days ago. They produced an impression on me. It is really a big job. I do not have time now to evaluate in details the results obtained. I want to make two remarks only.

First, I think, that the method of standardisation is very interesting, but it is disputable for the regions and sites where trees grow under extreme climatic conditions, for example at the polar timberline in Siberia. In such conditions the shape of age curve and the age of maximum growth are very changeable in different trees growing at the same site. It will be very interesting if you can present the age curve obtained for one such site, for example for the North Taymir Peninsula.

Second, I do not agree that in the northern Siberia the 15th century summers were warmer than those observed in the 20th century, at least in the Western and Middle Siberia. May be it is a result of stundartisation?

We suggest to inscribe in list of references the next papers:

1. Vaganov E.A., Shiyatov S.G., Mazepa V.S. Dendroclimatic study in Ural-Siberian Subarctic. - Novosibirsk "Nauka", Siberian Publishing Firm RAS, 1996. - 246 p. (in Russian).
2. Mazepa V.S. Influence of Precipitations on Tree-Ring Growth of Coniferous in Subarctic Regions of Eurasia //Lesovedenie, No. 6, 1999. - P.14-21. (in Russian).

Abstract. Influence of precipitation on tree-ring variability of coniferous trees in Subarctic regions of Eurasia has been shown. Depending on the region, significant ecological factor for tree growth are precipitation of autumn-winter, winter-spring and summer periods.

Ecological explanation of such influence has been given. On the base of relationships between tree-rings and rainfall the reconstructions of precipitation in different regions of Subarctic for last 200 years have been developed.

3. Mazepa V.S. Spatial Reconstruction of Summer Air Temperature in the North of the West Siberia since 1690 on the base of Tree-Ring Data.

//Siberian ecological journal, No. 2, 1999. - P.175-183. (in Russian).

Abstract. Opportunity of annual reconstruction of summer thermal conditions from Polar Urals (64-68°N, 64-68°E) up to Yenisei River (66-70°N, 86-89°E) is caused by high and sufficiently stable relationship between coniferous tree growth (*Larix sibirica*, *Picea obovata*) and corresponding climatic factors. Percent variance in tree-ring chronologies explained by climate (June-July temperature) in this extreme for growth of trees area reaches 50%. Spatial reconstruction of air summer temperature on the base of point reconstruction for 11 corresponding meteorostations has been developed. Analysis of reconstructed temperatures has shown their significant changes for last 300 years. The most strong fall of temperatures was observed in XIX century, but rise in temperature was observed in XVIII and XX centuries.

4. Mazepa V.S. Dendroclimatic reconstructing air summer temperatures since 1690 in subarctic regions of siberia. //Problems of ecological monitoring and ecosystem modelling, Volume XVII. - St.Petersburg Gidrometeoizdat, 2000. - P.170-187. (in Russian).

Abstract. The further development of many-year dendroclimatic study carried out in subarctic regions of Siberia and on the polar timberline, is given in this paper. Climatic factors determining the year-to-year and many-year tree-ring width variability were revealed, using multiple regression models. The spatial year-to-year reconstruction of air summer temperatures was made on the base of available dendroclimatic network. The reliability of spatial summer temperatures reconstruction in the boreal zone of the Urals and Siberia was evaluated. The temporal dendroclimatic zoning of the area investigated was carried out according to the chronology similarity. The regional border changes, depending on warm and cold periods, were shown. Five regional chronologies showing the nature of summer months thermic regime variability were developed. Extremely cold and warm

periods were revealed. The coldest periods are: the first half of XVII and XIX centuries. The warmest periods are: the second half of XVII, XVIII and middle of XX centuries.

To-day R. Hantemirov and A. Surkov will go to the Yamal Peninsula for subfossil wood collecting. I and V. Mazepa will go to the Polar Ural Mountains in some days.

Best regards,

Stepan

<mailto:stepan@ipae.uran.ru>

From: "Mick Kelly" <m.kelly@uea.ac.uk>
To: m.hulme@uea.ac.uk
Subject: Shell
Date: Wed, 05 Jul 2000 13:31:00 +0100
Reply-to: m.kelly@uea.ac.uk
Cc: t.oriordan@uea.ac.uk, t.o'riordan@uea.ac.uk

Mike

Had a very good meeting with Shell yesterday. Only a minor part of the agenda, but I expect they will accept an invitation to act as a strategic partner and will contribute to a studentship fund though under certain conditions. I now have to wait for the top-level soundings at their end after the meeting to result in a response. We, however, have to discuss asap what a strategic partnership means, what a studentship fund is, etc, etc. By email? In person?

I hear that Shell's name came up at the TC meeting. I'm ccing this to Tim who I think was involved in that discussion so all concerned know not to make an independent approach at this stage without consulting me!

I'm talking to Shell International's climate change team but this approach will do equally for the new foundation as it's only one step or so off Shell's equivalent of a board level. I do know a little about the Fdn and what kind of projects they are looking for. It could be relevant for the new building, incidentally, though opinions are mixed as to whether it's within the remit.

Regards

Mick

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From: "Raymond S. Bradley" <rbradley@geo.umass.edu>

To: Frank Oldfield <frank.oldfield@pages.unibe.ch>

Subject: Re: the ghost of futures past

Date: Mon, 10 Jul 2000 08:57:19 -0400

Cc: alverson@pages.unibe.ch, jto@u.arizona.edu, k.briffa@uea.ac.uk, mhughes@ltrr.arizona.edu, pedersen@eos.ubc.ca, whitlock@oregon.uoregon.edu, mann@multiproxy.evsc.virginia.edu

<x-flowed>Sorry this kept you awake...but I have also found it a rather alarming graph. First, a disclaimer/explanation.

The graph patches together 3 things: Mann et al NH mean annual temps + 2 sigma standard error for AD1000-1980, + instrumental data for 1981-1998 + IPCC ("do not quote, do not cite" projections for GLOBAL temperature for the next 100 years, relative to 1998. The range of shading represents several models of projected emissions scenarios as input to GCMs, but the GCM mean global temperature output (as I understand it) was then reproduced by Sarah Raper's energy balance model, and it is those values that are plotted. Keith pointed this out to me; I need to go back & read the IPCC TAR to understand why they did that, but it makes no difference to the first order result....neither does it matter that the projection is global rather than NH...the important point is that the range of estimates far exceeds the range estimated by Mann et al in their reconstruction. Keith also said that the Hadley Center GCM runs are being archived at CRU, so it ought to be possible to get that data and simply compute the NH variability for the projected period & add that to the figure, but it will not add much real information. However, getting such data would allow us to extract (say) a summer regional series for the Arctic and to then plot it versus the Holocene melt record from Agassiz ice cap....or....well, you can see other possibilities.

[.....At this point Keith Alverson throws up his hands in despair at the ignorance of non-model amateurs...]

But there are real questions to be asked of the paleo reconstruction. First, I should point out that we calibrated versus 1902-1980, then "verified" the approach using an independent data set for 1854-1901. The results were good, giving me confidence that if we had a comparable proxy data set for post-1980 (we don't!) our proxy-based reconstruction would capture that period well. Unfortunately, the proxy network we used has not been updated, and furthermore there are many/some/ tree ring sites where there has been a "decoupling" between the long-term relationship between climate and tree growth, so that things fall apart in recent decades....this makes it very difficult to demonstrate what I just claimed. We can only call on evidence from many other proxies for "unprecedented" states in recent years (e.g. glaciers, isotopes in tropical ice etc..). But there are (at least) two other problems -- Keith Briffa points out that the very strong trend in the 20th century calibration period accounts for much of the success of our calibration and makes it unlikely that we would be able to reconstruct such an extraordinary

period as the 1990s with much success (I may be mis-quoting him somewhat, but that is the general thrust of his criticism). Indeed, in the verification period, the biggest "miss" was an apparently very warm year in the late 19th century that we did not get right at all. This makes criticisms of the "antis" difficult to respond to (they have not yet risen to this level of sophistication, but they are "on the scent"). Furthermore, it may be that Mann et al simply don't have the long-term trend right, due to underestimation of low frequency info. in the (very few) proxies that we used. We tried to demonstrate that this was not a problem of the tree ring data we used by re-running the reconstruction with & without tree rings, and indeed the two efforts were very similar -- but we could only do this back to about 1700. Whether we have the 1000 year trend right is far less certain (& one reason why I hedge my bets on whether there were any periods in Medieval times that might have been "warm", to the irritation of my co-authors!). So, possibly if you crank up the trend over 1000 years, you find that the envelope of uncertainty is comparable with at least some of the future scenarios, which of course begs the question as to what the likely forcing was 1000 years ago. (My money is firmly on an increase in solar irradiance, based on the 10-Be data..). Another issue is whether we have estimated the totality of uncertainty in the long-term data set used -- maybe the envelope is really much larger, due to inherent characteristics of the proxy data themselves....again this would cause the past and future envelopes to overlap.

In Ch 7 we will try to discuss some of these issues, in the limited space available. Perhaps the best thing at this stage is to simply point out the inherent uncertainties and point the way towards how these uncertainties can be reduced. Malcolm & I are working with Mike Mann to do just that.

I would welcome other thoughts and comments on any of this!

Ray

At 01:34 PM 7/10/00 +0200, you wrote:

>Salut mes amis,

>

>I've lost sleep fussing about the figure coupling Mann et al. (or any
>alternative climate-history time series) to the IPCC scenarios. It seems to
>me to encapsulate the whole past-future philosophical dilemma that bugs me
>on and off (Ray - don't stop reading just yet!), to provide potentially the
>most powerful peg to hang much of PAGES future on, at least in the eyes of
>funding agents, and, by the same token, to offer more hostages to fortune
>for the politically motivated and malicious. It also links closely to the
>concept of being inside or outside 'the envelope' - which begs all kinds of
>notions of definition. Given what I see as its its prime importance, I
>therefore feel the need to understand the whole thing better. I don't know
>how to help move things forward and my ideas, if they have any effect at

>all, will probably do the reverse. At least I might get more sleep having
>unloaded them, so here goes.....
>
>The questions in my mind centre round the following issues. If I've got any
>one of them wrong, what follows in each section can be disregarded or (more
>kindly) set straight for my benefit.
>
>1. How can we justify bridging proxy-based reconstruction via the last bit
>of instrumental time series to future model-based scenarios.
>
>2. How can the incompatibilities and logical inconsistencies inherent in
>the past-future comparisons be reduced?
>
>3. More specifically, what forms of translation between what we know about
>the past and the scenarios developed for the future deal adequately with
>uncertainty and variability on either side of the 'contemporary hinge' in a
>way that improves comparability across the hinge.
>
>4. Which, if any, scenarios place our future in or out of 'the envelope'
>in terms of experienced climate as distinct from calculated forcing? This
>idea of an envelope is an engaging concept, easy to state in a quick and
>sexy way (therefore both attractive and dangerous); the future could leave
>us hoisted by our own petard unless it is given a lot more thought.
>
>1. I am more or less assuming that this can already be addressed from data
>available and calculations completed, by pointing to robust calibration
>over the chosen time interval and perhaps looking separately at variability
>pre 1970, if the last 3 decades really do seem to have distorted the
>response signatures for whatever reasons. I imagine developing this line of
>argument could feed into the 'detection' theme in significant ways.
>
>2 & 3. This is where life gets complicated. For the past we have biases,
>error bars that combine sources of uncertainty, and temporal variability.
>For the future we have no variability, simply a smooth, mean, monotonic
>trend to a target 'equilibrium' date. Bandwidths of uncertainty reflect
>model construction and behaviour. So we are comparing apples and oranges
>when we make any statement about the significance of the past record for
>the future on the basis of the graph. Are there ways of partially
>overcoming this by developing different interactions between past data and
>future models?
>
>My own thinking runs as follows: Take variability. Do we need to wait for
>models to capture this before building it into future scenarios? This seems
>unnecessary to me, especially since past variability will be the validation
>target for the models. Is there really no way of building past variability
>into the future projections? One approach would be to first smooth the
>past record on the same time-span as the future scenarios. This would get
>us to first base in terms of comparability, but a very dull and pretty
>useless first base in and of itself. It would, however, allow all kinds of

>calculations of inter-annual variability relative to a mean time line of
>the 'right' length. This in turn could be used in several ways, for
>example:
> - build the total range of past variability into the uncertainty
>bands of each future scenario.
> - take the 30,50 or 100 year period (depending on the scenario for
>comparison) during which
> there was the greatest net variability, or the greatest net fall
>in Temperature, or the
> greatest net increase in T. and superimpose/add this data-based
>variability on the mean
> trends.
> - take the n-greatest positive anomalies relative to the trend and
>use them to define an upper
> limit of natural variability to compare with the (to my mind)
>more realistic future scenarios.
>
>These and cleverer variants I cannot begin to think up seem to me to hold
>out the possibility of linking future projections of GHG forcing with what
>we know about natural variability in reasonably realistic ways and perhaps
>even of redefining the 'past data-future scenario' relationship in ways
>that benefit both the paleo-community and the quality of future
>projections.
>
>4. I also think the above kinds of exercise might eventually lead us
>towards a better definition of 'the envelope' and more confidence in
>deciding what is outside and what is not. The same sort of approach can be
>taken towards projections of P/E I imagine and, more particularly, at
>regional rather than global or hemispheric level.
>
>Sorry if all this sounds stupid or obvious. I got afflicted with the 'need
>to share' bug.
>
>Frank
>
>
>_____

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Climate System Research Center Web Site:

<<<http://www.geo.umass.edu/climate/climate.html>><http://www.geo.umass.edu/climate/climate.html>

Paleoclimatology Book Web Site (1999):

<<http://www.geo.umass.edu/climate/paleo/html>><http://www.geo.umass.edu/climate/paleo/html>

</x-flowed>

From: "Michael E. Mann" <mann@holocene.evsc.virginia.edu>

To: Frank Oldfield <frank.oldfield@pages.unibe.ch>

Subject: Re: the ghost of futures past

Date: Mon, 10 Jul 2000 13:37:30 -0400

Cc: rbradley@geo.umass.edu, jto@u.arizona.edu, keith.alverson@pages.unibe.ch, k.briffa@uea.ac.uk, pedersen@eos.ubc.ca, mhughes@ltrr.arizona.edu, whitlock@oregon.uoregon.edu

Thanks Frank,

My apologies...

Sorry, no, I hadn't looked in detail at your original email to Ray, only his response, and simply wanted to note that others have already jumped on this bandwagon, so Ray deserves neither all the blame, nor all the glory, depending on your perspective :)

And, as I stated, IPCC clearly considers such a plot not appropriate for prime time--so you won't see anything like this in the TAR.

What I find most useful, however, along the lines of what you discuss, is using empirical reconstructions as a baseline for comparison against model simulations of both free and forced variability. A number of studies have attempted this recently, and the results are encouraging from the point of view that (a) the coupled models appear to be getting the internal variability of mean global/hemispheric temperatures about right [this leads us in the direction of having greater faith in future scenarios from such models] and (b) the models, forced with paleoestimates of past volcanic, solar, and GHG radiative forcings, appear to be able to explain more than 50% of the variance in the paleo temperature reconstructions. A paper to appear in this Friday's "Science" by Tom Crowley describes some impressive results along these lines.

It is agreed that hydrological change and regional temperature anomalies superimposed on any large-scale temperature changes are of key importance from any practical point of view. And I think this is what we're all working towards, more regionally detailed reconstructions of climate fields (temperature, drought, slp, etc.) in past centuries. Clearly more high-resolution proxy evidence is necessary, in both time and space. I make many of these very points in a "Perspectives" article also to appear in Science on Friday, accompanying Tom Crowley's article.

Will appreciate any comments on it. Hope the above provides some clarification.

cheers,

mike

At 06:59 PM 7/10/00 +0200, you wrote:

>Hi Mike,

>

>Not sure if your reply implied you were taking my points seriously or not -

>I'm not even sure if Ray sent them on to you or you just received his

>reply! My reactions to the graphs on the website are that the temperature

>one does not address my points (but it does not aim to and I fully agree

>that if the projections are sufficiently reliable it hardly needs to!),

>that P/E is likely to be much more important than temperature per se and

>that the historical sea-level curve is not really acceptable - very much

>more high resolution work needs to be done on that before we have any real

>sense of past variability on decadal to century timescales.

>

>Cheers,

>

>Frank

>

>

>_____

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>

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>At 06:59 PM 7/10/00 +0200, Frank Oldfield wrote:

>Hi Mike,

>

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>that the historical sea-level curve is not really acceptable - very much

>more high resolution work needs to be done on that before we have any real

>sense of past variability on decadal to century timescales.

>

>Cheers,

>

>Frank

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>_____

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: keith.alverson@pages.unibe.ch
Subject: glossy
Date: Tue Aug 1 10:23:10 2000

Keith

I've sent you a few slides taken by Hakan Grudd as promised . I think these should be supplemented by a bit of a colourful timeseries - part of a chronology. It could be a piece of the Tornetrask series (northern Sweden) from where the pictures are taken - but I think a section of the 3-region average (Tornetrask, Yamal, Taimyr) possibly showing the 563 A.D. would be better. So I am sending a couple post script files and a suggested colour scheme. What do you think? I suggest a one hundred year section of the average series , showing annual values. Note that in these Figures , A.D. 536 is marked by a filled triange. Just showing the initiation of a dramatic cooling in A.D. 536 and the widespread cold summers of the 540's (a major vocano? if perhaps not as David Keys makes out in his recent book), or a comet (as Mike Baille says in his?) , is quite appealing.

Keith

From: Keith Briffa <k.briffa@uea.ac.uk>

To: joos <joos@climate.unibe.ch>

Subject: Re: climate reconstructions

Date: Fri Aug 4 15:10:06 2000

Dear Fortunat

I am pleased to hear from you. I have still not been in touch about the data I showed you in Vienna! As for your question - of course I will send the series you mention - but it is only an average of three regional tree-ring chronologies (Northern Sweden, Yamal,Taimyr) and not calibrated in terms of temperature. Nevertheless, it is representative of summer warmth over a large Russian region, We have recently submitted a paper describing a different standardization approach (for preserving low frequency variance) applied to a big high-latitude network of tree-density data. This yields regional (up to 600-year) calibrated reconstructions and a hemispheric curve - all representing april-sept season. I have asked my colleague Tim Osborn here to send the data and a copy of the papers to you, I am on the verge of leaving for 2 weeks so if you need more information contact him. As for other areas of the world - Phil Jones has an alternative Hemisphere curve and there are some southern hemisphere chronologies (temp. sensitive). There are short precip reconstructions for several spots - but systematic Palmer Drought Indices for the U.S. from about 1700. I will be happy to talk on the phone about all these in two weeks.

best wishes

Keith

At 11:01 AM 7/19/00 +0200, you wrote:

Dear Keith,

How are you? Hope everything is going well.

I am writing because I am interested in your climate reconstruction for the last millennium.

The Etheridge ice core data of CO2 indicate that CO2 was below average in the 17th and 18th centuries by a few ppm. Very few (1-2 points) of ice core C13 data (Francey tellus, 99) suggest that this drawdown was caused by additional terrestrial carbon storage (Joos et al, GRL, 99; Trudinger, Tellus, 99). We try to investigate this suggestion using the Lund-Potsdam-Jena dynamical global vegetation model (LPJ-DGVM).

A diploma student of mine, Philippe Bruegger, has used the Mann et al annual mean temperature patterns (2 EOFs only) in combination with the Etheridge CO2 record to drive the LPJ model. Instead of absorbing carbon, the model is releasing carbon due to a reduced CO2 fertilization effect in the model that outweighs any climatic effects. Thus, the model results is clearly not compatible with the ice core results.

Obviously, the study is hampered by the limitation of the climate reconstruction (as well as by the few C13 ice core data). Instead of

changes in monthly values of Temp and precip (and cloud cover) changes in ANNUAL mean temperature were used to force LPJ.

Could you or Phil Jones provide alternative forcing fields that focus e.g. more on summer temperature? Any info about precipitation?

I would also appreciate very much to obtain reprints of your most recent articles, namely the article in Quaternary Science Rev. 2000.

Thanks for any help you can provide.

Regards, Fortunat

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NEW FAX NUMBER; NEW FAX NUMBER; NEW FAX NUMBER; NEW FAX NUMBER;

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References

1. <http://www.climate.unibe.ch/~joos/>

From: Phil Jones <p.jones@uea.ac.uk>
To: tom crowley <tom@ocean.tamu.edu>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: mill records
Date: Mon Aug 7 13:58:54 2000

Tom and Mike,

What Tom said is essentially correct. Tim Osborn here recalibrated each series, as a composite, against the same NH series for the April-Sept average north of 20N (using land only data). All this does is rescale the series as it is simple regression ($y=ax+b$). Because y is based on temps wrt 61-90 this means that the axis is then wrt 61-90. Doing this we can then add the same instrumental temp series. It also brings the series together and the web page was just for illustrative purposes. For Mike's series you get pretty much the same result by subtracting 0.12 from Mike's numbers as this is the difference between Mike's base period and 1961-90.

There is nothing sinister going on ! I'll summarise this to Rob.

Cheers
Phil

PS I seem to be stirring up loads of emails about historical data. You are both on those emails so you can see what crap is being written and my (time wasting for me) replies. Apologies for replying. I should know better and keep quiet. We can all expect more of this if IPCC stays in roughly the same form pre-Victoria. It's relatively easy to knock historical records, so as long as it gets no worse than this we'll be fine.

From: "S. Fred Singer" <singer@sepp.org>
To: "Raymond S. Bradley" <rbradley@geo.umass.edu>
Subject: Re:Your msg about climate/energy policy
Date: Tue, 08 Aug 2000 11:55:23 -0400
Cc: mann@virginia.edu, pjm8x@rootboy.nhes.com

<x-flowed>Dear Ray

You sent me this op-ed (?) (Letter to editor?) about the need to convert the US from a carbon-based economy to a hydrogen-based economy. I can't guess why you wanted me to know your views, but it does help me to better understand what motivates your scientific work and judgment. It also throws some doubt about your impartiality in promoting the "hockey stick" temperature curve that a number of us have been critical of.

In any case, I doubt if espousal of this energy policy will help BP and ARCO discover a source of hydrogen somewhere.

You quote the "progressive" Business Council approvingly: "We accept the views of most scientists that enough is known about the science and environmental impacts of climate change for us to take actions to address its consequences." And from BP chairman : "the time to consider the policy dimensions of policy change is not when the link between greenhouse gases and climate change is conclusively proven, but when the possibility cannot be discounted and is taken seriously by the society of which we are part."

I note that BP and ARCO are still out there exploring for oil; they don't seem to be quite ready yet to put real money where their mouth is.

You call for the US to take leadership in stabilizing the climate. Perhaps the government will turn to you to learn how to do this. A far less ambitious goal would be to stabilize the atmospheric concentration of CO2. According to the IPCC this would require an emission reduction of 60 to 80 percent (with respect to 1990) --- WORLDWIDE.

Have you ever considered the consequences of such a policy -- assuming it could really be adopted?

Best wishes ,

Fred

At 10:34 AM 8/1/00 -0400, you wrote:

- > WASHINGTON, DC -- In August 1997, a few months before the Kyoto
- > Conference on Climate Change, the Global Climate Coalition (GCC)
- > helped launch a massive advertising campaign designed to prevent the
- > United States from endorsing any meaningful agreement to reduce global
- > carbon emissions. This group included in its ranks some of the world's most
- > powerful corporations and trade associations involved with fossil
- > fuels. The
- > campaign effectively undermined public support of U.S. efforts to lead the
- > international effort to stabilize climate.
- > While the public image of the GCC was that of a unified group, there was
- > dissent. John Browne, Chairman of British Petroleum, on May 19, 1997,
- > announced that "the time to consider the policy dimensions of policy change
- > is not when the link between greenhouse gases and climate change is
- > conclusively proven, but when the possibility cannot be discounted and is
- > taken seriously by the society of which we are part. We in BP have
- > reached that point."
- > BP withdrew from the Global Climate Coalition. Dupont had already left.
- > The following year, Royal Dutch Shell left.
- > In 1999, Ford withdrew from the GCC. A company spokesman noted,
- > "Over the course of time, membership in the Global Climate Coalition has
- > become something of an impediment for Ford Motor Company to
- > achieving our environmental objectives."
- > In rapid succession in the early months of 2000, Daimler Chrysler, Texaco,
- > and General Motors announced that they too were leaving the Coalition.
- > This accelerating exodus reflected the conflict emerging within GCC ranks
- > between firms that were clinging to the past and those that were planning
- > for the future.
- > Some of the exiting companies, such as BP Amoco, Shell, and Dupont,
- > joined a progressive new group, the Business Environmental Leadership
- > Council, which says, "We accept the views of most scientists that enough is
- > known about the science and environmental impacts of climate change for
- > us to take actions to address its consequences."
- > Membership requires companies to have programs for reducing carbon
- > emissions. BP Amoco, for example, plans to bring its carbon emissions to
- > 10 percent below its 1990 level by 2010, exceeding the Kyoto goal of
- > roughly 5 percent for industrial countries.
- > Dupont has already cut its 1990 greenhouse gas emissions by 45 percent
- > and plans to reduce them by 65 percent by 2010.
- > There is a growing acceptance among the key energy players that the
- > world is in the early stages of the transition from a carbon-based to a
- > hydrogen-based energy economy. In February 1999, ARCO CEO

> Michael Bowlin said, "We've embarked on the beginning of the Last Days
> of the Age of Oil." He then discussed the need to convert our
> carbon-based energy economy into a hydrogen-based energy economy.
> With the organization that so effectively undermined U.S. leadership in
> Kyoto no longer a dominant player in the global climate debate, the
> stage is
> set for the United States to resume leadership of the global climate
> stabilization effort.
> Raymond S. Bradley
> Professor and Head of Department
> Department of Geosciences
> University of Massachusetts
> Amherst, MA 01003-5820
>
> Tel: 413-545-2120
> Fax: 413-545-1200
> Climate System Research Center: 413-545-0659
> Climate System Research Center Web Page:
> <<http://www.geo.umass.edu/climate/climate.html>>
> Paleoclimatology Book Web Site (1999):
> <http://www.geo.umass.edu/climate/paleo/html>

S. Fred Singer, President
Science & Environmental Policy Project
9812 Doulton Court
Fairfax, VA 22032
<http://www.sepp.org>
Tel: 703-503-5064
e-fax 815-461-7448 (your fax will be sent as email to my
computer)

"The improver of natural knowledge absolutely refuses
to acknowledge authority, as such. For him, scepticism
is the highest of duties; blind faith the one unpardonable sin."

Thomas H. Huxley

"That theory is worthless. It isn't even wrong!" - W. Pauli

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>, "Folland, Chris" <ckfolland@meto.gov.uk>
Subject: Re: FW: Mann etal
Date: Fri, 11 Aug 2000 13:40:30 +0100
Cc: jfbmitchell@meto.gov.uk,k.briffa@uea.ac.uk

Chris and John (and Mike for info),

I'm basically reiterating Mike's email. There seem to be two lots of suggestions doing the rounds. Both are basically groundless.

1. Recent paleo doesn't show warming.

This basically stems back to Keith Briffa's paper in Nature in 1998 (Vol 391, pp678-682). In this it was shown that northern boreal forest conifers don't pick up all the observed warming since about the late 1950s. It was suggested that some other factor or a combination of factors related to human-induced pollution (e.g. nitrogen deposition, higher levels of CO₂, ozone depletion etc). Hence in a new paper submitted to JGR recently we develop a new standardization approach (called age banding) and produce a large-scale reconstruction (calibrated over the period 1881-1960 against NH land north of 20N) back to 1402. If you want a copy of this can you email Keith and he'll send copies once he's back from holiday.

This background is to illustrate how Singer et al distort things. The new reconstruction only runs to 1960 as did earlier ones based solely on tree-ring density. All the other long series (Mike's, Tom Crowley's and mine) include other proxy information (ice cores, corals, historical records, sediments and early instrumental records as well as tree-ring width data, which are only marginally affected). All these series end around 1980 or in the early 1980s. We don't have paleo data for much of the last 20 years. It would require tremendous effort and resources to update a lot of the paleo series because they were collected during the 1970s/early 1980s.

It is possible to add the instrumental series on from about 1980 (Mike sought of did this in his Nature article to say 1998 was the warmest of the millennium - and I did something similar in Rev. Geophys.) but there is no way Singer can say the proxy data doesn't record the last 20 years of warming, as we don't have enough of the proxy series after about 1980.

<http://www.co2.science.org/edit/editor.html> takes the argument further saying that as trees don't see all the warming since about 1960 the instrumental records recently must be in error (i.e. this group believes the trees and not the instrumental records). This piece by Idso and Idso seems to want to have the argument whichever suits them.

2. Everyone knows it was cooler during the Little Ice Age and warmer in the Medieval Warm Period.

All of the millennial-long reconstructions show these features, but they are just less pronounced than people believed in the 1960s and 1970s, when there was much less paleo data and its spatial extent was limited to the eastern US/N.Atlantic/European and Far East areas. The issue seems to revolve around the average temperatures we have for earlier centuries in the millennium. I use the argument that for the instrumental period we need sites located over much of the NH (land and marine) regions in order to claim we have a reasonable record for the whole hemisphere. We wouldn't dream of extending the NH series based on longer European records and in the extreme just CET, so with the paleo data we need records from as many regions as possible. The coverage still could be better, but it is far better than it was 25 years ago, when the ideas embodied in the MWP and LIA became sort of mainstream.

The typical comments I've heard, generally relate to the MWP, and say that crops and vines were grown further north than they are now (the vines grown in York in Viking times etc). Similarly, statements about frost fairs and freezing of the Baltic so armies could cross etc. Frost fairs on the Thames in London occurred more readily because the tidal limit was at the old London Bridge (the 5ft weir under it). The bridge was rebuilt around the 1840s and the frost fairs stopped. If statements continue to be based on historical accounts they will be easy to knock down with all the usual phrases such as the need for contemporary sources, reliable chroniclers and annalists, who witnessed the events rather than through hearsay. As you all know various people in CRU (maybe less so now) have considerable experience in dealing with this type of data. Christian Pfister also has a lifetime of experience of this. There is a paper coming out from the CRU conference with a reconstruction of summer and winter temps for Holland back to about AD 800, which shows the 20th century warmer than all others. Evidence is sparser before 1400 but the workers at KNMI (Aryan van Engelen et al.) take all this into account.

I hope this is of use and hasn't been a total waste of time.

In Victoria last month, did you discuss how the policymaker's summary will report the millennial temperature series ? Are there any tentative phrases you're working on a la Balance of evidence etc ? Is Chapter 12 thinking of a new sentence to supercede the above ? Any sentence on the millennium record should be in Ch. 2.

Cheers
Phil

Prof. Phil Jones
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Norwich Email p.jones@uea.ac.uk
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From: Phil Jones <p.jones@uea.ac.uk>
To: Benjamin Santer <e782144@popgun.llnl.gov>
Subject: JGR paper
Date: Fri Aug 18 17:19:46 2000

Ben,

Here a few main points about the paper. I've ignored minor English/wording things I spotted.

p4 It seems better to put the other anthro forcings before the natural get discussed. (top of page). ie Other heteorogeneous.. sentence should be before Stratospheric aerosols.

p4 Bottom. Could reference Delworth et al to illustrate the 'perfect' model argument. They reproduced reality 1 out of 5 attempts.

p5 Don't like phenomenology of ENSO, change to ENSO sequences ?

p6 middle. Emphasise that withe models you can look at a lot longer series.

p6 bottom. Whether the model was really 'perfect' Michaels would find some problem.

p7 2/3rd way down. Say something about Santer et al (2000a).

p9 Don't think you need to say you got the SOI from CRU.

p10 ECHAM4 has solar, but how much does it change by. Or is it constant ?

p11 end of 2. Presumably in combining the SAT and SST you used anomalies. Worthwhile saying.

p12-15 Section 3 gets to read like a recipe. It is important, but it might be better as an Appendix. Also I guess the amount of detail depends on success of other submissions. I think the section needs reworking a bit as the style changes somewhat.

Have you considered whether alpha and tau and t(ramp) can differ by a month between the surface and 2LT.

The lag you use is 7 months. The science paper of Tom's uses 6 months.

In the later tables I wasn't clear how raw and nofilt relate to each other. I guess all the Tables need longer captions with more explanation. I couldn't figure out what the () numbers referred to in the Tables.

p17 I wonder if it's possible to show in a diagram that the iterative scheme works and you're getting to a global rather local minimum.

p19 The higher 'ratios' get nearer to my 2, but only at the high end.

p20 The last 4 numbers in Table 3 have been multiplied by 0.1 .

p23 An interesting aside would be to show in one of the Tables how much change in the observations is due to volcanoes (ie show how much cooling due to this there has been). People will quote this value. It shows that 'natural' factors (solar/volcanoes) have led to cooling as solar effects will be very small over this time.

p24 Emphasise later that models and obs all show 2LT level changes more than surface.

p24 Say something about how good ECHAM4 is for ENSO, or refer to a paper.

p25-33 All good stuff, but it does take a time to read. Not a very helpful comment, I know, but I'm being a referee.

p33 Does Fig 7 use the same data as in Fig 5 ? One showing things through time, the other as a distribution.

p35 PCM crept into the Hamburg section, so it should be said here when the GISS section starts.

p38 Quantify the volcanic cooling. I mentioned this earlier.

p39 Not clear what independent components are wrt Smith et al (2000).

p42 Surface data has errors too.

p43 The last sentence of the acknowledgements is like a red rag to a bull for Michaels. Even the perceptive adjective will not placate him.

Have to go home now. I think I've covered most things I noticed.

Have a good weekend !

Cheers

Phil

From: Stephen H Schneider <shs@stanford.edu>
To: tkarl@ncdc.noaa.gov
Subject: Re: THC collapse
Date: Wed, 23 Aug 2000 10:43:29 -0700 (PDT)
Cc: Thomas Stocker <stocker@climate.unibe.ch>, Jerry Meehl
<meehl@meeker.ucar.edu>, Timothy Carter <tim.carter@vyh.fi>,
maureen.joseph@eci.ox.ac.uk, lindam@ucar.edu, m.hulme@uea.ac.uk,
peter.whetton@dar.csiro.au, giorgi@ictp.trieste.it, cubasch@dkrz.de,
ckfolland@meto.gov.uk, hewitson@egs.uct.ac.za, "Stouffer, Ron"
<rjs@gfdl.gov>, DEASTERL@ncdc.noaa.gov

Great Tom, I think we are converging to much clearer meanings across various cultures here. Please get the inconclusive out! By the way, "possible" still has some logical issues as it is true for very large or very small probabilities in principle, but if you define it clearly it is probably OK--but "quite possible" conveys medium confidence better--but then why not use medium confidence, as the 3 rounds of review over the guidance paper concluded after going through exactly the kinds of discussions were having now. Thanks, Steve

On Wed, 23 Aug 2000 tkarl@ncdc.noaa.gov wrote:

>
>
> Steve, I agree with your assesement of inconclusive --- quite possible is
> much better and we use 'possible' in the US National Assessment.
Surveys
> has shown that the term 'possible' is interpreted in this range by the
> public.
>
> Tom
>
>
>
>
> Stephen H Schneider <shs@stanford.edu> on 08/23/2000 03:02:33 AM
>
>
>
> To: Thomas Stocker <stocker@climate.unibe.ch>
>
> cc: Jerry Meehl <meehl@meeker.ucar.edu>, Timothy Carter
> <tim.carter@vyh.fi>, maureen.joseph@eci.ox.ac.uk,
> lindam@ucar.edu, m.hulme@uea.ac.uk,
> peter.whetton@dar.csiro.au, giorgi@ictp.trieste.it,
> Tom Karl/NCDC, cubasch@dkrz.de,
> ckfolland@meto.gov.uk, hewitson@egs.uct.ac.za,
> "Stouffer, Ron" <rjs@gfdl.gov>
>
>
>
> Subject: Re: THC collapse
>

> > DEar Jerry, Tim and Ron et al
> >
> > I agree that an abrupt collapse - abrupt meaning within less than a
> decade, say
> > - has not been simulated by any climate model (3D and intermediate
> complexity)
> > in response to increasing CO2. Some models do show for longer
> integrations a
> > complete collapse that occurs within about 100-150 years. If you put
that
> into
> > context of the apparent stability of THC during the last 10,000 years
or
> so,
> > this is pretty "abrupt".
> >
> > Following up on the discussion regarding THC collapse, I think the
> statement Ron
> > apparently added to Ch9 needs to be made more specific. In order to
keep
> Ch7 and
> > Ch9 consistent, I propose to Ron the following revision:
> >
> > "It seems that the likelihood of a collapse of the THC by year 2100
is
> less
> > than previously thought in the SAR based on the AOGCM results to
date."
> >
> > There is really no model basis to extend this statement beyond 2100
as
> evidenced
> > by the figures that we show in TAR. There are many models that now
run up
> to
> > 2060, some up to 2100, but very few longer.
> >
> > Also I should add for your information, that we add to Ch7 a
sentence:
> >
> > "Models with reduced THC appear to be more susceptible for a
> > shutdown."
> >
> > Models indicate that the THC becomes more susceptible to collapse if
> previously
> > reduced (GFDL results by Tziperman, Science 97 and JPO 99). This is
> important as
> > "collapse unlikely by 2100" should not tempt people to conclude that
THC
> > collapse is hence not an issue. The contrary is true: reduction means
> > destabilisation.
> >
> >
> > Best regards
> >

> > thomas
> > --
> > -----
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From: Keith Briffa <k.briffa@uea.ac.uk>

To: mhughes@ltrr.arizona.edu

Subject: Re: cool bristlecone, etc

Date: Fri Aug 25 15:19:20 2000

Hi again Malcolm

I am forwarding the data in another message (from Tim). I am sending the whole lot for simplicity. Please don't pass on until we hear whether the paper is accepted or not.

Remember that , although they are strongly correlated with them, these data are not identical in the high frequency domain to the equivalent data standardised using say a Hegershoff function. The main purpose here was to extract long-timescale variations and I still consider the inter annual to decadal variability to be better defined using the 'traditional' approach. For a first look anyway these are fine

best wishes

Keith

At 04:14 AM 8/24/00 -0700, you wrote:

Dear Keith,

It was good to talk with you this morning. This is a reminder about sending your Western North America banded record as you suggested. I suspect that you are right to think that it would eventually be best to use a customized banded set, but as a start, I think it would be good to compare the WNW record with the mean series Graybill and Idso used in their 1993 paper, and with the single site Campito Mountain record. I'll start with a simple graphical comparison and then move to comparing waveforms extracted by, for example, SSA. My hope is that we can fairly rapidly generate a note to something like GRL or JoC's new short format, putting a believable version of these records out there for general use.

Please reply to the mhughes@ltrr.arizona.edu address. I'm sending it from my other address as well as a 'belt-and-braces' approach because of recent e-mail problems. Looking forward to working on this with you, Cheers, Malcolm

Malcolm Hughes

Professor of Dendrochronology

Laboratory of Tree-Ring Research

University of Arizona

Tucson, AZ 85721

520-621-6470

fax 520-621-8229

From: "Ben Matthews" <ben@chooseclimate.org>
To: "Mike Hulme" <m.hulme@uea.ac.uk>
Subject: Re: interactive climate science-policy website,
Date: Tue, 5 Sep 2000 00:14:56 +0100
Reply-to: "Ben Matthews" <ben@chooseclimate.org>

Dear Mike,

Regarding my last message,

In case you wonder about my background, I have attached a 2-page version of my CV, in rich-text format, file bjhmcv2.rtf

My experience, ranging from laboratory work with CO₂ fluxes and marine algae, through to organising events at the UN climate negotiations, combined with a strong mathematical and linguistic background, is a somewhat unusual combination which perhaps makes me more a "jack of all trades" than a specialist. On the other hand, this has given me an interdisciplinary overview which may be valuable for bridging the gap between science and policy, appreciating dilemmas and uncertainties, and communicating these around the world.

However, Kyoto left me very disillusioned by the apparent lack of connection between climate science and policy -in the protocol there was not one sentence discussing what we need to do to stabilise the climate in the long term, based on scientific predictions. This made me wonder, what is the use of my intricate research on air-sea CO₂ exchange, if the policymakers ignore even the most basic knowledge? I left UEA and started working at home, developing interactive web graphics showing the link between per-capita emissions and global climate change. Eventually, I realised that working alone was neither effective nor sustainable, and this has led to unfortunate personal circumstances. Now I need the stimulus of working again in a team, in an institute, even if this requires sacrificing of my own ideas. I am not just looking for a "job", it is more important to me, to rejoin the research community, and feel I am making the best use of my skills. I hope you can help, if only to discuss the possibilities.

I have also attached a zip package containing the interactive java applets which I developed,
it's only 90K including supporting webpages and historical data.
Once unzipped (all in one directory), you have to open the file "starthere.html" in any java-enabled web browser.

I can send a self-extracting windows version if you prefer, on the other hand you may find it easier just to look at the website

www.chooseclimate.org/applet/

Currently, this uses only very crude formulae loosely based on IPCC SAR and GCI's C&C, -but the presentation is unique: you can adjust the parameters just by dragging controls with a mouse, and all the linked plots respond instantly. It's hard to describe in words, which is why I encourage you to have a look.

Ben

Dr Ben Matthews ben@chooseclimate.org,

----- Original Message -----

From: Mike Hulme <m.hulme@uea.ac.uk>

To: Ben Matthews <ben@chooseclimate.org>

Sent: 04 September 2000 13:38

Subject: Re: interactive climate science-policy website,

> Thanks for this note Ben.

>

> I would be interested in talking about your ideas at some stage,

> particularly in relation

> to our outreach strategy. We are appointing a Communications Manager very

> soon and you are

> welcome to attend the presentations as listed below:

>

> I would suggest that we arrange a meeting a little further down the line,

> once the Centre

> has started operating in its new premises after 2 October.

>

> Mike

>

> _____

Attachment Converted: "c:\eudora\attach\cca21.zip"

Attachment Converted: "c:\eudora\attach\bjhmcv2.rtf"

From: "Mick Kelly" <m.kelly@uea.ac.uk>
To: j.kohler@econ.cam.ac.uk, m.hulme@uea.ac.uk, simon.shackley@umist.ac.uk
Subject: Tyndall RP2 proposal, final version
Date: Thu, 07 Sep 2000 18:58:37 +0100
Reply-to: m.kelly@uea.ac.uk
Cc: n.adger@uea.ac.uk

Dear Mike

I have attached the final version of the RP2 outline proposal on the interaction between the flexible mechanisms and the WTO trade rules. Please jettison the previous draft.

As noted earlier, Neil and I see this project as delivering multiple benefits to the Tyndall Centre on the basis of a limited, 'value-added' investment, not least in terms of tying Shell International to the Centre. We also highlight the suggestion of a workshop on common themes to be held in a couple of years' time to link related projects across the research programmes (though this is not covered by the current proposal).

Regards

Mick

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United Kingdom
Tel: 44-1603-592091 Fax: 44-1603-507784
Email: m.kelly@uea.ac.uk
Web: <http://www.cru.uea.ac.uk/tiempo/>

Attachment Converted: "c:\eudora\attach\tyndall11.doc"

From: "Griggs, Dave" <djgriggs@meto.gov.uk>
To: 'TAR CLA list' <tar_cla@meto.gov.uk>, 'TAR LA list'
<tar_la@meto.gov.uk>
Subject: Uncertainties again
Date: Fri, 08 Sep 2000 18:02:09 +0100
Cc: 'TAR Review Editors' <tar_re@meto.gov.uk>, "'Watson, Bob'"
<rwatson@worldbank.org>, "'Moss, Richard'" <richard.moss@pnl.gov>,
"'Houghton, Sir John'" <jthoughton@ipccwgl.demon.co.uk>, "'Albritton,
Dan'" <aldirhoff@al.noaa.gov>, "'Swart, Rob'" <Rob.Swart@rivm.nl>,
"'Leary, Neil'" <nleary@usgcrp.gov>, "'McCarthy, Jim'"
<James_j_mccarthy@harvard.edu>, "'Stone, John'" <john.stone@ec.gc.ca>,
"'shs@leland.stanford.edu'" <shs@leland.stanford.edu>,
"'m.manning@niwa.cri.nz'" <m.manning@niwa.cri.nz>

Dear CLAs/LAs

As you all know, in my Victoria follow-up e-mail of 2 August I presented a summary of the agreement we reached in Victoria on a common use of terminology to express degree of likelihood in the TAR. At that time the word or term to be used for the central box of 33 to 66% had not been agreed and the word "inconclusive" was proposed for that category. Since that time there has been a lengthy discussion, including Working Groups II and III, regarding the best word to be used in this category. To cut a long story short the term we would now like you to use for this middle category is "medium likelihood". I am sorry I have not been able to canvas this around all of you but from the discussions this term was agreed by all to be the best compromise. In particular, it clearly maintains the scale as one of degrees of likelihood, whereas inconclusive could be confused as to whether a degree of likelihood was being expressed or whether there was insufficient information to conclude a likelihood. I attach a table showing what should now be the final scale.

During the discussions it became clear that in addition to making likelihood statements it is sometimes more appropriate to express statements in terms of a degree of confidence, and indeed several chapters use this terminology. As you know the Uncertainties Guidance paper by Richard Moss and Steve Schneider recommends a scale of confidence from Very Low to Very High confidence. WGII in particular are using this scale and so I would ask that, if you choose to express things in terms of a level of confidence, that you use the terms as they are laid down in the guidance paper. This in no way affects the use of the likelihood scale where this is more appropriate. For

example, if we are highly uncertain how well a model handles a particular process, we may have "very low confidence" in a model result which is highly dependent on this process. If we have no other corroborating evidence we may therefore conclude that there is insufficient information to assign a likelihood in this case. By following the guidance paper when expressing a level of confidence we will hopefully improve the consistency between the two reports. Incidentally, if there are instances in the WGII report where they are able express degrees of likelihood they are going to try and use our scale.

Thirdly, there has been a lot of discussion about the impression which the likelihood scale, if taken out of context, could give for low likelihood, high consequence events, such as a disintegration of the WAIS or a shutdown of the THC in the next 100years. Please bear in mind that policymakers must balance likelihood and consequence in deciding whether or not to take action. Therefore please take extra care when considering text for these types of issues as simply expressing them as "extremely unlikely" does not give the full picture. For example, you could say an aircraft was "extremely unlikely" to crash on its next flight but if there was a 1% chance I would not fly on it. While it is a true statement the right balance is only achieved when the consequence is also brought in to put the risk in context.

I apologise for this late change to our scale but I hope you all agree that it is an improvement. If anything is not clear about any of the above please do not hesitate to contact me.

Best regards

Dave

<<Agreed terminology2.doc>>

Dr David Griggs
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UK

Tel +44 (0)1344 856615
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Email: djgriggs@meto.gov.uk

Attachment Converted: "c:\eudora\attach\Agreed terminology2.doc"

From: "Mick Kelly" <m.kelly@uea.ac.uk>
To: m.hulme@uea.ac.uk, t.oriordan@uea.ac.uk
Subject: Shell International
Date: Mon, 11 Sep 2000 13:05:29 +0100
Reply-to: m.kelly@uea.ac.uk

Mike and Tim

Notes from the meeting with Shell International attached.

Sorry about the delay.

I suspect that the climate change team in Shell International is probably the best route through to funding from elsewhere in the organisation including the foundation as they seem to have good access to the top levels.

Mick

Mick Kelly Climatic Research Unit
University of East Anglia Norwich NR4 7TJ
United Kingdom
Tel: 44-1603-592091 Fax: 44-1603-507784
Email: m.kelly@uea.ac.uk
Web: <http://www.cru.uea.ac.uk/tiempo/>

Attachment Converted: "c:\eudora\attach\shell.doc"

From: GIORGI FILIPPO <giorgi@ictp.trieste.it>
To: Chapter 10 LAs -- Congbin Fu <fcb@ast590.tea.ac.cn>, GIORGI FILIPPO <giorgi@ictp.trieste.it>, Bruce Hewitson <hewitson@egs.uct.ac.za>, Mike Hulme <m.hulme@uea.ac.uk>, Jens Christensen <jhc@dmi.min.dk>, Linda Mearns <lindam@ucar.edu>, Richard Jones <rgjones@meto.gov.uk>, Hans von Storch <storch@gkss.de>, Peter Whetton <phw@dar.csiro.au>
Subject: On "what to do?"
Date: Mon, 11 Sep 2000 16:58:02 +0200 (MET DST)

Dear All

we heard the opinions of most LAs, namely Jens, Richard, Linda, Peter, and Hans as well as some interesting interpretations of my email (Linda says:

" You seem to be assuming that the most desirable result is if the SRES results have no contrasts with the IS92a results. I don't understand your reasoning on this." I do not have any particular desire on the new data. We said that one thing to look at was the agreement with the old data and thus I noticed that relaxing the criteria would yield a greater agreement). I would say that a broad range of opinions was covered, from one where the SRES should essentially be commented upon concerning their agreement with the old data to one in which all the old stuff should be replaced with SRES stuff. Some people want to make the BOX more central, others want to get rid of it.

Given this, I would like to add my own opinion developed through the weekend.

First let me say that in general, as my own opinion, I feel rather uncomfortable about using not only unpublished but also un reviewed material as the backbone of our conclusions (or any conclusions). I realize that chapter 9 is including SRES stuff, and thus we can and need to do that too, but the fact is that in doing so the rules of IPCC have been softened to the point that in this way the IPCC is not any more an assessment of published science (which is its proclaimed goal) but production of results. The softened condition that the models themselves have to be published does not even apply because the Japanese model for example is very different from the published one which gave results not even close to the actual outlier version (in the old dataset the CCC model was the outlier). Essentially, I feel that at this point there are very little rules and almost anything goes. I think this will set a dangerous precedent which might mine the IPCC credibility, and I am a bit uncomfortable that now nearly everybody seems to think that it is just ok to do this. Anyways, this is only my opinion for what it is worth.

Going to the problem at hand, I have a proposal that is in between the two extreme positions. I think the SRES runs should be included and highlighted in the chapter, but should not be the only source of our conclusions, partially also for the reasons I state above (I seem to remember that in Chapter 9 the SRES results were only a small section in the whole chapter in which it was said that they essentially confirmed previous findings). Also let me say that, as it currently stands, the box is essentially meaningless, because it simply repeats what is already said in the executive summary. With these premises here is my proposal:

1) We leave 10.3 more or less as it is, a discussion of published science on model behavior, uncertainty, some climate change runs. Perhaps we shorten it or something like that. I am not in favor of presenting Giorgi and Francisco-type plots for the SRES runs for the simple reason that they do not convey effectively what readers want. Proof is that we had all the plots there and we were accused of not having any results in the chapter !! I think people want something more direct, i.e. plots similar to the +/- one we had proposed in the BOX.

2) We make the BOX only with SRES results, i.e. the BOX becomes a summary presentation of the SRES projections. In this way we accomplish several objectives: we highlight the SRES results in a way that is of direct impact (after all this is what working group II people are really interested in); we can explicitly state that the results are preliminary and sort of differentiate them from the more IPCC-proper chapter material (of course we are not going to say so); we have a natural place for the BOX (end of 10.3), do not need to rewrite the whole thing and just need to make the proper connections with the rest of the chapter. All and all I think this is a feasible and clean solution. The rest of the material in the old box (sections a and c) was really just general material repetitive of what we were saying in various other part of the chapter.

3) In the executive summary we summarize what we believe are the confident patterns from the combination of old and new runs.

As to what should the SRES box look like. I hear people liked a lot our +/- plot, so we do the same types of plots, both for precip and temperature, one for the A2 and one for B2 scenarios, plus one or two paragraphs explaining the plots. This will portray agreement not only across models but also across what are now considered plausible scenarios. We can easily fit 4 plots in a page and if need be fit the 1-2 paragraphs on another page (I do not see anything wrong with a 1.5 page box). For precipitation I think the old criteria are fine. For temperature this

is what I propose. In the precip plots we had 4 sub-categories, (+, - large change, small change) plus the inconclusive, or whatever we decided to call it. Similarly, we could do 4 categories here
1) Amplification positive, 2) Amplification negative (i.e. less than the global average), 3) strong amplification (> 50%), 4) small amplification (between 25 and 50 %). I cannot visualize it at the moment, but I think this will work to figures analogous to the precip ones. Correct me if I am wrong.

To the two technical issues:

1) Do we soften our requirement, i.e. from n-1 to n-2 model agreement? I do not feel strongly about it but am more in favor of not softening the criterion. We are looking for confidence and model agreement and should have stringent requirements on it.

2) Do we include the outliers in the analysis? I say yes, not having time for more detailed analysis as to why they should not be included. In Chapter 9 they are presented as bracketing the answers not as being wrong.

This is the problem of not having published research on this. perhaps a paper would have excluded them on scientific grounds, but can we at this point? I am not sure we can have solid enough foundations to legitimate it. Besides, I have done analysis without them as well and things did not change almost at all.

To the operational issues:

1) I agree there is no time for a paper to be delivered before the Sept. 26 deadline. After the deadline however, and with some calm, I think we should have a paper on it.

2) Meeting or conference call. I myself am not keen on a meeting of the Europeans. Jens is not back until the end of the week, which means the meeting would have to be during the last week before deadline. With all that is still left to do on the chapter and other internal commitments I have,

I certainly could do without spending 2 days to do this (which is always the minimum it takes me to get anywhere and back)and I cannot do it over the weekend since I am not here.

It sounds like we would have to contact people by phone anyways (see Peter and Linda's messages), so why not a conference call directly?

>From the technical viewpoint Linda seems to be the best person to organize this. As soon as Jens is back perhaps? (Jens if you can read this

can you let us know when this is possible?).

3) We just got the MPI data and the full CCC ones (I guess some was lost in the previous run). We need to incorporate these so we have all models available. I and Bruce will interact on this.

4) I agree we should contact the TSU about it, but I also think we should have a proposal on it with less spread than current to present them.

Last but not least, please work on your section revisions (especially those who have nothing to do with the BOX) so at least we get that out of the way.

Cheers, Filippo

```
#####  
# Filippo Giorgi, Senior Scientist and Head, #  
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# The Abdus Salam International Centre for Theoretical Physics #  
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#####
```

From: GIORGI FILIPPO <giorgi@ictp.trieste.it>
To: Chapter 10 LAs -- Congbin Fu <fcb@ast590.tea.ac.cn>, GIORGI FILIPPO <giorgi@ictp.trieste.it>, Bruce Hewitson <hewitson@egs.uct.ac.za>, Mike Hulme <m.hulme@uea.ac.uk>, Jens Christensen <jhc@dmi.min.dk>, Linda Mearns <lindam@ucar.edu>, Richard Jones <rgjones@meto.gov.uk>, Hans von Storch <storch@gkss.de>, Peter Whetton <phw@dar.csiro.au>
Subject: more on "what to do"
Date: Tue, 12 Sep 2000 11:53:20 +0200 (MET DST)

Dear All

I think I heard replies to my last proposal from most of you. I have also had a phone conversation with Linda. So let me try to summarize the situation

1) From the replies I got, it sounds like at least the basic idea of my proposal is viable. In particular I read an at least semi-consensus, and certainly some strong individual positions, that the SRES material , since it is unpublished (and remember unreviewed until now), should not be presented as our sole or even primary source of conclusions. Now, I share that position and in fact quite strongly. Presenting such material breaks the proclaimed IPCC rules. Now the rules have been softened for this case, but remember that there are people around who are paid to find faults in the IPCC process and the last thing I want to do is being accused of having broken the rules. I think the TSU people are too optimistic and casual in the way they change the rules during the process and expect people to accept that this "just" happens. Remember what happened to Ben Santer after the SAR. Besides, I myself think that material for a document as important as the TAR cannot be drawn from last-minute barely quality checked and un-peered reviewed material (people have barely looked at the MPI run that was completed last friday !!). It is up to the IPCC to better plan these things and avoid the mess. Be it as it may, unless somebody is strongly against this position, I will assume that we can proceed from this basis.

2) Having said the above, it is also clear that we can present the IPCC data in some format. Chapter 9 is doing it (remember also in their case the SRES stuff is only a minor component of the chapter) so we can and I think we should because it is relevant and important material, but with the proper caveats clearly up front, i.e. that whatever we present is a preliminary analysis that has not undergone a publication process. It would be certainly strange and confusing to have the SRES discussed in Chapter 9 but not in our chapter in some form. Besides we went through a significant effort to get it and process it. I myself think that the SRES information is important to provide. It is just unfortunate, but not surprising, that it came around too late.

3) So the question is at this point how do we present the SRES. I suggested not to incorporate it within the text of 10.3, since 10.3 is our assessment of published research which has undergone peer and government review. I stand strongly by that suggestion. Obviously 10.3 might need a bit of rewriting to make it flow better with possibly different conclusions but not more than that. I then suggested to make the Box an SRES Box including the +/- format figures (I thought we needed 4, i.e. two for each scenario, but Linda pointed out we really need only 2, one for precip and one for temperature each including the two scenarios). Now this offers several advantages: we can say right up front that this is from a preliminary analysis; we can separate it cleanly from the rest of the "official " text; it gives direct info in a format that people seem to like. Two very legitimate comments were made on this. Peter said, if we give this more palatable format (the +/- figures) only for SRES data would it not implicitly give it too much attention? Linda said: why not present similar plots for the IS92 data? The obvious action which would address both of these concerns is to present similar plots for the IS92 data. This is certainly an option. The only problem I see is that I think the clear separation of published and unpublished results would be lost if we put it in the BOX. The alternative is to do those figures and put them in 10.3, leaving the SRES for the BOX. This could be a good option, although it might require significant effort. All and all, I am still in favor of an SRES-only box with a clear statement up front that gets us off the hook in case of problems (you can see it as a sort of disclaimer I guess).

So let's come to the next point: we need to decide on this and soon. The best way appears to be a conference call. Linda suggested thursday, which is fine with me. It now looks like Richard cannot organize this. So Linda I am afraid you are left with the organization of it. The call would have to be during European-South African afternoon - US morning and I am afraid I am not sure what time in Australia. problems is: Jens can you make it? I think Jens is the person in the group most strongly opposed to presenting SRES data, so it important he is in the conference call. It is also critical that Peter participates, given he has been the main player in all this. Now here is my proposal:

Conference call on thursday 3 p.m. Trieste-Hamburg time, which means 4 p.m. Cape town time, 2 p.m. Bracknell time, 9 a.m. Boulder time, 8 a.m. Fairbanks time and ??? Australia time. Linda is this feasible for you to organize? Is this ok for all? Conbin, are you available at all?

items of discussion:

Question 1): Do we do an SRES BOX with +/- figures?

Question 2): What are the technical details (n-1 vs. n-2 model agreement, inclusion of outliers, threshold for large vs. small vs. no change both for precip change and temperature amplification factor).

Question 3): Do we do similar figures for IS92 data which would either replace the current figures on IS92 in the text (I think this would be perfectly acceptable since it is simply a way to present in a different way published results).

Question 4): How do we incorporate the SRES results within the current executive summary

I hope that by thursday I will have all data to do all relevant figures. I need to get CCC control and MPI-DMI data from Bruce and dig out the old IS92 data. If not by thursday then hopefully by friday. Once I have the data I can easily directly calculate all the thresholds necessary for doing the relevant figures. I will then circulate all the material to you.

Needless to say that any data based on SRES that is circulated among us should NOT go any further (except for the chapter of course) until we decide what to do with it (a paper or something like that).

In the mean time, I will never tire to keep asking you to please work on the section revisions and let's get those out of the way.

Cheers, Filippo

```
#####  
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#####
```


From: "Whetton, Peter" <peter.whetton@dar.csiro.au>
To: 'Hans von Storch' <Hans.von.Storch@gkss.de>, Congbin Fu
<fcb@ast590.tea.ac.cn>, GIORGI FILIPPO <giorgi@ictp.trieste.it>, Bruce
Hewitson <hewitson@egs.uct.ac.za>, Mike Hulme <m.hulme@uea.ac.uk>, Jens
Christensen <jhc@dmi.min.dk>, Linda Mearns <lindam@ucar.edu>, Richard
Jones <rgjones@meto.gov.uk>, "Whetton, Peter"
<peter.whetton@dar.csiro.au>
Subject: RE: n-1 / n-2
Date: Thu, 14 Sep 2000 10:30:27 +1100

Dear all,

It could be viewed that using n-1 for 9 models where we used n-1 for five
models before is an implicit change in the stringency of our criterion.
When we had five models, agreement (0/5, 1/5, 4/5 or 5/5) could be
expected
37% of the time just by chance (ignoring the near zero case). With nine
models the equivalent figure for n-1 is only 3.5%, and it is still much
lower for n-2 (18%)... (assuming that my somewhat rusty probability
calculations are correct). It really depends on what we had understood
the
purpose of the criterion to be. I am not certain how much this was
discussed.

Also, I would prefer Friday night as well if it means that more
information
will be available.

Cheers

Peter

-----Original Message-----

From: Hans von Storch [mailto:Hans.von.Storch@gkss.de]
Sent: Wednesday, 13 September 2000 19:48
To: Congbin Fu; GIORGI FILIPPO; Bruce Hewitson; Mike Hulme; Jens
Christensen; Linda Mearns; Richard Jones; Hans von Storch; Peter Whetton
Subject: n-1 / n-2

Dear friends,

I have already indicated that I favour the n-1 version. Obviously, this
choice is arbitrary, but it was made BEFORE we did the analysis. By
changing the criterion AFTER we have seen the data, we may be targeted
by critics for biased rules. Using material, which is unpublished and
unreviewed is already a bit shabby (Hans Oerlemans is unwilling to
participate in the IPCC process because of a similar incident in the
1995 report!).

Hans

--

Hans von Storch

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From: Phil Jones <p.jones@uea.ac.uk>
To: <wsh@unite.com.au>
Subject: Re: TAR
Date: Mon Sep 18 16:23:04 2000
Cc: ckfolland@meto.gov.uk, tkarl@ncdc.noaa.gov

Warwick,

I did not think I would get a chance today to look at the web page. I see what boxes you are referring to. The interpolation procedure cannot produce larger anomalies than neighbours (larger values in a single month). If you have found any of these I will investigate. If you are talking about larger trends then that is a different matter. Trends say in Fig 2.9 for the 1976-99 period require 16 years to have data and at least 10 months in each year. It is conceivable that at there are 24 years in this period that missing values in some boxes influence trend calculation.

I would expect this to be random across the globe.

Cheers
Phil

Warwick,

Been away. Just checked my program and the interpolation shouldn't produce larger anomalies than the neighbouring cells. So can you send me the cells, months and year of the two cells you've found ? If I have this I can check to see what has happened and answer (1).

As for (2) and (3) we compared all stations with neighbours and these two stations did not have problems when the work was done (around 1985/6).

I am not around much for the next 3 weeks but will be here most of this week and will try to answer (1) if I get more details. If you have the names of stations that you've compared Olenek and Verhojansk with I would appreciate that.

Cheers
Phil

At 05:13 AM 9/14/00 +1000, you wrote:
>Dear Phillip and Chris Folland (with your IPCC hat on),

>Some days ago Chris I emailed to Tom Karl and you replied re the grid cells
>in north Siberia with no stations, yet carrying red circle grid point
>anomalies in the TAR Fig 2.9 global maps. I even sent a gif file map
>showing the grid cells barren of stations greyed out. You said this was
>due to interpolation and referred me to Phillip and procedures described in
>a submitted paper. In the last couple of days I have put up a page
>detailing shortcomings in your TAR Fig 2.9 maps in the north Siberian
>region, everything is specified there with diagrams and numbered grid
>points.

>[1] One issue is that two of the interpolated grid cells have larger
>anomalies than the parent cells !!!!?????

>This must be explained.

>[2] Another serious issue is that obvious non-homogenous warming in Olenek
>and Verhojansk is being interpolated through to adjoining grid cells with
>no stations, like cancer.

>[3] The third serious issue is that the urbanization affected trend from
>the Irkutsk grid cell near Lake Baikal, looks to be interpolated into its
>western neighbour.

>

>I am sure there are many other cases of this, 2 and 3

> happening.

>Best regards,

>Warwick Hughes (I have sent this to CKF)

>

From: mhughes@lrr.arizona.edu
To: tom crowley <tom@ocean.tamu.edu>
Subject: Re: old stuff
Date: Fri, 22 Sep 2000 06:22:50 -0700
Cc: <k.briffa@uea.ac.uk>

Dear Tom,

The difference between the Campito Mountain record and, for example, the one from the Polar Urals that you mention, is that there is no meaningful correlation between the Campito record and local temperature, whereas there is a strong correlation in the Polar Urals case. I give references to the work reporting this phenomenon at the end of this message, but I'm afraid I'm missing the references to the technical comments that are being responded to in the last two. If you examine my Fig 1 closely you will see that the Campito record and Keith's reconstruction from wood density are extraordinarily similar until 1850. After that they differ not only in the lack of long-term trend in Keith's record, but in every other respect - the decadal-scale correlation breaks down. I tried to imply in my e-mail, but will now say it directly, that although a direct carbon dioxide effect is still the best candidate to explain this effect, it is far from proven. In any case, the relevant point is that there is no meaningful correlation with local temperature. Not all high-elevation tree-ring records from the West that might reflect temperature show this upward trend. It is only clear in the driest parts (western) of the region (the Great Basin), above about 3150 meters elevation, in trees old enough (>~800 years) to have lost most of their bark - 'stripbark' trees. As luck would have it, these are precisely the trees that give the chance to build temperature records for most of the Holocene. I am confident that, before AD1850, they do contain a record of decadal-scale growth season temperature variability. I am equally confident that, after that date, they are recording something else. I'm split between Harvard Forest and UMASS these days, and my copy of your paper is not with me today. I'd be interested to know what the name of the site for the LaMarche central Colorado record was.

Cheers, Malcolm

Reference List

1. Graybill, Donald A., and Sherwood B. Idso. 1993. Detecting the Aerial Fertilization Effects of Atmospheric CO₂ Enrichment in Tree-Ring Chronologies. *Global Biogeochemical Cycles* 7, no. 1: 81-95.
2. LaMarche, V. C., D. A. Graybill, H. C. Fritts, and M. R. Rose. 1984. Increasing Atmospheric Carbon Dioxide: Tree Ring Evidence for Growth Enhancement in Natural Vegetation. *Science* 225: 1019-21.

3. ---1986. Carbon Dioxide Enhancement of Tree Growth At High Elevations. Science 231: 859-60.

4. ---1986. Technical Comments: Carbon Dioxide Enhancement of Tree Growth At High Elevations. Science 231: 860.

Quoting tom crowley <tom@ocean.tamu.edu>:

> Dear Malcolm and Keith,

>

> as I discuss in my Ambio paper the "anomalous" late 19th century warming
> also occurs in a LaMarche tree ring record from central Colorado, the

> Urals

> record of Briffa, and the east China phenological temperature record of
> Zhu.

>

> Alpine glaciers also started to retreat in many regions around 1850,

> with

> 1/3 to 1/2 of their full retreat occurring before the warming that

> commenced about 1920.

>

> The Overpeck et al Arctic synthesis also discusses warming before 1920 -

> that record matches very closely the Mann et al reconstruction in other

> details back to 1600.

>

> Unpublished work by us on coral trends also suggests slight warming

> between

> about 1850-1920.

>

> So, are you sure that some CO2 fertilization is responsible for this?

> May

> we not actually be seeing a warming?

>

> Tom

>

>

>

> Thomas J. Crowley

> Dept. of Oceanography

> Texas A&M University

> College Station, TX 77843-3146

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- > 979-847-8879 (fax)
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- >
- >
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From: Ben Santer <santer1@llnl.gov>

To: wigley@ucar.edu, roeckner@dkrz.de, ktaylor@zooks.llnl.gov, boyle@pcmdi.llnl.gov, sailes1@llnl.gov, p.jones@uea.ac.uk, doutriau@pcmdi.llnl.gov, jhansen@giss.nasa.gov, meehl@meeker.ucar.edu, bengtsson@dkrz.de

Subject: Status of our JGR paper

Date: Fri, 22 Sep 2000 12:36:38 -0700

Dear All,

I just wanted to keep you informed about the status of our draft JGR paper. First, thanks to all of you for your comments - they were very helpful. I am now in the process of revising the paper, and hope to have a new draft ready by Oct. 10th. After several discussions with Tom, I have decided to repeat the volcano/ENSO signal separation for the observed data and for the GSOP experiment.

The reason for this is that there was a conceptual flaw in what I had done previously. The flaw related to the determination of the "pre-eruption" reference temperature, used as a baseline for estimating the maximum volcanically-induced cooling. Let's call this baseline temperature "TBASE". Previously, I was estimating TBASE for Pinatubo and El Chichon from either the raw or Gauss-filtered temperature data at time $t=0$ (the eruption month). If I was calculating TBASE from the filtered data, the estimate of TBASE was biased by "contamination" from post-eruption cooling. In other words, since I was using a 13-term Gaussian filter, temperature values from $t=0 + 6$ months were influencing TBASE, likely leading to an underestimate of the true TBASE value. I've now modified the program so that TBASE is not computed from the filtered data; instead, it is an average of the temperature anomalies in the MREF months prior to the eruption. There is some sensitivity to the choice of MREF (I've been experiment with values ranging from 6-18 months), which again underscores the uncertainties inherent in separating ENSO and volcanic signals.

The maximum volcanically-induced cooling is still estimated using filtered data, but now I'm using a 5-term binomial filter rather than the 13-term Gaussian.

These changes require repeating most of the analyses in the paper. Preliminary results indicate that the revised estimation of TBASE increases the ratio of the Chichon/Pinatubo maximum coolings, and brings this closer to the ratio of the Chichon/Pinatubo radiative forcings.

Tom has also made a number of useful suggestions regarding reorganization and shortening of various sections of the manuscript. Hopefully the next iteration will be a little shorter than the current version of the paper!

I will be out of my office next week, but should be back by October 2nd.

With best regards, and thanks again for all your help,

Ben

--

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email: santer1@llnl.gov

From: Tom Wigley <wigley@ucar.edu>

To: Ben Santer <santer1@llnl.gov>

Subject: Re: Status of our JGR paper

Date: Fri, 22 Sep 2000 15:47:37 -0600

Cc: roeckner@dkrz.de, ktaylor@zooks.llnl.gov, boyle@pcmdi.llnl.gov, sailes1@llnl.gov, p.jones@uea.ac.uk, doutriau@pcmdi.llnl.gov, jhansen@giss.nasa.gov, meehl@meeker.ucar.edu, bengtsson@dkrz.de

Ben (or, really, everybody else),

I don't know whether you have all seen the paper analyzing the observed data that Ben and I sent to J. Climate ?? This is where the JGR paper began, and it is useful to compare both papers. In the J. Climate paper we assessed the best fits using a subjective balance of raw and lowpass filtered results. The reason for this was because of the difficulty of setting up an automated procedure -- which is the problem that Ben is currently having to deal with. In the next iteration of the JGR paper, the reason for moving to a more automated procedure will be explained. Both the subjective and automated procedures have their advantages and disadvantages. The latter procedure, of course, is in no way 'objective'. Many subjective choices have to be made in setting up the procedure. This is why the word 'automated' is used above, rather than 'objective'.

If you have not seen the J. Climate paper, let me know and I will send you a copy. There is a companion paper that has been accepted by GRL that I will send at the same time.

Cheers, Tom.

Ben Santer wrote:

>

> Dear All,

>

> I just wanted to keep you informed about the status of our draft JGR paper.

> First, thanks to all of you for your comments - they were very helpful. I am now

> in the process of revising the paper, and hope to have a new draft ready by Oct.

> 10th. After several discussions with Tom, I have decided to repeat the

> volcano/ENSO signal separation for the observed data and for the GSOP

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> The reason for this is that there was a conceptual flaw in what I had done

> previously. The flaw related to the determination of the "pre-eruption"

> reference temperature, used as a baseline for estimating the maximum

> volcanically-induced cooling. Let's call this baseline temperature "TBASE".

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> raw or Gauss-filtered temperature data at time t=0 (the eruption month).

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> biased by "contamination" from post-eruption cooling. In other words, since I

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> the uncertainties inherent in separating ENSO and volcanic signals.

>
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> but now I'm using a 5-term binomial filter rather than the 13-term Gaussian.

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> results indicate that the revised estimation of TBASE increases the ratio of the
> Chichon/Pinatubo maximum coolings, and brings this closer to the ratio of the
> Chichon/Pinatubo radiative forcings.

>
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> shortening of various sections of the manuscript. Hopefully the next iteration
> will be a little shorter than the current version of the paper!

>
> I will be out of my office next week, but should be back by October 2nd.

>
> With best regards, and thanks again for all your help,

>
> Ben

> --

> -----

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: k.briffa@uea.ac.uk, p.jones@uea.ac.uk
Subject: OOPS. RETURN EMAIL GLITCHES IN ORIGINAL
Date: Fri, 22 Sep 2000 15:52:15 -0400

>Date: Fri, 22 Sep 2000 15:50:05 -0400
>To: Tim Osborn <t.osborn@uea.ac.uk>
>From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
>Subject: Re: my visit
>Cc: srutherford@virginia.edu, k.briffa@uea, p.jones@uea
>Bcc: mhughes@lrr.arizona.edu, rbradley@geo.umass.edu
>In-Reply-To: <3.0.6.32.20000922092400.007ed450@pop.uea.ac.uk>
>References: <3.0.6.32.20000919101130.00aad100@multiproxy.evsc.virginia.edu> <3.0.6.32.20000919135642.008114b0@pop.uea.ac.uk>

>

>HI Tim,

>

>Very busy, so just a short response for the time being.

>

>Regarding our MBH98 and GRL99 datasets, I'm pretty sure that Scott put those
>on anonymous ftp for you some months ago. So you **should** already have had
>access to all the data we used. In fact, it was only a few select series of
>Malcolm's that weren't made available from the get-go. So data has never
>been an issue for us. I'm happy to hear that it is not an issue for
>you/keith/phil and that you are ready to make your density data available...

>

>A few points of clarification might help here:

>

>The revised method (based on ridge regression) is currently in development
>as far as paleoreconstruction is concerned (we have a paper to be submitted
>on application to the instrumental record only). We intend to test it on
>synthetic proxy datasets (as described in my previous email) before
>applying it to actual proxy data, so your visit, unfortunately, occurs at a
>time that is too premature for comparison with results from this method.

Rather, we were hoping

>you shared some of the interest along the lines of
>developmental/methodological
>issues.

>

>Comparison between warm-season reconstructions would be fine, but you should
>be aware of the extreme caveats with regard to our seasonal
>reconstructions, as spelled out in detail in our "Earth Interactions"

article. We don't do nearly as well for warm-season or cold-season as for annual-mean, and we believe this is consistent w/ the mix of seasonal information contained in the multiproxy dataset. Obviously, things are somewhat different for the more seasonally homogeneous density chronology dataset. So to us, this comparison might not

>seem as worthwhile as it would for you all, but we can do it if all provisos
>and caveats are fully recognized and embraced from the start...

>

>The idea of testing wavelet methods of distinguish contributions on
different timescales sounds like it is of interest to all of us, and
perhaps we can

>move in that direction during your visit.

>

>In any case, we'll have more than enough to do, talk about, investigate,
and no need to necessarily hammer it all out beforehand.

>

>Comments from others (Scott, Phil, Keith?) welcome,

>

>mike

>

>At 09:24 AM 9/22/00 +0100, Tim Osborn wrote:

>>At 10:11 19/09/00 -0400, you wrote:

>>>I will put you up at the "Red Roof Inn" for the 10 nights...

>>>Will have reservations made for you for the night of the 10th through 19th,

>>>checking out morning of the 20th...

>>

>>That sounds great. Thanks.

>>

>>

>>Mike,

>>

>>I've talked over various ideas with Keith and Phil (and I'm cc'ing this to
>>them as well as to Scott), and I've now made some slightly firmer/clearer
>>suggestions, combining your ideas and ours.

>>

>>(1) We're still keen to spend part of the time on reconstruction method
>>issues, since that is one of the specifics that our current funded project
>>needs to address. To avoid being too retrospective, we could do something
>>that combined both your Nature98 and your revised methods:

>>

>>(a) compare your summer/warm season reconstructions (old & new methods)
>>with our reconstructions of Apr-Sep temperature from tree-ring densities
>>(regional/hemispheric averages and spatial comparisons).

>>

>>(b) In (a), we would be comparing reconstructions based on different
>>palaeodata *and* different statistical reconstruction methods. So a better
>>approach would be to use your (old & new) methods with our tree-ring
>>density data set to reconstruct Apr-Sep temperature fields, and then
>>compare with our reconstructions. This would be a good way of comparing
>>methods.

>>

>>(c) We could exchange data/methods to continue comparisons after the end of
>>my visit. We would be keen, for example, to obtain your Nature98 & GRL99
>>datasets and software to play around with after my return. In exchange, we
>>can provide you with our tree-ring density data set and the reconstructions
>>that we have produced from it. Of course, such subsequent work would
>>continue to be collaborative, keeping each other informed/involved with the
>>work.

>>

>>(d) If the tree-ring density data provided useful "added value" to your
>>reconstructions (perhaps at the higher frequencies and providing finer
>>spatial detail?), then we could use an appropriate method (perhaps your new
>>revised one) to produce a new reconstruction using all palaeodata. Such a
>>reconstruction might prove to be an important and well-used product.

>>

>>(2) Of your two specific suggestions I quite strongly prefer the first.
>>The reason is that, again, our project specifically requires comparison of
>>palaeo and model data and the development of appropriate methods to do
>>this. Your first suggestion would take us along those lines. There are
>>two related strands here. The first is to use the model outputs to assess
>>the reliability of the reconstructions (i.e., following the ideas you laid
>>out in your e-mail), which is certainly of interest. The second is to use
>>the reconstructions to evaluate the model simulations of "natural"
>>variability. We've done some comparisons with the HadCM2 and HadCM3
>>simulations - I shall bring papers/results along. What we need to develop
>>further are ways of incorporating the paleo biases/errors in such
>>comparisons. We have begun this, but when I visit we might be able to come
>>up with better methods and apply them to Hadley Centre and/or GFDL
>>comparisons.

>>

>>Your second suggestion, while interesting, is less appealing at this stage,
>>principally because we won't have time to do everything. As it happens,
>>Keith and I have just submitted a paper (to that well-known(!) journal
>>"Dendrochronologia") about timescale-dependent calibration of tree-ring
>>data - I shall bring a copy with me. My feeling is that the quantity of
>>data overlap available for calibration would be a strongly limiting factor

>>in most timescale-dependent approaches, whether they use wavelets or some
>>other filtering-type approach. What interests me more would be the
>>application of wavelets to the full palaeorecords to facilitate in the
>>definition of timescale-dependent coherent patterns (PCs?), rather than
>>just to the calibration period. Anyway, we can talk these ideas over even
>>if there's no time to begin any work yet.

>>

>>I think that a chance to exchange preprints, data, and discuss ongoing
>>developments of our work and yours will, in itself, prove to be a useful
>>outcome of my visit.

>>

>>Best regards

>>

>>Tim

>>

>>

>>

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>>Senior Research Associate | fax: +44 1603 507784
>>Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
>>School of Environmental Sciences | web-site:
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>>Norwich NR4 7TJ | sunclock:
>>UK | <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

>>

>>

>>

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<http://www.evsc.virginia.edu/faculty/people/mann.html>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: mann@virginia.edu
Subject: No Subject
Date: Mon Sep 25 10:16:52 2000
Cc: t.osborn@uea.ac.uk,p.jones@uea.ac.uk

Dear Mike

I know Tim has communicated with you about plans for his visit to Virginia. We have discussed ideas here and I ,for one, am excited about the prospects of joint work. Thank you for agreeing to his visit and for taking the trouble to arrange things .

The purpose of this brief message is simply to reiterate what we said in our brief discussions in Venice - namely that it is our intention to work with you rather than in any sense of competition. Our motivation for wanting to do some of the detailed comparisons between the results of our work and your own is to understand the sources of uncertainty in both. We are also committed to doing some of this work by the terms of our current NERC grant. We wish to involve you as much as possible , get your advice , and solicit criticisms of our approach -especially in relation to the Palaeo-model comparisons .

Our EC proposal was not funded , but we wish to follow it up with another to PRESCIENT (a NERC Thematic Programme of research along the same lines), and again we would be happy to collaborate with you. Better two way communication between here and there would be a major help.

It is my feeling that the relatively short time Tim has with you , might be best spent getting to grips with the finer details of your "old" and "new" approaches, including the details and results of your other work that is only partly described in the publications (seasonal runs, different data sets etc.) and , most importantly, discussing approaches and philosophies for data-model comparison work. That way he could come away with some concrete plans , and the means of fulfilling them, on his return. Any time you can spare to discuss and liaise along these lines would be much appreciated. He has discussed the specifics of your suggestions and I am happy with the approach and prioritization he has expressed.

While he is with you , we can always exchange emails if any issues need wider discussion.

very best wishes

Keith

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re:
Date: Mon, 25 Sep 2000 16:06:01 -0400
Cc: t.osborn@uea.ac.uk, p.jones@uea.ac.uk, srutherford@virginia.edu

Hi Keith et al,

Thanks for your message.

This sounds fine. I do have to warn that with a full teaching courseload this semester, my own free time will necessarily be somewhat limited. Thus, Scott's involvement here will be key.

Scott has been dealing w/ the new methodology and analyses, and hence my concern w/ any plans that expect new analyses w/ our old methodology. The code is not especially user friendly, though Tim is welcome to use it. Scott will be able to devote a decent share of his time to these activities during Tim's visit, though this will necessarily have to be split with time devoted to activities that Scott is explicitly supported for by our NOAA grant (ie, the development of a synthetic proxy network from model data, and wavelet-based calibration methods, as detailed in my previous email).

So I'm sure we'll be able to find common ground. Tim will have free access to our data and codes, and can make the comparisons indicated below. We of course appreciate your willingness to make available to us the tree ring density data.

It may be interesting to do a (highly preliminary!) analysis of both proxy datasets with our expectation maximization ridge regression scheme, and that would certainly fit in well w/ both our agendas (your NERC grant, and our NOAA grant).

Hopefully, our 4-processor Dell server (running Linux) will be back up and running, so Scott can use our Sun server, while Tim will have the Dell server to himself if he needs it.

I hope the above all sounds good.

Best regards,

mike

At 10:16 AM 9/25/00 +0100, Keith Briffa wrote:

>Dear Mike

> I know Tim has communicated with you about plans for his visit
>to Virginia. We have discussed ideas here and I ,for one, am excited about
>the prospects of joint work. Thank you for agreeing to his visit and for
>taking the trouble to arrange things .
>The purpose of this brief message is simply to reiterate what we said in
>our brief discussions in Venice - namely that it is our intention to work
>with you rather than in any sense of competition. Our motivation for
>wanting to do some of the detailed comparisons between the results of our
>work and your own is to understand the sources of uncertainty in both. We
>are also committed to doing some of this work by the terms of our current
>NERC grant. We wish to involve you as much as possible , get your advice
>, and solicit criticisms of our approach -especially in relation to the
>Palaeo-model comparisons .
> Our EC proposal was not funded , but we wish to follow it up with another
>to PRESCIENT (a NERC Thematic Programme of research along the same lines),
>and again we would be happy to collaborate with you. Better two way
>communication between here and there would be a major help.

> It is my feeling that the relatively short time Tim has with you ,
>might be best spent getting to grips with the finer details of your "old"
>and "new" approaches, including the details and results of your other work
>that is only partly described in the publications (seasonal
>runs, different data sets etc.) and , most importantly, discussing
>approaches and philosophies for data-model comparison work. That way he
>could come away with some concrete plans , and the means of fulfilling
>them, on his return. Any time you can spare to discuss and liaise along
>these lines would be much appreciated. He has discussed the specifics of
>your suggestions and I am happy with the approach and prioritization he has
>expressed.

>While he is with you , we can always exchange emails if any issues need
>wider discussion.

>very best wishes

>Keith

>--

>Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
>Norwich, NR4 7TJ, United Kingdom
>Phone: +44-1603-592090 Fax: +44-1603-507784

>

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From: John Daly <daly@vision.net.au>
To: Chick Keller <cfk@lanl.gov>
Subject: Re: Hockey Sticks References
Date: Wed, 04 Oct 2000 08:47:50 +1000
Reply-to: daly@vision.net.au
Cc: VINCENT GRAY <vinmary.gray@paradise.net.nz>, Onar †m
<onar@netpower.no>, "John L. Daly" <daly@microtech.com.au>, "P. Dietze"
<p_dietze@t-online.de>, mmaccrac@usgcrp.gov, Michael E Mann
<mann@virginia.edu>, rbradley@geo.umass.edu,
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richard@courtney01.compulink.co.uk, McKitrick <rmckit@css.uoguelph.ca>,
Bjarnason <agust@rt.is>, Harry Priem <priem@dds.nl>, balberts@nas.edu

Dear all

Here's another MWE reference, originally announced by the Idso's. I
looked up the abstract from the South African Journal of Science and
here's its URL

http://www.gheiss.de/Personal/Abstracts/SAJS2000_Abstr.html

That puts the MWE and LIA into South Africa.

Cheers

John D.

--

John L. Daly
"Still Waiting For Greenhouse"
<http://www.vision.net.au/~daly>

"All science is numbers, but not all numbers is science"

From: Phil Jones <p.jones@uea.ac.uk>
To: Myles Allen <M.R.Allen@rl.ac.uk>
Subject: Re: Observed temperature for IPCC power spectra
Date: Wed Oct 4 08:58:48 2000
Cc: Curtis Covey <covey1@llnl.gov>, santer1@llnl.gov, jfbmitchell@meto.gov.uk

Myles and Curt,

Attached are the NH and SH averages from the new variance corrected analyses (HadCRUTv). When the paper comes out in JGR (probably early next year) you'll see that variance correction is only possible from 1870.

So in these files I've patched on the 1856-1869 data on the front so they are the same length. This early data is the same as the original version (HadCRUT). For the global series I still think the best way of producing this is by averaging the two hemispheres. HadCRUT is what you all probably have - it is on the CRU web page. Again I would produce the globe by averaging the hemispheres so what Chris Folland has for the globe may differ slightly as the HadC produce this as one domain.

The way the variance correction is achieved is by reducing the high-freq variance of each grid-box series. This means that when I update the series for 2000 some values for the last few years (1995-9) will be altered slightly.

I don't know much about Chapter 2, but I don't recollect there being any power spectrum diagrams. Probably left for the detection chapter. Do make sure the axes and units are well explained. Don't leave anything for the skeptics to cling to !

Cheers
Phil

At 05:16 PM 10/3/00 +0100, Myles Allen wrote:

>Hi Phil,

>

>If you could send me the latest version that chapter 2 are using, that
>would be great -- I certainly won't pass it on nor use it for anything
>else. Subtle differences in processing do make a difference to the visual
>appearance of the plot, and even though these differences are inside the
>noise indicated by the error bar, you can bet potential critics will
>ignore that.

>

>Do you show a power spectrum of global temperatures in chapter 2, and if
>so, how was it computed? It would certainly be tidy to make sure both are

>processed in the same way.

>

>Myles

>-----

>Myles R. Allen Phone (RAL): 44-1235-446480
>Space Science & Technology Department Ph (Oxford): 44-1865-272085
>Rutherford Appleton Laboratory Fax: 44-1235-445848
>Chilton, Didcot, OX11 0QX e-mail: m.r.allen@rl.ac.uk
>United Kingdom <http://www.climate-dynamics.rl.ac.uk/>

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>On Tue, 3 Oct 2000, Phil Jones wrote:

>

>>

>> Curt, Myles and anyone else,
>> As the data on the web site has an end date of 1994, I suspect you
>> may have an earlier version of the surface data (different form of
>> gridding and maybe a few other differences in data usage), so I suggest
>> you use the latest one, which can be got from the CRU web site.
>> If this relates to IPCC work, then Chapter 2 on the Observations is
>> going to go with a variance corrected version (corrects for changing
>> station numbers within individual grid boxes), but the effect of this
>> on the hemispheric and global temp series is small.
>> If anyone wants this new version (HadCRUTv) then I can send the hemispheric
>> and global series by email. The 'normal' version (HadCRUT) is on the
>> CRU website. This naming and the variance correction procedures are
>> discussed in a paper which has been accepted by JGR. It will not be out for
>> a while, as I've not yet sent the camera ready columns to the AGU.

>>

>> Cheers

>> Phil

>>

>>

>>

>> At 03:45 PM 10/2/00 -0700, Curtis Covey wrote:
>>> Myles Allen wrote: Dear Curt, Can you give me the ancestry of the "ObsJ"
>>> global mean temperature series
>>> We need the source, start date (I think I can work it
>>> out by matching bumps, but it would be better to be sure) and how it's
>>> been detrended for the figure caption.
>>>
>>>>
>>>>> sent you, the data is

>> >>>the "Jones" set used by the IPCC for its Second (1995)
>> >>> It's
>> >>> processed this particular data that
>> >>> don't remember exactly who gave it to me: either Phil
>> >>> should invite Phil's latest
>> >>>(including error bars) now that I'm updating our Web
>> >>>site <http://www-pcmdi.llnl.gov/cmip/powerspec.html>. See attached
>> >>>PostScript graphic. Regards,
>> >>>Curt

>> >>>

>> >>

>> > Attachment Converted: "c:\eudora\attach\tseries_obsJ.ps"

>> Prof. Phil Jones

>> Climatic Research Unit Telephone +44 (0) 1603 592090

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>> Norwich Email p.jones@uea.ac.uk

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>>

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>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: stepan@ipae.uran.ru,eavaganov@forest.akadem.ru
Subject: intas
Date: Fri Oct 6 10:30:24 2000

Stepan and Eugene

I have asked INTAS for an extension on the report period. Stepan some problem has now arisen regarding your final payment . I have asked Janet to sort this out and contact you directly.

I have to give an up to date report on chronology development and tree-line changes at the PAGES meeting in Avignon on October 24-26 and I would really appreciate some Figures that demonstrate the latest state-of-the-art in the Yamal and Taimyr (and any other good Russian evidence) . The focus of the meeting is High-resolution variability of the Holocene , and the long records and evidence of tree-line changes is particularly valuable. Later there will be some large review papers (with many authors) summarising the information from high latitudes, mid latitudes, the tropics etc. The form of these papers is not yet decided but you would be contributing authors. I am also (with Ray Bradley,Julie Cole and Malcolm Hughes) writing a Chapter on the last 10000 years (with a major emphasis on the last 1000) for the PAGES Synthesis book and I intend to include a summary Figure that includes your work - I hope this is O.K Malcolm has just asked for a letter of support from me for a project he is submitting to NSF , in which I believe you are both involved. I have sent it to him. I am still exploring when we can resubmit our own proposal to the EC, and I will write an application to The Leverhulme trust before the end of this year. I am still discussing the Holocene ADVANCE-10K issue and I will be in touch about your papers.

best wishes

Keith

From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Yamal treeline figures
Date: Mon, 9 Oct 2000 18:08:04 +0500
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith,

Stepan Shiyatov tell me that you need some figures concerning Yamal chronology and tree line dynamics to show somewhere in France.

Attached are archived files contained some figures.

File MAP - the map of region of research. Red dots - subfossil wood sites, green marks - recent northern border of larch along river valleys.

File FIGURES - in Excel format, contains several figures.

Sheet "Values-10" - data on northernmost position of trees and number of trees dated for corresponding year (decadal step)

Sheet "Treeline" - dynamics of treeline in Yamal during last 7000 years reconstructed using about 1000 subfossil wood remains. Recent treeline position is about 67°34'.

One year ago we supposed (C-14 data, Hantemirov, Shiyatov 1999) that significant drop of treeline (the transition from "middle" to "late" Holocene) was about 1700-1600 AD. According new data it was earlier (about 2550 BC). May be it is because of lack of data from region northward of 68°N (only 25 datings)?

Sheet "Treeline and Nu" - treeline dynamics and number of dated trees. May be number of trees reflects the long scale climate fluctuations as well.

Sheet "2600-all" - for last 4600 years: treeline dynamics, number of trees, 11 most cold summers for last 7000 years (according our version of reconstruction), most expressed frosts in July (reconstructed using junipers from Polar Urals, see file PATHOL, frost in 1626 BC - based on subfossil larch - you can put away it), summer temperatures reconstruction smoothed with 20- and 100-year filters (our version of reconstruction).

Sheet "Values-2" - values for preceding figures, in 2-years

step.

Sheet "Yam-Ur-fig" - comparing of treeline data for Yamal and Polar Urals upper treeline dynamics (data by S.G.Shiyatov)

Sheet "Yamal-Ural" - values for preceding figure, in 2-years step.

Sheet "Treeline-std" - treeline dynamics and 50-year standard deviations of summer temperatures (our version of reconstruction). This figure shows surprising high negative correlation. However may be both of them just reflect long scale climate fluctuations?

Sheet "Std" - 50-year standard deviations of summer temperatures (our version of reconstruction) .

File PATHOL - in Excel format, contains data and figure on pathological structures in tree rings of Siberian juniper (*Juniperus sibirica* Burgsd.). According our data (Hantemirov et al., 2000) the presence of frost rings provides evidence for frosts that occurred in late June or first days of July (frost rings in earlywood) and in the first half of July (frost rings in late wood). Long term and pronounced temperature drop in the middle of very warm period in the second half of July is the factor responsible for wood density fluctuations (false rings).

Please let me know when you receive this. Some time large messages get lost.

P.S. We (Eugene Vaganov, Stepan Shiyatov, Leonid Agafonov and I) will be in Birmensdorf from 23 till 29 October. Are you going to Switzerland after your meeting? We would be happy to see you there.

Best regards,
Rashit M. Hantemirov

Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: rashit@ipae.uran.ru

Fax: +7 (3432) 29 41 61; phone: +7 (3432) 29 40 92

Attachment Converted: "c:\eudora\attach\Map.zip"

Attachment Converted: "c:\eudora\attach\Figures1.zip"

Attachment Converted: "c:\eudora\attach\Pathol.zip"

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tim Osborn <T.Osborn@uea.ac.uk>
Subject: Re: JGR paper
Date: Thu Oct 19 17:55:41 2000

I am just having to go so I will think about the "should we?" . The answer to the "can we?" is yes. I have spoken to the person organising the editorial review and she has promised me she will get it to us in the next week or so. If we can get it back immediattely she says we can make the December issue. Therefore it is possible to do the edits if it means very little change to the text. I have also confirmed that we will pay 1500 dollars for the colour and they say they are working on these now. I really want to get this into the 2000 so I can include it in the RAE. Ed is here now and has some great looking extended PDSI reconstructions (1000 years) for the western US.

I am suspicious as to whether the negative trend in Mike's Hockey stick prior to the 20th century is not at least partly the result of a trend in the long high elevation western US trees he uses . Malcolm sent me some figures for the HIHOL meeting and in this work he cuts off the juvenile growth sections of the long tree data but does no detrending on the remainder. This might leave a linear age trend in these data. I remember that Mike in his long reconstruction , stated that the pc representing the western US stuff was essential for getting a verifiable result. Interesting , but only a diversion. We can discuss the JGR and other stuff in Avignon. Hope your weekend was a god one. I tend to agree a bout the NAO meeting- you could use the money (and perhaps time) to better effect.
At 04:24 PM 10/19/00 +0100, you wrote:

Keith,

have you had to produce the camera-ready copy for the age-banded JGR paper yet? If not, then is it possible to make some minor changes to it? For the comparison with the Mann et al. reconstruction, I had previously just taken their land&marine full northern hemisphere mean annual temperature time series and re-calibrated it against the instrumental land north of 20N Apr-Sep mean temperature time series. Well, I have not taken the Mann et al. spatial temperature field reconstructions, and computed a land north of 20N area mean.

I still have to re-calibrate it against the instrumental series because it is an annual rather than Apr-Sep mean. After doing all this, you'll be pleased to know that the final figure is only slightly different (the Mann et al. curve is very slightly more of an outlier during the 1500-1700 period, and is cooler and closer to observations post-1950, but not much different elsewhere). What does change, however, are the correlations. The correlations with instrumental data are slightly worse (from 0.76 to 0.73, and from 0.92 to 0.89 decadal), but I'm not sure that we show these anyway. But the cross-correlations between the Mann et al. and the other reconstructions (which we do show) are all stronger than previously - which now seems a little

unfair on them.

Cross-correlations between unfiltered series:

Mann versus: Jones, Briffa (ABD), Briffa (Torn+Tai+Yam)

before: 0.47, 0.36, 0.33

now: 0.50, 0.37, 0.34

Cross-correlations between 50-yr smoothed series:

Mann versus: Jones, Briffa (ABD), Briffa (T+T+Y), Overpeck, Crowley

before: 0.78, 0.43, 0.50, 0.86, 0.76

now: 0.81, 0.51, 0.55, 0.86, 0.78

I don't have a copy of the paper in front of me, but the 'before' values should match those in one of the tables. Some of the 50-yr smoothed new values are noticeably stronger.

Can we make these changes still, or is it too late? And do you think we should?

Cheers

Tim

From: Phil Jones <p.jones@uea.ac.uk>
To: Brendaw Morris <brendawmorris@earthlink.net>
Subject: Re: JOC Review
Date: Tue Oct 24 15:20:04 2000

Dear Brendaw,

My review of the paper JCL 3435 is attached. My recommendation is to accept the paper subject to minor changes. I don't wish to see it again. If there are any problems with the attachment, let me know and I can fax the 2 pages.

Cheers
Phil Jones

At 06:58 PM 10/7/00 -0400, you wrote:

>Professor Michael Mann, Editor of Journal of Climate, has suggested you as a
>possible reviewer of a paper entitled "Differential ENSO and volcanic
>effects on surface and tropospheric temperatures" (JCL-3435 by T. M. L.
>Wigley and B. D. Santer.

>
>Would you please let me know whether or not you will be able to do this
>review? If you accept, we ask that you complete your review by 11/24/00 (if
>possible). Hard copy or e-mail copies of reviews are very acceptable.

>
>Also, if you accept, please send your complete address including telephone
>and fax numbers for our files. Thank you so much.

>
>If you are unable to do this review, suggestions of other potential
>reviewers (and their e-mail addresses) for this paper would be greatly
>appreciated.

>
>Brenda W. Morris
>Editorial Assistant
>Journal of Climate

>
>
>
>
>

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: Figures for revised version of paper
Date: Wed Oct 25 14:38:07 2000

Ben,

I hope the surgery next week goes OK. Ruth and I are going away next week for a short break to Coldstream on the River Tweed. This was the holiday cottage Matthew had planned to go to for his honeymoon, but the fuel crisis around his wedding time precluded this. We were able to negotiate the cottage for a later date, as we could get a refund or claim on the insurance as a national emergency wasn't declared. So on Nov 1 we will think about you !

I've listed off the diagrams and will take the text when it comes, but I won't be able to send you any comments until the week of Nov 6.

Also just sent back comments to Mike Mann on the paper by Tom and you factoring out ENSO and Volcanoes. Felt like writing red ink all over it, but sent back a short publish subject to minor revision to Mike. This is the first time I've ever reviewed one of Tom's or your papers !

Copy of what I sent is attached. I forgot to sign it before sending it !

Again hope all is well later next week !

Cheers
Phil

At 06:37 PM 10/24/00 -0700, you wrote:

>Dear All,

>

>Sorry that it has taken me so long to revise our paper. As I mentioned in a
>previous email, I've had to repeat most of the calculations using an improved
>estimate of the pre-eruption reference level temperature (Tref). I've also had
>to look at the sensitivity of our results to uncertainties in Tref. I'd like to
>thank Tom for prompting me to take a critical look at this issue - it's an
>important one. I'd also like to thank the rest of you for all the comments that
>you've sent me. I hope I've addressed them adequately in the revised paper.

>

>Another major change is that, rather than giving results are based on a variety
>of different filtering options -- e.g., estimation of volcano parameters from

>unfiltered data (too noisy) and highly smoothed data (13-term Gaussian filter
>leads to underestimate of volcanically-induced cooling) -- we now only give
>results for our "best guess" filtering option,
>a five-term binomial filter. We still discuss sensitivities to tau (the volcanic
>signal decay time) and choice of ENSO index. Restricting attention to one
>filtering option reduces the length of Tables, and hopefully improves the
>clarity of the paper.

>

>I've rewritten the discussion of the iterative method, and we now make it clear
>that although this approach is automated, its implementation still involves a
>number of subjective decisions (filter choice, choice of averaging period for
>estimating pre-eruption reference temperature, choice of tau, etc.) Many of the
>changes made here attempt to address useful comments that I received from Tom.

>

>Lennart and Erich kindly provided me with the SLP data from the GSOP, GSO1 and
>GSO2 integrations. Recall that we did not have this data previously, and so our
>estimation of ENSO signals in GSO1 and GSO2 and of ENSO/volcano signals in GSOP
>was based on simulated Nino 3.4 SSTs only. We've now also used the (simulated)
>SOI to perform ENSO/volcano signal estimation.

>

>Section 5 (discussion of ECHAM4/OPYC results) has been completely rewritten,
>and the ordering of individual subsections should now be more logical. We
>discuss the simulated Pinatubo signal first, then the "ENSO component" of
>simulated temperature trends, and finally residual trends after the removal of
>volcano and ENSO effects.

>

>Today I'm sending you, as postscript attachments, the revised Figures for the
>paper. To simplify things I've encoded the Figure number at the top of the
>postscript file. I don't want to overload your mailboxes, so I'm sending the
>Figures in two separate mail messages. There should be 11 Figures in total.
>Tomorrow I'll send you the revised text of the paper and the Tables. Please let
>me know if you have any problems printing these files. Note that all Figures
>except Figure 7 are in color. Color is not essential for some of the Figures,
>and in the next day or two I'll prepare black-and-white versions of Figs 3, 5,
>6, 8, 9, 10 and 11. But for now I thought you might find it easier working with
>the color versions.

>

>I will be going in for surgery on November 1st, and am not sure how long it will
>be until I get back to my office. I realize that it may not be feasible to
>submit the paper before November 1st. But I'd really appreciate it if you could
>send me comments before November 1st. These will keep me occupied while I'm
>trying to get back on my feet!

>

>With best regards,

>

>Ben

>--

>-----

>Benjamin D. Santer

>Program for Climate Model Diagnosis and Intercomparison

>Lawrence Livermore National Laboratory

>P.O. Box 808, Mail Stop L-264

>Livermore, CA 94550, U.S.A.

>Tel: (925) 422-7638

>FAX: (925) 422-7675

>email: santer1@llnl.gov

>-----

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: Text and Tables of draft JGR paper
Date: Fri Oct 27 08:31:10 2000

Ben,

All received and printed. The weather forecast for the next few days is cold and windy, so I'll read this at the cottage in Coldstream. Hope everything goes OK later next week. I will email comments, hopefully on Nov 6, maybe Nov 7 if there is a lot of urgent things to do when I get back.

Cheers
Phil

At 05:18 PM 10/26/00 -0700, you wrote:

>Dear All,

>

>Here are the three postscript files with the title page, main text, and Tables
>for our draft JGR paper. Sorry it took me a bit longer to get these to you.
>Please let me know if you have any problems printing these files. You should
>already have all the Figures that I sent on Tuesday.

>

>I'll be in my office tomorrow and Monday and Tuesday of next week. After Tuesday
>the best way of getting in touch with me is by contacting PCMDI's secretary,
>Harriet Moxley (925-422-7638). I hope to be out of hospital and back in my
>office by November 10th. It would be nice if we could submit this paper shortly
>thereafter!

>

>With best regards, and thanks again for all your help,

>

>Ben

>--

>-----

>Benjamin D. Santer
>Program for Climate Model Diagnosis and Intercomparison
>Lawrence Livermore National Laboratory
>P.O. Box 808, Mail Stop L-264
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>Tel: (925) 422-7638
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>-----
>Attachment Converted: "c:\eudora\attach\volcano_tables2.ps"
>
>Attachment Converted: "c:\eudora\attach\driver_maintext.ps"
>
>Attachment Converted: "c:\eudora\attach\driver_titlepage.ps"
>

From: Mike Hulme <m.hulme@uea.ac.uk>

To: barker,vira

Subject: Fwd: BP funding

Date: Sat Nov 4 16:45:25 2000

Any idea who at Cambridge has been benefitting from this BP money?

Mike

From: "Simon J Shackley" <Mcysssjs@fs1.sm.umist.ac.uk>

Organization: umist

To: m.hulme@uea.ac.uk

Date: Thu, 2 Nov 2000 14:44:09 GMT

Subject: BP funding

Reply-to: Simon.Shackley@umist.ac.uk

CC: robin.smith@umist.ac.uk,

brian.launder@umist.ac.uk

Priority: normal

X-mailer: Pegasus Mail for Win32 (v3.12a)

dear TC colleagues

looks like BP have their cheque books out! How can TC benefit from this largesse? I wonder who has received this money within Cambridge University?

Cheers, Simon

17) BP, FORD GIVE \$20 MILLION FOR PRINCETON UNIVERSITY EMISSIONS STUDY

Auto.com/Bloomberg News

October 26, 2000

Internet: [1]http://www.auto.com/industry/iwirc26_20001026.htm

LONDON -- BP Amoco Plc, the world's No. 3 publicly traded oil company, and Ford Motor Co. said they will give Princeton University \$20 million over 10 years to study ways to reduce carbon-dioxide emissions from fossil fuels. BP said it will give \$15 million. Ford, the world's second-biggest automaker, is donating \$5 million. The gift is part of a partnership between the companies aimed at addressing concerns about climate change.

Carbon dioxide is the most common of the greenhouse gases believed to contribute to global warming.

London-based BP said it plans to give \$85 million in the next decade to universities in the U.S. and U.K. to study environmental and energy issues. In the past two years, the company has pledged \$40 million to Cambridge University, \$20 million to the University

of California at Berkeley and \$10 million to the University of Colorado at Boulder.

References

1. http://www.auto.com/industry/iwirc26_20001026.htm

From: Eric.Steig@sas.upenn.edu (via the vacation program)
To: k.briffa@uea.ac.uk
Subject: away from my mail
Date: Fri, 10 Nov 2000 09:53:09 -0500 (EST)

I am away for a couple of days. This is an automatic reply. I will
reply
to your mail regarding "reminder" when I return on Sunday.

From: Keith Briffa <k.briffa@uea.ac.uk>
To: jjzeeberg <jzeebe1@uic.edu>
Subject: Re: temperature time series
Date: Mon Nov 20 09:41:03 2000

Hi Jap

please see the following - I have had the data put on my web site and I will slowly put other data and Figures and Abstracts on there also. Let me know if you have a problem downloading the data. Good luck

Keith

The data you want are included in those listed under -

[1]<http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/>

At 03:56 PM 11/17/00 -0600, you wrote:

Dear Mr Briffa,

I remind you to send me your temperature reconstructions for northern Scandinavia and the Polar Urals.

JaapJan Zeeberg

At 02:55 PM 11/14/2000 +0000, you wrote:

>Dear Jap

>I am sorry , but your earlier message must have slipped through the net .

>I will try to look out the data and send them to you in the next couple of

>days or so. Please remind me on thursday if they have not arrived. Best wishes

>Keith

>

>At 02:14 PM 11/13/00 -0600, you wrote:

>

>>Dear Dr Briffa,

>>

>>You may not have received this message the first time I sent it (30/10);

>>

>>I am a PhD-student at the University of Illinois, Chicago. I study the

>>effect of North Atlantic modulated inputs of precipitation and summer warmth

>>on the glacier mass balance of Novaya Zemlya. Results will appear in the

>>January or March-issue of The Holocene.

>>

>>I would like to use your temperature reconstructions for the northern Urals

>>and northern Fennoscandia published in Nature 376, p. 156-159 (1995). I

>>plan to compare the temperature time series with grain size distributions of

>>three sediment cores obtained from Russkaya Gavan', a fjord at north Novaya

>>Zemlya. These cores span the past ~4 centuries.

>>

>>I could not find the requested time series in the NOAA data base and would
>>be grateful if you could provide them.

>>

>>Sincerely

>>

>>JaapJan Zeeberg

>>

>>

>>=====

>>JaapJan Zeeberg

>>

>>[2]<http://www2.uic.edu/~jzeebe1/news.htm>

>>

>>845 W. Taylor Street MC186

>>Chicago, IL 60607-7059, USA

>>

>>Phone: 312-996-3154

>>Fax: 312-413-2279

>>e-mail jzeebe1@uic.edu

>>

>>=====

>

>--

>Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,

>Norwich, NR4 7TJ, United Kingdom

>Phone: +44-1603-592090 Fax: +44-1603-507784

>

>

--

Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,

Norwich, NR4 7TJ, United Kingdom

Phone: +44-1603-592090 Fax: +44-1603-507784

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/>
2. <http://www2.uic.edu/~jzeebe1/news.htm>

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: New tree-ring density data
Date: Fri, 01 Dec 2000 12:58:19 -0500
Cc: srutherford@virginia.edu, mann@virginia.edu

Scott, Tim,

Here's the abstract.

If the results pan out, then several us us may want to be discussing this work on the talk circuit.

This is the first stab! Notice how safe (a very results-insensitive abstract!)

mike

XXVI General Assembly, Spring EGS Meeting

Comparison of Large-Scale Proxy-Based Temperature Reconstructions Over the Past Few Centuries

MANN, M.E.; RUTHERFORD, S; OSBORN, T.J.

OA28.0 Study of past climates: Climate of the past millennium

JOUZEL, J.; (co-conveners: JONES, P.D.; MANN, M.E.)

Comparison of Large-Scale Proxy-Based Temperature Reconstructions Over the Past Few Centuries

M.E. Mann(1), S. Rutherford(1), and T.J. Osborn(2)

(1) Univ. of Virginia, USA, (2) Climate Research Unit, Univ. East Anglia, UK

A promising approach to the problem of reconstructing patterns of past climate variability

involves the application of spatial climate field reconstruction (CFR)

techniques to networks of proxy

climate indicators (e.g., Mann et al 1998;2000--see

http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html).

This approach seeks to exploit the complimentary information in a diverse

network of proxy indicators by determining the most consistent relationships between these networks of data and the leading spatial patterns of climate variability during a recent "calibration" period of overlap with the modern instrumental record. The calibrated relationship is then used to estimate large-scale patterns of climate variability in the past from the proxy data. This method makes no assumptions regarding the relationship between a given proxy indicator and specific local annual/seasonal climate variable, but does assume that the proxy indicator is tied to some combination of large-scale patterns of climate variability. Alternatively, it is possible to estimate large-scale temperature patterns from a relatively homogenous network of proxy climate indicators (e.g., tree-ring density data--see Briffa et al, 1998) by invoking a local calibration between each climate indicator and the climate variable (e.g., summer temperature) of interest. This approach is more conservative in the amount of information it seeks to extract from the proxy data network, but it is free from assumptions regarding the large-scale patterns of past climate variability. Recent reconstructions of Northern Hemisphere annual-mean and warm-season temperature patterns using these respective approaches and data show some similarities, but also some important differences. Here we investigate these differences more closely, examining the sensitivity of Northern Hemisphere temperature pattern reconstructions to (a) the underlying proxy data used, (b) the particular method used to estimate large-scale patterns from these data, and (c) the target seasonality of the reconstruction. By controlling independently for each of these three factors, we gain insight into the reasons for differences between various proxy-based estimates of past large-scale temperature variability.

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (804) 924-7770 FAX: (804) 982-2137
<http://www.evsc.virginia.edu/faculty/people/mann.html>

From: Eric Steig <esteig@sas.upenn.edu>
To: <masson@lsce.saclay.cea.fr>, jouzel@lsce.saclay.cea.fr,
ddj@gfy.ku.dk, fujii@pmg.nipr.ac.jp, tas.van.omnen@utas.edu.au,
vimeux@lsce.saclay.cea.fr, <fisher@nrnl.NRCan.gc.ca>,
<ethompsom@magnus.acs.ohio-state.edu>, <Koerner@ess-dns2.gsc.nrcan.gc.ca>,
edw@geophys.washington.edu, clow@usgs.gov
Subject: No Subject
Date: Tue, 12 Dec 2000 11:55:29 -0500
Cc: <raynaud@glaciog.ujf-grenoble.fr>, k.briffa@uea.ac.uk,
steig@geophys.washington.edu

Dear Colleagues

At the HIHOL meeting in Avignon in October, several of us (Steig, van Ommen, Dahl-Jensen, Vimeux) agreed to write a review paper addressing Holocene climate change as viewed from polar ice core records. The main task of writing and organizing this paper has fallen upon Tas and Eric, and

we are writing to solicit your interest, support, and contribution. We would appreciate hearing from each of you with comments on our proposed plan, requests for clarification and (hopefully) data sets. We hope you will be interested in working with us on this project. Note that the deadline for completion is the end of March, 2001.

Although the question of Holocene climate change has obviously been addressed in numerous papers on individual ice core records (and most recently in the Masson et al. review of Antarctic records in QR), we believe that it would be valuable to select the best-understood, best dated, polar ice core data from both hemispheres and put them in a single paper. We also think that the paper should be limited only to

1) data that address directly the TEMPERATURE history at high latitudes -
-
the information
we get from isotopes and from borehole reconstructions -- as opposed e.g. to atmospheric circulation changes that one gets from the chemistry record,
and

2) discussion of the long-term variations, as opposed to short term variations such as the Little Ice Age.

The intention here is not to be exclusive of either people or ideas, but to limit the scope of the paper so that it is as definitive a document as possible. Of particular interest is the "simple" question of the timing and magnitude of the "thermal maximum", the subsequent Holocene cooling, and their relationship to insolation forcing. This was a major question at the HIHOL meeting and we do not believe it was adequately resolved there.

Our vision is a summary paper that not only reproduces already-published

work, but that carefully quantifies the uncertainties inherent in each of the reconstructions. Of particular interest are the possible effects of elevation change on the records, and uncertainties in the timescales. We cannot say a priori what the conclusions of this paper will be. An example might be that the "thermal maximum" was actually warmer than present - a major issue of contention in the popular literature - and was more-or-less simultaneous in both polar regions. If this is correct, it will be a useful service to the paleoclimate community to demonstrate it. Alternatively, we may find after carefully looking at the data that we CANNOT reach such a conclusion. This would be an equally important result.

How should we proceed? Our suggestion is that those who are willing to participate send their favorite ice-core based temperature reconstructions to us, providing the best available timescales and a brief description of the uncertainties you ascribe to the reconstruction. We will compile the data and produce both 1) a single file containing all the data, and 2) a PDF figure comparing all the independent temperature reconstructions. We can then initiate discussion around a common figure, so that everyone is looking at exactly the same information. The last 11,000 years would be considered the appropriate time interval to consider. We do not wish to confuse matters by including the glacial-interglacial transition!

Data sets that we think would be particularly important include the following. Note that we will probably need to include other authors. This is just a preliminary list and is not intended to exclude anyone. We are also aware that some of these data are so far unpublished but we hope that they could be included anyway, perhaps in "smoothed" form (?).

- 1) Isotope profiles from Vostok, Byrd (and EPICA, if possible), on the most-accepted timescales (Francoise).
- 2) Isotope profiles from Taylor Dome and Siple Dome, Dye 3 and GISP2 (Eric).
- 3) Isotope profile from Dome Fuji (Fujii)
- 3) Isotope profiles and borehole temperatures from Law Dome core(s) (Tas, Vin).*
- 4) Isotope data from GRIP (and from N-GRIP if possible) (Dorthe)
- 4) Borehole data from Taylor Dome, GISP2, Dye 3 (Gary, Ed).*
- 5) Borehole data from GRIP (and N-GRIP if possible) (Dorethe)
- 6) Isotope, meltlayer frequency, and borehole T data from the Canadian ice caps (David, Fritz)
- 7) Meltlayer data from other sites (GISP2 - Alley?)

*The Law and Taylor Dome records only go to mid Holocene but would still be very useful here!

Other suggestions for data sets and people to contact?

Again, please reply to this email with your comments, criticisms concerns, request for clarification and (hopefully) data sets!

Thanks!

Warm regards to all,

Eric Steig & Tas van Ommen

From: Eric Steig <steig@geophys.washington.edu>
To: Valerie Masson-Delmotte <masson@lsce.saclay.cea.fr>, Eric Steig
<esteig@sas.upenn.edu>
Subject: Re: HIHOL "optima"?
Date: Thu, 14 Dec 2000 10:30:38 -0500
Cc: jouzel@lsce.saclay.cea.fr, ddj@gfy.ku.dk, fujii@pmg.nipr.ac.jp,
tas.van.ommen@utas.edu.au, vimeux@lsce.saclay.cea.fr,
fisher@nrnl.NRCan.gc.ca, ethompso@magnus.acs.ohio-state.edu, Koerner@ess-
dns2.gsc.nrcan.gc.ca, edw@geophys.washington.edu, clow@usgs.gov,
raynaud@glaciog.ujf-grenoble.fr, k.briffa@uea.ac.uk, Valerie Masson
<masson@lsce.saclay.cea.fr>

Valerie, Françoise et al.

We also were surprised by the "conclusion" that there was a 9-7 ka optimum. This probably arose from a statement by Greg Zielinski regarding the Arctic records. In any case, the article by Dominique and Kieth was just a rough draft -- we have pointed out the mistake to them and I expect we will all see a final version anyway!

Regarding the subject of the HIHOL paper, we agree that there are already many papers published that discuss the temperature interpretation of isotopic records during the Holocene. What has not been done, however, is to include the best Holocene records from both polar regions in a single paper, nor to make a specific comparison of the timing and magnitude of the optimum (or optima). For example, the elevation effect on the long-term trends for East Antarctica has been discussed (Masson et al., 2000) but not quantified. Of course quantifying this effect is difficult but our paper could put useful error estimates, for example, on the amount of cooling in the late Holocene. We do not of course wish to compete with Sigfus, but his paper will be more limited in geographic focus than ours and will include new data that we will not use. It would be good to include NGRIP borehole temperatures if we can, but this is not necessary. Even the GRIP and GISP2 records show very clearly the Holocene optimum. Our suggestion would be to let Dorethe decide on that, in consultation with Sigfus.

In our vision, one of the key features of the Holocene article will be its deliberately limited scope and confinement to observation rather than speculation about causes of climate change. We think that to involve modelers and oceanographers makes it difficult to keep the focus and is rather beyond the intended purposes of the Holocene volume. Keep in mind that modelling was looked at separately at the HIHOL meeting and we believe that the modelers at the meeting are planning their own contribution to the

volume.

As mentioned earlier, we think the best way to get the paper going is to begin soon the process of simply collating data sets and putting them all on one graph. We can then discuss the details of the paper with the same image in front of each of us.

We hope that you can agree more-or-less with the above, and that others on our email list will also provide some input. We are of course open to further discussion!

Further comments?

Eric and Tas

At 12:07 PM 12/13/00 +0100, Valerie Masson-Delmotte wrote:

>

>Dear Eric and Tas, dear colleagues,

>

>First, thank you for your initiative in motivating a comparison of ice isotope and borehole temperature records from both hemispheres from the Holocene. We think that it is important to position this work with respect

>to other related studies. There are in particular several papers already discussing the temperature interpretation of isotopic records during the Holocene (see below for Greenland; correcting the isotopic profiles in Antarctica from trends due to SST or ocean isotopic composition changes, based on the deuterium excess).

>

>As Dorthe will probably confirm, there is an ongoing work conducted by Sigfus Johnsen to be submitted to Journal of Quaternary Sciences next year, aiming at comparing all the Greenland Holocene temperature and isotopic profiles (including North GRIP).

>

>Therefore we think that it important to better define the scope of the HILOL possible paper (comparing north and south Holocene isotopic records

>and discussing the climate mechanisms involved) more than discussing the temperature imprint on water isotope records for instance.

>

>Second, we are still under the shock of the HILOL conclusions, mentioning

>a widespread Antarctic temperature optimum supposedly seen in all ice cores

>between 9 and 7 ka BP! In our paper published in Quaternary Research in november 2000 (data presented by Francoise at HILOL), we had a careful comparison of 11 existing Holocene Antarctic isotopic records (but without Dome F, so without ice cores in the Atlantic sector). Although we

>had no control on the independent time scales of these ice cores, they are

>all precisely dated during the transition and there is no doubt from the

>simple view of the raw isotopic (deuterium or oxygen 18) data, that they
>all exhibit a clear optimum from 11.5 to 9 ka BP, followed by a relative
>minimum at around 8 ka BP. Now, the sites located around the Ross Sea
show
>a mid Holocene optimum (8 to 6 ka BP), whereas in East Antarctica (apart
>from Dome C and Taylor Dome) a third "warm" interval can be seen later
(6
>to 3 ka BP). This is why we were quite surprised to hear about an
optimum
>between 9 and 7 ka BP in Antarctica.

>
>Last, if the HILOL possible paper is supposed to discuss the different
>timing of the major optima in the north and the south high latitudes,
>then it would greatly benefit from including climate modellers using
>intermediate complexity models (such as CLIMBER) and oceanographers (to
>discuss the possible role of changes in the north Atlantic circulation
in
>the first half of the Holocene).

>
>In such a framework, we are obviously willing to participate in the
>climate mechanisms discussion and of course provide the isotopic data
>measured at LSCE (e.g. Dome B, Vostok, "old" Dome C and EPICA Dome C).
For
>Byrd, you need to contact the Danish group.

>
>Sincerely,

>
> Valerie and Francoise.

>
>

>Laboratoire des Sciences	LSCE UMR CEA/CNRS 1572 Bat 709
>du Climat et de l'Environnement	L'Orme des Merisiers CEA Saclay
>Tel. (33) 1 69 08 77 15	91 191 Gif sur Yvette cedex
>Fax. (33) 1 69 08 77 16	France

>

From: Martin Welp <Martin.Welp@pik-potsdam.de>
To: gberz@minichre.com, juergen.engelhard@rheinbraun.de,
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Ottmar.Edenhofer@pik-potsdam.de, Martin.Welp@pik-potsdam.de
Subject: ECF
Date: Thu, 01 Feb 2001 17:59:03 +0100

Dear friends of the ECF,

Attached I send you:

- An executive summary of the ECF (to be revised anytime on the basis of your suggestions),
- The current version of the ECF "Manifesto" (to be revised anytime on the basis),
- The minutes of the Amsterdam preparatory meeting of last November.

We do have an URL by now: <www.European-Climate-Forum.net>. We will gradually develop it, please feel free to make suggestions. We also have an internal section, see: <www.European-Climate-Forum.net/internal/>.

Her at PIK, Dr. Martin Welp will take care of ECF logistics for the time being. His e-mail address is: Martin.Welp@pik-potsdam.de.

As for the Logo search, I like these things very much, although I am really not knowledgeable at all. We might make a pre-selection and run a competition on our web-site, inviting cyber-visitors to give their opinion

- we will still be free to choose what we like best. Perhaps rather than looking for the single best Logo right now it is more fruitful to identify which proposals find enough appreciation to become part of the web competition.

And let us enjoy these more playful moments without neglecting the key challenge we are faced with: Defining first joint projects and reaching agreements with relevant stakeholders to actually carry them out.

As for the foundation technicalities, we are preparing a background document that we will send out soon.

A final remark on e-mail etiquette: Could we put the string "ECF" in the subject line of all e-mails dealing with ECF, in order to enable our various browser to filter these pearls out of the ocean of e-mails we have begun to live in?

Best regards,
Carlo Jaeger
and Martin Welp

Attachment Converted: "c:\eudora\attach\ECF executive summary.rtf"

Attachment Converted: "c:\eudora\attach\ECF_Jan_01.rtf"

Attachment Converted: "c:\eudora\attach\ECF minutes Amsterdam.rtf"

From: "John L. Daly" <daly@microtech.com.au>
To: Chick Keller <ckeller@igpp.ucsd.edu>
Subject: Re: Hockey Sticks again
Date: Sat, 10 Feb 2001 21:47:57 +1100
Reply-to: daly@microtech.com.au
Cc: "P. Dietze" <p_dietze@t-online.de>, mmaccrac@usgcrp.gov, Michael E Mann <mann@virginia.edu>, rbradley@geo.umass.edu, wallace@atmos.washington.edu, Thomas Crowley <tom@ocean.tamu.edu>, Phil Jones <p.jones@uea.ac.uk>, sfbtett@meto.govt.uk, daly@vision.net.au, onar@netpower.no, jarl.ahlbeck@abo.fi, richard@courtney01.compulink.co.uk, McKitrick <rmckit@css.uoguelph.ca>, Bjarnason <agust@rt.is>, Harry Priem <priem@dds.nl>, vinmary.gray@paradise.net.nz, balberts@nas.edu, Martin Manning <m.manning@niwa.cri.nz>, Albert Arking <arking@jhu.edu>, Sallie Baliunas <baliunas@cfa.harvard.edu>, Jack Barrett <100436.3604@compuserve.com>, Sonja Boehmer-Cristianse <sonja.b-c@geo.hull.ac.uk>, Nigel Calder <nc@windstream.demon.co.uk>, John Christy <christy@atmos.uah.edu>, cpaynter@greeningearthssociety.org, driessen@global-commpartners.net, dwojick@shentel.net, Myron Ebell <mebell@cei.org>, Ellsaesser <hughel@home.com>, John Emsley <j.emsley@ic.ac.uk>, Jim Goodridge <jdg@mcn.org>, gsharp@montereybay.com, Peter Holle <cog@escape.ca>, Douglas V Hoyt <dhoyt1@erols.com>, "W. S. Hughes" <wsh@unite.com.au>, Wibjorn Karlson <wibjorn.karlen@natgeo.su.se>, kidso@hotmail.com, Kirill Kondratyev <kirill.kondratyev@niersc.spb.ru>, "Dr. Theodor Landscheidt" <theodor.landscheidt@ns.sympatico.ca>, Ross McKitrick <rmckitri@uoguelph.ca>, omcshane <omcshane@wk.planet.gen.nz>, Pat Michaels <pmichael@cato.org>, pbrekke@esa.nascom.nasa.gov, "David M. Ritson" <dmr@SLAC.Stanford.EDU>, robert.balling@asu.edu, Tom Segalstad <t.v.segalstad@toyen.uio.no>, Fred Singer <singer@sepp.org>, Roy Spencer <roy.spencer@msfc.nasa.gov>, Hartwig Volz <Hartwig.Volz@rwedea.de>, Gerd-Rainer Weber <gerd-rainer.weber@gvst.de>, tlowery@ocean.tamu.edu, Rosanne D'Arrigo <druidrd@Ideo.columbia.edu>, k.briffa@uea.ac.uk

Dear Chick & all

> the first is Keith Briffa's rather comprehensive treatment of getting
> climate variations from tree rings: Annual climate variability in
> the Holocene: "interpreting the message of ancient trees", Quaternary
> Science Reviews, 19 (2000) 87-105. It should deal with many of the
> questions people raise about using them to determine temperatures.

Take this from first principles.

A tree only grows on land. That excludes 70% of the earth covered by water. A tree does not grow on ice. A tree does not grow in a desert. A tree does not grow on grassland-savannahs. A tree does not grow in alpine areas. A tree does not grow in the tundra

We are left with perhaps 15% of the planet upon which forests grow/grew. That does not make any studies from tree rings global, or even hemispheric.

The width and density of tree rings is dependent upon the following variables which cannot be reliably separated from each other.

sunlight - if the sun varies, the ring will vary. But not at night of course.

cloudiness - more clouds, less sun, less ring.

pests/disease - a caterpillar or locust plague will reduce photosynthesis

access to sunlight - competition within a forest can disadvantage or advantage some trees.

moisture/rainfall - a key variable. Trees do not prosper in a drought even if there's a heat wave.

snow packing in spring around the base of the trees retards growth temperature - finally!

The tree ring is a composite of all these variables, not merely of temperature. Therefore on the 15% of the planet covered by trees, their rings do not and cannot accurately record temperature in isolation from the other environmental variables.

In my article on Greening Earth Society on the Hockey Stick, I point to other evidence which contradicts Mann's theory. The Idso's have produced more of that evidence, and a new article on Greening Earth has 'unearthed' even more.

Mann's theory simply does not stack up. But that was not the key issue. Anyone can put up a dud theory from time to time. What is at issue is the uncritical zeal with which the industry siezed on the theory before its scientific value had been properly tested. In one go, they tossed aside dozens of studies which confirmed the existence of the MWE and LIA as global events, and all on the basis of tree rings - a proxy which has all the deficiencies I have stated above.

The worst thing I can say about any paper such as his is that it is 'bad science'. Legal restraint prevents me going further. But in his case, only those restraints prevent me going *much* further.

Cheers

John Daly

--

John L. Daly

'Still Waiting For Greenhouse'

<http://www.microtech.com.au/daly>

replies to: daly@microtech.com.au

PLEASE NOTE:

WEBSITE URL HAS BEEN CHANGED TO <http://www.microtech.com.au/daly>

EMAIL ADDRESS HAS BEEN CHANGED TO daly@microtech.com.au

BOOKMARKS AND ADDRESS ENTRIES, IF ANY, SHOULD BE AMENDED ACCORDINGLY.

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

To: Phil Jones <p.jones@uea.ac.uk>

Subject: Re: Wally

Date: Mon, 26 Feb 2001 09:03:51 -0500

Cc: mhughes@ltrr.arizona.edu, tom crowley <tom@ocean.tamu.edu>, rbradley@geo.umass.edu, tom@ocean.tamu.edu, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, mann@virginia.edu

Dear Phil,

Thanks for your response. I agree that I think these folks just don't quite seem to get it! Anyways, I've pasted in the text of Broecker's piece below (everything there but the figure. Trust me, the figure isn't worth looking at anyways). Will be very interested to hear your thoughts after reading this...

mike

PALEOCLIMATE:

Was the Medieval Warm Period Global?

Wallace S. Broecker*

The reconstruction of global temperatures during the last millennium can provide important clues for how climate may change in the future. A recent, widely cited reconstruction (1) leaves the impression that the 20th century warming was unique during the last millennium. It shows no hint of the Medieval Warm Period (from around 800 to 1200 A.D.) during which the Vikings colonized Greenland (2), suggesting that this warm event was regional rather than global. It also remains unclear why just at the dawn of the Industrial Revolution and before the emission of substantial amounts of anthropogenic greenhouse gases, Earth's temperature began to rise steeply.

Was it a coincidence? I do not think so. Rather, I suspect that the post-1860 natural warming was the most recent in a series of similar warmings spaced at roughly 1500-year intervals throughout the present interglacial, the Holocene. Bond et al. have argued, on the basis of the ratio of iron-stained to clean grains in ice-rafted debris in North Atlantic sediments, that climatic conditions have oscillated steadily over the past 100,000 years (3), with an average period close to 1500 years. They also find evidence for the Little Ice Age (from about 1350 to 1860) (3). I agree with the authors

that the swing from the

Medieval Warm Period to the Little Ice Age was the penultimate of these oscillations and will try to make the case that the Medieval Warm Period was global rather than regional.

One difficulty encountered when trying to reconstruct Holocene temperature fluctuations is that they were probably less than 1°C. In my estimation, at least for time scales greater than a century or two, only two proxies can yield temperatures that are accurate to 0.5°C: the reconstruction of temperatures from the elevation of mountain snowlines and borehole thermometry. Tree ring records are useful for measuring temperature fluctuations over short time periods but cannot pick up long-term trends because there is no way to establish the long-term evolution in ring thickness were temperatures to have remained constant. Corals also are not accurate enough, especially because few records extend back a thousand years. The accuracy of the temperature estimates based on floral or faunal remains from lake and bog sediments is likely no better than $\pm 1.3^\circ\text{C}$ (4) and hence not sufficiently sensitive for Holocene thermometry.

The Mountain Glaciation Record

At the Last Glacial Maximum, mountain snowlines throughout the world were on average about 900 m lower than today (5). On the basis of today's rates of temperature change with elevation, this required an air temperature cooling at the elevation of the glacier of about 5°C (and a corresponding tropical sea surface temperature cooling of about 3°C). During the Younger Dryas--a cold "spell" of about 1200 years during the last deglaciation--snowlines in the Swiss and New Zealand Alps dropped to about 300 m below the lowest levels reached in the subsequent Holocene.

Since their 1860 maximum at the end of the Little Ice Age, the retreat of Swiss glaciers represents a rise in snowline of about 90 m (6). If this rise could be attributed entirely to air temperature, the required warming would be between 0.5° and 0.6°C. However, simple considerations suggest that precipitation changes result in a negative feedback of about 20% (7). The warming required to account for the post-1860 retreat of Alpine snowlines would then be between 0.6° and 0.7°C.

The post-1860 glacier retreat is not confined to Switzerland. With the exception of Antarctica, it has been well documented everywhere on Earth where ice-covered mountains are present (2). There is no doubt that the Little Ice Age was global in extent and that the post-1860 warming was also global. In this regard, the Mann et al. (1) reconstruction is consistent with the mountain snowline record.

The Medieval Warm Period has also left its traces in the Swiss Alps.

Holzhauser has reconstructed the history of a larchwood aqueduct constructed by medieval farmers (8). It ran from a small mountain lake along the valley occupied by the Grosser Aletsch Glacier, supplying water to an Alpine village. The aqueduct was first constructed around 1200 A.D. (toward the end of the Medieval Warm Period). It was partially destroyed when the glacier advanced in 1240 A.D. and had to be totally rerouted after a further advance in 1370 A.D.

Swiss geologists and geomorphologists agree that the large moraines marking the maximum glacier extent during the Little Ice Age are a composite of debris left behind by a series of Holocene advances (9). For example, soils separating individual advance episodes have been found within the moraines. Precise dating has proven difficult, however, and the chronology of these prior advances remains uncertain.

Two recent studies of Holocene climate cycles in the Swiss Alps have greatly improved this situation. Both focus on establishing the times of glacial retreats rather than advances. Holzhauser (8), on the basis of radiocarbon dating of larchwood stumps exposed by the ongoing retreat of the Grosser Aletsch Glacier, finds warm episodes 2400 ± 300 and 1500 ± 200 calendar years ago. Hormes and Schlüchter (10-12) have dated wood and peat fragments that are being disgorged from beneath a number of Swiss glaciers. Radiocarbon dates of a large number of these samples cluster in three major groups centered at 8700, 6600, and 4300 calendar years before present. The correlation between these Alpine warm phases and the warm phases of Bond's North Atlantic ice-rafting record, although imperfect, is encouraging (see the figure).

Climatic oscillations during the Holocene. Circles show the ratios of iron-stained to total grains (for grains with diameters $>63 \mu\text{m}$) in a North Atlantic core (3). The chronology is taken from (22). The green (10-12) and yellow (8) boxes are based on radiocarbon dating on wood and peat formed when the glaciers had retreated to positions similar to or up-valley from those at present (see text).

CREDIT: FIGURE PREPARED BY AUTHOR FOR THIS PUBLICATION

Borehole Thermometry

Geothermal heat is produced deep inside Earth, and the shape of the vertical temperature profile measured in a borehole from any point on Earth's surface thus reflects the depth dependence of the thermal conductivity of the crustal material. The temperature at the surface does not remain constant, however, and the thermal profiles therefore have kinks that reflect past air temperature fluctuations. Mathematical

deconvolutions are used to reconstruct these fluctuations from the temperature profile, but because of smoothing due to diffusive spreading of past thermal anomalies, many different time histories fit the observed downhole temperature record. The modeler selects from these possibilities the temperature history with the least complicated shape. The details are thus lost, and only the broad features of the time history are captured.

Deconvolutions of thermal records from holes drilled through the polar ice caps reveal broad maxima that reflect the colder temperatures during glacial times. In Greenland boreholes, this broad glacial feature is preceded by a narrower one, which requires a temperature oscillation to have occurred in the late Holocene. The timing of this swing broadly matches that of the Medieval Warm Period to Little Ice Age oscillation. Its magnitude is about 2°C (13). The borehole temperature record for Greenland thus appears to reflect the climate changes thought to have led to the establishment and eventual abandonment of the Viking colonies in southern Greenland (2). It is also consistent with records in the Swiss Alps.

Far Field Evidence

Evidence for the Medieval Warm Period from other parts of the world exists but is spotty and/or circumstantial. From an analysis of 6000 continental borehole thermal records from around the world (14), Huang et al. conclude that 500 to 1000 years ago, temperatures were warmer than today, but that about 200 years ago, they cooled to a minimum some 0.2° to 0.7°C below present. However, as is the case for the thermal profiles in ice, those for continental boreholes are highly smoothed. Although suggestive, the fluctuation postulated by Huang et al. does not prove that the Medieval Warm Period was global in extent.

Evidence that climate during the latter part of the Medieval Warm Period was much different from today's comes from moisture records for the western United States. Stine has studied lodgepole pine trees rooted at 8 to 19 m depth in Lake Tenaya in the high Sierra Nevada (15). For the trees to have grown, the lake must have been nearly dry. In contrast, only once during the past 50 years has the lake not overflowed during snowmelt. Using radiocarbon dating and ring counting, Stine has shown that for 70 years before 1093 A.D., the lake stood at least 13 m below its outflow spillway, and for 141 years before 1333 A.D., it stood at least 11 m below its spillway (16). Stine has documented similar events at Mono Lake and the Walker River (17). He concludes that late in the Medieval Warm Period, California experienced several decade-long periods of profound drought.

If, as Bond et al. (3) suggest, the cyclic changes in ice-rafted debris composition reflect oscillations in the strength of the Atlantic's conveyor

circulation, one might expect temperature changes in Antarctica to have been opposite in phase to those in the North Atlantic, as was the case during the last deglaciation (18). Clow has carried out a deconvolution of the temperature record at the Antarctic Taylor Dome site (19). His reconstruction shows that the air temperature was 3°C colder during the time of the Medieval Warm Period than during that of the Little Ice Age. This record suggests that conditions in Antarctica underwent an antiphased oscillation during the Medieval Warm Period-Little Ice Age period.

The Case for a Global Event

The case for a global Medieval Warm Period admittedly remains inconclusive. But keeping in mind that most proxies do not have adequate sensitivity, it is interesting that those capable of resolving temperature changes of less than 1°C yield results consistent with a global Medieval Warm Period. To test whether this is indeed the case, we require Holocene snowline fluctuation records for tropical and Southern Hemisphere sites and continued studies of wood and peat exposed by the continuing retreat of Northern Hemisphere glaciers. As the world's mountain glaciers continue to retreat, ever more evidence for past Holocene warm episodes will be exposed.

One might ask why the strength of the Atlantic's conveyor circulation oscillates on a time scale of one cycle per 1000 to 2000 years. I suspect that it has to do with the export through the atmosphere of water vapor from the Atlantic to the Pacific Ocean. The magnitude of this export has been estimated to be $(0.25 \pm 0.10) \times 10^6$ m³/s (20). If this freshwater loss were not balanced by the export of salt from the Atlantic, the latter's salt content would rise at the rate of about one gram per liter each 1500 years. Such an increase in salt content would densify cold surface water by an amount equivalent to a 4 to 5 K cooling, thereby strongly altering the buoyancy of surface waters in the North Atlantic and hence their ability to sink to the abyss.

I believe that this salt export is not continuous but episodic. The salt content of the Atlantic periodically builds up until a strong conveyor circulation mode is turned on, causing the salt content to drain down. Eventually, a weak circulation mode kicks in, allowing the salt content to build up again. I have suggested previously (21) that an apparent mismatch between radiocarbon and chlorofluorocarbon-based estimates of the rate of deep-water formation in the Southern Ocean may reflect a change in circulation after the Little Ice Age.

The geographic pattern of Holocene climate fluctuations remains murky, but several things are clear. The Little Ice Age and the subsequent warming were global in extent. Several Holocene fluctuations in snowline, comparable in magnitude to that of the post-Little Ice Age warming,

occurred in the Swiss Alps. Borehole records both in polar ice and in wells from all continents suggest the existence of a Medieval Warm Period. Finally, two multidecade-duration droughts plagued the western United States during the latter part of the Medieval Warm Period. I consider this evidence sufficiently convincing to merit an intensification of studies aimed at elucidating Holocene climate fluctuations, upon which the warming due to greenhouse gases is superimposed.

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At 12:04 PM 2/26/01 +0000, Phil Jones wrote:

>
> Dear All,
> I was away over the weekend at Bowdoin College in Maine, giving a
>talk about the
> last 1000 years. There were three others as well on other paleo aspects,
>Richard Alley,
> Gary Clow and Wally Broecker ! The latter briefly mentioned to me that
>he had had
> something in last Friday's Science, which was getting at the Mann et al.
>series. He
> didn't have a copy so we've not seen it here yet. I tried to get a copy
>of Science on
> the bookstand at Logan airport last night - I guess it's not sold that
>way !
> Wally was going on about this 1500 yr cycle of Bond's, which seemed
>pretty flimsy.
> I was showing all the various series in a general talk - and I used some
>of the overheads
> from the upcoming Science paper. This is due to appear in the issue for
>the last week
> of April. It is all accepted now. I will forward if you'll all abide by
>the Science rules. Both
> Wally and Alley seem convinced that the climate of Greenland changed by
>10 C in
> the space of 2-3 years at times in the past (Y Dryas etc). I had long
>talks with both
> and they don't seem to have got their heads around spatial scales (local
>changes
> and hemispheric). Also they don't seem to realise where we are coming
>from. He
> has a downer on trees (believes all the multiproxy series depend
>exclusively on
> trees) but he thinks Ed Cook is a great scientist. The latter is true,
>but he might
> just think that because he's at Lamont. I did tell him that Keith's paper
>on the age
> banding is out in JGR. I should send him a reprint and maybe ask that great
> scientist to go and explain it to him ! Ed's in NZ at the moment. Also
>Wally believes
> much more in glacier advances/retreats. I'll get Keith to send him
>Sarah's paper

> where the long Tornetrask reconstruction is shown to agree with
Storglaciaren
> advance/retreat dates from moraine evidence. Also Sarah's been working on
>similar
> glaciers in the Swiss Alps with long tree-ring reconstructions. One
>interesting
> thing was he didn't seem to realise that a lot of the tree-ring
>reconstructions use
> density. Seemed to think they were all ring widths and there had to be
>moisture
> changes we were not accounting for.
> It is easy to respond to a Perspectives piece. Some of you did it
>with respect to
> one of mine. I'm not sure it will achieve much - it won't come out before
>the paper
> in the last week of April. I need to wait to see what he says. Our paper
>(me, Tim and
> Keith) clearly says that the MWP couldn't have been warmer (for the NH
>average)
> than the late 20th century.
> Another possible reason for not doing anything is that the IPCC
>report will be out
> soon. The summary is written in pretty clear language.
> The above is my first thoughts, not having read the piece and just
>got off the
> flight back.
>
> Best to ignore Wojciek. All he seems to want to do is deflect us into
>responding.
>
> Cheers
> Phil
>
>
>
>At 11:47 25/02/01 -0700, mhughes@ltrr.arizona.edu wrote:
>>Dear all,
>>What mechanism does "Science" have for responding to Perspective pieces?
Most
>>of the answer to Wally is contained within his own piece - he comments on
the
>>ambiguity of the record, which, in various ways, we have all done. What he
>>doesn't offer, however, is anything other than an anecdotal alternative. As
>>always, he seeks to damn (in this case with faint praise) the records or
>>work
>>that don't serve his purpose , and to elevate any scrap of evidence that
does

>>serve it. I think it will be important for us to stick closely to what we
>>have
>>written in published papers. CHEers, MAIcolm
>>
>>Quoting "Michael E. Mann" <mann@virginia.edu>:
>>
>>> Dear Phil, Ray,
>>>
>>> What do you guys think. If we're all on board, than an appropriately
>>> toned,
>>> "high road" response here might be appropriate. We don't want to engage
>>> Wally in a personal battle, but simply should correct the record where
>>> Wally has muddied it. Again, Phil et al do have a Science article in
>>> press
>>> that serves this purpose to some extent, so I'm especially interested in
>>> what
>>> Phil thinks (Phil?)...
>>>
>>> mike
>>>
>>> At 02:52 PM 2/24/01 -0700, mhughes@ltrr.arizona.edu wrote:
>>>>Dear Mike et al., I think we should definitely let Wojick stew in his
>>>> own
>>>>juice - as Mike pointed out to me the other day he, and his like, have
>>>> a
>>>>specific agenda, and anything we write will be pressed into the service
>>>> of
>>>> that
>>>>agenda. I'm not so sure about Wally. I share Tom's disinclination to
>>>> get
>>>> into a
>>>>street fight with Wally - generally I take the view that life's too
>>>> short and
>>>>uncertain for such activities. On the other hand, would we let such a
>>>> shoddy
>>>>piece of work(and editing) go by if it were from another author? There
>>>> are so
>>>>>many holes in Wally's argument, and such a selective choice of evidence
>>>> that it
>>>>>should beggar belief. One of the more obvious holes is that he writes
>>>>> of the
>>>>>Great Basin droughts of the 10th through 14th centuries as proof of
>>>>> warmer
>>>>>conditions then, but doesn't explain why we don't have such conditions
>>>>> now.
>>>>>Interestingly, Larry Benson, Dave Meko and others have good evidence
>>>>> that

>>> these
>>> >same multidecadal periods were marked by a great excess of
>>> precipitation
>>> just a
>>> >few hundred miles north in northern Nevada and California and southern
>>> Oregon.
>>> >He just hasn't grasped that the methods that are appropriate for
>>> tracking the
>>> >consequences of major changes in boundary conditions don't work in the
>>> late
>>> >Holocene. I've been trying to figure out the issue of "Was there a
>>> Medieval
>>> >Warm Period, and if so where and when" for a decade or so, and still
>>> have the
>>> >impression that the records for the 9th through 14th centuries are
>>> extremely
>>> >mixed. But then, I didn't come to the investigation with a certain
>>> knowledge of
>>> >the absolute truth, and have had to 'misfortune' to work with people
>>> who let
>>> >careful analysis get in the way - Henry Diaz, Ray and Mike, and others.
>>>
>>> >Anyway, the point of this rant is that I think we should give careful
>>> >consideration to making a measured response to Wally. Cheers, Malcolm
>>>>
>>>>
>>>>
>>>>
>>>>
>>>> Quoting "Michael E. Mann" <mann@virginia.edu>:
>>>>
>>>>> Hi Tom,
>>>>>
>>>>> Thanks for your quick reply. I agree with you entirely. I think its
>>>>> very
>>>>> unfortunate he's chosen to disinform the community rather than engage
>>>>> in
>>>>> a
>>>>> constructive dialogue (we tried the latter w/ him in a series of
>>>>> emails
>>>>> last
>>>>> year, but clearly to no avail).
>>>>>
>>>>> On the other hand, think that a war of words w/ Broecker would be
>>>>> exploited
>>>>> by the skeptics, and perhaps we should just try to let this thing
>>> die...
>>>>>

>>>> I'm not sure. I'd appreciate knowing what others think?
>>>>
>>>> mike
>>>>
>>>> At 10:25 AM 2/24/01 -0600, tom crowley wrote:
>>>>> >Mike,
>>>>>
>>>>> >I was not aware of the Broecker piece - I am dismayed but not
>>>>> surprised. I
>>>>> >do not know what to do - I personally cannot stand the combative
>>>>> personal
>>>>> >approach Broecker relishes but it does seem as if some rebuttal is
>>>>> called
>>>>> >for. Maybe you Ray Phil I and Malcolm could pen a response - we are
>>>>> >heading to Germany in a week, for a month, so I am not sure how much
>>> I
>>>>> can
>>>>> >keep up on this but it seems as if some response is called for.
>>>>>
>>>>>> >What think ye?
>>>>>>
>>>>>> >Tom
>>>>>>
>>>>>>
>>>>>>>> Dear Mike,
>>>>>>>>
>>>>>>>> Thanks for passing this along.
>>>>>>>>
>>>>>>>> Wojick of course completely misrepresents Broecker, and puts his
>>>>>>>> conventional intellectually dishonest spin on this.
>>>>>>>>
>>>>>>>> That having been said, it is a bit disappointing that Wally
>>> continues
>>>>>> to
>>>>>>>> cling to some of his flawed beliefs which aren't supported from
>>> either
>>>>>> our
>>>>>>>> best current understanding of the observations or of the results of
>>>>>>>> careful
>>>>>>>> modeling experiments. My own perception is that the climate
>>> community,
>>>>>>>> modelers as well as observationalists, simply don't take seriously
>>>>>>>> anymore
>>>>>>>> the idea that the history of climate change over the past 1000
>>> years
>>>>>> is
>>>>>>>> part of an internal oscillation. The sediment core evidence oft

>>> cited
>>>> by
>>>>> Broecker (e.g. Bond et al) for this is tremendously weak, and I, as
>>>>> well as
>>>>> the vast majority of my colleagues, simply don't buy it for even a
>>>>> second.
>>>>> But people don't like to challenge Broecker publically. He can and
>>>>> will
>>>>> play hardball.
>>>>>
>>>>>> There is an odd irony. Broecker refused to accept the modeling
>>>>>> evidence
>>>>>> that the 100 kyr ice age Pleistocene variations were part of an
>>>>>> internal
>>>>>> oscillation paced by insolation variations, favoring instead the
>>>>>> discredited notion that they were a direct response to (too weak)
>>>>>> eccentricity forcing, until the evidence became insurmountable
>>> (from
>>>>> my
>>>>>> adviser, Barry Saltzman, may he rest in piece, and people like Dick
>>>>>> Peltier). Ironically, Broecker then took credit for the very
>>>>> proposition he
>>>>>> had fought w/ tooth and nail.
>>>>>>
>>>>>>> Broecker is even more wrong, and unfortunately equally stubborn, in
>>>>>>> this case.
>>>>>>> And, again, the reason: because his pet theory, that climate
>>>>>>> variability is
>>>>>>> a simple millennial oscillation, is finally being challenged w/
>>> hard
>>>>>> data
>>>>>>> and hard facts.
>>>>>>>
>>>>>>>> Broecker misrepresents the nature of that data that we and others
>>> have
>>>>>>>> used, and misunderstands the source of the muted hemispheric trends
>>>>>>>> (there
>>>>>>>>> *is* a hemispheric "medieval warm period" and "little ice age",
>>> just
>>>>>>>> not of
>>>>>>>>> the magnitude or the distinctiveness that Broecker imagines).
>>>>>>>>> Individual
>>>>>>>>>> regions in our reconstructions, and Phils, and others, vary by
>>> several
>>>>>>>>>> degrees C, ie, the proxies we use have no problem whatsoever in
>>>>>>>>>> resolving
>>>>>>>>>>>> high-amplitude temperature variations in the past. The problem is

>> > that
>> >>> when
>> >>>>we look at the different regions we find that periods of cold and
>> >>> warm
>> >>>>often occur at very different times in different regions, and so in
>> > a
>> >>>>hemispheric or global average, a lot of purely regional variability
>> >>> cancels
>> >>>>out. The resulting trends are somewhat smaller. I remained
>> > befuddled
>> >>> as to
>> >>>>why Wally doesn't understand this point. Its been explained to him
>> >>> time and
>> >>>>time again. Maybe he's just not listening, or doesn't want to
>> >>> listen...
>> >>>>
>> >>>>>In fact, Tom Crowley has clearly shown that the observed millennial
>> >>>>>temperature reconstruction is precisely consistent w/ our
>> >>>> understanding of
>> >>>>>*forced* climate change over the past 1000 years (solar changes,
>> >>>> volcancic
>> >>>>>output, and recent greenhouse gas concentrations). There is, simply
>> >>>> put, no
>> >>>>>room for a global millennial internal oscillation. Regionally, such
>> >>>> types
>> >>>>>of climate phenomena, associated for example with changes in the
>> > North
>> >>>>>Atlantic ocean circulation, are supported by the observations. This
>> >>>>>explains why, for example, European temperature variations are
>> >>>> somewhat
>> >>>>>larger than those in other regions not effected so strongly by such
>> >>>> climate
>> >>>>>processes.
>> >>>>>
>> >>>>>Other recent perspectives, by Ray Bradley and myself provide a far
>> >>>> more
>> >>>>>balanced and nuanced (and less dogmatic or defensive) viewpoint.
>> > I'm
>> >>>> not
>> >>>>>sure a written response to Broecker is worthwhile (this is,
>> > afterall,
>> >>>> a
>> >>>>>"perspective" and everyone understands that a scientist may have a
>> >>>> flawed
>> >>>>>perspective). If Wally wants this to be his legacy, so be it...
>> >>>>>
>> >>>>>Phil and others have a review article coming out in the near future

>>>> which
>>>>>also provides a much more balanced perspective on the climate
>>> changes
>>>> of
>>>>>the past millennium, and will set the record straight once again
>>> (good
>>>>>timing Phil!). Science's embargo policy prevents me from saying
>>> much
>>>>> more
>>>>>at this time, but if Phil or anyone else wishes to comment further,
>>>>> I'd
>>>>>encourage it.
>>>>>>
>>>>>>Well, I've still got some snow to shovel here in Charlottesville!
>>>>> Happy
>>>>>>weekend to all,
>>>>>>
>>>>>>mike
>>>>>>
>>>>>>p.s. For those with electronic subscriptions, Broecker's latest
>>> piece
>>>>> can
>>>>>>be found here:
>>>>>>
>>>>>> PALEOCLIMATE:
>>>>>> Was the Medieval Warm Period Global?
>>>>>> Wallace S. Broecker
>>>>>> Science Feb 23 2001: 1497-1499. [Summary] [Full Text]
>>>>>>
>>>>>>><http://www.sciencemag.org/cgi/content/full/291/5508/1497>
>>>>>>>
>>>>>>>While my previous perspective piece is here:
>>>>>>> CLIMATE CHANGE:
>>>>>>> Lessons for a New Millennium
>>>>>>> Michael E. Mann
>>>>>>> Science 2000 July 14; 289: 253-254. (in Perspectives) [Summary]
>>>>>>> [Full
>>>>>>>Text]
>>>>>>>URL:
>>>>>>>
>>>>>>>>[http://www.sciencemag.org/cgi/content/full/289/5477/253?maxtoshow=&HIT](http://www.sciencemag.org/cgi/content/full/289/5477/253?maxtoshow=&HITS=10&its=10&RESULTFORMAT=&author1=Mann&searchid=QID_NOT_SET&stored_search=&FIRSTINDEX=&fdate=10/1/1995&tdate=2/28/2001)
>>>>>>>>S=10&h
>>>>>>>>
>>>>>>>>its=10&RESULTFORMAT=&author1=Mann&searchid=QID_NOT_SET&stored_search=&
>>>>>>>>FIRSTI
>>>>>>>>NDEX=&fdate=10/1/1995&tdate=2/28/2001
>>>>>>>>

>>>>and Bradley's is here:

>>>>

>>>>> PALEOCLIMATE: Enhanced: 1000 Years of Climate Change

>>>>> Ray Bradley

>>>>> Science 2000 May 26; 288: 1353-1355. (in Perspectives) [Summary]

>>>> [Full

>>>>>Text]

>>>>>

>>>>>URL:

>>>>

>>>>><http://www.sciencemag.org/cgi/content/full/288/5470/1353?maxtoshow=&HI>

>> TS=10&

>>>>

>>>>>hits=10&RESULTFORMAT=&author1=Bradley&searchid=QID_NOT_SET&stored_sear

>> ch=&FI

>>>>>RSTINDEX=&fdate=10/1/1995&tdate=2/28/2001

>>>>>

>>>>>>Dear Michael--The third point below has comments on the

>>> controversy

>>>>>>betweenyou and Broecker--I'd be interested in your response (did

>>>>> Wally not

>>>>>>just understand what your data show?).

>>>>>>

>>>>>>Mike

>>>>>>

>>>>>>Three Wojick Pieces on Climate Change.

>>>>>>I've been busy busy.

>>>>>>

>>>>>>David

>>>>>>

>>>>>>FIRST, the latest issue of Insight Magazine includes a

>>>>> point-counterpoint

>>>>>>between measly old me and the great Robert Watson. Boy has he got

>>>>>>credentials! Too bad he's wrong.

>>>>>>

>>>>>><<http://www.insightmag.com/archive/200103143.shtml>>

>>>>>>

>>>>>>>Symposium: Do scientists have compelling evidence of global

>>> warming?

>>>>>>>

>>>>>>>Yes: Rising sea levels worldwide and retreating Arctic glaciers

>>>> are

>>>>> ominous

>>>>>>>signs.

>>>>>>>

>>>>>>>By Robert T. Watson -- chairman of the UN Intergovernmental Panel

>>>> on

>>>>>>Climate Change, chief scientist at the World Bank and former chief
>>>>> science
>>>>>>advisor to the Clinton White House.
>>>>>>>
>>>>>>>No: Despite the overheated rhetoric, there is no new evidence of
>>>>> warming
>>>>>>>
>>>>>>>By David E. Wojick -- covers climate policy for Electricity Daily
>>> and
>>>>> is a
>>>>>>>science adviser to the Greening Earth Society
>>>>>>><<http://www.greeningearthsociety.org>>, as well as Undereditor of
>>> the
>>>>>>>Washington Pest <<http://www.WashingtonPest.com>>
>>>>>>>>
>>>>>>>>SECOND, the February 15 Eco-logic on-line has published "The Black
>>>>> Hole of
>>>>>>>Global Climate Government" by David Wojick, my detailed attack on
>>> the
>>>>>>>Framework Convention on Climate Change. It includes a lot of the
>>>>> actual
>>>>>>>treaty language.
>>>>>>>>
>>>>>>>><<http://www.eco.freedom.org/el/20010202/wojick.shtml>>
>>>>>>>>>
>>>>>>>>THIRD, here is a draft Electricity Daily article of mine. Seems
>>> I'm
>>>>>> not the
>>>>>>>>only one who thinks the IPCC is nuts.
>>>>>>>>>
>>>>>>>>>Climate Guru Kicks The Hockey Stick
>>>>>>>>>by David Wojick (dwojick@shentel.net)
>>>>>>>>>>
>>>>>>>>>>Global warming is natural and the recent warming is probably no
>>>>>> exception.
>>>>>>>>>This is the controversial argument made by prominent climatologist
>>>>>> Wallace
>>>>>>>>>S. Broecker in today's issue of Science.
>>>>>>>>>>
>>>>>>>>>>Broecker's bombshell bears the seemingly innocent title "Was the
>>>>>> Medieval
>>>>>>>>>Warm Period Global?" It may seem esoteric, but whether the
>>> apparent
>>>>>> warmth
>>>>>>>>>reported in Europe about 1000 years ago was global or simply local
>>> is
>>>>>>>>>becoming a central issue in climate science. What makes it

>>>> contentious is
>>>>>>the recent claims by the United Nations Intergovernmental Panel on
>>>> Climate
>>>>>>Change that the earth is warmer now than it has been for
>>> millennia,
>>>> and
>>>>>>that therefore human carbon dioxide emissions are to blame.
>>> Broecker,
>>>> a
>>>>>>leading figure at Lamont-Doherty Earth Observatory, Columbia
>>>> University,
>>>>>>questions both IPCC claims.
>>>>>>
>>>>>>The focus of the debate is a 1000-year temperature reconstruction
>>>> known in
>>>>>>climate circles as the "hockey stick". Produced in 1999 by M. E.
>>>> Mann, R.
>>>>>>S. Bradley, M. K. Hughes, the long handle of the hockey stick
>>> shows
>>>> global
>>>>>>temperatures for the first 8 centuries as basically unchanging,
>>>> followed by
>>>>>>the sharply up-tilting blade of the last 150 years or so. The Mann
>>> et
>>>> al
>>>>>>hockey stick is the central feature of the recently released IPCC
>>>> working
>>>>>>group one Summary for Policy makers, which claims to embody the
>>> best
>>>> of
>>>>>>climate science.
>>>>>>
>>>>>>Broecker does not like the hockey stick, nor the conclusions the
>>> IPCC
>>>> draws
>>>>>>from it. He says " A recent, widely cited reconstruction (Mann's)
>>>> leaves
>>>>>>the impression that the 20th century warming was unique during the
>>>> last
>>>>>>millennium. It shows no hint of the Medieval Warm Period (from
>>> around
>>>>>> 800
>>>>>>to 1200 A.D.) during which the Vikings colonized Greenland,
>>>> suggesting that
>>>>>>this warm event was regional rather than global. It also remains
>>>> unclear
>>>>>>why just at the dawn of the Industrial Revolution and before the

>>> emission
>>> >>>of substantial amounts of anthropogenic greenhouse gases, Earth's
>>> >>>temperature began to rise steeply. Was it a coincidence? I do not
>>> >>> think so.
>>> >>>Rather, I suspect that the post-1860 natural warming was the most
>>> >>> recent in
>>> >>>a series of similar warmings spaced at roughly 1500-year intervals
>>> >>>throughout the present inter-glacial, the Holocene."
>>> >>>
>>> >>>Broecker presents the evidence for a global Medieval Warm Period,
>>> >>> as
>>> >>> well
>>> >>>as for a Little Ice Age from around 1300 to 1860, when the present
>>> >>>temperature rise begins. He also argues that the "proxy" evidence
>>> >>> used by
>>> >>>Mann et al, such as tree ring data, is ill suited to the time
>>> >>> period
>>> >>> and
>>> >>>temperature variation -- less than one degree C -- in question.
>>> >>>
>>> >>>As he puts it, "In my estimation, at least for time scales greater
>>> >>> than a
>>> >>>century or two, only two proxies can yield temperatures that are
>>> >>> accurate
>>> >>>to 0.5 C: the reconstruction of temperatures from the elevation of
>>> >>> mountain
>>> >>>snowlines and borehole thermometry. Tree ring records are useful
>>> >>> for
>>> >>>measuring temperature fluctuations over short time periods but
>>> >>> cannot
>>> >>> pick
>>> >>>up long-term trends because there is no way to establish the
>>> >>> long-term
>>> >>>evolution in ring thickness were temperatures to have remained
>>> >>> constant."
>>> >>>
>>> >>>Broecker acknowledges that the proxy evidence is necessarily
>>> >>> somewhat
>>> >>>"murky", but his conclusion is that "climatic conditions have
>>> >>> oscillated
>>> >>>steadily over the past 100,000 years, with an average period close
>>> >>> to
>>> >>> 1500
>>> >>>years... The swing from the Medieval Warm Period to the Little Ice
>>> >>> Age was
>>> >>>the penultimate of these oscillations." The implication being that
>>> >>> some, if

>>>>>>not all, of the present warming is the natural swing out of the
>>>>>> Little Ice
>>>>>>Age, and that Mann et al, as well as the IPCC, are mistaken.
>>>>>>
>>>>>>
>>>>>>--
>>>>>>
>>>>>>
>>>>>>Dr. David E. Wojick
>>>>>>President
>>>>>>Climatechangedebate.org
>>>>>>Subscribe to the free debate listserv at
>>>>>> <http://www.climatechangedebate.org>
>>>>>>Non subscribers can follow the debate at
>>>>>> <http://www.eScribe.com/science/ClimateChangeDebate/>
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From: tom crowley <tom@ocean.tamu.edu>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
Subject: Re: Wally
Date: Mon, 26 Feb 2001 11:18:19 -0600
Cc: mhughes@lrr.arizona.edu, rbradley@geo.umass.edu, k.briffa@uea.ac.uk, p.jones@uea.ac.uk, t.osborn@uea.ac.uk

Mike,

you are really the most appropriate person to be the lead author on this -
I was just volunteering myself as the unfortunate soul who has to bear the
brunt of Wallys wrath

Tom

ps Peck would be fine of course but I don't know whether we want to get
him tangled up in the acrimony - we could of course ask for his comments
beforehand

>HI Tom,

>

>Thanks--I was thinking this too. Ray held out a real olive branch to Wally
>by the extremely balanced piece he wrote in Science last year (some of us
>thought he caved in a bit too much!). So there was absolutely no reason for
>Wally to write this piece.

>

>If Julie Uppenbrink gives us the go-ahead, I say lets do as Tom suggests. I
>think this has a lot more cachet if all on this list are willing to sign on
>as co-authors.

>

>Regarding primary authorship: On the one hand, it would be appropriate for
>me tsince it is primarily Mann et al that is explicitly under attack here,
>though all of us are implicitly under attack. However, I think the piece
>carries a lot more weight if it is authored by someone of Wally's stature,
>and I think Tom far better fits the bill in this regard. So if Tom is
>willing to bear the brunt of this, I would definitely endorse him being
>primary author.

>

>I would argue to include Peck too, but I think this would be a conflict for
>him, as he is pretty close to Wally. So best to leave it w/ the current
>group in my opinion. Lets pursue this further once Phil hears back from J.U...

>

>mike

>

>At 09:16 AM 2/26/01 -0600, tom crowley wrote:

>>Hi all,

>>

>>>I vote for a response - quick and to the point - itemized in fact.

>>

>>>The only problem is somehow has to volunteer to be the sacrificial lamb as
>>>first author - that person will almost certainly be badgered by Wally and
>>>probably charged with some trumped up unethical piece - he will also
>>>probably try to subvert the review process by contacting the Editor of
>>>Science. This is not paranoia - Wally did exactly this when some people
>>>(some at Lamont!) questioned his conveyor explanation for the LIA that came
>>>out in Science a year or so ago. He was actually screaming at some of
>>>these people in the Lamont lunch room.

>>

>>>That said, I say we must bite the bullet and do it - Wally doesn't like me
>>>anyway so it wouldnt make as much a difference to me if I volunteered to go
>>>to the slaughter but if there is anyone else who wants to take the lead,
>>>thats fine with me!!

>>

>>>Tom

>>

>>>ps as I indicated the other day I will be in only until this Friday after
>>>which I am out for a month - I could write enough to get us going and then
>>>hand it over to someone else to deal with the submission business (Mike?)

>>

>>

>>>>Thanks a bunch Phil,

>>>>

>>>>Will look forward to hearing back w/ more info. I talked to Dick Kerr last
>>>>week about related stuff (an IPCC article he's writing) and he made no
>>>>mention of this at all! I wonder who did commission this, and why?

>>>>

>>>>mike

>>>>

>>>>At 02:51 PM 2/26/01 +0000, Phil Jones wrote:

>>>>>A

>>>>>

>>>>>

>>>>>

>>>>>

>>>>>

>>>>>

>>>>> Mike,

>>>>> I've had a quick read and sent an email to Julia Uppenbrink to get her
>>>>>views as

>>>>> she commissioned our piece. Also asked about a response, particularly on
>>>>>the

>>>>> high and low frequency indicators. I was going to send Wally two papers
>>>>> (Sarah Raper's on linking trees and glaciers in J. Glaciol. and Brian
>>>>>Luckman's

>>>>> in The Holocene, where the two are also linked but only in a qualitative

>>>>way).
>>>> From the weekend it was clear he had no ideas about these. His lack of
>>>>knowledge
>>>> of density data in trees come through in the article as well.
>>>> In Maine he also went on at length about the Stine work. and seems to
>>>>in this
>>>> piece as well. Malcolm should know all about this.
>>>> I'm going to go home soon as I'm getting knackered, but I'll email you
>>>>Julia's
>>>> response. I think she'll find out who asked Wally to do it, as he
>>>>implied to me it
>>>> was.
>>>>
>>>> Cheers
>>>> Phil
>>>>
>>>> PS Meant to say at the start that I see your points. Thanks for pasting
>>>>it to us.

>>>>
>>>>
>>>>

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From: Phil Jones <p.jones@uea.ac.uk>
To: mhughes@ltrr.arizona.edu, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Wally
Date: Mon, 26 Feb 2001 12:04:32 +0000
Cc: <mhughes@ltrr.arizona.edu>, "Michael E. Mann" <mann@virginia.edu>, tom crowley <tom@ocean.tamu.edu>, "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, <rbradley@geo.umass.edu>, <tom@ocean.tamu.edu>, <mhughes@ltrr.arizona.edu>, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

<x-flowed>

Dear All,

I was away over the weekend at Bowdoin College in Maine, giving a talk about the

last 1000 years. There were three others as well on other paleo aspects,

Richard Alley,

Gary Clow and Wally Broecker ! The latter briefly mentioned to me that he had had

something in last Friday's Science, which was getting at the Mann et al.

series. He

didn't have a copy so we've not seen it here yet. I tried to get a copy

of Science on

the bookstand at Logan airport last night - I guess it's not sold that way !

Wally was going on about this 1500 yr cycle of Bond's, which seemed pretty flimsy.

I was showing all the various series in a general talk - and I used some

of the overheads

from the upcoming Science paper. This is due to appear in the issue for the last week

of April. It is all accepted now. I will forward if you'll all abide by

the Science rules. Both

Wally and Alley seem convinced that the climate of Greenland changed by 10 C in

the space of 2-3 years at times in the past (Y Dryas etc). I had long talks with both

and they don't seem to have got their heads around spatial scales (local

changes

and hemispheric). Also they don't seem to realise where we are coming from. He

has a downer on trees (believes all the multiproxy series depend exclusively on

trees) but he thinks Ed Cook is a great scientist. The latter is true, but he might

just think that because he's at Lamont. I did tell him that Keith's paper

on the age

banding is out in JGR. I should send him a reprint and maybe ask that great scientist to go and explain it to him ! Ed's in NZ at the moment. Also Wally believes much more in glacier advances/retreats. I'll get Keith to send him Sarah's paper where the long Tornetrask reconstruction is shown to agree with Storglaciaren advance/retreat dates from moraine evidence. Also Sarah's been working on similar glaciers in the Swiss Alps with long tree-ring reconstructions. One interesting thing was he didn't seem to realise that a lot of the tree-ring reconstructions use density. Seemed to think they were all ring widths and there had to be moisture changes we were not accounting for.

It is easy to respond to a Perspectives piece. Some of you did it with respect to one of mine. I'm not sure it will achieve much - it won't come out before the paper in the last week of April. I need to wait to see what he says. Our paper (me, Tim and Keith) clearly says that the MWP couldn't have been warmer (for the NH average) than the late 20th century.

Another possible reason for not doing anything is that the IPCC report will be out soon. The summary is written in pretty clear language.

The above is my first thoughts, not having read the piece and just got off the flight back.

Best to ignore Wojciek. All he seems to want to do is deflect us into responding.

Cheers
Phil

At 11:47 25/02/01 -0700, mhughes@ltrr.arizona.edu wrote:
>Dear all,
>What mechanism does "Science" have for responding to Perspective pieces?
Most
>of the answer to Wally is contained within his own piece - he comments on the
>ambiguity of the record, which, in various ways, we have all done. What he

>doesn't offer, however, is anything other than an anecdotal alternative.
As
>always, he seeks to damn (in this case with faint praise) the records
or
>work
>that don't serve his purpose , and to elevate any scrap of evidence that
does
>serve it. I think it will be important for us to stick closely to what
we
>have
>written in published papers. CHeers, MAlcolm
>
>Quoting "Michael E. Mann" <mann@virginia.edu>:
>
> > Dear Phil, Ray,
> >
> > What do you guys think. If we're all on board, than an appropriately
> > toned,
> > "high road" response here might be appropriate. We don't want to
engage
> > Wally in a personal battle, but simply should correct the record
where
> > Wally has muddied it. Again, Phil et al do have a Science article in
> > press
> > that serves this purpose to some extent, so I'm especially interested
in
> > what
> > Phil thinks (Phil?)...
> >
> > mike
> >
> > At 02:52 PM 2/24/01 -0700, mhughes@ltrr.arizona.edu wrote:
> > >Dear Mike et al., I think we should definitely let Wojick stew in
his
> > own
> > >juice - as Mike pointed out to me the other day he, and his like,
have
> > a
> > >specific agenda, and anything we write will be pressed into the
service
> > of
> > that
> > >agenda. I'm not so sure about Wally. I share Tom's disinclination to
> > get
> > into a
> > >street fight with Wally - generally I take the view that life's too
> > short and
> > >uncertain for such activities. On the other hand, would we let such
a
> > shoddy
> > >piece of work(and editing) go by if it were from another author?
There
> > are so

> > >many holes in Wally's argument, and such a selective choice of evidence
> > that it
> > >should beggar belief. One of the more obvious holes is that he writes
> > of the
> > >Great Basin droughts of the 10th through 14th centuries as proof of
> > warmer
> > >conditions then, but doesn't explain why we don't have such conditions
> > now.
> > >Interestingly, Larry Benson, Dave Meko and others have good evidence
> > that
> > these
> > >same multidecadal periods were marked by a great excess of
> > precipitation
> > just a
> > >few hundred miles north in northern Nevada and California and southern
> > Oregon.
> > >He just hasn't grasped that the methods that are appropriate for
> > tracking the
> > >consequences of major changes in boundary conditions don't work in the
> > late
> > >Holocene. I've been trying to figure out the issue of "Was there a
> > Medieval
> > >Warm Period, and if so where and when" for a decade or so, and still
> > have the
> > >impression that the records for the 9th through 14th centuries are
> > extremely
> > >mixed. But then, I didn't come to the investigation with a certain
> > knowledge of
> > >the absolute truth, and have had to 'misfortune' to work with people
> > who let
> > >careful analysis get in the way - Henry Diaz, Ray and Mike, and others.
> >
> > >Anyway, the point of this rant is that I think we should give careful
> > >consideration to making a measured response to Wally. Cheers,
Malcolm
> > >
> > >
> > >
> > >
> > >
> > >Quoting "Michael E. Mann" <mann@virginia.edu>:
> > >
> > >> Hi Tom,
> > >>
> > >> Thanks for your quick reply. I agree with you entirely. I think its
> > very

> > >> unfortunate he's chosen to disinform the community rather than engage
> > in
> > >> a
> > >> constructive dialogue (we tried the latter w/ him in a series of
> > emails
> > >> last
> > >> year, but clearly to no avail).
> > >>
> > >> On the other hand, think that a war of words w/ Broecker would be
> > >> exploited
> > >> by the skeptics, and perhaps we should just try to let this thing
> > die...
> > >>
> > >> I'm not sure. I'd appreciate knowing what others think?
> > >>
> > >> mike
> > >>
> > >> At 10:25 AM 2/24/01 -0600, tom crowley wrote:
> > >> >Mike,
> > >> >
> > >> >I was not aware of the Broecker piece - I am dismayed but not
> > >> surprised. I
> > >> >do not know what to do - I personally cannot stand the combative
> > >> personal
> > >> >approach Broecker relishes but it does seem as if some rebuttal
is
> > >> called
> > >> >for. Maybe you Ray Phil I and Malcolm could pen a response - we
are
> > >> >heading to Germany in a week, for a month, so I am not sure how
much
> > I
> > >> can
> > >> >keep up on this but it seems as if some response is called for.
> > >> >
> > >> >What think ye?
> > >> >
> > >> >Tom
> > >> >
> > >> >
> > >> >>Dear Mike,
> > >> >>
> > >> >>Thanks for passing this along.
> > >> >>
> > >> >>Wojick of course completely misrepresents Broecker, and puts his
> > >> >>conventional intellectually dishonest spin on this.
> > >> >>
> > >> >>That having been said, it is a bit disappointing that Wally
> > continues
> > >> to
> > >> >>cling to some of his flawed beliefs which aren't supported from
> > either
> > >> our

> > >> best current understanding of the observations or of the results
of
> > >> careful
> > >> modeling experiments. My own perception is that the climate
> > community,
> > >> modelers as well as observationalists, simply don't take
seriously
> > >> anymore
> > >> the idea that the history of climate change over the past 1000
> > years
> > >> is
> > >> part of an internal oscillation. The sediment core evidence oft
> > cited
> > >> by
> > >> Broecker (e.g. Bond et al) for this is tremendously weak, and I,
as
> > >> well as
> > >> the vast majority of my colleagues, simply don't buy it for even
a
> > >> second.
> > >> But people don't like to challenge Broecker publically. He can
and
> > >> will
> > >> play hardball.
> > >> >>
> > >> There is an odd irony. Broecker refused to accept the modeling
> > >> evidence
> > >> that the 100 kyr ice age Pleistocene variations were part of an
> > >> internal
> > >> oscillation paced by insolation variations, favoring instead the
> > >> discredited notion that they were a direct response to (too
weak)
> > >> eccentricity forcing, until the evidence became insurmountable
> > (from
> > >> my
> > >> adviser, Barry Saltzman, may he rest in piece, and people like
Dick
> > >> Peltier). Ironically, Broecker then took credit for the very
> > >> proposition he
> > >> had fought w/ tooth and nail.
> > >> >>
> > >> Broecker is even more wrong, and unfortunately equally stubborn,
in
> > >> this case.
> > >> And, again, the reason: because his pet theory, that climate
> > >> variability is
> > >> a simple millennial oscillation, is finally being challenged w/
> > hard
> > >> data
> > >> and hard facts.
> > >> >>
> > >> Broecker misrepresents the nature of that data that we and
others
> > have

> > >> used, and misunderstands the source of the muted hemispheric trends
> > >> (there
> > >> >>*is* a hemispheric "medieval warm period" and "little ice age",
> > just
> > >> not of
> > >> the magnitude or the distinctiveness that Broecker imagines).
> > >> Individual
> > >> regions in our reconstructions, and Phils, and others, vary by
> > several
> > >> degrees C, ie, the proxies we use have no problem whatsoever in
> > >> resolving
> > >> high-amplitude temperature variations in the past. The problem is
> > that
> > >> when
> > >> we look at the different regions we find that periods of cold and
> > >> warm
> > >> often occur at very different times in different regions, and so
> > in
> > a
> > >> hemispheric or global average, a lot of purely regional
> > >> variability
> > >> cancels
> > >> out. The resulting trends are somewhat smaller. I remained
> > befuddled
> > >> as to
> > >> why Wally doesn't understand this point. Its been explained to
> > him
> > >> time and
> > >> time again. Maybe he's just not listening, or doesn't want to
> > >> listen...
> > >> >>
> > >> >> In fact, Tom Crowley has clearly shown that the observed
> > >> millennial
> > >> temperature reconstruction is precisely consistent w/ our
> > >> understanding of
> > >> >>*forced* climate change over the past 1000 years (solar changes,
> > >> volcanic
> > >> output, and recent greenhouse gas concentrations). There is,
> > >> simply
> > >> put, no
> > >> room for a global millennial internal oscillation. Regionally,
> > >> such
> > >> types
> > >> of climate phenomena, associated for example with changes in the
> > >> North
> > >> Atlantic ocean circulation, are supported by the observations.
> > >> This
> > >> explains why, for example, European temperature variations are
> > >> somewhat
> > >> larger than those in other regions not effected so strongly by
> > >> such

> > >> climate
> > >> >>processes.
> > >> >>
> > >> >>Other recent perspectives, by Ray Bradley and myself provide a
far
> > >> more
> > >> >>balanced and nuanced (and less dogmatic or defensive) viewpoint.
> > I'm
> > >> not
> > >> >>sure a written response to Broecker is worthwhile (this is,
> > afterall,
> > >> a
> > >> >>"perspective" and everyone understands that a scientist may have
a
> > >> flawed
> > >> >>perspective). If Wally wants this to be his legacy, so be it...
> > >> >>
> > >> >>Phil and others have a review article coming out in the near
future
> > >> which
> > >> >>also provides a much more balanced perspective on the climate
> > changes
> > >> of
> > >> >>the past millennium, and will set the record straight once again
> > (good
> > >> >>timing Phil!). Science's embargo policy prevents me from saying
> > much
> > >> more
> > >> >>at this time, but if Phil or anyone else wishes to comment
further,
> > >> I'd
> > >> >>encourage it.
> > >> >>
> > >> >>Well, I've still got some snow to shovel here in
Charlottesville!
> > >> Happy
> > >> >>weekend to all,
> > >> >>
> > >> >>mike
> > >> >>
> > >> >>p.s. For those with electronic subscriptions, Broecker's latest
> > piece
> > >> can
> > >> >>be found here:
> > >> >>
> > >> >> PALEOCLIMATE:
> > >> >> Was the Medieval Warm Period Global?
> > >> >> Wallace S. Broecker
> > >> >> Science Feb 23 2001: 1497-1499. [Summary] [Full Text]
> > >> >>
> > >> >><http://www.sciencemag.org/cgi/content/full/291/5508/1497>
> > >> >>
> > >> >>While my previous perspective piece is here:
> > >> >> CLIMATE CHANGE:

> > >> >> Lessons for a New Millennium
> > >> >> Michael E. Mann
> > >> >> Science 2000 July 14; 289: 253-254. (in Perspectives)
[Summary]
> > >> [Full
> > >> >>Text]
> > >> >>URL:
> > >>
> >
>>http://www.sciencemag.org/cgi/content/full/289/5477/253?maxtoshow=&HIT
> S=10&h
> > >>
> >
>>its=10&RESULTFORMAT=&author1=Mann&searchid=QID_NOT_SET&stored_search=&
> FIRSTI
> > >> >>NDEX=&fdate=10/1/1995&tdate=2/28/2001
> > >> >>
> > >> >>and Bradley's is here:
> > >> >>
> > >> >> PALEOCLIMATE: Enhanced: 1000 Years of Climate Change
> > >> >> Ray Bradley
> > >> >> Science 2000 May 26; 288: 1353-1355. (in Perspectives)
[Summary]
> > >> [Full
> > >> >>Text]
> > >> >>
> > >> >>URL:
> > >>
> >
>>http://www.sciencemag.org/cgi/content/full/288/5470/1353?maxtoshow=&HI
> TS=10&
> > >>
> >
>>hits=10&RESULTFORMAT=&author1=Bradley&searchid=QID_NOT_SET&stored_sear
> ch=&FI
> > >> >>RSTINDEX=&fdate=10/1/1995&tdate=2/28/2001
> > >> >>
> > >> >>>Dear Michael--The third point below has comments on the
> > controversy
> > >> >>>betweenyou and Broecker--I'd be interested in your response
(did
> > >> Wally not
> > >> >>>just understand what your data show?).
> > >> >>>
> > >> >>>Mike
> > >> >>>
> > >> >>>Three Wojick Pieces on Climate Change.
> > >> >>>I've been busy busy.
> > >> >>>
> > >> >>>David
> > >> >>>
> > >> >>>FIRST, the latest issue of Insight Magazine includes a
> > >> point-counterpoint

> > >> between measly old me and the great Robert Watson. Boy has he got
> > >> credentials! Too bad he's wrong.
> > >>
> > >> <<http://www.insightmag.com/archive/200103143.shtml>>
> > >>
> > >> Symposium: Do scientists have compelling evidence of global
> > warming?
> > >>
> > >> Yes: Rising sea levels worldwide and retreating Arctic glaciers
> > are
> > >> ominous
> > >> signs.
> > >>
> > >> By Robert T. Watson -- chairman of the UN Intergovernmental
Panel
> > on
> > >> Climate Change, chief scientist at the World Bank and former
chief
> > >> science
> > >> advisor to the Clinton White House.
> > >>
> > >> No: Despite the overheated rhetoric, there is no new evidence
of
> > >> warming
> > >>
> > >> By David E. Wojick -- covers climate policy for Electricity
Daily
> > and
> > >> is a
> > >> science adviser to the Greening Earth Society
> > >> <<http://www.greeningearthsociety.org>>, as well as Undereditor
of
> > the
> > >> Washington Pest <<http://www.WashingtonPest.com>>
> > >>
> > >> SECOND, the February 15 Eco-logic on-line has published "The
Black
> > >> Hole of
> > >> Global Climate Government" by David Wojick, my detailed attack
on
> > the
> > >> Framework Convention on Climate Change. It includes a lot of
the
> > >> actual
> > >> treaty language.
> > >>
> > >> <<http://www.eco.freedom.org/el/20010202/wojick.shtml>>
> > >>
> > >> THIRD, here is a draft Electricity Daily article of mine. Seems
> > I'm
> > >> not the
> > >> only one who thinks the IPCC is nuts.
> > >>

> > >> Climate Guru Kicks The Hockey Stick
> > >> by David Wojick (dwojick@shentel.net)
> > >>
> > >> Global warming is natural and the recent warming is probably no
> > >> exception.
> > >> This is the controversial argument made by prominent
climatologist
> > >> Wallace
> > >> S. Broecker in today's issue of Science.
> > >>
> > >> Broecker's bombshell bears the seemingly innocent title "Was
the
> > >> Medieval
> > >> Warm Period Global?" It may seem esoteric, but whether the
> > >> apparent
> > >> warmth
> > >> reported in Europe about 1000 years ago was global or simply
local
> > >> is
> > >> becoming a central issue in climate science. What makes it
> > >> contentious is
> > >> the recent claims by the United Nations Intergovernmental Panel
on
> > >> Climate
> > >> Change that the earth is warmer now than it has been for
> > >> millennia,
> > >> and
> > >> that therefore human carbon dioxide emissions are to blame.
> > >> Broecker,
> > >> a
> > >> leading figure at Lamont-Doherty Earth Observatory, Columbia
> > >> University,
> > >> questions both IPCC claims.
> > >>
> > >> The focus of the debate is a 1000-year temperature
reconstruction
> > >> known in
> > >> climate circles as the "hockey stick". Produced in 1999 by M.
E.
> > >> Mann, R.
> > >> S. Bradley, M. K. Hughes, the long handle of the hockey stick
> > >> shows
> > >> global
> > >> temperatures for the first 8 centuries as basically unchanging,
> > >> followed by
> > >> the sharply up-tilting blade of the last 150 years or so. The
Mann
> > >> et
> > >> al
> > >> hockey stick is the central feature of the recently released
IPCC
> > >> working
> > >> group one Summary for Policy makers, which claims to embody the
> > >> best

> > >> of
> > >> >>>climate science.
> > >> >>>
> > >> >>>Broecker does not like the hockey stick, nor the conclusions
the
> > IPCC
> > >> draws
> > >> >>>from it. He says " A recent, widely cited reconstruction
(Mann's)
> > >> leaves
> > >> >>>the impression that the 20th century warming was unique during
the
> > >> last
> > >> >>>millennium. It shows no hint of the Medieval Warm Period (from
> > around
> > >> 800
> > >> >>>to 1200 A.D.) during which the Vikings colonized Greenland,
> > >> suggesting that
> > >> >>>this warm event was regional rather than global. It also
remains
> > >> unclear
> > >> >>>why just at the dawn of the Industrial Revolution and before
the
> > >> emission
> > >> >>>of substantial amounts of anthropogenic greenhouse gases,
Earth's
> > >> >>>temperature began to rise steeply. Was it a coincidence? I do
not
> > >> think so.
> > >> >>>Rather, I suspect that the post-1860 natural warming was the
most
> > >> recent in
> > >> >>>a series of similar warmings spaced at roughly 1500-year
intervals
> > >> >>>throughout the present inter-glacial, the Holocene."
> > >> >>>
> > >> >>>Broecker presents the evidence for a global Medieval Warm
Period,
> > as
> > >> well
> > >> >>>as for a Little Ice Age from around 1300 to 1860, when the
present
> > >> >>>temperature rise begins. He also argues that the "proxy"
evidence
> > >> used by
> > >> >>>Mann et al, such as tree ring data, is ill suited to the time
> > period
> > >> and
> > >> >>>temperature variation -- less than one degree C -- in question.
> > >> >>>
> > >> >>>As he puts it, "In my estimation, at least for time scales
greater
> > >> than a

> > >> century or two, only two proxies can yield temperatures that
are
> > >> accurate
> > >> >>>to 0.5 C: the reconstruction of temperatures from the elevation
of
> > >> mountain
> > >> >>>snowlines and borehole thermometry. Tree ring records are
useful
> > for
> > >> >>>measuring temperature fluctuations over short time periods but
> > cannot
> > >> pick
> > >> >>>up long-term trends because there is no way to establish the
> > >> long-term
> > >> >>>evolution in ring thickness were temperatures to have remained
> > >> constant."
> > >> >>>
> > >> >>>Broecker acknowledges that the proxy evidence is necessarily
> > somewhat
> > >> >>>"murky", but his conclusion is that "climatic conditions have
> > >> oscillated
> > >> >>>steadily over the past 100,000 years, with an average period
close
> > to
> > >> 1500
> > >> >>>years... The swing from the Medieval Warm Period to the Little
Ice
> > >> Age was
> > >> >>>the penultimate of these oscillations." The implication being
that
> > >> some, if
> > >> >>>not all, of the present warming is the natural swing out of the
> > >> Little Ice
> > >> >>>Age, and that Mann et al, as well as the IPCC, are mistaken.
> > >> >>>
> > >> >>>
> > >> >>>--
> > >> >>>
> > >> >>>
> > >> >>>Dr. David E. Wojick
> > >> >>>President
> > >> >>>Climatechangedebate.org
> > >> >>>Subscribe to the free debate listserv at
> > >> <http://www.climatechangedebate.org>
> > >> >>>Non subscribers can follow the debate at
> > >> >>><http://www.eScribe.com/science/ClimateChangeDebate/>
> > >> >>>
> > >> >>>
> > >> >>>
> > >> >>>
> > >>
> >
>>

> > >> >>

Professor Michael E. Mann

> > >> >> Department of Environmental Sciences, Clark Hall
> > >> >> University of Virginia
> > >> >> Charlottesville, VA 22903

> > >>
> >

>>

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> > >> >Thomas J. Crowley
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UK

</x-flowed>

From: PARRYML@aol.com
To: tgcia@meto.gov.uk
Subject: Proposed TGCIA meeting: 30th May to 1st June, Amsterdam
Date: Tue, 27 Feb 2001 08:32:21 -0500 (EST)

Dear TGCIAers:

A proposed date/place for the next TGCIA meeting is: 9.00 on Wednesday 30th May to 14.00 on Friday 1st June at Shell International Bldg, Amsterdam. Rob Swart and colleagues at WGIII TSU have kindly agreed to be local hosts. I suggest this date after consulting with 9 TGCIA members present at WGII plenary at Geneva last week. The window is narrow between IPCC and SUBSTA meetings (the latter is now almost certainly delayed until mid June). Please put this date in your diary, but also let me know of any major conflict with IPCC/UNFCCC-type schedules.

Unless I hear to the contrary(*let us say by Monday 5th March*), the proposal is that this date stands .

This meeting is particularly important because top of the agenda from our last meeting is consideration of developing a 'one-stop-shop' for data and guidance for scenario-based climate impacts assessment, which would lay the foundations for compatible research for the next IPCC assessment (whatever form it may take). We might well also consider what recommendations to make concerning the form of the next assessment (a subject probably on the agenda of the IPCC London Plenary in September).

More follows next week, assuming these dates hold, about agenda and arrangements.

Kind regards,
Martin parry

Prof. Martin L. Parry
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Web: <http://www.uea.ac.uk/env/jei>

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@multiproxy.evsc.virginia.edu
Subject: Fwd: RE: Science issue Feb 22/23
Date: Tue, 27 Feb 2001 10:14:09 +0000
Cc: mhughes@ltrr.arizona.edu,rbradley@geo.umass.edu,tom@ocean.tamu.edu, k.briffa@uea.ac.uk,t.osborn@uea.ac.uk

<x-flowed>

Mike et al,
Sorry about the multiple sendings. I've forgotten my glasses and couldn't see I'd missed a comma.
Another thing to point to is the special issue of Climatic Change by Astrid Ogilbie and Trausti Jonsson. They point to the LIA not being very appropriate in Iceland.

Cheers
Phil

Mike,
So Julia handled it. Even she thought it was handwaving, but it passed the usual Science review process. Obviously this isn't great as none of us got to review it. Odd that she didn't send it to one of us here as she knew we were writing the article she asked us to ! Anyway that is water under the bridge.
As for authorship we have this article coming out so this rules us out. Tom isn't keen and he's away. Wally told me he didn't reckon Tom, so Tom has got the right vibes. Julia is asking us to go ahead and hinting at a joint response.
One possibility is either you or Macolm taking the lead. Malcom and Henry wrote the MWP piece in Climate Change in 94. Keith and I think something pointed about the MWP is the way to go. Could add in that even the two warming periods in the 20th century don't show warming everywhere - especially the early 20th century.
Remember that we are all basically averaging long series together and if one site shows a big warming/cooling then the average will to a lesser extent.
Also bring in

a few of the papers where people have compared tree based reconstructions with glacial advances/retreats (eg Raper et al in J. Glaciology and Luckman et al in the Holocene. Also there are more in that Interhemispheric Linkages Book of Vera and work by Ricardo Villalba and others).

Basically need to point to a load of literature that we would expect someone writing an article of this type to be aware of. Also the North Atlantic isn't the last word in NH and global averages. Clearly said in Hughes and Diaz and papers therein.

Also the latest IPCC report will use and reference the latest curves, but from 1400 they are not that different from Bradley and Jones (1993), so why the fuss now.

Clearly the MWP is the issue that has got a few worked up, but we have concluded nothing that couldn't have been gleaned in 1994. Maybe we're stating it more clearly now, but the recent warmth of the 1990s is a factor as well.

Cheers
Phil

>From: "Julia Uppenbrink" <Juppenbrink@science-int.co.uk>
>To: "Phil Jones" <p.jones@uea.ac.uk>
>Subject: RE: Science issue Feb 22/23
>Date: Mon, 26 Feb 2001 17:05:45 -0000
>X-Mailer: Microsoft Outlook IMO, Build 9.0.2416 (9.0.2910.0)
>Importance: Normal

>
>Dear Phil
>
>Thanks for your message regarding Wally Broecker's Perspective. I am of course aware of this Perspective coming out - I did handle it - I realized that it was perhaps a bit handwaving in parts but I thought the message was interesting and the article passed the usual screening. But we are always open to criticism! So please do send a letter to us; you can send it directly to me, and you may cowrite it with Tom Crowley and Mike Mann or you can send separate letters (if the concerns overlap a lot then one letter is perhaps better than several). The letter will be handled through our letters department, and we will get a response from Wally plus possibly outside review before we make a decision to publish.

>
>I look forward to receiving your letter.
>
>Best wishes
>

> Julia

>

>-----Original Message-----

>From: Phil Jones [mailto:p.jones@uea.ac.uk]

>Sent: 26 February 2001 14:40

>To: Julia Uppenbrink

>Subject: Science issue Feb 22/23

>

>

>

>>

>> Dear Julia,

> I don't know if you have seen the Perspectives piece in last

>week's issue of

> Science by Wally Broecker. I guess it was nothing to do with you and it

>contains

> several inaccuracies and sweeping statements. I accept it is a personal

>view

> and I've not seen the issue yet , only a copy that I was ironically given

>by Wally

> Broecker as we were both guest speakers at a meeting at Bowdoin College,

>ME

> on Saturday. I got back this morning to Norwich.

> I talked to Wally about it over the weekend and will send him a few

>reprints

> pointing out a few of the things he should have read. Some things he

>states are just

> wrong.

> I don't want to change the article already accepted, but what are

>the possibilities

> of writing a response to Wally's piece in a later issue. I've been

>contacted by a couple

> of people in the US about Broecker's piece (Mike Mann and Tom Crowley),

>who are

> quite unhappy about it and would like to respond. They both know about

>the invited

> piece and wanted me to comment, hence my email to you. The invited piece

>does

> address some of the issues, but not the link between high and low

>frequency

> proxy series.

>

> Best Regards

> Phil

>

>

>

>

>

>

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From: "Thomas L. Delworth" <td@gfdl.noaa.gov>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
Subject: Re: letter to Science
Date: Thu, 01 Mar 2001 08:19:45 -0500
Cc: tom@ocean.tamu.edu, hpollack@geo.lsa.umich.edu,
mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, p.jones@uea.ac.uk,
k.briffa@uea.ac.uk

Dear Mike et al,

I offer the following comments on your letter for your consideration.

It seems to me there are 2 primary issues to address:

- (A) what does proxy evidence say about whether the Medieval Warm period was global
- (B) what do we know about potential mechanisms for the Medieval Warm period
 - (i) evidence for a forced phenomenon
 - (ii) evidence for internal variability

Issue (A) is currently dealt with in your sections (1) and (2). One point that could be perhaps conveyed more clearly is the necessity of using the spatial information conveyed in (multi) proxy reconstructions, rather than overly interpreting sets of local proxy evidence. I felt this point could have been stressed more, and is one which the casual reader may not appreciate.

Issue (B, Bi) is in your section (3). I suggest a more explicit mention of conclusions with regard to the Medieval Warm period in recent work on this topic. The first statement in this section doesn't provide (I don't think) explicit evidence to back itself up. The sentence starting "These results ..." could be more explicit about what those studies show with respect to the Medieval Warm period, in addition to the more general statement about the partitioning between forced and internal variability. A reader could ask "Ok, if 50% of the variability is explained by volcanic and solar forcing, that doesn't exclude the other 50% playing a strong role for events such as the Medieval Warming." Such a question could be dealt with in advance by stating what role these studies suggest for radiative forcing in the Medieval Warm period.

For issue (Bii), I would suggest being explicit that it is incumbent upon authors to provide some evidence to support their speculation. What evidence can the author provide to support his speculation concerning the role of the THC in the Medieval Warm period? Rather than explicitly stating this is not a likely mechanism, I would contrast the speculation he has offered on this topic to the stronger (in my opinion) evidence provided by modeling studies to

support the idea of the importance of radiative forcing.

... a few more minor comments

(1) I agree with the overall message you are conveying, but might choose somewhat differing wording in a place or two. The statement is made "(1) It cannot reasonably be argued that the Middle Ages were as warm as the 20th century at global or hemispheric scales." This might be a bit strong ... I would think one can have a reasoned discussion on this topic. Perhaps something like "We strongly disagree with the assertion that the Middle Ages were as warm as the 20th century at global or hemispheric scales."

(2) In the second to last sentence, I would add the qualifying phrase "on planetary scales" after the text "... responsible for centennial-millennial changes ...".

Regards,
Tom Delworth

ps The central issue is one that I have not been heavily involved in, and thus don't think it's appropriate for me to sign on as an author. Good luck, and please send me a copy of your final submission.

pps I previously provided to Tom correlations between the THC and global/hemispheric temperature based on a 900 year run of our R30 coupled model. These correlations were relatively low (0.27), but probably significant. The applicability of those correlations to the issue of the Medieval Warming may not be strong. If the Medieval Warming is a multi-century event, then I should really be looking at the correlations of low frequency (>50 years) filtered model output from a run of several millenia duration. Thus, the 900 year run may not be applicable. I will revisit this topic using a multi-millennial R15 coupled run, but probably won't have any results today. I don't think that would change the essential conclusions, however. I recall that experiments with the R15 model in which the THC was substantially weakened through the addition of fresh water to the North Atlantic provided strong regional temperature anomalies, but their global expression was small. These experiments are being repeated with the higher resolution model.

In light of these issues, I suggest that the focus be not so much on saying the THC cannot be responsible for the Medieval Warming, but rather on saying (1) there is strong evidence for a substantial role of radiative forcing, and (2) the burden is on the author to provide evidence for the role of the THC.

?

"Michael E. Mann" wrote:

> Dear Colleagues,
>
> Below is a draft of a short letter to Science that Tom Crowley and I
> have put together, after discussing w/ Phil, Ray, and Malcolm. We
> feel that a reply to Broecker's recent "Perspectives" piece is
> warranted to correct several misconceptions that Wally unfortunately
> chose to perpetuate (attached as an html file FYI). We have been given
> encouragement to submit this by Julia Uppenbrink at Science.
>
> We are working under a very tight timeline owing to Tom's travel
> schedule (leaves on an extended travel on friday) so we would greatly
> appreciate it if you could respond ASAP w/ comments, suggestions, etc.
> Please note that we are currently near the length limitations (and
> probably shouldn't include more than 15 references) so we're looking
> to sharpen and hone, but not lengthen the piece at this point.
>
> Thanks in advance for your feedback,
>
> mike
>
> _____
>
> Medieval Warming Redux
> In a recent "Perspectives" opinion piece, W. Broecker suggests that
> the
> "hockey stick" reconstruction of climate change over the past 1000
> years -
> with extreme warming only in the late 20th century - is incorrect, and
> that
> the so-called "Medieval Warm Period" was at least as warm as the 20th
> century and due to oscillations in the thermohaline circulation. To
> reach
> this conclusion, Dr. Broecker rejects traditional empirical "proxy"
> climate
> indicators of past climate (e.g. tree ring, ice core, coral, and long
> historical documentary records) that are the foundation of a number of
>
> hemispheric reconstructions, as well as our current best physical
> understanding of the factors controlling climate at
> century-to-millennial
> timescales. We disagree with Broecker on several major points:
> (1) It cannot reasonably be argued that the Middle Ages were as warm
> as the
> 20th century at global or hemispheric scales. Although regional warmth
>
> during the Middle Ages may have sometimes been significantly greater
> than
> present, four different hemispheric-scale reconstructions (Jones,
> Mann,

> Briffa, Crowley) have been completed for the last 1000 years -- all of
> them
> showing warmth in the Middle Ages that is either no warmer or
> significantly
> less than mid-20th century warmth. This is because it has been known
> for a
> quarter of a century that the timing of warmth during the Middle Ages
> was
> significantly different in different regions (Lamb, Dansgaard,
> Hughes).
> Failure to take this observation into account can lead to serious
> errors in
> the inference of hemispheric temperature trends. Although one analysis
> of
> heat flow measurements suggests warmer temperatures than the surface
> proxies during the Middle Ages (Huang and Pollack, GRL. 1997), the
> considerable sensitivity of the resulting trends to a priori
> statistical
> assumptions has lead borehole researchers to restrict their attention
> to
> the more reliably interpretable temperature fluctuations during the
> past
> five centuries (Huang and Pollack, Nature). Our conclusion is also
> supported by measurements from tropical glaciers indicating an
> unprecedented level of recent warming with respect to the last
> 1,000-2,000
> years (Thompson).
> (2) High-resolution proxy climate records which form the foundation of
>
> recent hemispheric temperature reconstructions are far more reliable
> indicators of century-to-millennial scale climate variability than is
> implied by Broecker. The potential limitations in interpreting
> long-term
> climate change from proxy indicators such as tree rings, have been
> long
> recognized by dendroclimatologists (e.g., Cook "segment curse" paper)
> and
> are almost always taken into account in framing interpretations of
> long-term trends. For example, Mann et al (1999) verified that a
> significant subset of multiple-millennial length tree ring and ice
> core
> proxy climate indicators used to reconstruct the trend over the past
> millennium passed rigorous statistical tests for fidelity at the
> millennial
> timescale, and that the basic attributes of the hemispheric
> reconstruction
> using more recent non-tree ring proxies available over the past few
> centuries yielded essentially the same result as that based on both
> tree
> ring and non-tree ring based information (Mann et al, Earth
> Interactions,
> 2000). Several independent reconstructions (Jones et al and Crowley
> and
> Lowery), using a wide variety of proxy climate indicators and

> different
> statistical approaches, yield similar hemispheric temperature trends.
> Even
> the centennial-scale changes within the so-called "Little Ice Age" of
> the
> 15th-19th centuries are largely in agreement. Furthermore these
> centennial
> changes have been shown to be in "agreement" , rather than "in
> opposition"
> (as argued by Broecker) with evidence from alpine glacial advances
> (Raper
> reference).
> (3) Physical considerations show that external forcing, not internal
> variability, played the dominant role in the transition from the
> "Medieval
> Warm Period" to "Little Ice Age" (these terms are used loosely and
> are, in
> fact, ill advised in the context of hemispheric or global temperature
> changes -see e.g. Bradley and Jones, 1993; Hughes and Diaz, 1994). One
> of
> the major points of Broecker's argument is that changes in the
> thermohaline circulation are a primary driver of climate change on
> this
> time scale. These results do not consider recent modeling studies
> (Free,
> Crowley) that demonstrate at a high significance level (>99%) that
> about
> 50% of the pre-anthropogenic (pre-1850) variance can be explained by
> changes in volcanism and low frequency solar irradiance. Although the
> latter term is still not well constrained from observational studies,
> there
> are a number of independent lines of evidence suggesting such changes
> (Hoyt, Lean, Lockwood).
> (4) It is not justifiable to argue that changes in the thermohaline
> circulation cause significant hemispheric or global changes in
> temperature.
> Although changes in the conveyor play a major role in the Atlantic
> Basin,
> to a first approximation changes in ocean circulation simply
> redistribute
> heat on the planet without significantly raising global temperature,
> or
> even hemispheric temperature. This conclusion is born out by very low
> correlations between warmth in the Greenland sector and the
> hemispheric
> indices over the last 1000 years (Crowley footnote ref.), a low
> correlation
> that is shared by coupled model experiments (Delworth citation)? In
> fact,
> sediment core data from the subtropical North Atlantic often cited as
> indicative of a distinct "Medieval Warm Period" and "Little Ice Age"
> (Keigwin Sargasso Sea), has recently been shown to be more consistent
> with
> changes in the North Atlantic Oscillation (Keigwin and Pickart),

> implying a
> zero sum pattern of regionally alternating warm and cold superimposed
> on
> far more modest hemispheric variations over the past 1000 years. This
> pattern itself may be forced, rather than internal in nature, and
> would
> explain the limited evidence for more dramatic cold and warm periods
> in
> regions such as Europe (see Mann, Sci Perspective, 2000).
> The above arguments lead us to conclude that, although the conveyor
> may be
> changing, radiative forcing perturbations were primarily responsible
> for
> centennial-millennial changes in the last 1000 years, with attendant
> implications for interpretation of earlier Holocene oscillations (e.g,
>
> Denton and Karlen). Furthermore, the weight of evidence indicates that
> the
> late 20th century hemispheric warming is significantly greater than
> the
> Middle Ages.

>
> Michael E. Mann
> Thomas J. Crowley
> WHO ELSE???

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Thomas L. Delworth

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

To: tom crowley <tom@ocean.tamu.edu>

Subject: Re: Science letter

Date: Fri, 02 Mar 2001 12:00:03 -0500

Cc: "Raymond S. Bradley" <rbradley@geo.umass.edu>, mhughes@ltrr.arizona.edu, k.briffa@uea.ac.uk, tom@ocean.tamu.edu, p.jones@uea.ac.uk, td@gfdl.noaa.gov, hpollack@geo.lsa.umich.edu

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Thanks for clarifying Tom,

Yes, these are my sentiments as well, and I would conditionally sign-on to this effort. In the meantime, I think there is a lot of good science to be done!

mike

At 10:53 AM 3/2/01 -0600, tom crowley wrote:

>Dear All,

>

>A few more comments re Mikes note - Mike and I thought that if we cannot
>make a case to our colleagues, why muddy the waters further (as either
>Keith, Malcolm, or Ray said)?

>

>That said, I don't think this has been wasted time. I still think a
>thoughtful short paper on the subject of Holocene climate change would be
>useful, this time stating it from OUR perspective (i.e., not focusing
>exclusively on Broeckers message). By broadening this it may be more
>interesting; we could also include a couple of figures and maybe add some
>input from Tom Delworth and Henry Pollack. I would be willing to take a
>crack at this, and if anyone wants to CONDITIONALLY sign on, I would be
>more than happy to include you.

>

>I probably would not begin this until late April, after our trip to Germany
>and the meeting in Virginia.

>

>Tom

>

>ps fyi I counted the average spacing between the warm and cold
>oscillations in the iron oscillations illustrated by Broecker. Regardless
>of whether warm or cold are used, the mean spacing is indeed 1.5 k,
>although the s.d. is 0.4k HOWEVER, the mean spacing between the four main
>warm phases illustrated by Broecker on the same figure is, believe it or
>not, 2.15! much closer to the solar peak. This calls to mind the
>interesting (and clever) Wigley and Raper paper in Proc. Roy. Soc. (1990)
>indicating that, given the uncertainties in chronology, solar forcing plays
>a role in Holocenn climate change. It therefore seems that the conveyor
>is indeed oscillating but the time scale of the larger scale CLIMATE shifts
>may be more regulated by solar, with volcanism adding some stochastic
>contribution. Something like this is worth adding to the proposed Eos
>piece.

>

>Tom

>

>

>

>Thomas J. Crowley

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</x-flowed>

From: Chick Keller <ckeller@igpp.ucsd.edu>
To: "Michael E Mann" <mann@virginia.edu>, <rbradley@geo.umass.edu>, "Phil Jones" <p.jones@uea.ac.uk>, <k.briffa@uea.ac.uk>, tom crowley <tom@ocean.tamu.edu>, "Jonathan Overpeck" <jto@u.arizona.edu>, Tom Wigley <wigley@ucar.edu>, Mike MacCracken <mmaccrac@usgcrp.gov>
Subject: Some thoughts on climate change proxy temperatures in the last 1,000 yrs
Date: Fri, 2 Mar 2001 15:54:57 -0800

<x-rich>Folks,

Two points here:

1. I read with some consternation Wally Broecker's latest piece in Science (23Feb. 2001). First you can all take up some other topic since Wally says only Boreholes and treeline changes are accurate enough to do low frequency trends. What does he mean by "only two proxies can yield temperatures that are accurate to 0.5°C"? and do you agree that tree rings and sediments, etc are not sufficiently accurate to exhibit correct low frequency trends?

2. Here are some references to recent Holocene time-frame records you probably have seen, but just in case... I found them interesting without knowing how good or representative they are.

Surprisingly they were given me by one who cited them as examples of evidence for a MWP and LIA. I read them differently but they caused me to consider one question I hadn't heard discussed (see below).

Based just on these four, one comes to the following tentative conclusions and observations:

Conclusions:

*MWP was a generally warm time interspersed with coolings and not well synchronized hemispherically or globally.

*LIA was global and capable of better (but not completely) synchronized large amplitude variations

*20th Cent. was the only time when all records agree (tree ring problems with CO2?)

MIGHT THIS RELATIVE UNIFORMITY BE USED AS A CHARACTERISTIC OF 20TH CENTU WARMING THAT SETS IT APART FROM PREVIOUS CLIMATE CHANGES?

*Borehole inversion is too smoothed to be of much use but it does indicate a larger temp amplitude if it weren't smoothed. ~1.5°C

And this brings up my question. How one averages these records.

One way would be to note that the temperature amplitude (1000 - 1950) for each is ~1.5°C. Thus you could conclude that hemispheric/global climate varied by over a degree Celcius (although with regional differences)

Another way would be to average the records. The resulting temperature amplitude would be smaller because extremes would cancel since variability is large and each region's extremes occur at different times.

Thus, if people simply looked at several records they would get the impression that temperature variations were large, ~1.5°C. Imagine their surprise when they see that the ensemble averages you publish have much smaller amplitude.

Comparison of amplitudes is given below (although difficult to do since amplitude depends on averaging so these are very approximate).

Approximate Temperature Amplitudes for period 1000-1950

Mann et al 1999	~0.5
Jones et al 1998	~0.8
Crowley and Lowery	???
Briffa 2000	???
Dahl, Jensen	~1.5
Huang, et al	~0.8 (500 yrs only)
Overpeck et al	~1.3 (400 yrs, polar only)
Bradley & Jones (93)	~0.7 (600 yrs only)

(Not surprising that the contrarians take great exception to Michael's small amplitude.)

This is important in the current debate even, it would appear, with people like Wally. I have been looking for what the real issue is between researchers like yourselves and skeptical scientists. Politics and agendas aside, I think it is close to this.

Anyone looking at the records gets the impression that the temperature amplitude for many individual records/sites over the past 1000 years or so is often larger than 1°C. They thus recognize that natural variability is unlikely to generate such large changes unless the sun is having more effect than direct forcing, or there is some fortuitous but detectable combination of forcings. And they see this as evidence that the 0.8°C or so temperature rise in the 20th century is not all that special.

The community, however, in making ensemble averages gets a much smaller amplitude ~0.5°C. which they say shows that reasonable combinations of solar direct plus volcanos and internal variability with the help of THC can indeed explain this AND the 20th century warming is unique.

Thus, the impass--one side pointing to large temperature variations in many records around the globe and the other saying "yes, but not synchronous and so averaged hemispherically no big deal.

But, just replying that lack of synchronous events (sometimes by a few decades) is the reason might not be enough. It seems to me that we must go one step further. We must address the question: what forcings can generate large amplitude temperature variations over hundreds of years, regional though they may be (and, could these occur at different times in different regions due to shifting heat inertia patterns)? If we can't do this, then there might be something wrong with our rationale that the average is low amplitude even though many regions see high amplitude. This may be the nubbin of the disagreement, and until we answer it, many careful scientists will decide the issue is still unsettled and that indeed climate in the past may well have varied as much or more than in the last hundred years.

(Also, I note that most proxy temperature records claim timing errors of +-50 years or so. What is the possibility that records are cancelling each other out on variations in the hundred year frame due simply to timing errors? as in hitting or missing C&L's triple warming peak 1000-1200 AD)

Regards,

Referendes to proxy temp records

<excerpt>(1) Bodri, L. and V.Cermak Climate change of the last millennium inferred from borehole temperatures: Regional patterns of climatic changes in the Czech Republic - Part III, Global and Planetary Change, 21, 225-235. 1999

As with other borehole data the record is incredibly smoothed. It has essentially three warming features.

from 1000 to after 1500 there is a broad warming pulse;

1550-1750 cooling

1750-1850 warming

1850-1900 cooling

1900-1950? rapid warming <underline>Total amplitude ~1°C (1.5°C if not smoothed?)

</underline>

I don't know what to make of the more than 500 year warming pulse. Most records show warming either in the 1100's or 1200's but usually not both.

The rest of the record looks reasonable given the smoothing.

</excerpt>

<excerpt>

</excerpt>(2) Filippi, M.L., Lambert, P., et al, Climatic and anthropogenic influence on the stable isotope record from bulk carbonates and ostracodes in Lake Neuchatel, Switzerland during the last two millenia, Jour. of Paleolimnology, 21, 19-34, 1999

<excerpt> Graph actually begins at 805 AD (all dates are advertised as +-50 yrs)

Starts out warm but already cooling which it does till about 1150.

warms till 1242, second peak 1298 then cools to minimum at 1500

warms significantly to 1600 then cools to about half of 1500 max and essentially stays that way till 1850 when cools to 1500 level again and immediately rebounds

into 1950s and still warming. <u>Total amplitude
~2.5°C</u>

</excerpt>(3) Naurzbaev, M.M. and E.A.Vaganov, Variation of early summer and annual temperature in east Taymir and Putoran (Siberia) over the last two millennia inferred from tree rings, JGR 105, 7317-7326, 2000

Interesting record.

<excerpt>moderately cool 800-950,

rapid warming to max 1000 dip ~1050, recovers till ~1180

cools fast to minimum ~1250,

</excerpt> warms to max ~1400

cools to 1450 slight cooling till 1700

<excerpt>warms to ~1780

rapid cooling to ~1830

rapid warming till ~1930 <u>Total Amplitude
~1.5°C</u>

</excerpt>(4) Wilson, A.T., Hendy, C.H. and Reynolds, C.P., Short-term climate change

and New Zealand temperatures during the last millennium, Nature 1979,

<excerpt> 315-317,

Used stalagmites ($\delta^{18}O$ proxy)

</excerpt> This is a strange record, but the authors compare it favorably with the

<excerpt> central England record.

1100 starts and warms in two pulses one at 1250, min at 1300, big max at 1400, followed by dive to minimum 1450

rises to max 1500

drops to min 1600

rises a bit 1700 and into 1850

drops to minor min1880 rises after that <u>Total Amplitude
~1.5°C</u>

STRANGE RECORD

</excerpt>

Charles. "Chick" F. Keller,

IGPP.SIO.UCSD - Attn: Chick Keller

9500 Gilman Drive

La Jolla, CA 92093-0225

(858) 822-1510 office

(858) 456-9002 home

Is the noticeable increase in surfers off Scripps Beach a possible
indication of global warming?

</x-rich>

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: verification results
Date: Wed, 14 Mar 2001 14:34:11 -0500
Cc: srutherford@virginia.edu, mann@virginia.edu

Hi Tim,

That all sounds great, and indeed, the 19th century will be a *hot* topic (pun intended) as we try to rectify Tom's model response w/ the instrumental record and proxy reconstructions. Ironically, the 19th century is one in most dispute over the past millennium, it seems!

You accurately summarize what my understanding is of the breakdown of lead roles. I don't see any reason for changing that. I think Scott and I will have our hands full w/ the other items, so if you can take the lead role on the MXD paper (comparing the two methods, etc.) that would be great.

My intention is to give you and Scott full credit for anything I show at meetings that is a result of mutual collaboration. Of course, both of you are co-authors of my EGS talk.

So all sounds great! Scott: when Tim sends revised plots, can you prepare some revised ppt files and let me know when they are available to download? Hope to get all this straightened away next week after I return from the frozen north (Michigan)...

mike

That soundsAt 07:05 PM 3/14/01 +0000, Tim Osborn wrote:

>>Thanks alot, these look good. I think we're really making some good
>>progress now.

>>

>>Just to confirm, my understanding is that you're next working on a similar
>>plot showing the
>>comparison of the REG-EM results w/ the straight gridbox age-banded
>>estimates you and Keith have produced over the longer period (ie, back to
>>1600 or so?). It would be great to be able to show those at EGS.

>

>Mike, you're welcome to show these results at EGS. I had to leave early
>today (Wednesday) as my wife was ill, but I'll be back at work tomorrow.
>What I'll do first is just to modify the figures I've already sent to you,

>comparing the verification REG-EM run with instrumental data over the
>1856-1900 period. What I want to do is to modify the final map so that the
>grid boxes that actually have tree-ring sites in them are highlighted in
>some way. Then we can visualise more clearly whether the 'local'
>information is much better than the 'non-local' information. I was in a bit
>of a hurry with my e-mail earlier, I didn't mention that the map is based on
>all those grid boxes with at least 20 years of instrumental data during the
>1856-1900 period. I found the year-by-year pattern correlations quite
>informative too, and was particularly impressed by the fact that there were
>no really poor years! (at least that's my recollection, not having the plots
>in front of me at home).

>
>Having modified the map as described, I'll repeat the analysis but comparing
>the 1404-1855 period of the full reconstruction from REG-EM with our
>existing year-by-year maps and quasi-hemispheric averages. I shall compare
>them against our "traditionally-standardised" version, since it would be
>unfair to compare them with the age-banded version. The year-by-year maps
>we have already got are calibrated on a grid-box by grid-box basis
>(individually) using simple linear regression between the density series and
>the instrumental temperature. This gives us coverage for those grid boxes
>with density data in them. We throw away those that do not correlate
>significantly with their local grid box temperature. That leaves around 100
>boxes, with fewer further back in time. We then try to reconstruct all
>remaining northern hemisphere grid boxes, using principal component
>regression (PCs of the calibrated density used as predictors on a grid box
>by grid box basis), but only actually retain those that have significant
>correlations during an independent verification period. So we gain quite a
>few more grid boxes, again time-varying. So we have this (perhaps rather
>odd!) combination of local regression plus principal component regression
>producing our maps. I shall use this set of year-by-year maps for the
>comparison with REG-EM, though as with the instrumental temperatures, I'll
>sometimes highlight or subsample just those with trees in (i.e. those
>locally-calibrated).

>
>Our original plan for carving up the analysis/papers was for me to take the
>lead on the comparison of methods with the same data set, Scott on the
>comparison of data sets with the same method, and Mike to concentrate on the
>19th century stuff including verification against the instrumental data etc.
> I saw Tom Crowley last week and he showed some results indicating how
>critical the 19th century is for getting a good match between his forced
>model results and the various proxy reconstructions - so the 19th century
>could certainly be a hot topic. Phil Jones would be useful here as he may
>know of more early instrumental data from Europe that might help (depending

>upon homogeneity!). Anyway, I'm refreshing our minds about the 3-way split
>of work because: (i) this might be an appropriate point to confirm that such
>a split is still the best way to go (I'm still happy with it); and (ii) to
>point out that the REG-EM comparisons with our existing density-based maps
>falls into the bit that I'm to take the lead on - so while I'm completely
>happy for you to show these at EGS or other meetings, I'd still like to
>write the comparisons up for a journal paper.

>
>>p.s. Tim: are you going to be at EGS? I know Phil will...Also, I'm hoping
>>that one of the 3 of you can make it to the Charlottesville workshop in
>>April. You and Phil have both indicated you can't go, I think? At present,
>>Keith hasn't yet confirmed. It would be a shame not to have him, you, or
>>Phil present. Can you suggest some sort of "alternate" (Schweingruber?) the
>>Europeans might invite if Keith can't make it. Thanks...

>
>I can't make it to EGS, as I have work to prepare for my 3 talks I'm giving
>at NCAR in the first week in April! For the Charlottesville workshop, I
>spoke to Keith yesterday and I think he has now booked his flights - so I'd
>take that as confirmation. He's in touch with Julie Jones at GKSS about it.
> I put in a good word about how pleasant Charlottesville was!

>
>Best regards to you both,

>
>Tim

>
>
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From: Tim Osborn <t.osborn@uea.ac.uk>
To: mann@virginia.edu, srutherford@virginia.edu
Subject: Re: verification results
Date: Thu Mar 15 16:38:31 2001

Mike & Scott,

I've redone the verification against instrumental temperatures for 1856-1899. Previously I'd used 1856-1900, but I've now realised that 1900 is not part of the verification period (the pattern correlation = 1 gave it away!). So I've now stopped in 1899. It makes virtually no difference to the quasi-hemispheric series and their correlations. What it does affect is the grid-box by grid-box temporal correlations, since I was previously using one perfect value at the end of each series. So the correlations are mostly a bit lower now, though still fairly good I think. There's a reasonable area with $r > 0.3$. Signal to noise should increase fairly dramatically if some kind of regional averaging were done. I've outlined the boxes that actually have chronologies in them. There's not enough instrumental data to verify the more northern ones, but the European and USA ones do well (r in range 0.5 to 0.9). The more distant oceanic regions are a bit poorer, except the northern Indian Ocean. So that's it for the verification, for the moment.

I've compared the 1404-1855 (i.e., pre-instrumental) reconstruction with the Briffa et al. and Osborn et al. reconstructions. Correlations are all quite high (0.7 to 0.85) for the quasi-hemispheric series, while the pattern correlations average around 0.6. The box-by-box temporal correlations show many boxes with r in the range 0.6 to 1.0, indicating little sensitivity to the method used. One notable feature of the latter results is that there's less agreement in the boxes that actually have trees than those don't! There's two different interpretations of this that I'm working on, which seem equally possible. More later. I was going to send the time series and maps from this comparison, but I've just realised that I'm using anomalies from two different baselines (1961-90 for ours, 1901-60 for REG-EM) so the % variance explained and the time series aren't right - that'll have to wait till Friday now.

Tim

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: comparison with our existing reconstructions
Date: Fri, 16 Mar 2001 14:25:57 -0500 (EST)
Cc: Scott Rutherford <srutherford@virginia.edu>, Mike Mann <mann@virginia.edu>

Dear Tim, Scott

On the road w/ tenuous email connection so have to be brief. This sounds good. Hoping we can have age-banded connections by the end of tnext week so I can show in Nice! Scott: can you rectify the comparisons that Scott is producing w/ your own comparisons that show more of a discrepancy ?

Thanks,

mike

Fri, 16 Mar 2001, Tim Osborn wrote:

> Dear Mike & Scott,
>
> Attached is "traditional.ps", comparing the 1404-1855 (i.e.
> pre-instrumental) REG-EM reconstruction with our existing Osborn et al.
> maps and Briffa et al. quasi-hemispheric series (see refs below). Neither
> the REG-EM nor the existing reconstructions use the age-banded trees, so
> low frequencies are suppressed. [Scott - thanks for the new age-banded
> results, but I probably won't get to them till next week due to other
> commitments.]
>
> The time series comparisons are, as you see, quite good - thought you'd
> expect this as we're comparing two methods but identical data! Red is
> REG-EM, black is from the Osborn et al. existing reconstructions (then
> averaged into quasi-hemispheric means), while blue is from Briffa et al.
> (where we average the tree-density into regions/hemisphere *before*
> calibrating against regional/hemispheric temperature). Blue & black agree
> quite closely, so all correlations and % var explained are between red and
> black.
>
> Timeseries are:
>
> '0-90' = full spatial average over each of our existing maps.

- > '0-70' = full spatial average over each of the REG-EM maps.
- > 'masked' indicates REG-EM maps are masked by the time-dependent coverage of
- > our existing maps.
- > 'land20-90' or 'land20-70' indicates only land grid boxes north of 20N are
- > averaged.
- > 'treeboxes' indicates only those grid boxes that contain tree-ring sites
- > are averaged together.
- >
- > The pattern correlations range from 0.2 to 0.8, with a mean of 0.6
- > (approx). Fairly consistent then. The pattern of temporal correlations is
- > reasonable, ranging from 0.0 to 0.9, with a mean of 0.6 (approx).
- >
- > Comments:
- > (1) Time series generally have less variance in REG-EM, especially early
- > on, though masking of data brings them closer to our time series.
- > (2) Getting the mean level correct (I've converted REG-EM to behave like
- > anomalies from 1961-90 mean) helps with the % variance explained considerably.
- > (3) The temporal correlations are poorer for boxes containing trees than
- > those that do not!
- >
- > The decreased variance early in the REG-EM [comment (1)] is, I guess,
- > because the fewer the records with data, the earlier the
- > truncation/weighting function kicks in etc. and therefore the less the
- > variance that is reconstructed. As the 'skill' of REG-EM decreases, the
- > more the values are filled in with something near to their mean, I seem to
- > recall. This raises the question that the early values might be biased
- > towards the observational mean? If so, it might be better to replace box
- > values by missing values when their expected 'skill' becomes fairly low.
- >
- > Comment (3) can be explained two ways. In the non-tree boxes our two
- > methods (REG-EM and principal component regression) have similarities, and
- > given the common input data, one would expect similar reconstructions -
- > which the high correlations indicate. In the tree boxes, however, the
- > difference is our approach uses only local information, while REG-EM still
- > uses non-local information too. So, either (i) our reconstructions are
- > poorer *because* we're ignoring non-local information, or (ii) REG-EM
- > reconstructions are poorer *because* real local variations are partly
- > masked by regional-scale variations. It might be possible to choose either
- > (i) or (ii) as a preferred explanation, using verification or other
- > consideration, but I'd prefer to stick with (i) and (ii) as being equally
- > possible and therefore justifying both approaches. This is politically
- > better too! What I get out of the comparison is that the REG-EM is
- > producing variability that is highly correlated with our method, given the

> same input data. The main concern is the difference in variance and hence
> absolute anomalies. We should look at this again when I've compared the
> age-banded stuff too.

>
> Another long e-mail, but I hope that this is useful (especially for EGS)
> and will form the basis of a comparison of methods paper.

> Have a good weekend!

> Tim

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>
> Briffa KR, Osborn TJ, Schweingruber FH, Harris IC, Jones PD, Shiyatov SG
> and Vaganov EA (2001) Low-frequency temperature variations from a northern
> tree-ring-density network. Journal of Geophysical Research 106, 2929-2941.

>
> Osborn TJ, Briffa KR, Schweingruber FH and Jones PD (2001)
> Annually-resolved patterns of summer temperatures over the Northern
> Hemisphere since AD 1400 from a tree-ring-density network. In preparation.

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From: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>
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Christensen <torben.christensen@planteco.lu.se>, Wolfgang Knorr
<Wolfgang.Knorr@bgc-jena.mpg.de>, Wolfgang Lucht <Wolfgang.Lucht@pik-
potsdam.de>
Subject: Vulnerability in ATEAM
Date: Fri, 16 Mar 2001 22:17:24 +0100
Reply-to: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>

Content-Type: text/plain; charset=ISO-8859-1
X-MIME-Autoconverted: from 8bit to quoted-printable by
spdmraac.compuserve.com id QAA21095

Dear everybody,

I am still busy compiling the report from the kickoff meeting (and I
also still await some input pieces from some of you...).

For those of you who could not be there, let me just say that I
enjoyed very much to see the group here, and to witness the really
lively and productive discussions. Let's keep it that way.

While U wait for the report - I would like to get you thinking about
the project again by circulating the second draft of a small piece
which is edging towards a working definition of vulnerability, mostly
written by Richard and with input from Pete, Miguel and myself. All
comments are welcome. This is not intended for publication of course,
but it could be a start of something more substantial in due course.

So please send me the elements still missing for the overall report,
and comment to the four authors about the vulnerability piece.

Best regards,

Wolfgang

--

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NOTE: IF YOU NEED TO SEND ATTACHMENTS TO ME, PLEASE:

- 1) avoid sending MS-Word *.doc files (send rtf instead)
 - 2) if the attachments exceed 500kB, contact me before sending anything
-

PS: Sticking to my promise to avoid attachments, I send the plain
ascii text here. Some time Monday you should find the pdf of it on the
web site.

Internal ATEAM document ôTowards a definition of
vulnerabilityàö û do not cite
Draft version 2.0 (16/3/01)

TOWARDS A DEFINITION OF VULNERABILITY
OF ECOSYSTEM FUNCTIONING TO GLOBAL CHANGE

Richard J.T. Klein, Pete Smith, Miguel B. Ara·jo and Wolfgang
Cramer

This document aims to stimulate the discussion of
vulnerability to global change, which is a key feature of the
EU project Advanced Terrestrial Ecosystem Analysis and
Modelling (ATEAM). The goal of ATEAM is to develop an
operational quantitative assessment of vulnerability across
European ecosystems. The rationale for this assessment and its
initial elements are also found in this document.

Common features in present definitions of vulnerability

Vulnerability is a multi-dimensional concept that has been a
topic of study in many different scientific disciplines,
ranging from anthropology and psychology to economics and
ecology. As such, it has been defined and assessed in many
different ways for many different purposes. The scientific
literature provides many examples of vulnerability
assessments, each with their own explicit or (more often)
implicit interpretations of what vulnerability means to the
object of study.

In spite of this diversity the various interpretations of vulnerability have a number of things in common:

1. Vulnerability is always an attribute of a system, in the broadest meaning of the term. Systems that may be vulnerable include individual people, communities, countries, economic sectors, landscapes, resources, ecosystems and so on. Importantly, in ATEAM the system of interest is not ecosystems per se but the set of functions that ecosystems perform in providing goods and services to human society.
2. Vulnerability always refers to some potential of or exposure to harm or damage. It is therefore meaningful to specify exactly to which forcing a system is thought to be vulnerable. In ATEAM multiple forcings are considered, all related in some way to global change. In response to needs expressed by the European Commission these forcings are the increasing atmospheric concentration of CO₂, the climate change that is the result of this increasing concentration, as well as the effects of changing land use and land-use policies.
3. Definitions of vulnerability tend to capture some notion of the extent to which the system would be unable to avoid, defend itself against, cope with, adjust to or otherwise prevent or minimise potential harm or damage. This mechanism of damage prevention or minimisation (termed adaptation in the context of climate change) is important because it defines the difference between the potential harm or damage and the actual or residual impacts that will occur. It can be argued that if a stress-exposed system has the ability to avert the potentially severe impacts that could ensue from this stress, then it is not vulnerable (footnote 1).

The first assessments of vulnerability to climate change (such as the First and Second Assessment Reports of the IPCC and many national vulnerability studies) were carried out without considering adaptation as an important aspect of vulnerability. These assessments implicitly assumed present-day behaviour and activities to continue unchanged in the future, irrespective of how they would be affected by climate change. By ignoring adaptation these studies did not distinguish between potential and residual impacts and thus their results represented serious overestimates of the system's vulnerability. On the other hand, the studies served to generate awareness of the potential magnitude of impacts and of the need for adaptation.

A recent discussion of vulnerability: the IPCC Working Group II

Each of the aforementioned features of vulnerability was incorporated in the proposed definition of vulnerability in

the IPCC Working Group II Third Assessment Report, which was as follows:

The degree to which a system is sensitive to and unable to cope with adverse impacts of climatic stimuli. Vulnerability is a function of a system's exposure and its adaptive capacity.

However, the IPCC Working Group II Plenary meeting in Geneva (13-16 February 2001) adopted a somewhat modified and expanded definition in the final, government-approved version of the Summary for Policymakers. The adopted definition no longer captures the important notion that vulnerability depends on both potential impacts and the inability to cope with these impacts, as was indicated by the word "and" in the first sentence of the above definition:

The degree to which a system is susceptible to, or unable to cope with, adverse effects of climatic change, including climate variability and extremes. Vulnerability is a function of the character, magnitude and rate of climate variation to which a system is exposed, its sensitivity, and its adaptive capacity.

Building blocks for a definition to be used in ATEAM

The former definition of vulnerability captures the various aspects of vulnerability discussed above but it is likely to be too broad to be made operational in ATEAM. ATEAM addresses the interaction between ecosystems and society and in particular the provision of goods and services by ecosystems for human use. Of relevance to ATEAM are therefore not only the exposure and adaptive capacity of ecosystems to climate change but also the adaptive capacity of human systems in relation to a change in the provision of ecosystem goods and services. To develop a meaningful definition of vulnerability for ATEAM it could be useful to explore a number of related concepts: risk, sustainability and resilience.

A relatively widely accepted interpretation of risk is that it is a function of the probability of occurrence of an event combined with an estimate of the magnitude of its impact. For example, in the context of species conservation risk can be seen as a measure of the probability that a negative event (i.e., a threat) combined with the individual species's response to these events (i.e., an indicator of species's vulnerability) would lead a species to extinction (Araujo and Williams, 2000).

Amongst the many definitions of sustainability, a useful one is based on the conservation and substitutability of different types of capital: human-made capital, natural capital, human capital and social capital (Serageldin and Steer, 1994). Sustainable development, of which the most widely used

definition is "development that meets the needs of the present without compromising the ability of future generations to meet their own needs" (WCED, 1987), prescribes that the total stock of capital does not decrease over time. Whether or not substitution and compensation of different types of capital are allowed depends on the preferred level of sustainability (cf. weak versus strong sustainability).

The relationship between sustainability and ecosystem vulnerability is based on the extent to which external forcings lead to a decrease in natural capital and thus in the potential of ecosystems to provide goods and services for human use. A possible (anthropocentric) definition of sustainability in the context of ATEAM could therefore be:

The ability of an ecosystem to provide humans with goods and services in the present, without compromising the ability of future human generations to obtain these ecosystem goods and services in the future.

The concept of resilience is well known in ecology, although two distinct interpretations of the term exist. As defined by Holling (1973), resilience determines the persistence of relationships within a system and is a measure of the ability of these systems to absorb changes and still persist. According to Pimm (1984), however, resilience describes the speed with which a system returns to its original state following a perturbation. Holling (1973), on the other hand, considered this to be the stability of a system, whilst Pimm (1984) referred to stability as the combination of resilience, resistance, persistence and variability.

In an attempt to define the resilience of the Dutch coast, Klein et al. (1998) distinguished between a morphological, an ecological and a socio-economic component of coastal resilience, each of which represents another aspect of the coastal system's capacity to cope with perturbations. They described coastal resilience as a measure of the extent to which a coast is able to respond to external pressures without losing actual or potential functions:

The resilience of the coast is its self-organising capacity to preserve actual and potential functions of coastal systems under the influence of changing hydraulic and morphological conditions. This capacity is based on the (potential) dynamics of morphological, ecological and socio-economic processes in relation to the demands that are made by the functions to be preserved.

Given the focus of ATEAM on ecosystem services, we might want to work towards a similar type of definition of vulnerability, whereby vulnerability could be described in terms of the likelihood that an ecosystem loses a significant amount of its capacity to provide goods and services that are important to

society. A definition that includes the temporal dimension of global change and sustainability could describe vulnerability in terms of the risk of ecosystem sustainability being compromised. Before suggesting a "final" definition, however, we would like to invite views and suggestions from the entire ATEAM consortium.

References

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- Serageldin, I. and A. Steer (eds.), 1994: *Making Development Sustainable: From Concepts to Action*. Environmentally Sustainable Development Occasional Paper Series No. 2, World Bank, Washington DC, iii+40 pp.
- (WCED) World Commission on Environment and Development, 1987: *Our Common Future*, Oxford University Press, Oxford, UK, xv+383 pp.

1 In this document we do not elaborate on the possible different interpretations of adaptation. Adaptation will be the subject of more detailed discussion at a later stage, aimed at an appropriate (semi-) quantitative operationalisation.

Attachment Converted: "c:\eudora\attach\vCard.vcf"

From: "Michael E. Mann" <mann@virginia.edu>
To: p.jones@uea.ac.uk, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk
Subject: problem
Date: Wed, 04 Apr 2001 14:10:07 -0400
Cc: mann@virginia.edu, rbradley@geo.umass.edu

<x-flowed>

Phil et al,

There is a problem w/ figure 4 (and discussion thereof) in your paper to appear in Science. Unfortunately, I didn't catch this until I re-read the paper just now. You haven't shown the right Mann et al NINO3 reconstruction. Are you sure you have used the *cold-season* NINO3 reconstruction, as discussed (and available) in the Mann et al Earth Interactions paper, and not the annual mean reconstruction!!

http://www.ngdc.noaa.gov/paleo/ei/ei_reconsb.html

I don't believe that has the trend that the series you show does. That NINO3 series agrees closely ($r=0.63$) w/ the Stahle et al series (once the sign has been flipped on that series, and the off-by-one-year date convention is taken into account), far closer than what you have shown. I'm pretty sure you've used the wrong series.

Moreover, it is inappropriate to refer (as you do) the Nino3 reconstruction as an SOI reconstruction, no matter whether it has been renormalized, sign-switched, etc. There are fundamennal differences between the low-frequency behavior of NINO3 and SOI, (consider for example the 20th century!) and they aren't dynamically equivalent! To say there is a "long-term trend" in our "SOI reconstruction" is extremely misleading. There is a long-term trend in our *NINO3* reconstruciton. Only Stahle produced an SOI reconstruction, and it is only meaningful to correlate the two at annual timescales where they should similarly reflect largely interannual ENSO variability.

Moreover, I don't think this is true (or as true) of our colld-season NINO3 series, which is the right one to use. Hopefully, you still have a chance to change this in the galleys, etc.

Thanks in advance for your attention to this,

mike

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</x-flowed>

From: Mike Hulme <m.hulme@uea.ac.uk>
To: s.torok
Subject: Fwd: RE: kyoto survey - press inquiry from the THES
Date: Thu Apr 5 11:59:31 2001

Simon,

Could you - or Vanessa - buy a THES today from the paper shop and check this out. I would quite like to draft a short letter to THES as suggested by Steve. But I need to see how the issue was presented in this week's issue.

Thanks,
Mike

From: "Farrar, Steve" <steve.farrar@thes.co.uk>
To: 'Mike Hulme' <m.hulme@uea.ac.uk>
Subject: RE: kyoto survey - press inquiry from the THES
Date: Thu, 5 Apr 2001 09:45:33 +0100
X-Mailer: Internet Mail Service (5.5.2653.19)

Dear Mike,

thanks for that. I feel terrible but despite the pain it cost to reply to the survey, the deadline has now passed. We had such a high response rate that we decided to run the piece in this week's paper while the issue of the US withdrawl from the protocol was still high in everyone's mind. So I cannot include your responses. However, you make a number of very significant points, not least your reply to question 2 on the strength of the evidence and the political framework outlined in your final sentences. I wonder - and I know this is pushing it - whether you might consider rearranging some of these sentences to form a brief letter to the editor for the following week's paper? I would like this issue to stay alive in the THES and allow the paper to play a small role in persuading as many scientists as possible to take part in a scientific/political debate that may contribute to influencing those people who *can* change things. Not an original objective, I know, but the THES does have a fairly unique position within the academic community and hence a responsibility. Anyhow, sorry for the bad news

best wishes

Steve

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-----Original Message-----

From: Mike Hulme [[2]mailto:m.hulme@uea.ac.uk]

Sent: 04 April 2001 19:57

To: Farrar, Steve

Subject: Re: kyoto survey - press inquiry from the THES

Steve,

I hate these sort of questionnaires since Y or N answers are barely adequate. However, I've given it a go with some other comments (by the way, Prof. Trevor Davies is Head of my School here at UEA - I am only Director of a Centre within the School, albeit a highly relevant one!). You can quote me if appropriate, but let me know before hand.

Mike

At 12:30 02/04/01 +0100, you wrote:

>Dear Mike,

>

>hope you're well. I am conducting a survey of heads of UK university
>departments of environmental science for the Times Higher Education
>Supplement. I am keen to explore views concerning the United States and
>the Kyoto agreement. I wonder if you could answer the following Yes/No
>questions when you get a moment. Note, I will not identify you unless you
>specifically state that you do not mind being quoted.

>

>I do hope you can help

>

>all the bets

>

>Steve

>

>1: Do you believe human activities are at least in part responsible for
>driving global climate change?

YES

>2: Do you feel the evidence for this is sufficiently strong to start
>reducing emissions?

NO - to reduce emissions requires more evidence than that humans are altering climate. We need to know something about the potential risks associated with future climate change, whether these risks can be minimised through adaptive action and then have some socially negotiated basis for deciding about the necessity and extent of desirable emissions reductions. On none of these issues do we have a good basis to work from. The precautionary principle, if chosen, would imply start reducing emissions now - but I am not convinced a blind application of the precautionary principle in this case is the most appropriate instrument.

>3: Do you think the measures proposed at Kyoto were too weak, correct, or
>too strong?

The 5.2% emissions reduction by 2010 by Annex I countries were not driven by science but by real-politik. By definition they were the best achievable. The real issue however is not about target setting - it's about the dynamics of change worldwide in energy technologies, investment strategies, consumer and community behaviour and aspirations, etc. It is *these* things that in the end will deliver a safer climate - not the Protocol per se. More attention should be directed at the diverse and myriad set of actions needed to decarbonise our societies.

>4: Are you disappointed that George Bush has abandoned the Kyoto agreement?
YES - but it is too early to say that Kyoto is dead. The USA does not have the power of veto - and Bush will have to propose some climate management strategy of his own. We wait and see.

>5: Should the rest of the world press on with the agreement without the
>United States?

Probably YES. This can be achieved and should provide valuable lessons in global climate management which we can learn from in the long-term.

>6: Do you feel the US should be allowed to count carbon sequestration
>measures such as planting new forests towards any carbon emissions
>reduction target?

YES. The UK are doing it in their national climate change programme so why not the USA?

>7: Are you optimistic that there will be a new emissions control agreement
>within the next 12 months?

A 'new' one? We haven't got one yet. I would think maybe not in the next 12 months, but the critical issues about global climate management will be clearer.

>8: Should the Kyoto preliminary targets be watered down to gain the
>Americans' support?

NO. If the USA don't like them, let them not ratify or propose a strategy of their own.

>If you would like to add any comments to this survey as to the
>implications of the US's rejection of Kyoto for the planet, what UK can do
>about it or what role scientists can play in this debacle, please do so.

In a literal sense the implications for global climate are trivial - what will affect the course of global climate (and only then climate beyond about 2030 - up until then climate is pretty much pre-determined by inertia in the system) in the long-run are the effects of cumulative decisions taken by many, many people/governments/businesses over the next 10-20 years. Let's not kid ourselves that the USA President is more powerful than he would like to think. The planetary system is much bigger than one 4-year term of a US president.

The UK is playing a key role both within the negotiating machinery of the FCCC, in pioneering new scientific analyses, and in working out new forms of adapting to climate change. This momentum in the UK is not going to be

halted by Bush.

Scientists need to be there to point out the long-term nature of the problem - it is not a classic political issue where a one-term government can solve or worsen the problem. Scientists need to point out that for long-term planetary management we need new analytical tools, new criteria for investment decisions, a new appreciation of the concept of global citizenship. What climate change forces us to do is to think about the influence we are having on the quality of life for the next generation but one - not our own generation or even our children's generation. Conventional politics is not a system geared up for this challenge.

>*****

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>

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1. <http://www.thes.co.uk/>
2. <mailto:m.hulme@uea.ac.uk>
3. <http://www.thes.co.uk/>
4. <http://www.tyndall.uea.ac.uk/>

From: "Michael Mann" <memann00@hotmail.com>
To: T.Osborn@uea.ac.uk
Subject: RE: problem
Date: Thu, 05 Apr 2001 15:37:18 -0000
Reply-to: mann@virginia.edu
Cc: k.briffa@uea.ac.uk, mann@virginia.edu, p.jones@uea.ac.uk

<x-flowed>

Hi Tim,

THanks for looking into this so quickly. I agree w/ your assessment. It is probably just the fact that the signal of interest in SOI and NINO3 is really the interannual signal, and this is not evident in the low-frequency component shown, which emphasizes discrepancies that are actually small compared to amplitude of the interannual signal present in both Stahle et al and Mann et al. So I would urge showing the annual reconstructions in this case, rather than smoothed for this reason...

In IPCC we only chose to show 1700 to present, which is a better calibrated/verified interval than back to 1650, so I'd encourage you guys to restrict it to 1700-present if you can. Other than that, I think it is important to acknowledge that SOI and NINO3 have different low-frequency trends over the 20th century, and might well have different trends in the past. It is true that many of the proxies used are sensitive to the SOI (e.g. mexican tree rings), but others are sensitive to Pacific SST (e.g. corals from GBR, New Caledonia, Galapagos) and our claim is that the calibration process will select out the best estimate of the temperature patterns, rather than SLP patterns, associated w/ ENSO, from the multiproxy network. In the future, we'll be going after SLP reconstruction too, and it'll be interesting to see what the difference is.

I hope that clarifies. Please let me know if I can be of any further help, provide further clarification, etc. Thanks again,

mike

>From: Tim Osborn <T.Osborn@uea.ac.uk>
>To: "k.briffa" <k.briffa@uea.ac.uk>, Michael Mann <memann00@hotmail.com>,
> "p.jones" <p.jones@uea.ac.uk>, "T.Osborn" <T.Osborn@uea.ac.uk>,
> mann <mann@virginia.edu>
>CC: rbradley <rbradley@geo.umass.edu>

>Subject: RE: problem

>Date: Wed, 4 Apr 2001 23:02:35 +0100

>

>>Thanks for getting back to me so quickly. I could be wrong, but i just
>>want to make sure. The cold-season NINO3 is far more consistent w/ DJF

>SOI

>>and Stahle's recon, so I just want to be sure that is the one that

>>is shown.

>

>>>>Are you sure you have used the *cold-season* NINO3

>>>>reconstruction, as discussed (and available) in the Mann et al Earth

>>>>Interactions paper, and not the annual mean reconstruction!!

>>>>

>>>>http://www.ngdc.noaa.gov/paleo/ei/ei_reconsb.html

>>>>

>>>>I don't believe that has the trend that the series you show does. That

>>>>NINO3 series agrees closely ($r=0.63$) w/ the Stahle et al series (once

>>>>the sign has been flipped on that series, and the off-by-one-year date

>>>>convention is taken into account

>

>Dear all,

>

>I've found a machine with telnet and have been able to check my files &

>programs. The file I'm using matches the ninocold-recon.dat file

>downloadable

>from the ei_reconsb.html. It also correlates at $r=0.63$ with Stahle. I

>don't

>have access to plotting here, so I cannot investigate further the reason

>for

>the apparent mismatch, though I wonder whether it is due to the heavy

>(30-yr)

>smoothing used in the Science paper - much more smoothing than is typically

>used when looking at ENSO! These 30-yr differences are in fact quite small

>in

>comparison with some of the interannual variations, and perhaps the series

>would look very much more alike if unfiltered? Anyway, as far as I can

>tell,

>the figure is ok.

>

>>>>Moreover, it is inappropriate to refer (as you do) the Nino3

>>>>reconstruction

>>>>as an SOI reconstruction, no matter whether it has been

>>>>renormalized, sign-switched, etc. There are fundamennal differences

> >>between
> >> >the low-frequency behavior of NINO3 and SOI, (consider
> >> >for example the 20th century!) and they aren't dynamically equivalent!
>To
> >> >say there is a "long-term trend" in our "SOI reconstruction"
> >> >is extremely misleading. There is a long-term trend in our *NINO3*
> >> >reconstruciton. Only Stahle produced an SOI reconstruction, and it is
> >>only
> >> >meaningful to correlate the two at annual timescales where they should
> >> >similarly reflect largely interannual ENSO variability.
>
>Phil/Keith - I've got a copy of our paper with me and I agree with what
>Mike
>says above, but on the other hand the lack of space constrains us. I
>wonder
>whether we can squeeze anything in at the proofs stage (have you had them
>yet
>Phil?). With a quick read I couldn't actually spot the phrase "long-term
>trend", but we could still add something about SOI and SST being coupled on
>interannual time scales and possibly doing somewhat different things on
>longer
>timescales. Mike - would you not agree, however, that your predictors
>(excluding corals) are mainly remote from the Nino 3 SST region and that
>they
>are likely responding via atmospheric teleconnection patterns and therefore
>perhaps should pick up the SOI even if calibrated against Nino 3 SST? Feel
>free to disagree - just wanted to get your reaction!
>
>Best regards
>
>Tim
>
>

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</x-flowed>

From: tom crowley <tom@ocean.tamu.edu>
To: Chick Keller <ckeller@igpp.ucsd.edu>
Subject: Re: Low Frequency signals in Proxy temperatures:
Date: Sat, 28 Apr 2001 09:54:18 -0500
Cc: tom@ocean.tamu.edu, p.jones@uea.ac.uk, rbradley@geo.umass.edu, k.briffa@uea.ac.uk, mann@virginia.edu, mhughes@ltrr.arizona.edu

Chick,

look at the instrumental record! there are huge differences between different regions - Alaska has warmed substantially while eastern North America cooled after the 1950s. locking onto local records, no matter how beautiful, can lead to serious errors. If the ice cores are so infallible why do they give substantially different stories for grip and gisp2 over the last 1500 years?

the bottom line is that one cannot make a robust case that decadal hemispheric temperatures over the last 1500 years were even as warm as the late 20th century, much less warmer.

Tom

>
>Well said indeed! This helps me to slowly understand what's being
>done and why.
>
>My nagging problem remains however, and that's that there seem to be
>too many paleo records published that show much larger amplitude
>variations. Now many can be explained, but some look more robust.
>For example I think most people are wondering about the total
>disagreement between isotope temperatures from GISP II and borehole
>temperatures from GRIP and Dye 3. Here the usual land use caution
>doesn't apply since I don't think the ice above the boreholes has
>changed much?
>
>And if I understand Tom Crowley's note to me, his reconstruction
>averaged normalized records, thus missing large amplitude variations
>such as the Keigwin Sargasso one, which he used, but which shows a
>large amplitude signal tantalizingly similar to the GRIP/Dye 3
>records. (Tom used GISP II which essentially has no low frequency
>amplitude)
>
>So I read all the papers, and am impressed by the painstakingly
>careful work, but still wonder about a world in which the
>hemispherical low frequency temperature amplitude could be (see Jones
>et al Science this week) only about 0.4°C between 1000 and about
>1950, while parts of the world could have seen amplitudes of up to
>2°C in the same period. I suppose you could say that, given natural
>forcing only, there can be much larger variance from the mean
>(spatially and temporally) than in the past hundred and fifty years
>when GHG forcing is forcing more uniformity, but does this make sense?
>
>This is why I keep asking questions about the ability of various
>proxies to return low frequency information.
>
>Anything you could say about this would be greatly appreciated.
>

>Finally consider this. I read recently (don't know the pedigree of
>this number but it WAS published!!) that Milankovitch cooling at this
>point in the Holocene should be about 0.4°C/millennium (other plots
>I've seen would suggest about 2.3 to 1/2 of that). If that's true,
>then all the cooling since the year 1000 is Milankovitch and there's
>no room for variations in solar activity and multiple volcanic
>eruptions. Now I'm not saying this is the best way to think about
>such things, but it does remind us that much of the cooling seems to
>have been due to Milankovitch, and, given the small amplitude of the
>proxy records, that is a bit worrisome. What do people think about
>this?

>

>Regards,

>

>

>Well said Malcolm...

>

>mike

>

>p.s. Chick: You might want to check out the review article by Jones
>et al in the latest Science...

>

>At 01:16 PM 4/26/01 -0700, Malcolm K. Hughes wrote:

>>Dear Chick - some thoughts on a couple of the points you raised,

>>Cheers, Malcolm

>>1. There is no reference to the ABD in MBH 98 and 99 because
>>the technique

>>was not available at that time - see the dates on Keith's publications that
>>describe it.

>>2. There are significant regions where the ABD method is not needed,
>>because the trees live much longer than those in the Schweingruber
>>network that

>>Keith has been using, and grow under conditions that make only very
>>conservative

>>standardization necessary. There is a growing body of evidence that these
>>tree-ring records can capture century-to-millennial change accurately (Hughes
>>and Graumlich, 1996 and Hughes and Funkhouser 1998, for example). In
>>fact, the

>>MBH reconstruction before AD 1400 was largely based on these.

>>3. Keith has pooled information from extremely large regions

>>(presumably to
>>get large enough samples), whereas we (MBH) have been particularly
>>interested in

>>spatial variability, ruling out the use of ABD.

>>4. The ABD method is new, needs testing, and, I predict, will be
>>modified

>>as it is tested.

>>5. The benefit of annual resolution is that direct calibration and

>>cross-validation against instrumental records is possible with a
>>high degree of

>>rigor. We are relaxing this condition somewhat in our ongoing analyses,
>>and it

>>will be interesting to see how the uncertainties increase as one includes
>>records with poorer temporal resolution. This is an issue that the

>>advocates of

>>such records do not address, so far as I can see.

>>

>>

>>

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>>Laboratory of Tree-Ring Research
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>

>

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> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

>

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>Is the noticeable increase in surfers off Scripps Beach a possible
>indication of global warming?

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From: Edward Cook <drdendro@ldeo.columbia.edu>

To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

Subject: Re: hockey stick

Date: Wed, 2 May 2001 15:25:41 -0400

Cc: tom crowley <tom@ocean.tamu.edu>, esper@ldeo.columbia.edu, Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, mhughes@ltrr.Arizona.edu, rbradley@geo.umass.edu, p.jones@uea.ac.uk, srutherford@virginia.edu

Hi Mike,

No problem. I am quite happy to work this stuff through in a careful way and am happy to discuss it all with you. I certainly don't want the work to be viewed as an attack on previous work such as yours. Unfortunately, this global change stuff is so politicized by both sides of the issue that it is difficult to do the science in a dispassionate environment. I ran into the same problem in the acid rain/forest decline debate that raged in the 1980s. At one point, I was simultaneously accused of being a raving tree hugger and in the pocket of the coal industry. I have always said that I don't care what answer is found as long as it is the truth or at least bloody close to it.

Cheers,

Ed

>Hi Ed,

>
>This is fair enough, and I'm sorry if my spelling out my concerns
>sounded defensive to you. It wasn't meant to be that way.

>
>Lets figure this
>all out based on good, careful
>work and see what the data has to say in the end. We're working towards
>this ourselves, using revised methods and including borehole data, etc.
>and will keep everyone posted on this.

>
>I don't in any way doubt yours and Jan's integrity here.

>
>I'm just a bit concerned that the result is getting used publically, by
>some, before it has gone through the gauntlet of peer review.
>Especially because it is, whether you condone it or not, being used as
>we speak to discredit the work of us, and Phil et al, this is dangerous.
>I think there are some legitimate issues that need to be sorted out
>with regard to the standardization method, and would like to see
>this play out before we jump to conclusions regarding revised estimates
>of the northern hemisphere mean temperature record and the nature of
>the "MWP".

>
>I'd
>be interested to be kept posted on what the status of the manuscript is.

>
>Thanks,

>
>mike

>
>On Wed, 2 May 2001, Edward Cook wrote:

>
>> Hi Mike,

>>
>>> A few quick points Ed,

>>>
>>>> These "Wally seminars" are self-promoting acts on Broecker's part, and I
>>>> think the community has to reject them as having any broader significance.
>>>> If Broecker had pulled this w/ Ray, Malcolm, Keith, Phil, and Tom around,

>> >he wouldn't get away w/ such a one-sided treatment of the issue. I've been
>> >extremely troubled by what I have heard here.

>>
>> It appears that you are responding in a way that is a bit overly defensive,
>> which I regret. I am not supporting Broecker per se and only explained in a
>> very detailed fashion the origin of the work by Esper and me and how it was
>> presented to refute a very unfair characterization of tree-ring data in
>> Wally's perspective piece. The fact that Esper compared his series with
>> Jones, Briffa, and Mann et al. should not be viewed as an attack on your
>> work. It was never intended to be so, but it is was a clearly legitimate
>> thing to do. As I said, I have no control over Broecker. But it is unfair
>> and indeed incorrect to start out by dismissing the "Special Wally
>> Seminars" as self-promoting acts. To say that is simply wrong. He doesn't
>> bring people in to only express support for his point of view or pet
>> theory, as you are implying. So, I suggest that you cool down a bit on this
>> matter. It detracts from the scientific issues that should properly be
>> debated here. This is the only point on which I will defend Broecker.

>>
>> >I'm also a bit troubled by your comparisons w/ glacial advances, etc. and
>> >how these correlate w/ your reconstruction. Malcolm, Ray, Phil, and others
>> >have been over this stuff time and again, and have pointed out that these
>> >data themselves don't support the notion of globally-synchronoous changes.
>> >You seem to be arguing otherwise? And with regard to association w/
>> >volcanic forcing, Tom has already shown that the major volcanic events are
>> >captured correctly in the existing reconstructions, whether or not the
>> >longer-term trends are correct or not...

>>
>> I am not arguing for "globally-synchronous changes" and never have. To
>> quote what I said about neo-glacial advances, some of the fluctuations in
>> Esper's series "correspond well with known histories of neo-glacial advance
>> in some parts of the NH". Note the use of the word "some" in that quote.
>> That is a fair statement and why shouldn't I say it if it is true,
>> coincidentally or not. Whether or not it argues for "globally-synchronous
>> changes" is up to you. I would never argue that everything happening on
>> multi-decadal time scales is phase-locked across the NH. That would be a
>> silly thing to say. But it is perfectly valid to point out the degree to
>> which independent evidence for cold periods based on glacier advances
>> appears to agree with a larger-scale indicator of temperature variability. I
>> thought this is how science to supposed to proceed. I also don't see your
>> point about volcanic forcing. I mentioned this purely in the spirit of the
>> work of Crowley and others to suggest that the Esper series is probably
>> capturing this kind of signal as well. It has nothing to do with the issue
>> of centennial trends in temperature. You are reading far more into what I
>> wrote than I ever intended or meant.

>>
>> >Re the boreholes. Actually, if Tom's estimates are correct, and it is also
>> >correct that the boreholes have the low-frequency signal correct over the
>> >past few centuries, we are forced to also accept Tom's result that the
>> >so-called "MWP", at the hemispheric scale, is actually even COOLER relative
>> >to present than our result shows! That was clear in Tom's presentation at
>> >the workshop. So lets be clear about that--Tom's work and the boreholes in
>> >no way support Broecker's conclusion that the MWP was warmer than we have
>> >it--it actually implies the MWP is colder than we have it!

>> >Tom, please speak up if I'm not correct in this regard!
>>
>> I am not saying that Tom's results are wrong. And, I am certainly not
>> saying that Broecker is right. I merely described the results of a new
>> analysis of a somewhat new set of long tree-ring records from the
>> extra-tropics. My statement that the MWP appeared to be comparable to the
>> 20th century does not imply, nor was it meant to imply, that somehow the
>> 20th century temperature is not truly anomalous and being driven by
>> greenhouse gases. To quote from my email, "I would not claim (and nor would
>> Jan) that it exceeded the warmth of the late 20th century. We simply do not

>> have the precision or the proxy replication to say that yet." Note the use
>> of the word "precision". This clearly relates to the issue of error
>> variance and confidence intervals, a point that you clearly emphasize in
>> describing your series. Also note the emphasis on "late 20th century". I
>> think that most researchers in global change research would agree that the
>> emergence of a clear greenhouse forcing signal has really only occurred
>> since after 1970. I am not debating this point, although I do think that
>> there still exists a significant uncertainty as to the relative
>> contributions of natural and greenhouse forcing to warming during the past
>> 20-30 years at least. Note that I also tried to emphasize the
>> extra-tropical nature of this series, and it may be that the tropics do not
>> show the same strength of warming. But I do argue strongly that we do not
>> have the high-resolution proxy data needed to test for a MWP in the
>> tropics. Please correct me if I am wrong here.

>>
>> >We are in the process of incorporating the borehole data into the
>> >low-frequency component of the reconstruction. The key difference will be
>> >that they are going to be calibrated against the instrumental record and
>> >weighted by the spatial coherence within the borehole data rather than what
>> >Pollack has done. I expect the results will be different, but in any case
>> >quite telling...

>>
>> Fine.

>>
>> >I'll let Malcolm and Keith respond to the issues related to the
>> >standardization of the Esper chronologies, though it immediately sounds to
>> >me quite clear that there is the likelihood of of having contaminated the
>> >century-scales w/ non-climatic info. Having now done some work w/
>> >chronologies in disturbed forests myself now (in collaboration w/ Dave
>> >Stahle), I know how easy it is to get lots of century-scale variability
>> >that has nothing to do w/ climate. I imagine the reviewers of the
>> >manuscript will have to be convinced that this is the case w/ what Esper
>> >has done. I'm very skeptical. I'm also bothered that Broecker has promoted
>> >this work prior to any formal peer review. There are some real issues w/
>> >the standardization approach and there is a real stretch in promoting this
>> >as a hemispheric temperature reconstruction.

>>
>> I appreciate your skepticism and I hope that Jan and I can convince you
>> otherwise. I also encourage you to continue getting your shoulders sore and
>> hands dirty on tree-ring sampling and analysis. Esper's analysis is not
>> perfect. Nor is anyone else's who works in this game. But if Esper's series
>> is wrong on century time scales, then Jones and Briffa are wrong too. If
>> Esper's series is also wrong on inter-decadal time scales, then your series
>> is wrong as well because on that time scale of variability, his series
>> agrees very well with yours. So, I would be very cautious about declaring
>> that Esper's series is in some sense invalid. Finally, as I have said ad
>> nauseam, I have no control over what Broecker thinks or does beyond
>> presenting to him a convincing case for the ability of certain tree-ring
>> series to preserve long-term temperature variability. And again, "I also
>> tried to emphasize the extra-tropical nature of this series." Please give
>> me a break here.

>>
>> >Finally, what is the exact spatial distribution of the sparse data he used.
>> >Scott R. drove home the point regarding the importance of taking into
>> >account spatial sampling in his talk at the workshop. A sparse
>> >extratropical set of indicators, no matter how
>> >locally-temperature-sensitive they are, will not, unless you're *very*
>> >lucky w/ the locations, be an accurate indicator of true N. Hem temp. In
>> >general it will overestimate the variance at all timescales. The true N.Hem
>> >temperature (ie, weighted largely by tropical ocean SST) has much less
>> >variance than extratropical continents. There may be a large apples and
>> >oranges component to the comparisons you describe.

>> I know your argument and I am sensitive to it, hence my emphasis on
>> "extra-tropical". So, don't look for disagreement on the importance of the
>> tropical SSTs to any estimate of NH temperatures. But let's be honest here.
>> Your reconstruction prior to roughly AD 1600 is dominated by extra-tropical
>> proxies. So, in a way, you are caught in the same dilemma as all other
>> people who have tried to do this.

>>
>>> We've shown that are reconstructions in continental extratropical regions
>>> have lots more variance and variability. It is, as we have all shown, the
>>> averaging over many regions that reduces the amplitude of variability. Our
>>> regional reconstructions show far more significant warm and cold periods.
>>> But they cancel out spatially!

>>
>> Understood, but it is still unclear how this all happens as your
>> reconstruction proceeds back in time with an increasingly limited and
>> spatially-restricted set of proxies. Confidence limits that you place on
>> your series is laudable and I agree, to first order, that the MWP in your
>> series could easily have been cooler than what you show. But it implicitly
>> assumes that the estimates are equally unbiased (or equally biased for that
>> matter) back in time. I don't know if that is an issue here, but I believe
>> that the issue of bias using an increasingly sparse number of predictors
>> scattered irregularly over space has not been investigated. Please correct me
>> if I am wrong here.

>>
>>> If a legitimate argument were to be made that we have significantly
>>> underestimated, within the context of our uncertainty estimates, the
>>> amplitude of the MWP at the hemispheric scale, I'd be the first to accept
>>> it (note that, as Phil et al pointed out in their recent review article in
>>> Science, we do not dispute that temperatures early in the millennium,
>>> within the uncertainty estimates, may have been comparable to early/mid
>>> 20th centuries--just not late 20th century temperatures).

>>
>> We are in agreement here. See my earlier comments.

>>
>>> Frankly though Ed, I really don't see it here. We may have to let the
>>> peer-review process decide this, but I think you might benefit from knowing
>>> the consensus of the very able group we have assembled in this email
>>> list, on what Esper/you have done?

>>
>> Of course, I know everyone in this "very able group" and respect their
>> opinions and scientific credentials. The same obviously goes for you. That
>> is not to say that we can't disagree. After all, consensus science can
>> impede progress as much as promote understanding.

>>
>> Cheers,

>>
>> Ed

>>
>>> Comments or thoughts?

>>>
>>> cheers,

>>>
>>> mike

>>>
>>> At 10:59 AM 5/2/01 -0400, Edward Cook wrote:

>>>>
>>>> Ed,

>>>>
>>>> heard some rumor that you are involved in a non-hockey stick
>>>> reconstruction

>>>>
>>>> of northern hemisphere temperatures. I am very intrigued to learn about
>>>> this - are these results suggesting the so called Medieval Warm

>>>>
>>>> Period may

>>>>
>>>> be warmer than the early/mid 20th century?

>>>>

>>>>>any enlightenment on this would be most appreciated, Tom

>>>>

>>>>

>>>>

>>>>>Thomas J. Crowley

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>>>>

>>>>>Hi Tom,

>>>>

>>>>>As rumors often are, the one you heard is not entirely accurate. So, I

>>>>>will

>>>>>take some time here to explain for you, Mike, and others exactly what was

>>>>>done and what the motivation was, in an effort to hopefully avoid any

>>>>>misunderstanding. I especially want to avoid any suggestion that this work

>>>>>was being done to specifically counter or refute the "hockey stick".

>>>>>However, it does suggest (as do other results from your EBM, Peck's work,

>>>>>the borehole data, and Briffa and Jones large-scale proxy estimates) that

>>>>>there are unresolved (I think) inconsistencies in the low-frequency

>>>>>aspects

>>>>>of the hockey stick series compared to other results. So, any comparisons

>>>>>with the hockey stick were made with that spirit in mind.

>>>>

>>>>>What Jan Esper and I are working on (mostly Jan with me as second author)

>>>>>is a paper that was in response to Broecker's Science Perspectives

>>>>>piece on

>>>>>the Medieval Warm Period. Specifically, we took strong exception to his

>>>>>claim that tree rings are incapable of preserving century time scale

>>>>>temperature variability. Of course, if Broecker had read the

>>>>>literature, he

>>>>>would have known that what he claimed was inaccurate. Be that as it may,

>>>>>Jan had been working on a project, as part of his post-doc here, to

>>>>>look at

>>>>>large-scale, low-frequency patterns of tree growth and climate in long

>>>>>tree-ring records provided to him by Fritz Schweingruber. With the

>>>>>addition

>>>>>of a couple of sites from foxtail pine in California, Jan amassed a

>>>>>collection of 14 tree-ring sites scattered somewhat uniformly over the

>>>>>30-70 degree NH latitude band, with most extending back 1000-1200 years.

>>>>>All of the sites are from temperature-sensitive locations (i.e. high

>>>>>elevation or high northern latitude. It is, as far as I know, the largest,

>>>>>longest, and most spatially representative set of such

>>>>>temperature-sensitive tree-ring data yet put together for the NH

>>>>>extra-tropics.

>>>>

>>>>>In order to preserve maximum low-frequency variance, Jan used the Regional

>>>>>Curve Standardization (RCS) method, used previously by Briffa and myself

>>>>>with great success. Only here, Jan chose to do things in a somewhat

>>>>>radical

>>>>>fashion. Since the replication at each site was generally insufficient to

>>>>>produce a robust RCS chronology back to, say, AD 1000, Jan pooled all of

>>>>>the original measurement series into 2 classes of growth trends:

>>>>>non-linear

>>>>>(~700 ring-width series) and linear (~500 ring-width series). He than

>>>>>performed independent RCS on the each of the pooled sets and produced

>>>>>2 RCS

>>>>>chronologies with remarkably similar multi-decadal and centennial

>>>>>low-frequency characteristics. These chronologies are not good at

>>>preserving high-frequency climate information because of the scattering of
>>>sites and the mix of different species, but the low-frequency patterns are
>>>probably reflecting the same long-term changes in temperature. Jan then
>>>averaged the 2 RCS chronologies together to produce a single chronology
>>>extending back to AD 800. It has a very well defined Medieval Warm
>>Period -
>>>Little Ice Age - 20th Century Warming pattern, punctuated by strong
>>decadal
>>>fluctuations of inferred cold that correspond well with known histories of
>>>neo-glacial advance in some parts of the NH. The punctuations also appear,
>>>in some cases, to be related to known major volcanic eruptions.
>>>
>>>Jan originally only wanted to show this NH extra-tropical RCS
>>chronology in
>>>a form scaled to millimeters of growth to show how forest productivity and
>>>carbon sequestration may be modified by climate variability and change
>>over
>>>relatively long time scales. However, I encouraged him to compare his
>>>series with NH instrumental temperature data and the proxy estimates
>>>produced by Jones, Briffa, and Mann in order to bolster the claim that his
>>>unorthodox method of pooling the tree-ring data was producing a record
>>that
>>>was indeed related to temperatures in some sense. This he did by linearly
>>>rescaling his RCS chronology from mm of growth to temperature
>>anomalies. In
>>>so doing, Jan demonstrated that his series, on inter-decadal time scales
>>>only, was well correlated to the annual NH instrumental record. This
>>result
>>>agreed extremely well with those of Jones and Briffa. Of course, some of
>>>the same data were used by them, but probably not more than 40 percent
>>>(Briffa in particular), so the comparison is based on mostly, but not
>>>fully, independent data. The similarity indicated that Jan's approach was
>>>valid for producing a useful reconstruction of multi-decadal temperature
>>>variability (probably weighted towards the warm-season months, but it is
>>>impossible to know by how much) over a larger region of the NH
>>>extra-tropics than that produced before by Jones and Briffa. It also
>>>revealed somewhat more intense cooling in the Little Ice Age that is more
>>>consistent with what the borehole temperatures indicate back to AD 1600.
>>>This result also bolsters the argument for a reasonably large-scale
>>>Medieval Warm Period that may not be as warm as the late 20th century, but
>>>is of much(?) greater significance than that produced previously.
>>>
>>>Of course, Jan also had to compare his record with the hockey stick since
>>>that is the most prominent and oft-cited record of NH temperatures
>>covering
>>>the past 1000 years. The results were consistent with the differences
>>shown
>>>by others, mainly in the century-scale of variability. Again, the Esper
>>>series shows a very strong, even canonical, Medieval Warm Period - Little
>>>Ice Age - 20th Century Warming pattern, which is largely missing from the
>>>hockey stick. Yet the two series agree reasonably well on inter-decadal
>>>timescales, even though they may not be 1:1 expressions of the same
>>>temperature window (i.e. annual vs. warm-season weighted). However, the
>>>tree-ring series used in the hockey stick are warm-season weighted as
>>well,
>>>so the difference between "annual" and "warm-season weighted" is probably
>>>not as large as it might seem, especially before the period of
>>instrumental
>>>data (e.g. pre-1700) in the hockey stick. So, they both share a
>>significant
>>>degree of common interdecadal temperature information (and some, but not
>>>much, data), but do not co-vary well on century timescales. Again,
>>this has

>>>>all been shown before by others using different temperature
>>>>reconstructions, but Jan's result is probably the most comprehensive
>>>>expression (I believe) of extra-tropical NH temperatures back to AD 800 on
>>>>multi-decadal and century time scales.
>>>>
>>>>Now back to the Broecker perspectives piece. I felt compelled to refute
>>>>Broecker's erroneous claim that tree rings could not preserve long-term
>>>>temperature information. So, I organized a "Special Wally Seminar" in
>>>>which
>>>>I introduced the topic to him and the packed audience using Samuel
>>>>Johnson's famous "I refute it thus" statement in the form of "Jan
>>>>Esper and
>>>>I refute Broecker thus". Jan than presented, in a very detailed and well
>>>>expressed fashion, his story and Broecker became an instant convert. In
>>>>other words, Wally now believes that long tree-ring records, when properly
>>>>selected and processed, can preserve low-frequency temperature variability
>>>>on centennial time scales. Others in the audience came away with the same
>>>>understanding, one that we dendrochronologists always knew to be the case.
>>>>This was the entire purpose of Jan's work and the presentation of it to
>>>>Wally and others. Wally had expressed some doubts about the hockey stick
>>>>previously to me and did so again in his perspectives article. So, Jan's
>>>>presentation strongly re-enforced Wally's opinion about the hockey stick,
>>>>which he has expressed to others including several who attended a
>>>>subsequent NOAA meeting at Lamont. I have no control over what Wally says
>>>>and only hope that we can work together to reconcile, in a professional,
>>>>friendly manner, the differences between the hockey stick and other proxy
>>>>temperature records covering the past 1000 years. This I would like to do.
>>>>
>>>>I do think that the Medieval Warm Period was a far more significant event
>>>>than has been recognized previously, as much because the high-resolution
>>>>data to evaluate it had not been available before. That is much less
>>>>so the
>>>>case now. It is even showing up strongly now in long SH tree-ring series.
>>>>However, there is still the question of how strong this event was in the
>>>>tropics. I maintain that we do not have the proxies to tell us that now.
>>>>The tropical ice core data are very difficult to interpret as temperature
>>>>proxies (far worse than tree rings for sure and maybe even unrelated to
>>>>temperatures in any simple linear sense as is often assumed), so I do not
>>>>believe that they can be used alone as records to test for the
>>>>existence of
>>>>a Medieval Warm Period in the tropics. That being the case, there are
>>>>really no other high-resolution records from the tropics to use, and the
>>>>teleconnections between long extra-tropical proxies and the tropics are, I
>>>>believe, far too tenuous and probably unstable to use to sort out this
>>>>issue.
>>>>
>>>>So, at this stage I would argue that the Medieval Warm Period was probably
>>>>a global extra-tropical event, at the very least, with warmth that was
>>>>persistent and probably comparable to much of what we have experienced in
>>>>the 20th century. However, I would not claim (and nor would Jan) that it
>>>>exceeded the warmth of the late 20th century. We simply do not have the
>>>>precision or the proxy replication to say that yet. This being said, I do
>>>>find the dismissal of the Medieval Warm Period as a meaningful global
>>>>event
>>>>to be grossly premature and probably wrong. Kind of like Mark Twain's
>>>>comment that accounts of his death were greatly exaggerated. If, as some
>>>>people believe, a degree of symmetry in climate exists between the
>>>>hemispheres, which would appear to arise from the tropics, then the
>>>>existence of a Medieval Warm Period in the extra-tropics of the NH and SH
>>>>argues for its existence in the tropics as well. Only time and an enlarged
>>>>suite of proxies that extend into the tropics will tell if this is true.
>>>>
>>>>I hope that what I have written clarifies the rumor and expresses my views

>>>>more completely and accurately.

>>>>

>>>>Cheers,

>>>>

>>>>Ed

>>>>

>>>>=====

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>>>>=====

From: "Michael E. Mann" <mann@virginia.edu>
To: Ed Cook <drdendro@Ideo.columbia.edu>
Subject: Re: Comments on "Extending NAO Reconstructions ..."
Date: Thu, 17 May 2001 13:15:02 -0400
Cc: Juerg Luterbacher <juerg@giub.unibe.ch>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, Scott Rutherford <srutherford@virginia.edu>

<x-flowed>

Hi Ed,

On the road, but just had to chime into this debate briefly.

What you say is of course true, but we have to start somewhere. Step #1 is producing a reconstruction. Without some reasonable estimate of uncertainty, a reconstruction isn't very useful in my opinion. Step #2 is producing some reasonable estimate of uncertainty. In my mind, this is based on looking at the calibration residuals, seeing if they pass some basic tests for whiteness, normality, etc., looking at the verification statistics, and seeing if this continues to hold up in an independent sample. It is important to use the longest instrumental records we have for independent verification where possible. Of course, there may be additional biases in the predictors that are difficult to identify even in a relatively long verification interval (e.g., ultra low-frequency problems w/ fidelity). Step #3 is trying to evaluate this as best we can (looking at the frequency domain structure of the predictors themselves, seeing if there is loss of variance at very long timescales, looking at the robustness of long-term trends to standardization issues, etc.), etc...I see this as a successive series of diagnostics and self-consistency checks that iterate towards getting a reasonable handle on the uncertainties. This is the approach that we have taken, and I think it is the most appropriate...

I firmly believe that a reconstruction w/ out some reasonable estimate of uncertainty is almost useless! If the community wants to use paleodata for signal detection, model validation, etc. I believe that this is absolutely essential to do, whether or not we can do a perfect job.

I would be very surprised if Hans would disagree w/ my statement above!

anyways, my two cents on the matter...

mike

At 09:50 AM 5/17/01 -0400, Ed Cook wrote:

>Hi Juerg,

>

>I've done an admittedly quick read of your paper "Extending NAO
>Reconstructions Back to AD 1500" and find it to be fine overall. One slight
>correction on pg. 3 concerning the Cook et al. (1998) recon. The tree-ring
>records used also included some from England, as well as the eastern US and
>northern Fennoscandia. On pg. 10, sentence 8-9 in Conclusions, the wording
>is a little confusing. You say "Including station pressure of Gibraltar and
>Reykjavik as predictors in 1821 lead to a decrease of the confidence

>estimates". This almost sounds like you are doing worse when adding in
>Gibraltar and Reykjavik, when I know you mean the opposite. So, a change in
>wording to something like "... lead to increased confidence in the
>estimates of monthly NAO". Also in Table 1, is the Cullen R4 NAO
>reconstruction the one with instrumental data in it? If so, it has used
>some of the same data as yours. I don't recall if R4 is the one with
>instrumental data. But if it is, you ought to mention that.

>
>
>On a thematic note that doesn't have much direct bearing on the paper as it
>stands now (but which may be of interest to Keith, Phil, and Mike as well),
>I have growing doubts about the validity and use of error estimates that
>are being applied to reconstructions, such as those you have applied in
>Fig. 3. First, as you say at the end of the paper, there is a clear
>frequency dependence in the strength of relationship between the actual and
>proxy-estimated data that is not being considered, i.e. "the SE ... become
>smaller when considering low-pass filtered time series" (pg. 10). The
>assumption of the error estimates as now estimated and applied is that the
>error variance is truly white noise, i.e. equally distributed across all
>frequencies. That is surely not the case. This is different from questions
>about autocorrelated residuals, which tell you nothing about the frequency
>dependence of the quality of the estimates. This is where classic
>regression theory falls down. It is based on the notion that each
>observation is a random sample with no time history or frequency domain
>structure. When we use long time series of observations (climate or proxy)
>to reconstruct some climate variable, we are also using predictors that
>have time series structure and history that cannot vary in a completely
>random fashion even if the data could be completely resampled. This is
>because they represent a series of prior "observations" of
>climatic/environmental conditions. This lack of randomness of the
>observations used for reconstructing past climate again causes me to doubt
>the validity of the error estimates being applied. The degree to which the
>reconstruction can actually vary from year to year within the prescribed
>error limits is itself constrained by the time history of the observations
>themselves used for reconstruction. In contrast, the 2SE limits in your
>Fig. 3 prior to 1821 contain almost all of the estimates. This result could
>be used to claim that there is effectively no useful time history of
>variation in the NAO reconstruction prior to 1821 because each estimate may
>fall with equal probability anywhere in the error envelop. I would regard
>this interpretation as completely wrong. Thus, I would say that the decadal
>period of above-average winter NAO in your reconstruction around AD 1700 is
>real, assuming that the predictors used are providing unbiased estimates,
>even though it is fully enclosed by the 2SE limits that intersect zero.
>This is getting towards the debate with Von Storch over "most probable"
>estimates. I am probably not explaining myself well here and undoubtedly
>need to think more about it. But I really think that error bars, as often
>presented, may potentially distort and unfairly degrade the interpreted
>quality of reconstructions. So, are the error bars better than nothing? I'm
>not so sure.

>
>Cheers,

>
>Ed

>
> >Hello Ed
> >
> >thanks very much for your nice mail. I hope these little
> >comments were useful for you and yes of course
> >we hope too that we can merge the data base sometime
> >later on. This would be great.
> >
> >Do you think that you could send me some comments
> >on our paper by tomorrow?
> >Is your paper for the Orense book?
> >
> >Many greetings and till later
> >
> >Juerg
>
>
>=====

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From: "Michael E. Mann" <mann@virginia.edu>
To: christy@nsstc.uah.edu
Subject: Re: FYI: Fwd: Re: IPCC
Date: Thu, 24 May 2001 11:33:02 -0400
Cc: rbradley@geo.umass.edu, tkarl@ncdc.noaa.gov, tom crowley
<tom@ocean.tamu.edu>, mhughes@ltrr.arizona.edu, jto@u.arizona.edu,
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<x-flowed>

John:

For future reference, I think its also important to clarify for you what the Dahl-Jensen, Clow et al borehole results actually show (see Dahl-Jensen et al, "Past Temperatures Directly from the Greenland Ice Sheet", Science, 282, October 1998).

In fact, the results show that the amplitude of variability over the past 1000+ years differs by a factor of 2 between the GRIP and Dye 3 borehole estimates (the latter only 865 km to the south). This is an example of extreme regional-scale variability, which should give pause to those who want to draw large-scale inferences.

However, even more importantly, they show in the case of Dye 3, the mid 20th century warm period in the record actually exceeds the Medieval warm peak! (see Fig 4, lower panel, blue curve). So here we have two temperature histories less than 1000 km apart in Greenland, which give different stories regarding the level of Medieval warmth, with at least one of the histories conforming precisely to the hemispheric trends presented in IPCC chapter 2 (note that in the chapter, we actually discuss the evidence of conflicting temperature trends in Greenland, though not specifically referring to Dahl-Jensen et al).

So do I understand correctly that you are referring to the results of Dahl-Jensen et al as conflicting with what we say in the chapter? At the face of it, this argument has no merit whatsoever. I think we should all use a better explanation from you, since you seem to be arguing publically that the Dahl-Jensen et al record undermines what we've said in the chapter.

Thanks in advance,

mike

p.s. I've cc'd in Eric Steig, a collaborator of Clow's and a Greenland & Antarctic Ice Core expert, to make sure my facts above have been presented accurately. Perhaps Eric would be kind enough to forward my email to Gary

Clow, and Gary can let us know directly if he disagrees with any of my remarks above.

At 03:30 PM 5/23/01 -0400, Michael E. Mann wrote:

>John,

>

>I appreciate your reply.

>

>However, I don't agree at all w/ your assessment. It was determined early

>on that the ice core borehole results would be discussed in the context of

>the millennial-scale variability section, as they arguably don't have the

>resolution to address the timescales relevant to the past 1000 years. So

>this was in Jean's domain, not mine, and if the cross-references between

>the sections aren't clear enough in that regard, that is indeed our fault.

>

>However, there is considerable discussion of the fact that the

>Arctic/North Atlantic regions are inappropriate for inferences into

>hemispheric-scale temperature patterns, and this remains fundamentally

>from any reasonable treatment of the underlying climate dynamics that

>influence that region.

>

>The various hemispheric temperature reconstructions discussed in our chapter (the emphasis was on the commonality between them), including

Mann

>et al, Jones et al, Briffa et al, Crowley and Lowery, and others, make

>considerable use of just about all of the available reliable low-res and

>high-res paleo data available, and come to a clear consensus regarding the

>relative warmth of the Medieval period at the hemispheric/global scale.

>Crowley's modeling results come to the same conclusion, and it entirely

>independent of

>any empirical paleoclimate reconstructions.

>

>You misrepresent the Mann et al reconstruction--it is not based on "tree rings", but uses all high-resolution proxy information commonly

available.

>We have shown, in fact, that our reconstruction is robust to the

>inclusion/exclusion of tree ring information. The Crowley and Lowery

>reconstruction, which is discussed in our chapter, makes use of almost no

>tree ring data, and employs lower-resolution proxy indicators, including

>the very records (Keigwin, Lamb's central England temperature record,

>GISP2 018) that are often used to argue for a warmer MWP, and yet comes to

>the same conclusion. And Tom shows that when averaged across the

>hemisphere, a warmer-than-present-day MWP just doesn't hold up.

>

>Our treatment of this subject in the chapter was far more careful, far

>more inclusive and detailed, and far more nuanced than you give us

credit

>for. Your comments below remain disturbingly selective and myopic, and we
>have dealt w/ similar comments many times over...
>
>If ABC is looking to do a hatchet job on IPCC so be it (this doesn't
>surprise me--Stossel has an abysmal record in his treatment of
>environmental issues, from what I had heard), but I'll be very disturbed
>if you turn out to have played into this in a way that is unfair to your
>co-authors on chapter 2, and your colleagues in general. This wouldn't
>have surprised me coming from certain individuals, but I honestly
expected
>more from you...
>
>Mike
>
>>Date: Wed, 23 May 2001 13:50:49 -0500
>>From: John Christy <christy@nsstc.uah.edu>
>>X-Mailer: Mozilla 4.04 (Macintosh; I; PPC)
>>To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
>>Subject: Re: IPCC
>>
>>Hi Mike:
>>
>>Here's what happened. ABC News 20/20 with Stossel wanted me to be part
>>of a segment that will air at the end of June on the climate change
>>issue. Specifically the piece will be dealing with the alarmist
>>rhethoric that tends to be found in the media. I am more than happy to
>>talk about that because I've been very disappointed with what has gone
>>on even with respect to some of the IPCC elders and their
pronouncements
>>for forthcoming disasters.
>>
>>In one of the pre-interviews they asked about the "Hockey Stick". I
>>told them of my doubts about the intercentury precision of the record,
>>especially the early part, and that other records suggested the period
>>1000 years ago was warmer. I remember saying that "you must give the
>>author credit for including the large error bars for that time series
in
>>the figure." I also specifically said that the most precise record of
>>century scale precision, Greenland Borehole temps, was very important
to
>>note but that the figure was not in the IPCC. I then looked quickly at
>>the IPCC reference list and saw the citation of Dahl-Jensen and assumed
>>that it was at least commented on in the 1000 year time series material
>>and told ABC as much.
>>
>>ABC called back a few days later and said they couldn't find a
reference
>>to the Greenland stuff in the IPCC discussion of the past 1000 years.
>>So I read the final version, and ABC was right. I said this was an
>>omission that should not have happened - and that I take part of the
>>blame because I had mentioned it at each of our Lead Author meetings.
>>
>>Last Thursday night, I was one of the guys flown to NY City for the

>>taping of the show. There was only one question on this particular
>>issue (it was even after Stossel had left the room) and I gave much the
>>same answer as I indicated above (as best as I can remember)- that the
>>"Hockey Stick" (I don't think I used the term "Hockey Stick", and I'm
>>almost positive I did not mention your name at any point) is one
>>realization of temperatures but that other data are not included and
>>that I had thought the "other" data were clearly mentioned in the IPCC,
>>but weren't. I mentioned the large error bars (as a credit to you) and
>>that I was partly to blame for this omission. If they use my remark,
>>they could slice and dice it to make it as provocative as possible.

>>

>>Four of us were taped for almost 2 hours, and from this they will
select

>>about 8 minutes, so I doubt my remarks will make the show. When
Stossel

>>came back in after all was said and done, he said to me that I might be
>>a good scientist but I didn't have the emotion and passion necessary to
>>excite the audience. In one way, that is a compliment I suppose. I
>>think Pat M. will have a good chunk of air time (I don't remember
>>whether he added any comments on the 1000-year time series, but he may
>>have).

>>

>>Whatever is shown, just keep it in context. There is no way a clear
>>scientific point with all the caveats and uncertainties can come across
>>in such venues. However, I do agree with Stossel's premise (though I
>>don't know what the piece will actually look like so I may be
>>disappointed) that the dose of climate change disasters that have been
>>dumped on the average citizen is designed to be overly alarmist and
>>could lead us to make some bad policy decisions. (I've got a good
story

>>about the writers of the TIME cover piece a couple of months ago that
>>proves they were not out to discuss the issue but to ignore science and
>>influence government.)

>>

>>It is not bad science to look at arguably the most precise measure of a
>>point temperature (actually two boreholes) when that point shows a 600+
>>year period of greater warmth than today. On that time scale, the
>>equivalent spatial scale is much larger than any of the regional
>>oscillations we now identify. But, there are several other (admittedly
>>less robust) measures that suggest greater warmth 1000 years ago that
>>are outside the N. Atlantic area. I just don't think tree rings, if
>>averaged over a century, can tell us which century was warmest. We've
>>never had two complete, independent centuries of global instrumental
>>data (separated by more than one century) to even test this idea. (By
>>the way, I came to my own conclusions long before Broekers piece
>>appeared.) This is an area of further work that I promoted to the NRC
>>about 2 months ago (more funding for Paleo work to assess intercentury
>>precision of all proxy records.)

>>

>>Regarding the IPCC. The IPCC TAR is good, but it is not perfect nor
>>sacred and is open to criticism as any document should be. In some
>>cases it is already outdated. Some of the story lines used to generate
>>high temperature changes are simply ridiculous. The IPCC is us. We
are

>>under no gag rule to keep our thoughts to ourselves. I thought our
>>chapter turned out pretty good overall, and I attribute that to the
>>open, working relationship we all had (some other chapter groups did
not
>>experience this) and to the tireless efforts of our convening lead
>>authors.

>>

>>Good to hear from you.

>>

>>John C.

>>

>>

>>--

>>*****

>>John R. Christy

>>Director, Earth System Science Center voice: 256-961-7763

>>Professor, Atmospheric Science fax: 256-961-7751

>>Alabama State Climatologist

>>University of Alabama in Huntsville

>><http://www.atmos.uah.edu/atmos/christy.html>

>>

>>Mail: University of Alabama in Huntsville, Huntsville AL 35899

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>

>

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</x-flowed>

From: Kevin Trenberth <trenbert@cgd.ucar.edu>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fwd: Recent Paper from the Competitive Enterprise Institute
Date: Thu, 24 May 2001 11:35:06 -0600 (MDT)
Reply-to: <trenbert@ucar.edu>
Cc: <rbradley@geo.umass.edu>, <tkarl@ncdc.noaa.gov>, tom crowley <tom@ocean.tamu.edu>, <mhughes@ltr.arizona.edu>, <jto@u.arizona.edu>, <rbradley@geo.umass.edu>, <p.jones@uea.ac.uk>, <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>

Mike:

You are right: this is a disinformation campaign.
Some remarks

1) On the Christy et al grl paper, I sent the following to John following the IPCC Shanghai mtg.:

Date: Mon, 22 Jan 2001 15:39:20 -0700 (MST)
From: Kevin Trenberth <trenbert@cgd.ucar.edu>
To: John Christy <christy@atmos.uah.edu>
Subject: your grl paper

John:

Just back from IPCC. One surprise was the strong Saudi delegation distributed your recent grl paper and wanted it inserted into the SPM! In spite of the fact that you are a lead author on Chapter 2, the paper is referenced, etc. In fact Simon Brown was there.

Chris Folland made a comment about his hypothesis for this: related to changes/growth in ships. My hypothesis focusses on the buoy data. See our recent paper submitted to jgr:

<http://www.cgd.ucar.edu/cas/papers/jgr2001b/jgr2.html> also

http://www.cgd.ucar.edu/cas/papers/jgr2001a/jgr_interann.html

This shows that during and following El Nino there is an anomalous flux of heat out of ocean into atmosphere in the east Pacific of order 50 W m⁻² over many months: so ocean T warms relative to air. During La Lina flux goes other way. i.e. air warms relative to ocean.

So your results must be affected by 1997-98 event at end of series and that may explain trend differential.

Hope this helps
Regards
Kevin

i.e. the result is not as advertized.

=====

2) wrt Lindzen's paper

Here is the text from my recent Senate testimony

The determination of the climatic response to the changes in heating and cooling is complicated by feedbacks. Some of these can amplify the original warming (positive feedback) while others serve to reduce it (negative feedback). If, for instance, the amount of carbon dioxide in the atmosphere were suddenly doubled, but with other things remaining the same, the outgoing long-wave radiation would be reduced and instead trapped in the atmosphere. To restore the radiative balance, the atmosphere must warm up and, in the absence of other changes, the warming at the surface and throughout the troposphere would be about 1.2\deg C. In reality, many other factors will change, and various feedbacks come into play, so that the best IPCC estimate of the average global warming for doubled carbon dioxide is 2.5\deg C. In other words, the net effect of the feedbacks is positive and roughly doubles

the response otherwise expected. The main positive feedback comes from increases in water vapor with warming.

In 2001, the IPCC gave special attention to this topic. The many issues with water vapor and clouds were addressed at some length in Chapter 7 (of which I was a lead author, along with Professor Richard Lindzen (M.I.T.), and others). Recent possibilities that might nullify global warming (Lindzen 2001) were considered but not accepted because they run counter to the prevailing evidence, and the IPCC (Stocker et al., 2001) concluded that "the balance of evidence favours a positive clear sky water vapour feedback of the magnitude comparable to that found in the simulations."

===

Here is a more complete rebuttal, written March 23 to MacCracken.

Subject: Re: Recent Lindzen paper

Kevin Trenberth

1) The paper is based on very simple conceptual ideas that do not mesh with reality. Fig. 2 is simply not correct. For a more correct view of the overturning see:

Trenberth, K. E., D. P. Stepaniak and J. M. Caron, 2000: The global monsoon as seen through the divergent atmospheric circulation. {J. Climate}, 13, 3969--3993.

This paper also shows that the flow in the tropics is dominated by transients (and thus mixing) of all kinds. The mean overturning is only about a third of the daily mean variance for a month and much less if the intra diurnal variations and interannual variations are included.

2) The "observations" analysis makes absolutely no sense to me at all. There is a totally inadequate description of what is done and no way to decipher what a dot in Fig 5 or Fig 6 is. Given 20 months, and daily values (how was that done?) why are there only about 330 points? Why isn't Fig 6 part of Fig. 5?

In any event the results are totally at odds with other evidence. Here I refer to the Goes Precipitation Index which uses 3 hourly data on OLR, and thus on high cloud, as an index of rainfall, and it is clear from many studies that OLR generally decreases (convection and high cloud increase) with SST, the reverse of the relationship in Fig. 5.

Moreover the whole conceptual basis for anything here is surely flawed. As stated, on short time scales SST is not changing. But clouds are NOT caused by local SST, rather they arise from either transients, like the MJO, or for the ITCZ and SPCZ (which are major operators in this region), they come from moisture convergence ($P \gg E$) and so it is the patterns of SST (gradients) as well as where the warmest water is that determines where the convergence and clouds occur. Now in the warm pool, the convergence is focussed more on the edges, as that is where the pressure gradients are greater, and so the convergence is not where SST is necessarily highest.

In any case, moisture is not equal to cloudy air. Many analyses show that moisture is much more extensive, see for example Trenberth, K. E., and C. J. Guillemot, 1998: Evaluation of the atmospheric moisture and hydrological cycle in the NCEP/NCAR reanalyses. {Climate Dyn.}, {14}, 213--231.

Even with such results, other factors need to be considered.

One process might be

High SST => convergence => rainfall and cloud

OR

Less cloud => more solar radiation => higher SST

Those give opposite relations and both operate. The latter is more important in the Indian Ocean where subsidence (from the Pacific) dominates. However, it also operates over the oceans in the region in question in northern summer, because that is the monsoon season, and the main convection is over land, meaning subsidence over the ocean.

None of this is sorted out in any way in this paper.
In fact it is so bad in this regard I do not know how it got published.

In Fig 5 etc, no correlations are given, nor are their significance levels. My rough estimate is that the correlation is about 0.2 to 0.3 and that is significant if the 330 or so points are independent. But why should I have to guess at that.
Again I would question the editorial and review process.

3) Finally, I refer you to chapter 7 of IPCC which is a more balanced assessment. Lindzen was a coauthor of that with me and others. Lindzen wrote 7.2.1 and the same figure 1 in the BAMS article was included as 7.1 in chapter 7 along with similar ones from models, showing that these things are fully simulated in good models, although better with higher resolution. Anyway, his arguments were fully considered in chapter 7 and you can read it to see the result. The whole of 7.2.1, including 7.2.1.1, 7.2.1.2 and 7.2.1.3 was put together originally by Lindzen, Pierrehumbert and Le Treut, but basically the final version was rewritten by me to provide better balance. Pierrehumbert is an agnostic of sorts: disbelieves everything including models but seems to have faith in simple theories. Le Treut was sound on the modeling. I did not change the substance of what they prepared, I did reshape it and polish and it ended up in a form they accepted.

Note at the end it clearly states:
"the balance of evidence favours a positive clear sky water vapour feedback of the magnitude comparable to that found in the simulations."

The 4 subsections together are quite long and thoroughly air the issue, much more so than any previous IPCC report. For those of you who do not have it: 7.2.1 "Physics of the water vapour and cloud feedbacks" (draft written by Lindzen) is 1.3 pages, 7.2.1.1 (I think Pierrehumbert) "Water vapour feedback", is 1 page, 7.2.1.2 "Representation of water vapour in models" is 1.5 pages (Le Treut) and 7.2.1.3 "Summary on water vapour feedbacks" is half a page or so.

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On Thu, 24 May 2001, Michael E. Mann wrote:

> FYI. I received this from a colleague. This gives you some idea of who is
> behind this latest disinformation push.
>
> A note to all regarding the Broecker piece, which has been heavily referred
> to in this and other similar recent pieces (though it is an opinion piece,
> and not peer-reviewed).
> A response by Bradley, Briffa, Crowley, Hughes, Jones, and Mann appears in
> tomorrow's issue of "Science". This response simply points out that old
> fallacies that are simply reiterated in Broecker's piece...
>
> mike
>
>

>> remains "feedback" effects on the climate. The conventional
>>explanation
>> by proponents of global warming theory always assumes that
>> human-induced increases in atmospheric concentrations of
>>greenhouse
>> gases, primarily carbon dioxide, could lead to catastrophic
>>warming of
>> the planet. Man-made greenhouse gas emissions, however, are
>>only an
>> indirect cause of the forecasted warming. A doubling of
>>carbon dioxide
>> concentrations alone would lead to slight warming of about
>>one degree
>> Celsius (1.8 degrees Fahrenheit) over the next 100 years.
>>This small
>> amount of warming, according to standard global warming
>>theory, speeds
>> up evaporation, thereby increasing the amount of water vapor
>>(a major
>> greenhouse gas) in the atmosphere. This "positive water
>>vapor feedback"
>> effect is where most of the predicted warming comes from.
>>This
>> assumption has never been tested.
>>
>> A recent study in the Bulletin of the American Meteorological
>>Society
>> suggests that the reverse is true.[2] The authors find a
>>negative water
>> vapor feedback effect that is powerful enough to offset all
>>other positive
>> feedbacks. Using detailed daily observations of cloud cover
>>from
>> satellites in the tropics and comparing them to sea surface
>>temperatures,
>> the researchers found that there is an "iris effect" in which
>>higher
>> temperatures reduce the warming effect of clouds.
>>
>> According to a NASA statement about the study, "Clouds play a
>>critical
>> and complicated role in regulating the temperature of the
>>Earth. Thick,
>> bright, watery clouds like cumulus shield the atmosphere from
>>incoming
>> solar radiation by reflecting much of it back into space.
>>Thin, icy cirrus
>> clouds are poor sunshields but very efficient insulators that
>>trap energy
>> rising from the Earth's warmed surface. A decrease in cirrus
>>cloud area
>> would have a cooling effect by allowing more heat energy, or
>>infrared
>> radiation, to leave the planet."[3]
>>
>> The researchers found that a one degree Celsius rise in ocean
>>surface
>> temperature decreased the ratio of cirrus cloud area to
>>cumulus cloud
>> area by 17 to 27 percent, allowing more heat to escape.
>>
>> In an interview, lead author Dr. Richard S. Lindzen said the
>>climate
>> models used in the IPCC have the cloud physics wrong. "We
>>found that
>> there were terrible errors about clouds in all the models,
>>and that that will
>> make it impossible to predict the climate sensitivity because

>>the
>> sensitivity of the models depends primarily on water vapor
>>and clouds.
>> Moreover, if clouds are wrong, there's no way you can get
>>water vapor
>> right. They're both intimately tied to each other." Lindzen
>>argues that
>> due to this new finding he doesn't expect "much more than a
>>degree
>> warming and probably a lot less by 2100." [4]
>>
>> The study is the best empirical confirmation to date of the
>>negative
>> feedback hypothesis proposed by Lindzen early on in the
>>global warming
>> debate. It builds on earlier empirical work by Drs. Roy
>>Spencer of NASA
>> and William Braswell of Nichols Research Corporation. Their
>>1997 study
>> also cast doubt on the assumption of a positive water vapor
>>feedback
>> effect. [5] They found that the tropical troposphere, the
>>layer of air
>> between 25,000 and 50,000 feet, is much dryer than climate
>>modelers
>> previously thought. Further empirical work will no doubt
>>confirm whether
>> this phenomenon is common throughout the tropics, which act
>>as the
>> Earth's exhaust vents for escaping heat.
>>
>>
>> Black Carbon. In 1995, the IPCC had to explain in its Second
>> Assessment Report why its previous predictions of global
>>temperature
>> change were nearly three times larger than observed in the
>>actual
>> temperature record. The SAR concluded that emissions of
>>sulfate
>> aerosols from burning coal were offsetting the warming that
>>should be
>> caused by carbon dioxide levels in the atmosphere. Sulfate
>>aerosols,
>> according to this explanation, reflect incoming solar
>>radiation back to
>> space, thereby cooling the planet.
>>
>>
>> The TAR takes the sulfate aerosol idea even further. The SAR
>>had
>> predicted a temperature rise of 1 to 3.5 degrees C (1.8 to
>>6.3 degrees F)
>> over the next 100 years. The TAR goes even further,
>>anticipating a 1.4 to
>> 5.8 degrees C (2.52 to 10.44 degrees F) rise in temperature.
>>The
>> extreme case scenario of a 5.8 degrees C of warming, for
>>instance, is
>> based partly on assumptions that the whole world will raise
>>its level of
>> economic activity to that of the U.S., will equal U.S. per
>>capita energy
>> use, and energy use will be carbon intensive. The primary
>>assumption
>> behind the new scenario, however, is that sulfate aerosol
>>emissions will
>> be eliminated by government regulation, giving carbon dioxide
>>free

>> reign.[6]
>>
>> Sulfate aerosols, then, are a key component of catastrophic
>>global
>> warming scenarios. Without them, the IPCC cannot explain why
>>the
>> earth is not warming according to their forecasts, nor can
>>they
>> reasonably claim that global warming will lead to
>>catastrophes of biblical
>> proportions.
>>
>> A new study in Nature eliminates sulfate aerosols as a
>>corrective for the
>> models. [7] The author, Mark Jacobson, a professor with the
>>Department
>> of Civil & Environmental Engineering at Stanford University,
>>examines
>> how black carbon aerosols affect the Earth's climate. Unlike
>>other
>> aerosols that reflect solar radiation back into space, black
>>carbon (soot)
>> absorbs solar radiation, thereby raising atmospheric
>>temperatures.
>>
>> Until now the warming influence of black carbon was thought
>>to be minor,
>> leading researchers to ignore it. James Hansen, with the
>>Goddard
>> Institute for Space Studies, in a paper published in August
>>2000, first
>> suggested that black carbon plays an important role in global
>> warming.[8] Jacobson found "a higher positive forcing from
>>black carbon
>> than previously thought, suggesting that the warming effect
>>from black
>> carbon may nearly balance the net cooling effect of other
>>anthropogenic
>> aerosol constituents."
>>
>> There you have it. Soot offsets the cooling effect of other
>>aerosols,
>> meaning we are back at square one. Scientists still do not
>>have a
>> plausible explanation for why the Earth has failed to warm in
>>line with
>> climate model results. Indeed, all the prognostications of
>>the IPCC are
>> wrong if the Nature study is right.
>>
>>
>> Natural Cycles. The main propaganda device of the TAR is the
>>"hockey
>> stick graph." The graph is a temperature record derived from
>>tree rings
>> dating back to 1000 AD and running through 1900, with the
>>20th century
>> thermometer-based temperature data attached at the end.[9]
>>It claims to
>> show that global temperatures have remained steady or even
>>decreased
>> during the last millennium until the industrial age, when
>>there was an
>> anomalous warming represented by the blade of the hockey
>>stick. The
>> hockey stick is largely bogus, however. The margin of error
>>is so large
>> that nearly any temperature trend could be drawn to fit

>>within it.
>>
>>
>>
>> The hockey stick features prominently in all of IPCC Chairman
>>Robert
>> Watson's speeches, and to the uninitiated it is very
>>persuasive. Senator
>> John McCain (R-AZ), for example, expressed alarm when he saw
>>the
>> graph at Commerce Committee hearings last May.
>>
>>
>> Watson uses the hockey stick to claim that current warming is
>>greater
>> than at any other time in the last 1,000 years. The Medieval
>>Warm
>> Period (MWP) and the Little Ice Age (LIA) were two naturally
>>occurring
>> events during the last millennium where the range of global
>>temperature
>> change exceeded that of the 20th century. During the MWP,
>>global
>> temperatures were higher than they are today. The MWP,
>>however, does
>> not show up in the hockey stick graph.
>>
>> The hockey stick has effectively been dismantled in a recent
>>study in
>> Science, however.[10] Wallace Broecker, of the
>>Lamont-Doherty Earth
>> Observatory, argues that the MWP and the LIA were indeed
>>global
>> phenomena. Referring to the hockey stick, Broecker notes, "A
>>recent,
>> widely cited reconstruction leaves the impression that the
>>20th century
>> warming was unique during the last millennium. It shows no
>>hint of the
>> Medieval Warm Period (from around 800 to 1200 A.D.) during
>>which the
>> Vikings colonized Greenland, suggesting that this warm event
>>was
>> regional rather than global. It also remains unclear why just
>>at the dawn
>> of the Industrial Revolution and before the emission of
>>substantial
>> amounts of anthropogenic [manmade] greenhouse gases, Earth's
>> temperature began to rise steeply."
>>
>>
>> Broecker reviewed several scientific studies which
>>reconstruct the Earth's
>> temperature history into the distant past using various
>>proxies. He
>> concludes, "The post-1860 natural warming was the most recent
>>in a
>> series of similar warmings spaced at roughly 1500-year
>>intervals
>> throughout the present interglacial, the Holocene." [11] In
>>other words,
>> the current warm period may just be attributable to natural
>>cycles.
>>
>>
>>
>> Flawed Temperature Data. The National Oceanic and
>>Atmospheric
>> Administration (NOAA) claimed that the year 2000 was the

>>sixth
>> warmest since 1880. Other temperature records find less
>>warming.[12]
>> Last year was only the 14th warmest, or 9th coolest, year
>>since 1979
>> according to the satellite temperature record,[13] and only
>>the 9th
>> warmest, according to records that include only measurements
>>from
>> meteorological stations.[14]
>>
>> The NOAA data, which is cited by government officials and the
>>news
>> media, may be the least accurate, according to a study that
>>recently
>> appeared in Geophysical Research Letters.[15] The NOAA
>>datasets "are
>> a mixture of near-surface air temperatures over land and sea
>>water
>> temperatures over oceans," according to lead author Dr. John
>>Christy,
>> professor of atmospheric science and director of the Earth
>>System
>> Science Center at the University of Alabama in Huntsville.
>>
>> Since actual air temperature data over many large ocean areas
>>are
>> nonexistent, the NOAA uses sea surface temperatures as a
>>"proxy,"
>> assuming that sea surface temperatures and air temperatures
>>move in
>> lock step. This is not the case, according to the data
>>compiled by
>> Christy and his colleagues at the Hadley Centre of the United
>>Kingdom's
>> Meteorological Office, who worked on the study. The
>>researchers used
>> buoy data in the tropical Pacific Ocean to compare "long-term
>>(8-20 year)
>> trends for temperatures recorded one meter below the sea
>>surface and
>> three meters above it."
>>
>> What they found was a significant discrepancy. "For each
>>buoy in the
>> Eastern Pacific, the air temperatures measured at the three
>>meter height
>> showed less of a warming trend than did the same buoy's water
>> temperatures at one meter depth," the study said. The
>>difference is a
>> near-surface seawater warming trend of 0.37 degrees C per
>>decade and
>> an air temperature trend of only 0.25 degrees C per decade
>>during the
>> 20-year period tested. Replacing the sea surface
>>temperatures with the
>> air temperature data reduces the Earth's global warming trend
>>by a third,
>> from 0.19 to 0.13 degree C per decade.
>>
>> This is significant due to difficulties with reconciling the
>>various global
>> temperature data sets, particularly the discrepancy between
>>tropospheric
>> temperatures measured by satellites that show little to no
>>warming, and
>> the surface-based temperature data that show slightly more
>>warming.

>> Last year, the National Research Council stated that both
>>temperature
>> records are correct and speculated about an explanation.[16]
>>
>> This brings up another problem, however. The standard
>>explanation of
>> the greenhouse effect suggests warming occurs first five
>>kilometers
>> above the earth's surface in the atmospheric layer known as
>>the
>> troposphere. How events at the surface are connected to what
>>happens
>> high in the atmosphere is not clear, but it is believed that
>>surface
>> warming would follow tropospheric warming through climatic
>>processes
>> such as air circulation.[17] If both temperature records are
>>correct, then
>> this explanation of the greenhouse effect is wrong. Christy
>>et al. brings
>> the surface temperature data into closer agreement with the
>>satellite
>> data, suggesting that a better explanation for the
>>discrepancy is flawed
>> surface data.
>>
>> Progressive Science. At a press conference at the National
>>Press
>> Club on April 18, Mr. Jan Pronk, chairman of the Sixth
>>Conference of the
>> Parties of the United Nations Framework Convention on Climate
>>Change
>> said most issues were still on the table in the ongoing Kyoto
>>negotiations
>> but the scientific basis of catastrophic global warming could
>>not be
>> questioned. That would be like going back ten years, he
>>said. This is a
>> myopic and erroneous view of science. Science is not static
>>but
>> dynamic. It reaches tentative conclusions at best, and those
>> conclusions constantly give way to new data. The IPCC is a
>>static
>> process, however. The Third Assessment Report is already
>>obsolete and
>> it has not even been released yet. With these four recent
>>studies, it may
>> be time to bid catastrophic global warming theory a warm
>>farewell.
>>
>>
>>
>>
>>
>> [1] "Evidence of Rapid Global Warming Accepted by 99 Nations,"
>>Environment News Service, January 22,
>> 2001.
>> [2] Richard S. Lindzen, Ming-Dah Chou, and Arthur Y. Hou, "Does the
>>Earth Have an Adaptive Infrared Iris?,"
>> Bulletin of the American Meteorological Society, 82:417-32, March
>>2001.
>> [3] <ftp://www.gsfc.nasa.gov/pub/PAO/Releases/2001/01-18.htm>
>> [4] "Is Globe Warming? Sure, But Far Less than Alarmists Say,"
>>Tech Central Station
>> (<http://www.techcentralstation.com/BigShotFriday.asp>), March 5,
>>2001.
>> [5] Roy W. Spencer and William D. Braswell, "How Dry is the
>>Tropical Free Troposphere? Implications for

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Stepan Shiyatov <stepan@ipae.uran.ru>
Subject: Re: Article and money
Date: Fri Jun 8 13:38:08 2001

Stepan

it is just pressure of work. I am afraid the final report did not go to INTAS . I will do it this week! I still expect we will get the money outstanding - just late . Sorry.

Keith

At 02:22 PM 5/31/01 +0600, you wrote:

Dear Keith,

Thank you for the print of collaborative article published in the J. of Geophysical Research I have received some days ago. The article is very interesting and, I think, these reconstructions will be used by many researchers of different disciplines.

At the end of the last year Janet asked me to send the account of the bank to transfer the rest money of the INTAS project (737 Euros). I have sent you the necessary form to transfer the money for my name, but the Ekaterinburg Branch of Bank for Foreign Trade did not receive the money until now. Do you know the reason?

This summer I am very busy. Along with Fritz Schweingruber and his team (four persons) we will visit many sites (using helicopter) on the North of European and Siberian Arctic and Subarctic (from the Lower of Pechora river in the west to the Lower of Khatanga river in the East). We will try to find a new sources of subfossil wood material between the Yamal Peninsula and Taimyr Peninsula, on the one hand, and between the Yamal Peninsula and Kola Peninsula, on the other hand. The second aim is to collect samples from living trees of different ages for estimating biomass changes.

After this trip I and my post-graduate student will be working in the Polar Urals (large-scale mapping of forest-tundra ecosystems over the forest-tundra ecotone for three time intervals: the beginning of the XXth century, the 1960ties and the 2000ties.

At the end of September I intend to be in Davos.

Best Regards,

Stepan

stepan@ipae.uran.ru

--

Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
Norwich, NR4 7TJ, United Kingdom

Phone: +44-1603-593909 Fax: +44-1603-507784

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>, Thomas R Karl <Thomas.R.Karl@noaa.gov>
Subject: Re: NRC report on climate change
Date: Tue, 12 Jun 2001 08:46:36 +0100
Cc: trenbert@ucar.edu,"Michael E. Mann" <mann@virginia.edu>, rbradley@geo.umass.edu,tom crowley <tom@ocean.tamu.edu>, mhughes@ltrr.arizona.edu,k.briffa@uea.ac.uk, Folland Chris <ckfolland@meto.gov.uk>

<x-flowed>

Dear All,

I'd just like to echo all the points made by Mike and Kevin. The logic behind saying that there isn't enough paleo data before 1600 yet there may have been even early millennia which experienced warming of almost 2 C per millennium escapes me. As Kevin points out they have mixed up all the various factors that force climate on interannual to intermillennial timescales. One of the main points of IPCC is to synthesize the science, with particular reference to potential future changes. Changes in the distant past (glacial and deglaciation) are of less relevance to the 21st century because of differences in boundary conditions. The last few hundred to a thousand years are clearly more important to the near future. At least from my quick reading there seems no explicit reference to changes in the thermohaline circulation.

Perhaps the paleo people on this list need to redouble their efforts to emphasize the importance of the last few thousand years, stressing absolute dating, calibration and verification. Another issue that is mixed up in the report (apart from the forcing) is spatial scales. I will try and address these at the Chicago meeting. What are the 4 useful sites ?

I just hope in the US that people read the full IPCC reports and the summaries, rather than this hastily cobbled together document. I also hope that Europeans don't read it. It has already got some air time here and may get some more with Bush here this week. Issues like star wars and capital punishment were commented upon whilst I came to work. Kyoto wasn't mentioned.

Cheers
Phil

At 10:45 11/06/01 -0400, Michael E. Mann wrote:

>Hi Tom,
>
>Thanks for your message. I know how hard you worked to make the report as
>balanced as possible, and realize this experience must have been a bit
>frustrating for you, after all the careful and hard work you and Chris put

>into our IPCC chapter. While the idea that the limited panel involved in
>the NAS report can provide an improved or more objective assessment of the
>science relative to IPCC seems, of course, ridiculous to a lot of us. But
>I'm very thankful you were on the panel. Needless to say, my criticism
>below is in no way directed towards you, but rather some of the other
>panel members whom I think did a real injustice to the science.

>
>Having seen the list of authors and reviewers of the report, I think I
>have a pretty good idea what the source of a good deal of that skepticism
>is and I think much of it is spurious and unfair. There are legitimate
>caveats and uncertainties--I think we've been very honest about these in
>our publication, and we (as Phil, Keith, and others) are working earnestly
>to improve the reconstructions. But the claims we make (e.g. the
>anomalousness of recent warmth) are guided by the substantial
>uncertainties in the reconstructions, which of course take into account
>uncertainty due to increasingly sparse information back in time, and I
>have yet to see any legitimate argument that our reconstruction (or Phils,
>Toms, Keiths, etc.) is "wrong" within the context of the diagnosed
>uncertainties. Unfortunately, much of the criticism that has been advanced
>recently is knee-jerk and unsubstantiated, particularly with regard to
>dendroclimatological issues (which Malcolm and Keith can comment on best).
>Much of this has to do w/ a lack of understanding of tree ring information
>(to be honest Tom, I didn't see one name in the list of authors or
>reviewers of the NAS report whom I think is qualified to comment on
>dendroclimatological climate reconstruction and its strengths and
>weaknesses, and that is a real problem. In such a vacuum it is easy, for
>example, for Wally to wave around some highly non-standard, un
>peer-reviewed tree-ring analysis that he has been promoting (which Ed Cook
>himself, a co-author on this, admits makes use of a questionable
>standardization approach), in an attempt to dismiss all other climate
>reconstructions which use tree ring information.

>
>The criticism that there are only "4 useful sites" for reconstructing
>climate over the past 1000 years is especially irksome and ignorant. Does
>Tom C. agree that there are only 4 meaningful records that contribute to
>his reconstruction? Does Phil, or Keith? Where does that number come from?
>The same source as R.L.'s GHG sensitivity factor of 1.0 (i.e., the ether)
>I suspect.

>
>The discussion of paleo in the report (which I realize you had very
>limited control over) is disturbingly misleading and flawed to many of us
>who actually work in this area. There are throwaway statements about
>millennial trends of 2 C in global temperatures being typical during the
>early Holocene that have no basis in fact. They are again probably based
>on this increasingly disturbing notion that Arctic ice core borehole
>thermometry or other ice core information tells us anything at all about
>the hemisphere let alone globe. A small number of scientists are really
>misleading the scientific community in this regard. How odd that the panel
>was happy to claim that there were millennial periods with 2 degree C
>warming in global temperature during the holocene (for which there is no
>reliable empirical evidence whatsoever) and yet focuses its skepticism on
>much more detailed and careful assessments of the most recent millennium.
>I think you can see why some of us are frustrated by this type of
>inconsistency, and suspect some degree of bias or agenda at work. There
>was a clear bias in the panel in the promotion of ice cores (which sample
>a very limited portion of the globe and are very questionable in their

>ability to say *anything* about hemispheric or global temperature
>variations). I am disturbed by this because the NAS report shouldn't have
>been promoting a particular specific area of funding. It seems to have.

>
>Finally, with regard to one of the primary supposed discrepancies in the
>paleo record of the past 1000 years, temperature reconstructions from
>boreholes vs. other proxies, I'll be presenting some results in Chicago
>which I think you'll all find quite elucidating. Turns out there is no
>discrepancy after all. More on that soon. I'll also try to confront both
>the "real" and "imagined" sources of uncertainty and bias in
>paleoreconstructions in my presentation there, and we should all be able
>to have a very healthy discussion of this.

>
>I really think that there was a bias in this panel which cannot be
>considered representative of the community as a whole. So I vote that we
>not over-react. I'm anxious to see Lindzen, Broecker, or Mike Wallace
>publish a peer-reviewed critical analysis of proxy data over the past 1000
>years. Until that day, I take their comments w/ a shaker of salt...

>
>mike

>At 09:41 AM 6/11/01 -0400, Thomas R Karl wrote:

>>Kevin,

>>
>>I agree with most of your points. It was a very interesting Panel. I should
>>emphasize however, that the Paleo record (at least the last 1000 years)
>>has many
>>critics, and we really need to show how the data prior to 1600 stands
>>up. Some
>>contend there are only 4 good sites in the first part of the record. I
>>am not sure
>>of this, perhaps Mike and others will explain this in Chicago.

>>
>>Regards, Tom

>>
>>Kevin Trenberth wrote:

>>
>>> FYI
>>>
>>> Some comments on the NRC/NAS report on the IPCC and global warming
>>>
>>> Kevin Trenberth
>>> 6/7/2001
>>>
>>> While the report overall is an endorsement of the IPCC report and the
>>> process, it has a lot of "buts" in it, and the overall tone is to somewhat
>>> downplay the problem. It does not focus on policy relevant issues. The
>>> report was done in a very hurried fashion and perhaps as a result,
>> there are
>>> several factual errors or misstatements and there are errors of
>> omission. My
>>> impression is that it tends to overstate the caveats and need for
>> questioning
>>> of results and understate the certainties and likelihoods.
>>>
>>> 1. In dealing with natural variability, there are two aspects that are

>>> mixed in this report. There is natural variability of climate
>>> that is tied to external forcings, such as variations in the sun,
>>> volcanoes, and the orbital variations of the Earth around the sun. The
>>> latter is the driver for the major ice ages and interglacials. The
>> second
>>> kind of natural variability is that internal to the climate system
>> arising
>>> from interactions between the atmosphere and ocean, such as El
>> Nino, for
>>> instance. This variability occurs even in an unchanging climate.
>>>
>>> In the section dealing with this and in the summary, both kinds of
>>> variability are discussed as if they are the second kind. Glacial to
>>> interglacial differences are discussed without any mention of the known
>>> causes and as if these can happen without a cause. This is
>> misleading at
>>> best. A consequence is that there is no clear statement that the
>>> recent warming is outside the realm of natural variability - and that a
>>> cause is needed. And the cause is human induced changes in the
>>> atmospheric composition.
>>>
>>> 2. The report does not clearly address issues in attribution of recent
>>> climate change to human activities. At the end of p 3 in the
>> summary it
>>> makes an equivocal statement. It avoids the issue that the recent
>>> temperature increase is outside any estimates of natural variability
>>> without any forcings. What else is the warming due to?
>>>
>>> On p 14, it does not sum up the forcings and make a clear statement
>> about
>>> the total. Nowhere does it say that the recent warming has to be
>> because
>>> of an increase in heating. This reasoning also put limits on how large
>>> aerosol cooling can be.
>>>
>>> On p 17, the ambiguity over the term "natural forcing" is used to
>> say that
>>> a causal link can not be unequivocally established. It does not mention
>>> estimates of variability from the paleo record and how well they
>> agree (or
>>> not) with model estimates.
>>>
>>> It does not note on p 17 that many models show the signal of
>> greenhouse gas
>>> effects emerging from the noise of natural variability about 1980. The
>>> attribution statement is weak.
>>>
>>> 3. Several statements about the hydrological cycle, rainfall, and
>> warming are
>>> misleading and even wrong. One direct consequence of this is that
>>> statements about changes in extremes are missing, understated and
>> incorrect.
>>> Another is to understate the threats in the tropics and subtropics.
>>>
>>> It begins in the first sentence of the summary: "Greenhouse gases are
>>> accumulating as a result of human activities, causing surface air

>>> temperatures and subsurface temperatures to rise." Later in the
>> paragraph
>>> it states "Secondary effects are suggested by computer model
>> simulations
>>> and basic physical reasoning. These include increases in rainfall
>> rates
>>> and increased susceptibility of semi-arid regions to drought."
>>> While the first statement is true, is is misleading. The increased
>>> greenhouse gases cause increased heating (also called radiative
>> forcing in
>>> this report). It is also referred to as "warming". The latter term is
>>> ambiguous and misused in this report, by confusing where it should mean
>>> "heating" versus where it should mean "increased temperature". So
>> while
>>> some of the increased heating does in fact cause an increase in surface
>>> temperature, much of the heating goes into evaporation of surface
>>> moisture. This changes the moisture content of the atmosphere and
>>> rainfall. This increase in the hydrological cycle is NOT a secondary
>>> effect, it is a primary one.
>>>
>>> Moreover, the increase in atmospheric moisture content is much
>> greater than
>>> the increase in evaporation, because it is controlled by the
>> temperature
>>> (which determines the water holding capacity of the atmosphere
>> through the
>>> so-called Clausius Clapeyron effect) while the evaporation is
>> controlled
>>> by the surface heating. For doubled CO2, evaporation and the overall
>>> hydrological cycle speeds up by about 3%, but the moisture in the
>>> atmosphere increases by about 6% per degree C, or about 15% for a
>> doubling
>>> of CO2.
>>>
>>> The rainfall intensity is determined by the available moisture, and
>> so it
>>> increases at about the latter rate. But the total precipitation
>> increases
>>> only at the former rate, and so the frequency of precipitation must
>>> decrease in some way. This also means that the residence time for
>> water
>>> vapor increases in a world with increased heating. The increased
>> drying
>>> means increased risk of drought everywhere, not just semi-arid
>> locations,
>>> and increased intensity increases risk of floods. These increases
>> in risk
>>> of extremes are direct consequences and are not adequately
>> mentioned. In
>>> the section on "Future climate change", p 19, one statement is
>> wrong: "An
>>> increase in the recycling rate of water in the hydrological cycle is
>>> anticipated in response to higher global average temperatures." The
>>> increased hydrological cycle is in response to increased heating, not
>>> increased temperatures (and may not occur if only the temperature is
>>> increased). The term "recycling" is normally used to refer to

>> moisture that
>>> evaporates and precipitates in the same catchment, and is
>> misleading here.
>>>
>>> A consequence of all this is that in the summary on p 4 in
>> addressing the
>>> question "What will be the consequences of global warming (e.g.,
>> extreme
>>> weather, ...)..." , there is no statement about increased risks of
>> extremes
>>> of floods and droughts, and heat waves. It also underplays the
>> risks of
>>> increases in pests and diseases (like fungal diseases) in agriculture.
>>>
>>> 4) The report contends that emissions in the last decade have averaged
>> less
>>> than in IPCC predictions, notably for CO2 and methane. However, the
>> IS92c
>>> scenario had flat CO2 emissions till 2020 and then declining
>> emissions to
>>> 2100, and for methane values projected are quite close to those
>> observed.
>>> In any case they are not forecasts but scenarios, to be used for
>> planning
>>> purposes. Statements in the summary on p 4 and on p19 are misleading.
>>> Also, the claim that CO2 emissions will accelerate for mid-range
>> estimates
>>> is not true: those have emissions increasing at a close to constant
>> rate.
>>>
>>> 5) The report dodges the issue of what is a "safe" level of
>> concentration of
>>> greenhouse gases, and has a strong US bias. It does not list on p
>> 21, for
>>> instance, the vulnerability of small island States to sea level
>> rise and
>>> of poorer countries to all aspects of climate change. Again it avoids
>>> discussion of changes in extremes. It is also incorrect in stating
>> "The
>>> largest changes occur consistently in the regions of the middle to high
>>> latitudes." This is true only for temperature and NOT for
>> precipitation
>>> (also p 8) perhaps because of the issues raised in item 2).
>>> Therefore it understates the threats to tropical countries.
>>>
>>> Some details:
>>>
>>> p 6: The accepted value of forcing for doubled CO2 with a stratosphere in
>>> adjustment (which occurs rapidly) is 3.5 W m-2, not 4.
>>>
>>> p 11: sheep are just as much a source of methane as cows and cattle.
>>>
>>> p 24: the list of variables needed for an observing system should include
>>> those for the ocean.
>>>
>>> -----

>>> Kevin E. Trenberth e-mail: trenbert@ucar.edu
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>>> *****

>
>

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From: "Michael E. Mann" <mann@virginia.edu>

To: "Dr. Nanne Weber" <weber@knmi.nl>

Subject: Re: workshop report

Date: Mon, 18 Jun 2001 11:50:15 -0400

Cc: "Michael E. Mann" <mann@virginia.edu>, Julia Cole <jcole@geo.arizona.edu>, rbradley@geo.umass.edu, jto@u.arizona.edu, storch@gkss.de, wanner@giub.unibe.ch, tom crowley <tom@ocean.tamu.edu>, k.briffa@uea.ac.uk

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Hi Nanne,

Thanks for your comments. I've asked Julie Cole, who is attempting a revised draft, to incorporate your suggestions. Hans or you should also provide a revised paragraph 7 that is more to your liking than what I wrote.

I'm requesting that Julie wait until the end of this week (Friday, Jun 22) to give the others time to get their comments in also. Then, after Julie provides me w/ her revised draft, I'll try to make a few more small changes and sent that onto the group for suggested final changes.

I hope this sounds acceptable to all concerned?

thanks,

mike

At 03:22 PM 6/18/01 +0000, Dr. Nanne Weber wrote:

>Hi Mike (and others),

>

>Below follow some comments on the draft report for EOS that you send
>around. The

>general outline is fine for me. Responding to Julie's comment on the
>large-scale/regional

>reconstruction issue: I guess that the three different approaches
>mentioned are not

>necessarily restricted to large-scale. Especially (1) can be for all
>scales, (2) will work

>better for large scales, but (3) could be very well applied to regional
>scales

>like African monsoon or NAO. However, I do think that this 'scales
>issue'

>should be addressed explicitly in the text (as indicated in my
>comments).

>

>We can not cover all of the workshop in a small EOS report, but I do
>think

>that there should be more emphasis on the different model strategies
>presented,

>process-based proxy modeling and some more mention of historical
>documentary data.

>

>I am willing to take my share in the rewriting task. Just let me know
>what is most

>convenient for you.

>

>

>One practical point: the Netherlands funding agency is called National

>Research Program (NRP) of the Netherlands (KNMI is my affiliation, but

>it

>did not pay the bill)

>
>Thanks,
>
>Nanne
>=====

>
>
>First para, first sentence: name all boundary conditions relevant
>for geological timescales (astronomical forcing, orography, GHG
>concentrations)
>or none.

>
>First para, fifth sentence: Three distinct approaches have in
>reconstructing
>the LARGE-SCALE AND REGIONAL climate history of past centuries and
>millenia.

>
>First para, point (3): the assimilation of paleoclimatic proxy data
>directly
>into (leave out 'forced') climate model integrations (using statistical
>models to upscale the proxy data to large-scale climatic patterns), in a
>manner
>conceptually etc.,.

>
>
>
>Second para: can be written in a more condense manner. One ore two
>sentences
>discussing the large-scale versus regional climate issue should be
>added. For example:
>(i) add after the second sentence ('The first method...'): This holds
>for
>spatial scales ranging from local (in the case of site-by-site
>calibration) to
>large scale (in the case of pattern calibration, e.g. ENSO and NAO) and
>up to hemispheric/global.
>(ii) add just before 'It was our belief that a meeting': The second and
>third
>approaches are more suitable for reconstructing the actual large-scale
>climatic
>state, as the local climate is inherently noisy and only to a limited
>amount determined by external forcing or related to large-scale patterns
>
>like e.g. the NAO.

>
>Second para, modify the description of the third approach as follows:
>The third approach can be thought of....., but it is nudged
>toward the actual observed large-scale climatic state at the time
>resolution provided by the proxy data. This method is more resistant to
>the potential biases.....model-based approaches, but it is relatively
>untested to the application of proxy data.

>
>
>
>Fourth para: leave out second sentence "A frequency-domain..." (too much
>
>technical detail, in a too condensed form to be understandable to a
>general
>reader of EOS).

>
>
>
>Fifth para: very much biased toward the modeling of large-scale, forced
>signal.
>My go at modelling paragraph(s):
>Three types of modelling experiments were distinguished: free
>simulations without
>any external forcing, giving insight into the patterns and timescales of
>
>internally-generated variability, forced simulations and simulations
>constrained
>by the assimilation of proxy data. Examples were presented, where models
>used ranged
>from an energy balance model (EBM), an intermediate-complexity climate
>model (EMIC) to
>atmospheric and coupled General Circulation Models (GCM). Simulations
>with an
>EBM as well as a GCM appear to explain variations over
>century-to-decadal timescales
>in proxy-based reconstructions of the Northern Hemisphere temperature
>over the past millenium, using estimated changes in radiative forcing
>(solar
>irradiance changes, volcanic activity, GHG and aerosol concentrations).
>Discrepancies,
>however, etc.... (a bit long as it is now).
>
>Process-based models of glaciers and sea level were used to generate
>synthetic
>records of these low-frequency proxies on the basis of EMIC and GCM
>simulations,
>using unforced runs as well as orbital and solar-forced runs.
>Over longer timescales simulated glacier lengths and sea level
>variations
>can be used to validate the models response in climatic parameters
>which are not well constrained by existing proxy data, like the
>hydrological cycle.
>In addition, model-data intercomparisons can be carried
>out on the level of the proxy itself rather than on the level of
>reconstructed
>climatic variables. Such process-based models require an understanding
>of local meteorological processes as well as the complicated (physical,
>biological or
>chemical) processes determining the proxy itself. A promising new model
>of
>tree-ring growth was presented.
>
>A new data-assimilation approach to paleoclimatic reconstruction DATUN
>(..)
>was discussed at length....
>This paragraph is not very clear as it is. I can have a go at it,
>but maybe Hans should.
>
>
>
>Para seven: This could be much shorter. Several points are mentioned
>here
>for the first time--> move up te earlier paragraphs (as indicated above)
>

>...currently emphasized high-resolution proxies such as tree rings,
>HISTORICAL
>DOCUMENTARY DATA, corals and ice cores. In addition, low-frequency
>climate
>variability may be reconstructed from low-resolution proxies such as
>borehole
>records, glaciers, foraminifera in marsh cores indicative of sea level
>as well
>as lake and ocean sediments which are not necessarily laminated.
>Process-based proxy models would enable to better exploit the
>information
>contained in proxy records and help to resolve the origin of apparent
>discrepancies between the different data sources. It is also important
>to
>better constrain the histories of radiative forcings prior to AD 1600.
>It was
>strongly felt that there should be an emphasis on developing
>projects....

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From: Martin Welp <Martin.Welp@pik-potsdam.de>

To: gberz@munichre.com, tloster@munichre.com, ccarraro@unive.it, juergen.engelhard@rheinbraun.de, guentherr@wwf.de, bhare@ams.greenpeace.org, klaus.hasselmann@dkrz.de, m.hulme@uea.ac.uk, carlo.jaeger@pik-potsdam.de, martin.welp@pik-potsdam.de

Subject: ECF: Agenda of the telephone conference 2 July 2001

Date: Thu, 28 Jun 2001 18:56:00 +0200

Dear member of the ECF steering committee,

The next telephone conference takes place on Monday, 2 July 2001 at 17.00-18.00 CET. The agenda is as follows (it may be modified at the beginning of the meeting):

1. Minutes of previous telephone conference (Draft sent by email on 14.6.2001) (5 Min.)
2. ECF preparatory meeting in Brussels (15 Min.)
(Agenda, Inputs: project descriptions, Outputs: workplan, sketch of a position paper)
3. ECF as an Association and/or Foundation (15 Min.)
4. Three priority projects (15 Min.)
5. Varia (10 Min.)

Important!! Please check that the telephone number where you want to be called is correct.

Gerhard Berz 089-3891 5290
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Bill Hare 030-44678765
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Carlo Jaeger 0331-288 2601
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Reminder:

General information about the ECF can be found at the ECF website:

<http://www.European-Climate-Forum.net/>

Background documents and internal information (e.g. the programme of the Brussels meeting):

<http://www.european-climate-forum.net/internal/>

Your feedback on these sites is more than welcome!

The ECF Flyer is available now! I will send all members of the steering committee 20 copies. If you need more of them please let me know.

Best regards,
Martin Welp

--

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From: Keith Briffa <k.briffa@uea.ac.uk>

To: "Dr. Reinhard Böhm" <r.boehm@zamg.ac.at>, <p.jones@uea.ac.uk>, <maugeri@mailserver.unimi.it>, <t.nanni@isao.bo.cnr.it>, <m.brunetti@isao.bo.cnr.it>, <Dietmar.Wagenbach@iup.uni-heidelberg.de>, <jones@gkss.de>, <widmann@gkss.de>, <storch@gkss.de>

Subject: Re: ALPIMOD-brainstorming

Date: Fri Jun 29 15:10:11 2001

Hi everyone

I have been through the ideas and offer a few (aptly non organised) comments. First Phil is away and will not be able to comment until later.

First, the project needs more explicit focus. The call will focused on natural variability . We are offering a detailed analysis of the variability of climate in the Alpine Region that focuses on CLIVAR timescales - basically very high resolution and not extending much beyond a few centuries. The project incorporates instrumental , model and palaeodata . The inter-relationships between these will be studied to gain an understanding of the nature and mechanisms of the climate variability - but is this enough. I feel it needs to be linked with a strong element of understanding the range of social/economic impacts of this variability.

Perhaps looking at aspects such as avalanches, forest damage, floods, tourism etc.? I merely put this out as a straw man . I feel the EC are putting a lot of emphasis on this aspect of research and incorporating research and researchers in these or similar areas will be a big plus.

As for the specific points in the brainstorming document -

The Dendro aspect :

I think it is essential to update the Alpine tree-ring chronologies that are available .

This is because they are a proven asset but many questions regarding tree-productivity (in relation to observed 20th century climate variability) simply can not be addressed without doing this. Many were collected over 20 years ago. The additional data would then allow new processing techniques to be employed and vital questions concerning the changing responses of tree-growth to explored. The most efficient way to do this is to involve several groups working in the Alps , (Thank you for sending the Thesis by Giorgio Strumia which is certainly a very impressive piece of work) I would think Rupert Wimmer's group and the Birmensdorf group would be ideal (Fritz Schweingruber has retired but Jan Esper has joined them in his place - I can ask them to be involved but this depends on what the group here think are the priorities and how much we see as the overall budget and institutional allocations). I should say here that I think we would require money for a single person who could , if it is agreed, work on aspects of tree-ring processing and relationships with climate in association with the other tree-ring groups, but also work with the climate and model data , especially with a view to exploring the statistical inter-relationships and dynamical associations between the different climate data sets. There is also the French tree-ring group at Marseille? Perhaps though not all need to partners - ALSO I am thinking of putting together a European Tree-ring project (or suggesting it as part of a large European integrated proxy study of Holocene variability) so if this happened there could be a link between it (involving some of the groups mentioned) and this proposal. The Swiss might be interested to produce selected site tree-ring density/updates which I think would be very valuable and I will speak to them without commitment as you ask.

As for some of the climate analysis possibilities mentioned, I very much like the ideas of detailed ,local climate comparisons with the larger CRU (and CRUder!) data. We are very interested in the association between time dependence in the relationships between circulation changes and changes in Temp. and Prec. Also changes in the nature of climate seasonality , and also extreme events (frost frequency , drought, intense rainfall). The detailed analyses of these characteristics also compliments the interpretational work on the tree-ring and glacier mass balance (and socio economic foci) data.

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Very best wishes to all

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Some remaining questions:

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Can everybody live with roughly 300.000 Euro (This would result into somewhere between 1.5 and two millions, which we heard is a magnitude preferred by the commission). Please consider not only the sum of money but also how to spend it and how to fill it with a reasonable equivalent in work amount.

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One last technical remark: Please send your comments and mails not only to Vienna, but also to the other groups (or at least to those You believe would be interested in what You write). I do not think this would spoil too much our mail boxes and it has the advantage to include the whole intellectual power of our group into the construction phase of the proposal.

Looking forward to Your replies, ideas, time tables and anything else

With best regards

Reinhard

--

Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
Norwich, NR4 7TJ, United Kingdom
Phone: +44-1603-593909 Fax: +44-1603-507784

From: "Ian Harris (Harry)" <harryharris@btinternet.com>
To: list@norwichgreenparty.org
Subject: Re: [ngp-list] Press Release 'Global Warning' talk establishes West Norfolk Green Party
Date: Mon, 2 Jul 2001 10:24:05 +0100
Reply-to: list@norwichgreenparty.org

<x-flowed>*sigh*

At 9:43 am +0100 2/7/01, Williams, Derek wrote:

>No, it's very dangerous to make predictions like this and IMO doesn't
>help the cause. Even without human activities, natural things like big
>volcanoes can easily disrupt the climate in such a way as to swamp the
>signs of global warming and indeed produce severe weather conditions as
>a casual glance at met records for the past couple of hundred years
>quickly shows (frozen Thames etc)
>
>One example of how this could work: A big volcano erupting a massive
~~~~~ hello Dan :-)  
>amount of ash could change the albedo of the earth by enough to counter  
>the warming effect of the increased CO2 (albedo - and sorry if the  
>selling is wrong - measures the reflectivity of the earth, more smoke in  
~~~~~ \*grin\*  
>the atmosphere reflects more radiation back to space). Cutting the
>forests down has the same effect, both because of the smoke from the
>burning trees and the resulting cleared ground, which is why on photos
>of building sites the bare earth looks white.
>
>Over simplification does no-one any good.

You're hardly any better, Derek: this is hardly a 'Nature' paper, is it?

You're talking about volcanic events that have a very different duration than the warming effects we're talking about. Major eruptions show up very clearly in the tree ring records going back centuries, but that's because you can pick out a one-to-three year spike rather than a prolonged cooling effect.

A rudimentary understanding of albedo is all very well, but since the radiative heat input from the Sun is still poorly understood (surprisingly) we can't deduce too much. In any case relying on mass deforestation or a prolonged series of major volcanic eruptions is hardly an attractive alternative to giving up burning what are finite

resources anyway.

Have a look at <http://www.cru.uea.ac.uk/> - particularly <http://www.cru.uea.ac.uk:80/cru/info/warming/>. We're looking at an *unprecedented* acceleration in temperature, and it's not due to a sudden lack of volcanic eruptions. Even if it turns out to be naturally-occurring, who's willing to take that chance? We should be trying to wean ourselves off of unsustainable energy generation and use anyway.

Cheers

Harry

--

Ian Harris - "Harry"
Climatic Research Unit
University of East Anglia
Norwich NR4 7TJ

Telephone: +44 1603 593818
Email: i.harris@uea.ac.uk

The content of this email should not be construed to represent the views of the Climatic Research Unit as a whole, nor of any other member of the Unit. If in doubt, please seek clarification before attribution.

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>

To: Keith Briffa <k.briffa@uea.ac.uk>, "Dr. Reinhard Böhm" <r.boehm@zamg.ac.at>, <maugeri@mailserver.unimi.it>, <t.nanni@isao.bo.cnr.it>, <m.brunetti@isao.bo.cnr.it>, <Dietmar.Wagenbach@iup.uni-heidelberg.de>, <jones@gkss.de>, <widmann@gkss.de>, <storck@gkss.de>

Subject: Re: ALPIPMOD-brainstorming

Date: Tue, 03 Jul 2001 15:01:17 +0100

Dear All,

Here are a few more comments on ALPIPMOD.

Ideas are probably not very well ordered. First, you should try for a 3 year

project

and second, although here for most of the next three months (apart from odd days) I

probably

couldn't justify a meeting. I am intending on resubmitting another proposal to the October EU round. This one will involve some of the group from ADVICE. Its aim will be to develop

a daily MSLP dataset for Europe and the Atlantic (30-70N by 70W-50E). After the dataset

is produced in the first year, the second and third year will see various analyses

performed

and comparisons of several GCM runs performed at the Hadley Centre. This new project

will probably go to 2.4.1 which will be a different area from yours which will be 2.1.4.

Thus I

would hope that your proposal could be developed over email.

The above dataset would go back to 1850. This is the period which from the IMPROVE

project

is just beyond how far we think we can reliably go back with daily data. Several papers

from

the IMPROVE project (Moberg et al., 2000 in JGR and several others in press in a special

issue

of Climatic Change) have come to the about 1870 date. We have much earlier data for the 8

sites but ensuring strict homogeneity of the daily series seems doubtful for some types of

extreme measures prior to about 1870. Pressure seems better than temperature. Some sites

are better than others. Monthly is fine for all.

All the IMPROVE and ADVICE data can be used by the ALPIPMOD project. I have a summer

student updating the 51 monthly MSLP sites from ADVICE, amongst other things.

As for your ideas, I think you need some overarching theme. The atlas and CD of all

the data may be one, but it also needs to address some scientific issues which can be

shown to have relevance to the public.

I like the idea of making use of the Alpine orography looking at changes in lapse

rates and

the use of high and low elevation air pressures. The latter is a totally independent

method of

looking at the warming and can be used back to the late 18th century. The Alps have the

longest records of any mountaineous records of any region of the world. Also I am a

strong

advocate of changes in the influence of features such as the NAO (and other circulation

indicators) on surface climate. You can clearly look at these changes over the last 200

years with all the data you have.

Another important issue to a lot of climatologists is the relative surface warming

compared

to the MSU2LT data in the lower troposphere. Although this is hemispheric in extent, we

can

look with the longer Alpine records as to changes in lower level lapse rates over 200+

years.

Related to this tropical ice caps are disappearing at alarming rates in Peru, Tanzania and

in Tibet (Lonnie Thompson's work). Lonnie has calculated that the ice cap on Kilimanjaro

will not be there by 2015 at its present rate of retreat. Lonnie has some local

temperature

series for about 40 years which show a small warming yet the ice caps are going fast. Why?

These ice caps have all been cored and have ice during the MWP times yet some aren't

producing layers now !

My idea is to use the better known histories of the Alpine glaciers to see if they

are

also melting at accelerated rates than simple temperature averages would imply. Keith

mentioned the forward modelling approaches to determine positions in the past (and then

relate these to moraine termini). Do these models still function in the last 20 years?

Lonnie

thinks a lot of the tropical melting is due to sublimation, which isn't accounted for by

the

degree day models. The elevational sunshine records may be important here and with

temperature a particular season may be much more important than the other three.

All the above is just ideas, but getting all the data together (instrumental and tree

ring

as well glacier termini and mass balance) allows us to be able to model the glaciers

better than

anywhere else. All Europeans will be interested in whether Alpine glaciers are going to disappear and there will be clear impacts on biodiversity at the high elevations and tourism.

Another impact area is on the use of glacier meltwater and runoff in hydropower generation.

These are all good issues to use in the social and economic pages that need to be written.

Cheers

Phil

At 15:10 29/06/01 +0100, Keith Briffa wrote:

Hi everyone

I have been through the ideas and offer a few (aply non organised) comments. First Phil is away and will not be able to comment until later.

First, the project needs more explicit focus. The call will focused on natural variability . We are offering a detailed analysis of the variability of climate in the Alpine Region that focuses on CLIVAR timescales - basically very high resolution and not extending much beyond a few centuries. The project incorporates instrumental , model and palaeodata . The inter-relationships between these will be studied to gain an understanding of the nature and mechanisms of the climate variability - but is this enough. I feel it needs to be linked with a strong element of understanding the range of social/economic impacts of this variability.

Perhaps looking at aspects such as avalanches, forest damage, floods, tourism etc.? I merely put this out as a straw man . I feel the EC are putting a lot of emphasis on this aspect of research and incorporating research and researchers in these or similar areas will be a big plus.

As for the specific points in the brainstorming document -

The Dendro aspect :

I think it is essential to update the Alpine tree-ring chronologies that are available .

This is because they are a proven asset but many questions regarding tree-productivity (in relation to observed 20th century climate variability) simply can not be addressed without doing this. Many were collected over 20 years ago. The additional data would then allow new processing techniques to be employed and vital questions concerning the changing responses of tree-growth to explored. The most efficient way to do this is to involve several groups working in the Alps , (Thank you for sending the Thesis by Giorgio Strumia which is certainly a very impressive piece of work) I would think Rupert Wimmer's group and the Birmensdorf group would be ideal (Fritz Schweingruber has retired but Jan Esper has joined them in his place - I can ask them to be involved but this depends on what the group here think are the priorities and how much we see as the overall budget and institutional allocations). I should say here that I think we would require money for a single person who could , if it is agreed, work on aspects of tree-ring processing and relationships with climate in association with the other tree-ring groups, but also work with the climate and model data , especially with a view to exploring the statistical inter-relationships and dynamical associations between the different climate data sets. There is also the French tree-ring group at Marseille?

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Phone: +44-1603-593909 Fax: +44-1603-507784

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Hans von Storch <Hans.von.Storch@gkss.de>
Subject: Re: EOS report
Date: Tue, 03 Jul 2001 15:04:58 -0400
Cc: "Michael E. Mann" <mann@virginia.edu>, Julie Jones <jones@gkss.de>, Julia Cole <jcole@geo.arizona.edu>, rbradley@geo.umass.edu, jto@u.arizona.edu, weber@knmi.nl, wanner@giub.unibe.ch, tom crowley <tom@ocean.tamu.edu>, k.briffa@uea.ac.uk, Martin Widmann <Martin.Widmann@gkss.de>

<x-flowed>
HI Hans,

Yes--it was the discussion of this in the De Bilt meeting report that led me to think this was envisioned in a broadened version of the DATUN approach. I thought the idea was that you would eventually use a forward biological/physical model to scale up from a given proxy an estimate of say precipitation or temperature for an atmospheric model gridpoint and use that to nudge say the slp or 500 mb field into a particular configuration. This is clearly more ambitious than what you are doing now, and I suppose I was blurring the distinct efforts of Nanne and colleagues with that of yours and colleagues. I makes much more sense at present to only use a statistically-based upscaling of the proxy data. The other possibility remains intriguing, but we are certainly far off from doing that in my opinion as well. I'm actually quite relieved to find out that I was wrong in assuming that this is the direction the DATUN approach was going.

thanks for the clarification,

mike

At 08:32 PM 7/3/01 +0200, you wrote:

>Hi folks,
>"forward models" can only deal with "weather -> proxy", but we need "proxy
>> circulation". If we had forward models, and we should certainly strive
>to develop such models, we could generate large data sets of consistent
>pairs "weather, proxy" and then derive empirically (neural nets?) the
>needed inverse relationship. (Actually, this method is used at our lab to
>evaluate the informational value of remotely sensed data about water
>quality in coastal seas.) But the inverse relationship is not
>process-based but necessarily phenomenological.
>
>I think the need for forward models was spelled out in the report about he
>De Bilt meeting in 1999 (see EOS paper by Weber and me).

>
>Regards

>
>Hans

>
>At 13:52 03.07.01 -0400, Michael E. Mann wrote:
>>Dear Julie et al,

>>
>>Then I apologize--I thought the idea in DATUN was to at least eventually
>>incorporate physical or biologically-based models of proxies into the
>>upscaling effort in addition to/in place of statistical upscaling. There
>>was lots of discussion of this, and I recall Hans early on having
>>described to me plans to use physical models of proxies in the process
>>(though I could be mistaken), so I thought that was a planned component
>>of DATUN, and the work that you described (ie, using empirical CCA
>>techniques) was just a preliminary empirical approach. But from what
>>Martin and you have told me, this is not the case, and there is no plan
>>in DATUN to use physical/biological forward models of proxies. If someone
>>out there still believes this is *not* the case please let me know!
>>Otherwise, the wording will be clarified to indicate that it is a
>>"statistical" and not physical/biological model that is used to upscale
>>the proxy information.

>>
>>That simplifies things quite a bit...

>>
>>mike

>>
>>At 07:18 PM 7/3/01 +0200, Julie Jones wrote:

>>
>>>Hi Mike

>>>
>>>I'm getting very confused now!

>>>
>>>If you mean 'forward modelling', by what I term upscaling, this is done
>>>in exactly the same way as most other climate reconstructions,
>>>i.e. calibrating proxy data against climate data using linear multivariate
>>>statistical methods (in this case I use CCA), so has the same errors

>>>inherent in it as other reconstructions where proxy data has been
>>>calibrated against large-scale climate, or climate indices.
>>>
>>>If your idea is that such large-scale climate reconstructions may have
>>>additional uncertainties compared to local empirical models, where proxy
>>>data are calibrated against local climate records, I agree that this is
>>>so - but I think this applies to all such non-local reconstructions, so
>>>should maybe go in the paragraph which discusses reconstructions of
>>>regional climate variability to keep things consistent.
>>>
>>>The additional potential source of error specific to the DATUN method
>>>compared to the other climate reconstructions, whether local or
>>>large-scale, is in the 'nudging' to assimilate the climate reconstructions
>>>obtained as above into the GCM, which should probably go into the text, so
>>>we could perhaps change the end of the paragraph to read:
>>>
>>>.....This method is more resistant to biases specific to
>>>purely empirical or model-based approaches but it is relatively untested
>>>using proxy data, and prone to additional uncertainties in the nudging
>>>method used to assimilate the proxy data.

>>>Am I on the right track, or have I missed something?

>>>
>>>
>>>cheers

>>>
>>>Julie
>>>
>>>*****

>>>Dr. Julie M. Jones
>>>Institute for Coastal Research
>>>GKSS Forschungszentrum
>>>Max-Planck-Strasse
>>>D-21502 Geesthacht
>>>Germany

>>>
>>>e-mail: jones@gkss.de
>>>phone: +49 (0)4152 871845
>>>fax: +49 (0)4152 871888
>>>*****

>>>
>>>On Tue, 3 Jul 2001, Michael E. Mann wrote:

>>>
>>> > Dear All,
>>> >
>>> > I am working on preparing a final version of the workshop report based on
>>> > Julie (C)'s revisions, and comments thusfar recieved.

>>> >
>>> > There is one instance below in which it seems especially important
>>> > that we
>>> > agree on the wording, so I wanted to give you my revised wording now and
>>> > let you comment on it if you see any problem:

>>> >
>>> > The third approach represents a hybrid of the first two; it
>>> > prescribes the
>>> > dynamics of the system using model physics, but aims to reproduce the
>>> > historical climate evolution by "nudging" the model towards reconstructed
>>> > climate estimates. This method is more resistant to biases specific to
>>> > purely empirical or model-based approaches but it is relatively untested
>>> > using proxy data, and prone to additional uncertainties in the forward
>>> > models employed to describe proxy-climate relationships.

>>> >
>>> > I think the latter statement is important because the assumption in the
>>> > forward model is *not* the same assumption as in empirical
>>> > reconstructions

>>> > (I take a slight issue w/ Julie J in this regard). The forward modeling
>>> > makes some universal assumptions regarding e.g. tree growth patterns. The
>>> > empirical calibration approach calibrates the individual trees against
>>> > local meteorological/climate records. It doesn't make any universal
>>> > assumptions, though the local calibration may be flawed! In other words,
>>> > we're not saying that one method is better than the other, but the
>>> > potential pitfalls are definitely different! I think this needs to be
>>> > expressed, hence my revised wording. Julie J should let me know if
>>> > there is

>>> > a problem w/ this, since she and Julie C spent some time parsing the
>>> > wording on the paragraph in question.
>>> >

>>> > Thanks,

>>>>
>>>> mike
>>>>
>>>> At 07:43 PM 6/28/01 +0200, Julie Jones wrote:
>>>>
>>>> >Hi Julie
>>>>
>>>>>Yes, that works, although if I could ask for one extra word -
>>>>>
>>>>>...but it is also limited by potential...
>>>>>
>>>>>cheers
>>>>>
>>>>>Julie
>>>>>
>>>>>
>>>>>*****
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>>>>>Germany
>>>>>
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>>>>>*****
>>>>>
>>>>>On Thu, 28 Jun 2001, Julia Cole wrote:
>>>>>
>>>>>> Hi Julie,
>>>>>>
>>>>>> First, sorry for the author oversight! I did not change that from
>>>>>> Mikes original, which did not have you on it, but he told me you
>>>>>> should be added.
>>>>>>
>>>>>> I like all your suggestions. I would alter the wording of the last
>>>>>> one a bit maybe, to use somewhat fewer words. Does this work? (68
>>>>>> words instead of 78). We are tight on space.
>>>>>>
>>>>>> The third approach represents a hybrid of the first two; it
>>>>>> prescribes the dynamics of the system using model physics, but aims
>>>>>> to reproduce the historical climate evolution by "nudging" the model
>>>>>> towards reconstructed climate estimates. This method is more
>>>>>> resistant to biases specific to purely empirical or model-based
>>>>>> approaches, but it is limited by potential instabilities in the
>>>>>> proxy-climate relationships and is relatively untested using proxy
>>>>>> data.
>>>>>>
>>>>>> cheers, Julie
>>>>>>
>>>>>>
>>>>>>>Dear All,
>>>>>>>
>>>>>>>Thanks Julie and Mike for your work on the paper. I have just a few
>>>>>>>sentences where I suggest alterations.
>>>>>>>
>>>>>>>1. First paragraph:
>>>>>>>
>>>>>>>'State-of-the-art climate models are also being applied to
>>>>>>> analyze late
>>>>>>>'Holocene climate sensitivity, upscale paleodata to large-scale
>>>>>>>'reconstructions, and simulate proxies themselves'
>>>>>>>
>>>>>>>I suggest changing to
>>>>>>>
>>>>>>>'State-of-the-art climate models are also being applied to
>>>>>>> analyze late
>>>>>>>'Holocene climate sensitivity, assimilate large-scale climate
>>>>>>>'reconstructions from palaeodata, and simulate proxies themselves.'
>>>>>>>
>>>>>>>
>>>>>>>2. Paragraph2, last sentence:
>>>>>>>
>>>>>>>'...patterns of atmospheric circulation, just as meteorological
>>>>>>>'information is assimilated into numerical weather forecasting
>>>>>>> models (von

>>> >>> >Storch et al. 2000).'

>>> >>> >

>>> >>> >I suggest changing to

>>> >>> >

>>> >>> >...patterns of atmospheric circulation, in a conceptually

>>> >>> >similar way to

>>> >>> >the assimilation of meteorological information into numerical

>>> >>> >weather

>>> >>> >forecasting models (Weber and von Storch 1999; von Storch et al.

>>> >>> >2000)

>>> >>> >

>>> >>> > - the Weber and von Storch reference is already in the

>>> >>> >reference

>>> >>> >list.

>>> >>> >

>>> >>> >3. Paragraph 3,

>>> >>> >

>>> >>> >'The third approach represents a hybrid of the first two; it

>>> >>> >prescribes

>>> >>> >the dynamical evolution of the system from climate physics but

>>> >>> >is "nudged" toward the observed climate by the proxy data. This

>>> >>> >method

>>> >>> >is more resistant to the biases specific to purely empirical or

>>> >>> >purely

>>> >>> >model-based approaches, but it is limited by potential instabilities

>>> >>> >in the proxy-climate relationships and by imperfections in the

>>> >>> >upscaling

>>> >>> >models, and it is relatively untested using proxy data.'

>>> >>> >

>>> >>> >I would suggest changing to the following (As the upscaling

>>> >>> >models are

>>> >>> >produced in exactly the same way as other

>>> >>> >climate reconstructions, so there are no extra imperfections in the

>>> >>> >upscaling models than in other climate reconstructions).

>>> >>> >

>>> >>> >'The third approach represents a hybrid of the first two; it

>>> >>> >prescribes

>>> >>> >the dynamics of the system using model physics, but is aimed

>>> >>> >at reproducing the historical climate evolution by "nudging" the

>>> >>> >model

>>> >>> >states towards towards climate estimates obtained by the first

>>> >>> >approach. Although this approach also requires the stability

>>> >>> >assumption in the statistical models, it is hoped that it is more

>>> >>> >resistant to the biases specific to purely empirical or purely

>>> >>> >model-based

>>> >>> >approaches; it is however relatively untested.'

>>> >>> >

>>> >>> >

>>> >>> >Finally, I've been missed off the list of authors! - and the

>>> >>> >address for

>>> >>> >Hans and myself should be GKSS Research Centre, Geesthacht.

>>> >>> >

>>> >>> >Best regards

>>> >>> >

>>> >>> >Julie

>>> >>> >

>>> >>> >

>>> >>> >

>>> >>> >*****

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>>> >>> >

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</x-flowed>

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t.oriordan@uea.ac.uk, ccarraro@unive.it, tol@dkrz.de
Subject: No Subject
Date: Wed, 11 Jul 2001 09:58:13 +0200

Dear Friends,

A few remarks before the meeting of tonight and tomorrow,

I am sure that our meeting will make clearer the different objectives of ECF, in particular regarding the articulation between the scientific agenda and activities in direction to stakeholders and policy-makers.

I would like to stress that I will attend the ECF meeting not only in the name of the Cired, but also in view of preparing the involvement of the Institut Laplace in ECF, namely the community of climate modellers, with which we develop a long term research program. I would like to explain hereafter in a few words what should be, in my view the priorities of ECF, in terms of scientific agenda:

Given recent Ipcc experience, the first priority would be to progress in direction to integrated models. Indeed the lessons of the Ipcc are twofold:

- first the Sress scenarios confirm the possibility of generating very different emissions growth scenarios over the long run, but the consistency between the Storylines and the numerical scenarios remain uncertain; this uncertainty and vagueness reveals a more fundamental limitation of the state of the art of economic modelling over the long run, in particular to provide an explicit picture of linkages between structural changes (infrastructure transportation, urban forms that govern the energy content of final consumption, industrial structure and the so-called dematerialisation), innovation and both macro and micro economic drivers (productivity, growth and price-signals). This makes very difficult to detect where are the real bifurcations, the real policy-parameters and to make much progress in the understanding of the timing of policy responses,

- second the sections on 'damages ' have make some progress but remain weak in terms of the social and economic implications. More precisely they deal mostly with impacts on physical parameters (sea-level rise), in a few cases adress impacts on humans (tropical diseases), but all this does not give a comprehensive picture of social and economic damages (once discounted the effect of adapation),

One of the scientific objective of ECF should be to be prepared to provide in a few years for a convincing contribution in future exercises like the SRES and in the future Ipcc rounds. This passes first through two parallel efforts:

- on long term economic modelling where the limitations of existing tools are obvious despite real progress; this relates basically to three challenges:

- a macroeconomic framework insuring the consistency between prices and quantities at any point in time without necessarily resorting to the modelling tricks relying on the conventional neo-classical growth theory; these 'tricks' assume indeed perfect foresight, efficient markets and the absence of strategic or routine behaviours; New conceptual frameworks about endogenous growth theory allow for such a move, but there is a gap between advances in pure theory and empirical modelling,

- the endogeneisation of technical change and more precisely to develop this endogeneisation in such a way that the information coming from sectoral models in energy, transportation or agriculture is not lost (this comes back to the bottom-up/top-down controversy); note that one key challenge here is to progress in direction to transportation and agriculture

- an explicit treatment of expectations and uncertainty; one key issue indeed is that the stabilisation of expectations over the long run is the main driver of technical change, consumption patterns and structural adaptation.

- on 'coupling' economic and climate models: here there are two routes, either to develop coupling methods between large-scale models or to develop interface compact modules, reduced forms of large scale models. Both routes are valid, however, in the following years, to develop integrated models made up with reduced forms of larger models seems more promising; thanks to tractable and numerically controlable models, in will be easier to reveal

the key mechanisms at work and to introduce uncertainties. This will pass through progress in the representation of carbon cycle (including sequestration) in such models and, more importantly in the representation of damages and adaptation, which rises rather fundamental conceptual issues that explain what seems to be the second priority in my view.

The second priority relates to the joint question of damages and precautionary principle:

- part of the agenda is covered by Mike Hulme's paper and I will not elaborate here on other dimensions I would link to include and how to assess a cost. I will simply insist of the fact that we need to set up a taxonomy of damages in economic terms, this means as resulting not of the climate transformation per se but from the joint effect of inertia and uncertainty (to pass to Riviera to the beaches of Normandy in not a cost in itself in a world restabilized around a new climate equilibrium; what matter are the transition costs and the generated variability of climate).

Moreover I would insist for adopting deliberately a worldview because, fundamentally, climate change will generate a new human geography, and not

to be restricted to the European subcontinent,

- this should lead to develop in parallel stochastic decision modelling

tools to disentangle the many dimensions and views about the precautionary

principle and, I take some risks in saying that, in a symmetric treatment of climate damages and nuclear risks (we cannot avoid to try and put some rationale in this discussion which is one of the reason for the failure of

the EU tax in 1992 and of COP6, and which will be an 'hidden' division line

within the EU)

The third priority should be the topic 1 made by Klaus. For me the two first modelling efforts I described briefly are outmostly important to bring new insights for responding the question of the instruments.

However,

we have, before waiting for the achievement of a new generation of models (which will respond to point 2 and 3 of Klaus's paper), it matters to develop in parallel a specific programm on international coordination architecture given the failure of COP6 and the lack of understanding of economic and social implications of the selection of this architecture (coordination through prices or quantities, full agreement or partial expanding coalition, issue linkages, perceived equity etc ...). This workprogramm should build on advances on the role of economic and non economic instruments in fostering innovation, and on the distributive static and dynamic implications of such instruments.

These are very brief remarks, simply to give you some ideas about my current perspectives.

From: Edward Cook <drdendro@ldeo.columbia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: N(eff) and practicality
Date: Tue, 24 Jul 2001 08:49:14 -0400
Cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>

Hi Tim,

Thanks for the remarks. We can certainly spend some time talking through some of the points raised. I guess I am still finding it difficult to believe that an rbar of 0.05 has any operational significance in estimating Neff. It is kind of like doing correlations between tree rings and climate: a correlation of 0.10 may be statistically significant, but have no practical value at all for reconstruction. The same goes for an rbar of 0.05 in my mind. I agree that what I suggested (i.e. testing the individual correlations for significance and only using those above the some significance level for estimating rbar) is somewhat ad hoc and not theoretically pleasing. However, it is also true that correlations below the chosen significance threshold are "not significantly different from zero" and could be ignored in principle, just as we would do in testing variables for entry into a regression model. This would clearly muddy (a nice choice of words!) the rbar waters, I admit.

In terms of the problem I am working on (computing bootstrap confidence limits on annual values of 1205 RCS-detrended tree-ring series from 14 sites), it is hard to know what to do. Certainly, using Neff will result in almost none of the annual means being statistically significant over the past 1200 years. I don't believe that this is "true". Other highly conservative methods of testing significance result in a very high frequency of similarly negative results, i.e. the test of significance in spectral analysis that takes into account the multiplicity effect of testing all frequencies in an a posteriori way (see Mitchell et al. 1966, Climatic Change, pg. 41). If you use this correction, virtually no "significant" band-limited signals will ever be identified in paleoclimatological spectra. So, this test has very low statistical power. I think that this is the crux issue: Type-1 vs. Type-2 error in statistical hypothesis testing. The Neff correction greatly increases the probability of Type-2 error, while virtually eliminating Type-1 error. So, truth or dare.

Consider one last "thought experiment". Suppose you came to Earth from another planet to study its climate. You put out 1,000 randomly distributed

recording thermometers and measure daily temperatures for 1 Earth year. You then pick up the thermometers and return to your planet where you estimate the mean annual temperature of the Earth for that one year. How many degrees of freedom do you have? Presumably, 999. Now, suppose that you leave those same recording thermometers in place for 20 years and calculate 20 annual means. From these 20-year records, you also calculate an rbar of 0.10. How many degrees of freedom per year do you have now? 999 or 9.9? What has changed? Certainly not the observation network. Does this mean that we can just as accurately measure the Earth's mean annual temperature with only 10 randomly placed thermometers if they provide temperature records with an rbar of 0.00 over a 20 year period? I wouldn't bet on it, but your theory implies it to be so. Surely, one would have more confidence (i.e. smaller confidence intervals) in mean annual temperatures estimated from a 1000-station network.

Cheers,

Ed

>Ed,

>

>re. your recent questions about Neff and rbar etc...

>

>I've thought a bit about these kind of questions over the past few years,
>but have never completely got my head around it all in a satisfactory way.

>I agree with what Phil said in his reply to you. Also, your idea of
>subsampling 40% of the cores at a time sounds reasonable, though I don't
>think it would be possible to write a very elegant statistical
>justification! Anyway, I just wanted to add a couple of points to what
>Phil said:

>

>(1) Even for very low rbar, the formula certainly works for
>idealised/synthetic cases (i.e. with similar standard deviations and
>inter-series correlations etc.). For example, I just generated 1000 random
>time series (each 500 elements long) with a very weak common signal,
>resulting in rbar=0.047. n=1000 was the closest I could get to n=infinity
>without waiting for ages for the correlation matrix to be computed! The
>formula:

>

> $neff = n / (1 + [n-1]rbar)$

>

>which reduces to $neff = 1 / rbar$ for $n=infinity$ gives $neff = 20.83$. For
>such a low rbar, neff seems rather few? The mean of the variances of the

>1000 series was 1.04677. If I took the "global-mean" timeseries (i.e. the
>mean of the 1000 series, then it's variance was 0.05041. The ratio of
>these variances is 20.77 - almost the same as neff! If our expectation
>that neff should be higher than 20.83 was true, then the variance of the
>mean series should have been much lower than it was. It should be easy to
>try out similar synthetic tests with various options (e.g. shorter time
>series, sets of series with differing variances, subsets with higher common
>signal (within-site) combined with subsets with weaker common signal
>(distant sites) etc.) to test the formula further.

>
>(2) I agree that \bar{r} is computed from sample correlations rather than true
>(population) correlations.

>(a) For short overlaps, the individual correlations will rarely be
>significant. But the true correlations could be higher as well as lower,
>so \bar{r} could be an underestimate and neff could be an overestimate! Maybe
>you have even fewer than 20 degrees of freedom!

>(b) I did wonder whether the sample \bar{r} might be a biased estimate of the
>population \bar{r} , given that the uncertainty ranges surrounding individual
>correlations are asymmetric (with a wider range on the lower side than the
>higher side). But I've checked this out with synthetic data and the \bar{r}
>computed from short samples is uncertain but not biased.

>(c) Just because \bar{r} is only 0.05 does not mean that you need series 1500
>elements long to be significant - that would be the case for testing a
>single correlation coefficient. But \bar{r} is the mean of many coefficients
>(not all independent though!) so it is much easier to obtain significance.
>Not sure how you'd test for this theoretically, but a Monte Carlo test
>would work, given some assumptions about the core data. For 100 cores,
>each just 20 years long, a quick Monte Carlo test indicates that an \bar{r} of
>0.05 is indeed significant - therefore $\bar{r}=0.05$ in your case with > 100
>cores, many of which will be > 20 years long, should certainly be significant.

>
>Looking forward to your visit! We can discuss this some more.

>
>Tim

>
>

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=====
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From: "Stephan Singer" <SSinger@wwfepo.org>
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Subject: response
Date: Fri, 17 Aug 2001 15:56:33 +0200

Dear Mr Hasselmann,

thanks for the draft position of the ECF. I do believe it is very good first approach to position the needs of a science-based climate policy in the future. I do particularly like the quasi-goal of a long-term 0-emission target supported by the scientific community. However, there are a few amendments I like to propose:

a) I do not agree at all that the focus on the short term "dictated by the 10 year Kyoto horizon has tended to obscure longer term issues".

In the contrary, if we were to agree on longer-term and deeper targets - what we all want I suppose - there must be a starting point somewhere in the next years. I do agree that the 1 CP targets are moderate and will be diluted by all kinds of loopholes. But given the economic and political nature of this treaty, more is/was not reachable by the international community. I prefer an unperfect agreement coevering the globe (almost!) as a starter over an perfect agreement that will never be agreed upon. And - probably more important - the recent Bonn agreement will give the signal to the main polluters that the atmosphere is not a free sewer any more. At best, they won some time - but the ultimate message is, that the train towards deeper targets has started. This may impact future industrial investment and legislative decision making much deeper than the targets of the 1 CP itself as it provides some basic certainty. Having said this, the next important discussion round on a political level will resume about "adequacy of commitments" of the next CPs. that is the build-in logic of both the treaty and the Convention. Here countries will start to address targets for 2013-2018. Thus, there is an approach to the long-term issues. It is a transient process over time. And, please believe me, almost everyone I talked to in the past who complained about the "short-term" focus of the treaty as opposed to a long-term global strategy had not in mind to strengthen environmental effectiveness - these voices mostly reflected the desire to fully delay any early action after all. And without early action and without short term focus we will never get to the longer-term targets.

In short, I believe, a scientific approach should foster the architecture of the KP and that of the Convention and the need for further target-setting processes in the future by all parties - and that is intrinsincally embedded in the process.

In that respect, it is probably scientifically correct to state that the "Kyoto reductions have negligible impacts on global warming" but it would be politically naive to conclude that this means Kyoto is only "symbolic". It is much more.

b) I have problems with the focus on solar as the sole beneficiary of a 0-emission society. Still, I still like to focus on those measures that are not implemented yet and can provide the bulk of future emissions reductions mostly cost-effectively - that is energy efficiency in its various forms and various applications. And renewables are those who benefit most from energy efficiency as each renewable kWh provides more service, km or goods.

Generally, I like a broader approach to renewables. It is not "one takes it all" solar what will save the world from climate change. We need many forms of renewables according to the cultural, political and economical circumstances in the various regions. In some it may be solar thermal power or PV, in others it is off-shore wind, and in many rural areas it may be biomass or geo-thermal energy. And let us not forget the challenge of producing hydrogen from renewable sources as another ultimate fuel.

c) How do we deal with equity? I believe it has to be addressed in one way or the other - and I mean much more than the usual GHG emissions per capita approach. This would include compensation/adaptation funding for poor and vulnerable developing countries - but also how to deal with targets for (certain) developing countries in the next CPs.

best regards
Stephan Singer
WWF International

From: Klaus Hasselmann <klaus.hasselmann@dkrz.de>
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Subject: Re: response to response
Date: Sat, 18 Aug 2001 13:39:00 +0200

<x-flowed>

>Dear Stephan (I suggest we use the anglo-saxon first-name form, coupled
>with "Sie" if we slip into German)

I agree with all of your points and hope you will contribute to finding
the
right language in our position paper to reflect both the need for long-
term
goals and the value of at least starting off with something one can build
upon. One of my motives was to help keep the door open for those who wish
to join the process later without too much embarrassment. I also agree
that
we need to investigate all technological options. I am certainly not an
expert in this field and am willing to learn from those who see more
Global
Mitigation Potential in some of the currently proposed technologies than
I do.

With best regards
Klaus

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</x-flowed>

From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Matilda Lee" <matildalee1@hotmail.com>
Subject: Re: Request from The Ecologist magazine
Date: Tue Aug 21 09:41:10 2001

See comments embedded from me below I would appreciate receiving a copy of the magazine when published. Thank you.

My affiliation is provided below.

Mike

At 15:15 14/08/01 +0000, you wrote:

Yes-very much so! Your response would be greatly appreciated. Thanks!

From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Matilda Lee" <matildalee1@hotmail.com>
Subject: Re: Request from The Ecologist magazine
Date: Tue, 14 Aug 2001 16:08:55 +0100
Been away on holiday - is this still relevant?

Mike

At 10:10 03/08/01 +0000, you wrote:

Dear Sirs:

The Ecologist, a London-based internationally recognized environmental magazine, will be publishing a Special Edition on Climate Change in September. For this edition, we believe it would be extremely useful to gather the opinions of the top climatologists on an issue for which there is growing interest by those concerned with climate change.

This issue is addressed in Article II of the United Nations Framework Convention on Climate Change, which states:

"The ultimate objective of this Convention and any related legal instruments that the Conference of the Parties may adopt is to achieve, in accordance with the relevant provisions of the Convention, stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system. Such a level should be achieved within a time-frame sufficient to allow ecosystems to adapt naturally to climate change, to ensure that food production is not threatened and to enable economic development to proceed in a sustainable manner."

Furthermore, the need to address the issue of atmospheric concentrations was recently reaffirmed by Michael Zammit Cutajar, Executive Secretary of the UNFCCC, who stated at the closing session of the IGBP in Amsterdam on 13 July 2001,

"I believe that the political process on climate change would be greatly assisted by agreement on a target for atmospheric concentrations, at least an intermediate target. This would give a sense of where the whole international community should be heading and a basis for apportioning responsibility for getting there."

We would be very appreciative if you would send a return email with your response to the following questions for publication in The Ecologist Special Edition on Climate Change.

-At what levels do you think we should aim to stabilize carbon dioxide concentrations in the atmosphere and why?

I do not believe we have any sure basis for establishing what a 'non-dangerous' level should be. This is so for several reasons:

- what is 'dangerous' depends on what measures are taken to adapt to climate change.

550ppm may be 'safe' in one assumed future world but 'dangerous' in another.

- the concept of 'danger' is not one that science can pronounce on. Such a level has to be negotiated via a social and political process. This negotiation has also to take place in the context of other risks that society is exposed to, i.e., we may be prepared to run a higher risk with climate change if it means we can divert greater resources to reducing global poverty.

- the basis for establishing 'danger' is contested. One could argue that 'dangerous' climate change is change in climate that leads to the death of just *one* person; or argue that some benefit/cost ratio should be used; or argue that if a sovereign state is extinguished (e.g. a Pacific atoll nation) then that is the definition of 'dangerous'.

Thus you can see that I do not believe we can arbitrarily choose 550ppm or 650ppm, as done by many scientific pronouncements (including the IPCC and others), and claim that is our target. This can only be done by using the instruments of social and political discourse on an international scale.

What we can say is that the higher the concentration of CO₂ reached the greater the likely risks associated with that concentration will be. But this is a relative argument, not an absolute one.

-What does that level equate to in terms of percentages of emissions reductions and by what date should we aim to reach that level?

So you see this second question I cannot answer. What we need to be doing, while we debate the first question, is to put in place measures/mechanisms/processes that will now, and in the future, give us greater flexibility of choice about different energy systems that have different carbon ratings. The process is more important than the targets, as the Kyoto negotiations have amply demonstrated.

In 10 years time, what we regard as 'dangerous' climate change will be very different from today - and different again in the year 2020. We therefore need an emissions reduction strategy that is flexible and reflexive to the changing demands of society.

We are aware that there is currently no consensus within the scientific community on what an appropriate level for atmospheric concentrations is.

Indeed not - and there never can be. This question is not appropriately answered by science - it has to be answered by society! This is a very important point to get across.

Our aim in this endeavour is to share with our readers the values considered relevant to this debate to illustrate why a consensus is difficult to achieve.

Exactly so - and in the end it is a matter of risk assessment and risk management. And with most matters of risk, it is the perception by different individuals that matters more than any quasi-objective estimate of risk. Temperamentally I take more risks than does my wife - my concept of dangerous climate change is likely therefore to be quite different from hers. Writ large and across the nations of the world, this is the problem of climate change management.

Thank you in advance for your consideration.

Sincerely,
Matilda Lee
The Ecologist

Get your FREE download of MSN Explorer at [1]<http://explorer.msn.com/intl.asp>

Get your FREE download of MSN Explorer at [2]<http://explorer.msn.com/intl.asp>

References

1. <http://explorer.msn.com/intl.asp>
2. <http://explorer.msn.com/intl.asp>

From: Rob Swart <Rob.Swart@rivm.nl>

To: wigley@ucar.edu

Subject: Re: TGCIA scenario recommendations

Date: Mon, 27 Aug 2001 11:39:11 +0200

Cc: m.hulme@uea.ac.uk, parryml@aol.com, Rob Swart <Rob.Swart@rivm.nl>, steve smith <ssmith@pnl.gov>, s.raper@uea.ac.uk, Tsuneyuki MORITA <t-morita@nies.go.jp>, tim.carter@fmi.fi

Dear Tom,

Thanks for your message and papers. The problem is clearly one of the science-policy interface. If science cannot demonstrate that it makes a difference in terms of avoided climate change and impacts if GHG concentrations are stabilised, why bother? Currently a Danish guy, Björn Lomborg, is making the headlines again (Guardian, New York Times, Economist), TV programmes, etc.) telling the public (and policymakers) not only that there aren't any environmental problems, but also, even if climate change may be real, it does not make any sense at all to do something about it, since efforts to control GHG emissions are expensive and the mitigation would not make any difference at all anyway in terms of avoiding negative consequences. Very popular message. Now clearly, scientists should clearly explain what they can say about this issue. My expectation would indeed be that comparing climate changes resulting from reference cases and from stabilization cases would not be distinguishable until well into the 2nd half of the century (like in the GRL paper), but if this is so, so be it. 2050 seems a lot closer now in 2001 (2050 is THIS century and our childrens' lifetime) than it was in 1999 (when 2050 was something of the next century and some abstract next generations). It is a matter of communication skills to get the message across about the long timescales and inertia of the systems involved, and the difficulty of identifying the climate change signal in the noise of natural variability. I would be curious what your opinion is about the UK work of Nigel Arnell, Martin Parry, John Mitchell and others, analysing the (significant) avoided impacts of 550 stabilisation from an IS98a reference. Another strategy of concerned scientists may be not to do these analyses at all in order to avoid a possible result that the differences between reference and stabilisation can not be demonstrated in a scientifically credible and unambiguous way and hence climate policy action may be obstructed. To me, this does not seem to be the honest way to go.

I am not sure what this all implies for the planned recommended stabilization runs. Your points about the climate sensitivity and non-CO2 gases are well taken. I am not sure the sulfur emissions in the proposed post-SRES scenarios would make a lot of difference, since already in the SRES base cases sulfur emissions are pretty low, and these would only be slightly different (usually lower) in the stabilisation cases. You suggest "carefully constructed idealized scenarios". Do you mean carefully constructed from the climate system point of view in order to get "distinguishable results", or carefully constructed from the socio-economic

point of view so as to analyse real-world consistent and plausible futures (the latter is what Morita's exercise tried to achieve)? My answer would be: both.

I'd like to reflect a little bit more on this and since I am a scenario expert rather than a climate expert, await reactions from people more expert in the area of climate modelling, like Sarah, Mike and Tim, and Martin himself as chair of the TG CIA.

Thanks again,

Rob

Tom Wigley
<wigley@ucar.edu>
25-08-01 01:47
Please respond to wigley

To: Rob Swart <Rob.Swart@rivm.nl>
cc: parryml@aol.com, tim.carter@fmi.fi, m.hulme@uea.ac.uk, s.raper@uea.ac.uk, Tsuneyuki MORITA <t-morita@nies.go.jp>, steve smith <:ssmith@pnl.gov>, (bcc: Rob Swart/RIVM/NL)
Subject: Re: TG CIA scenario recommendations

Rob and others,

The key thing with doing stabilization runs with AOGCMs is (as Rob says) that the different cases "would have to be distinguishable from one another". This is the crux of the problem (in fact, it is a non-trivial problem even to define what is meant by "distinguishable from one another").

A few years ago we decided to try to do some matched no-climate-policy and (550ppm) stabilization runs where the two scenarios had some semblance of realism. (It turns out that the only similar work is that done by the Hadley Ctr, but the scenarios they used are highly idealized.) Our runs were also idealized in that we only changed CO2 -- in the best scientific tradition of changing only one thing at a time to assess sensitivities. The first results of our exercise (using CSM) are in Dai et al., J. Climate 14, 485-519, 2000. A number of things were clear from this. First, one cannot tell much from single realizations of the two cases -- ensemble runs are essential. Second, as we already knew

from running simple models, the no-policy and stabilization runs diverge only slowly. Even after 50 years, the two are only just distinguishable at the global-mean level; so, clearly, differences at the regional level (especially for precipitation) would not be detectible above the noise of natural variability.

So our next step was to do ensembles of 5, this time using PCM instead of CSM (this paper is in press in BAMS -- for a pdf preprint, look at www.cgd.ucar.edu/cas/adai/). Even then, for ensemble means, the separation between the no-policy and stabilization cases is slow. So I devised an extended no-policy case out to 2200 (50 years beyond where the CO2 level stabilizes in the stabilization run), and we extended some of the runs out to 2200. This work is in press in GRL (and downloadable from the above site). Additional important results come from these experiments. One important result is that, even for precipitation, the *patterns* of change are not detectibly different between the no-policy and stabilization runs. A second important result is that, for most of the world the intra-ensemble differences are similar to or greater than the underlying signals of change. Distinguishing the no-policy and stabilization runs therefore presents a much greater challenge than any of you probably realize.

There are two issues to keep in mind, however. The first is that PCM and CSM have quite low climate sensitivities. So, will things be different if one used a more sensitive model? I suspect not in any major way. The reason is because inter-annual variability tends to be higher in more sensitive models, so the signal-to-noise ratio may not change much. This also applies to the intra-ensemble noise, since the root cause of these intra-ensemble differences is the internal variability of the model.

The second issue is that we have only changed CO2 in our experiments. We know that attempts to stabilize CO2 via emissions reductions also affect SO2 emissions -- so perhaps the no-policy and stabilization cases might be more distinguishable if one accounted for these concomitant SO2 effects? I have addressed this issue at the global-mean level in a paper on stabilization that I will attach to this email. (A more extensive analysis is in another paper, with Steve Smith as my co-author, that I am not ready to share with anyone just yet.) My judgment, as someone with quite a lot of experience in this area, is that having full spatial details will not make the problem any easier; since, as the spatial scale is reduced so the noise increases.

My recommendation from all this is that, first, you read the attached paper (and I would welcome feedback on this) and the three above-mentioned Dai et al. paper. Then, you might want to re-consider what your strategy should be. In my view, I do not think we as a community are at the stage where we can blindly develop paired no-policy and stabilization scenarios and simply feed them into AOGCMs to see the consequences. I believe that carefully constructed idealized scenarios (perhaps based on what Morita is doing) will provide much more useful

information. You are already probably well aware of the need to do ensemble runs, and I don't need to remind you how computationally expensive this can be.

I hope these comments, and the papers, are useful. I'm sorry that it is impossible for me to come to the Barbados meeting, but I am willing to help in any way that I can.

Best wishes (and good luck), Tom.

Rob Swart wrote:

>
> Dear Sarah, Tom, Tsuneyuki, Martin, Mike and Tim,
>
> Back from holidays I found your email exchange. Let me first apologize that
> I did not inform Sarah about this TGCIA action. I remembered from the
> IPCC-TGCIA meeting ? apparently wrongly - that Mike and/or Tim would inform
> Sarah, as they would be in touch with her anyway (I did not even have
> Sarah's email address at the time). Let me also reiterate the reason for
> Tsuneyuki's invited proposal. In order to have comparable GCM results
> available and impact studies based on these results at the time of the IPCC
> Fourth Assessment Report, and taking into account that GCM teams are
> unlikely to perform dozens of runs, the IPCC-TGCIA (chaired by Martin)
> intends to recommend a limited set of both baseline and stabilization
> scenarios for such runs. In this way, impact modellers in the coming
> years
> could base their analysis on different runs from different GCMs for the
> same socio-economic scenario(s). Evidently, teams are free to run
> whatever
> scenario they think interesting, but comparability would be preferable,
> and
> many teams have proven responsive to IPCC-TGCIA recommendations in the
> past
> as I understand it.
>
> The TGCIA has reached agreement on which 4 of the 40 SRES baseline
> scenarios would be most interesting (see meeting report: 4 scenarios
> (A1FI,
> A2, B1 and B2) for 3 time periods 2020s, 2050s and 2080s). The next
> question was: since a (maybe "the") core policy question is what the
> benefits (or avoided impacts) would be of stabilizing GHG concentrations
> at
> various levels, and since impact analysis should be based directly on GCM
> results rather than on results from simple climate models/IA models, it
> would be useful to also recommend a limited set of stabilization cases.
To

> make this a sensible effort, all the cases would have to be distinguishable
 > from one another from a GCM viewpoint. This may allow for combining various
 > scenarios which may be very different socio-economically, but would give
 > very similar climate results for this century, such as the B1 and 550, and
 > the 650 and B2 cases. The stabilization cases would be selected from the
 > following table, of which the cells contain available (post-SRES) scenario

> runs:

| | 450 ppm | 550 ppm | 650 ppm | 750 ppm |
|------|---------|---------|---------|---------|
| A1T | | | | |
| A1B | | | | |
| A1FI | | | | |
| A2 | | | | |
| B1 | | | | |
| B2 | | | | |

>
 > It was suggested to select 2-4 cases from the more than 70 scenarios runs
 > in the post-SRES programme co-ordinated by Tsuneyuki. Tom, it may well be
 > that your "post-WRE" work serves the same purpose, but the rationale for
 > selecting post-SRES cases would be: consistency with the SRES narratives
 > and numbers of the IPCC, and the much-acclaimed multi-model characteristics
 > of the (post-)SRES work. To downsize the 70-odd cases to 2-4 cases and
 not
 > burden Sarah too much, it was suggested to have one model (MAGICC) run a
 > subset of some 10-15 cases which seemed to make sense. Please also note
 > that not all 70-odd cases are useable, either because they do not have
 all
 > relevant GHG gases, or there have been questions about the
 > consistency/quality of their assumptions, e.g. a correct simulation of
 the
 > SRES base case by teams participating in post-SRES but not in SRES
 (right,
 > Tsuneyuki?). More importantly, Tsuneyuki used his intimate knowledge of
 all
 > cases and their distribution over base cases and stabilization levels to
 > recommend 13 cases. This selection was discussed with me and Naki during
 a
 > brief meeting in Washington in June and seemed to be a very appropriate

> one.

>

> I noted the remark by Sarah that mean climate change results would be
> rather be model-independent (for a given climate sensitivity), while
> Tsuneyuki notes the large differences in the post-SRES work. These
> differences may not have to do with different approaches with respect to
> the carbon cycle or radiative forcing calculations, but rather with the
> freedom modellers had (or rather: took) in selecting the time path
> (beyond
> 2100) towards stabilization/time horizon, and the changes in emissions of
> non-CO2 GHG in the stabilization analyses which focused primarily on CO2
> stabilization. This would need to be clarified in detail for the runs to
> be
> selected, and I suggest that only those runs are further used for which
> the
> authors provide sufficient information on these issues.

>

> Concluding, I would like to ask Sarah, if she would be willing to take
> the
> material provided by Tsuneyuki and perform the required calculations for
> the 13 cases (radiative forcing, global mean temperature and sea level
> rise, right, Mike/Tim?) within the next 1-2 months. The results would be
> discussed electronically in a small group (the addressees of this
> message)
> in October/November and a preliminary proposal based on these discussions
> would be the input for a discussion on this issue during the next TG CIA
> meeting in Barbados, in November. Tom's recent work may be useful for
> this
> discussion as well, and I wonder if the mentioned (draft) papers could be
> distributed to this group or even the full TG CIA.

>

> Kind regards,

>

> Rob

>

> Dr. Rob Swart
> Head, Technical Support Unit
> Intergovernmental Panel on Climate Change Working Group III: Mitigation
> P.O. Box 1
> 3720 BA Bilthoven
> Netherlands
> tel. 31-30-2743026
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Attachment Converted: "c:\eudora\attach\9-550TEM1.XLS"

From: Mike Hulme <m.hulme@uea.ac.uk>
To: Klaus Hasselmann <klaus.hasselmann@dkrz.de>, Carlo.Jaeger@pik-potsdam.de, Martin Welp <Martin.Welp@pik-potsdam.de>, schellnhuber@pik-potsdam.de, Ottmar.Edenhofer@pik-potsdam.de, tol@dkrz.de, ccarraro@helios.unive.it, ccarraro@unive.it, juergen.engelhard@rheinbraun.de, baldur.eliasson@ch.abb.com, hourcade@msh-paris.fr, ola.johannessen@nrsc.no, gretz@maill.tread.net, bill.hare@ams.greenpeace.org, SSinger@wwfepo.org, guentherr@wwf.de, gberz@munichre.com
Subject: Re: ECF position paper
Date: Fri Aug 31 17:37:14 2001

Klaus,

A few belated comments on your 1st draft which is looking promising:

a. we need to be careful about using concepts/terms such as 'unacceptable' global warming.

As I think Richard Tol says, we do not have any sound basis for determining what

constitutes 'dangerous' climate change. Is it one life lost? a nation-state inundated?

or some more utilitarian exceedance of a benefit/cost ratio? Does every citizen on the

planet have a vote or just each government? We should draw attention to the rather flimsy

basis upon which notions of safe or dangerous, tolerable or unacceptable climate change are

debated. In the end of course there are lots of things we may view as 'unacceptable' (war

for example), yet they happen and we survive. I think this is an area rich for research

and we could draw out some of the dimensions.

b. later on you use the idea of balancing abatements costs vs. the risks of climate

change. I think we need to use the language of risk here and to draw upon insights

developed by risk analysts (academic and professionals) about how we frame the climate

change problem in risk terms. The differential perceptions of risks, inc. climate ones,

therefore becomes central in addressing point a.

c. the proposed ECF project on changes in extreme weather is of course a necessary first

step towards the quantification of climate risks. This should be one of the justifications

for work in this area. It is also the case that better understanding of these changes will

yield insights into how adaptation does or should proceed, at both environmental systems

and institutional systems levels.

d. re. nuclear energy in a climate protection portfolio, the ECF should be bold and should

question and expose assumptions made on both sides of the debate about the up and

down-sides of this technology. It is rising higher on the UK agenda and there will be some challenging times ahead in this country about its rightful place and role.

I look forward to seeing the second draft,

Mike

At 14:24 11/08/01 +0200, Klaus Hasselmann wrote:

Dear colleague:

I was requested on the 6.August telephone conference by the ECF skeleton board and the

members of the former ECF steering committee to coordinate the writing of an ECF position

paper, as agreed upon at the ECF meeting in Brussels on July 12.

It was proposed that we complete the position paper and present it to the press about a

week in advance of the Marrakech COP 7 meeting in November this year.

I suggest the following timetable:

1) preliminary agreement on the structure and contents of the paper by the end of this

month,

2) production of first draft in September,

3) detailed discussion of first draft on 2nd October in Potsdam (an additional day ahead

of the 3-4.October ECF meeting, which was proposed on 6.August to discuss the details of

the various projects agreed upon at the Brussels meeting)

4) completion of the paper in October.

5) November: presentation of the paper

I would hope that apart from the 2nd October meeting we can achieve our task by e-mail.

But a meeting may be necessary in September. If so, we should try to combine it with one

of the other project meetings that will be taking place in September.

Everybody is invited to participate. Please feel free to copy this mail to other ECF

members or potential members who I may have missed.

It has been suggested that the position paper should be short, about 5 pages, plus some

appendices if necessary. To get the discussion going, I propose the attached structure as

straw man. Please note that many of the points I have listed are my own views, and I

will be happy to - and expect to - modify them based on your responses.

With best regards

Klaus

Prof. Dr. Klaus Hasselmann

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From: "Michael E. Mann" <mann@virginia.edu>

To: Ed Cook <drdendro@ldeo.columbia.edu>

Subject: Re: Esper/Cook paper

Date: Mon, 10 Sep 2001 10:35:13 -0400

Cc: "Malcolm K. Hughes" <mhughes@ltrr.arizona.edu>, "Michael E. Mann" <mann@virginia.edu>, Crowley_Hegerl <tcrowley@nc.rr.com>, jto@u.arizona.edu, rbradley@geo.umass.edu, Jan Esper <esper@wsl.ch>, srutherford@gso.uri.edu, p.jones@uea.ac.uk, k.briffa@uea.ac.uk

<x-flowed>

Hi Ed,

Just to reiterate one more key point--Superimposing the two series and their uncertainties is not the whole story (although it is a definite improvement over just showing the two reconstructions on top of each other w/ know assessment of uncertainty). However, doing the above still only poses the question:

apple +/- [uncertainty in apple] =? orange +/- [uncertainty in orange]

As we discussed in a previous email exchange (based on the correlations you calculated between instrumental series w/ the trend removed) , the two reconstructions should probably only share about 60% or so variance in common in the best case scenario, where there is no uncertainty at all, owing simply to the differing target regions/season...

So we need to be very careful w/ the following statement which you made in your previous email:

"If so, this would not mean that the series are not significantly different from each other. One can't dismiss the highly systematic differences at multi-centennial timescales quite so easily."

I'm not sure you can justify that statement based on sound statistical reasoning!

I agree w/ your following statement "Why these differences are there is the crux question."

However,I hope the discussion will accurately reflect the fact that the leading hypotheses to be rejected in answering that question are 1) random uncertainty in the two series owing to differing data quality and sampling, etc. can explain the difference and 2) systematic differences owing to differing target region and seasonality can explain any residual differences after (1).

That may be a tough standard to beat, but it *is* the approach that Tom, Phil, Keith, and I have all been taking in addressing the issue of whether our different reconstructions are or are not inconsistent and the conclusion has in general been (see e.g. IPCC which was really a consensus of many of us, though admittedly only I was a lead author) that, despite notable differences in the low-frequency variability, the different reconstructions probably cannot be considered inconsistent given the uncertainties and differences in seasonality/spatial sampling. I have a hard time understanding why the same standard should not be applied to comparisons w/ your current reconstruction?

Does your RCS reconstruction really not fall in the mix of all the other reconstructions? Is it truly an outlier w/ respect to Phil's, Tom's, MBH, and other existing N. hem reconstructions that are based on different seasonality and regional sampling???

We've probably had enough discussion now on this point, so I'll leave it to you to discuss the results in the way you see most fit, but I really hope you take the above points into account, in fairness to the previous work...

I look forward to seeing the final manuscript in one form or another, in any case,

cheers,

mike

At 08:10 AM 9/10/01 -0400, Ed Cook wrote:

>I do intend to put in a new Fig. 5 that will compare the mean RCS with MBH,
>including each series' confidence limits. This will be done on low-pass
>filtered data (probably 40 year because of what Mike has sent me). I am
>sure that there will be significant overlap of confidence limits,
>especially prior to AD 1600, when they are quite wide in MBH. If so, this
>would not mean that the series are not significantly different from each
>other. One can't dismiss the highly systematic differences at
>multi-centennial timescales quite so easily. Why these differences are
>there is the crux question.

>
>Cheers,
>
>Ed

>>Dear Ed and Jan,
>>I have a couple of general comments, and then some specific little things
>>that
>>may be helpful. It is possible that some of the answers to my questions
>>may be
>>in the two manuscripts in review or in press (TRR and Dendrochronologia) to
>>which you refer.
>> It seems that your results are consistent with the general shape and
>>some of the detail of the MBH99 series, apart from departures before 1200
>>and in
>>the 19th century. As the two datasets are largely, but not completely,
>>independent, this is an important result. At the time when your
>>replication is
>>weakest, there appear to be differences between the linear and
>non-linear RCS

>>curves and the MBH series. Before about 1200 your dataset is dominated by
>>material from four sites, I think - Polar Urals, Mongolia, Quebec and the
>>Taimyr
>>Peninsula. It therefore seems to me that it is important to make the
>>kinds of
>>direct graphical comparisons that Mike suggests of both your series and
>>the MBH
>>series (superimposed and with their confidence limits shown). Perhaps the
>>differences you note are not robust, and then there would seem to be little
>>reason to seek climatological explanations. I suggest that the graphical
>>comparison Mike suggests will be important since it should allow some
>>assessment
>>of the extent to which MBH and others have or have not underestimated
>>temperature in the AD 1000-1400 period, if your arguments hold up.

>> I think that a reasonable reader would have some questions about this
>>particular application of the RCS approach. Maybe an expansion of the
>>footnote
>>might help. How does the determination of the form of the regional
>>standardization curve itself depend on replication within each sampled
>>population? Do we know that the regional standardization curve does not vary
>>with time? Or, do we know that the regional standardization curve does
>>not vary
>>with climate on multicentennial timescales? If so, how? Is it not quite
>>possible
>>that the level of the part of the curve for, say, trees between ages 100
>>and 300
>>is set by climate in the early life of the tree, or that it is itself
>>directly
>>determined by contemporaneous temperatures? A number of these questions
>>occur to
>>me because I have been struggling with RCS in the Yakutia material I have
>>been
>>working on with Gene Vaganov. We have a very good situation for the
>>application
>>of the method, with a couple of hundred samples for which we have pith - no
>>estimate needed. Even so, the resulting chronology, once calibrated, gives
>>impossible temperatures in the early part of the millennium. They imply mean
>>early summer temperatures of up to 18 degrees Celsius, which, at 70 degrees
>>north would have led to massive ecological and geomorphological change.

> I can
>>find no evidence for this. I would not be at all surprised if an

>>examination of
>>the Taimyr material you used were to show the same thing. I say this
>>because I
>>know Mukhtar Nuarzbaev's RCS chronology from the Taimyr shows these very
> high
>>levels at precisely the same time as the Yakutia material. Perhaps Mukhtar
>>and I
>>are misapplying the RCS method - a real possibility at least as far as I am
>>concerned. Alternatively, there is some problem with RCS that we have yet to
>>identify.
>>We are all stuck with a more fundamental problem, which is that we have no
>>way
>>to calibrate multicentennial variations. You have used one method of
>>producing
>>chronologies with greater low frequency variability, one that has some very
>>appealing characteristics. There are other ways the same objective could be
>>reached, but we do not have a simple way to choose between them in most
>>cases. I
>>do think it would be interesting to compare the RCS for the Sierra Nevada
>>material you used, if it contains enough samples to do that, with the Great
>>Basin upper forest border network, as highgraded to only contain samples
> with
>>minimum segment length of 500 years, and very conservatively detrended.
>>
>>Here are some specific points:
>>In the penultimate line on page 2 you refer to 1,205 tree ring series
> from 14
>>locations. Some readers will for sure be confused by the word "series" in
>>this
>>case - how about "core samples" or "radii" or "trees"?
>>Page 3 - I need to check this, but I think the segment lengths in the
>>relevant
>>series in the MBH99 analyses are much longer than 400 years.
>>Page 5 - The differences of timing in high values between the linear and
>>non-linear chronologies are actually quite striking. I think if you and I
>>were
>>looking at a couple of subsamples from a single site we would put these
>>differences down to inadequate sample depth.
>>Page 6 - you talk about the two series (RCS and MBH) disagreeing strongly,
>>but
>>at the moment there is no basis available to the reader to see how strongly.
>>This comes back to Mike's suggestion of a direct graphical comparison with
>>confidence limits, etc.
>>
>>Hope this helps, Cheers, Malcolm
>>
>
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</x-flowed>

From: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>

To: "Michael E. Mann" <mann@virginia.edu>, Ed Cook <drendro@ldeo.columbia.edu>

Subject: Re: Esper/Cook paper

Date: Mon, 10 Sep 2001 12:40:42 -0700

Cc: "Malcolm K. Hughes" <mhughes@ltrr.arizona.edu>, Crowley_Hegerl <tcrowley@nc.rr.com>, jto@u.arizona.edu, rbradley@geo.umass.edu, Jan Esper <esper@wsl.ch>, srutherford@gso.uri.edu, p.jones@uea.ac.uk, k.briffa@uea.ac.uk

Dear Ed - Didn't Keith Briffa also come up with a more marked LIA than MBH99 in his age-band work? If this turns out to be right, it should eventually be easier to find the sources of the differences between the reconstructions, just by virtue of there being not only many more tree-ring data for that period, but also more other, data, such as documentary. Cheers, Malcolm

Malcolm Hughes

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From: Ed Cook <drdendro@ldeo.columbia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: the real message
Date: Mon, 10 Sep 2001 16:45:18 -0400

Hi Keith,

You probably haven't seen the newest version, which has not yet been submitted, but I CLEARLY state that several of the data sets/sites used in the paper have been used before and I reference all of the relevant papers. I never implied anywhere that this was the first successful use of RCS. I also reference your Quat. Sci. Rev. paper and your Age Banding paper. I also state in the concluding section that what has been shown is not new, but it is somewhat novel (the separation of the data into RC curve classes and the regionalization of the data on the scale described) and informative. I stand by that completely. So, the version I am working on covers (hopefully) some of your concerns/complaints. I will do my best to be "fair" before I finally submit it. However, this is a Report to Science (~2500 word limit), so I can't do the kind of review of the literature and detailed discussion of results that would be possible in more normal size papers.

Sorry for sounding a bit testy here. I've been fielding a whole raft of questions, comments, and criticisms from Mike Mann, Tom Crowley, and Malcolm Hughes. Some of them useful, many of them tiresome or besides the point. I never wanted to get involved in this quixotic game of producing the next great NH temperature reconstruction because of the professional politics and sensitivities involved. All I wanted to do was demonstrate with Jan that Broecker was wrong, something that you have obviously done a few times before but in journals that Broecker and others don't follow closely (I guess. I should also say that the amount of ignorance about tree rings in the global change/paleo/modeling community is staggering given what has been published. Like it or not, they simply don't read our papers.). In so doing, it seemed reasonable to compare the RCS chronology against the hockey stick because that is the series that Broecker was railing against. That is why I didn't bother to compare the series against all the other records produced by you, Phil, and others. Jan originally did that, but I chose to restrict the comparison to tighten the focus of the paper. More reference to your results is clearly justified, so maybe I was wrong here.

This all reinforces my determination to leave this NH/global temperature reconstruction junk behind me once I get this paper submitted. It's not

worth the aggravation. However, the paper is something that I need to do for Jan. And I still think it is a good paper.

Cheers,

Ed

>What I really mean is that you have written this paper implying that you
>are getting low-frequency NH temperatures out of tree-ring data for the
>first time- using the RCS. You set up this question then use a lot of data
>in your analysis and the RCS as though they have not been analysed like
>this before and then show you get more of a LIA than Mann , while
>ignoring the fact that I have already produced calibrated summer
>temperature curves (in the Science Perspective piece) from RCS ring width
>data in Sweden , Urals , Taimyr and (in the JGR paper) using banded
>density - which both show more low frequency than MBH. The real question is
>whether MBH use data in tropical and mid latitudes that supress what is
>really a high latitude summer signal in their northen predictors ? I just
>don't think you are being very fair here- despite how many times you cite
>me (perhaps the citations should anyway reflect the useful contributions
>to a particular area even if they number more than a token couple)
>that's off my chest now

>cheers

>Keith

>

>--

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: Ed Cook <drendro@Ideo.columbia.edu>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Esper/Cook paper
Date: Mon Sep 10 20:34:13 2001
Cc: "Malcolm K. Hughes" <mhughes@ltrr.arizona.edu>, Crowley_Hegerl <tcrowley@nc.rr.com>, jto@u.arizona.edu, rbradley@geo.umass.edu, Jan Esper <esper@wsl.ch>, srutherford@gso.uri.edu, p.jones@uea.ac.uk

Ed
I still believe you are not showing sufficient comparisons with series besides the MBH ; necessary to demonstrate the true extent of "new" information in this work. At the very least this needs to acknowledge that other (and other tree-ring-based) series are out there , that use at least some of the data you employ , and use the RCS method to process may of their constituent series - i.e. the Northern chronology series shown in my QSR paper. What is similar and what is different in your series and this one? You give the impression here that you are using the RCS and new data to demonstrate the possibility of getting more low frequency signal from tree-ring data - but then you base this on a comparison with MBH only. Surely what is needed here is to establish WHY MBH don't get as much LIA for example . By not showing that other tree-ring data that have also shown a LIA , and not exploring why MBH does not (despite using some of the same -and note -already RCS standardised data) is perhaps confusing rather than clarifying the issue. When we discussed this here, I also suggested the need to show separate "north" and more "south" curves ,separated in your data set, to try to get at least some handle on the independent expression of the centennial trends in a region south of the over-exploited northern network . At the very least it should be clearly stated that many of the site data used here and in previous work (see our Science perspectives piece) are common and other series already produce more low-frequency signal than is implied in MBH . Sorry for this rushed comment but I wanted to get this point over as we had talked about it before but you don't seem to have taken it on board.

cheers

Keith

At 02:51 PM 9/10/01 -0400, Ed Cook wrote:

Hi Mike et al.,

Okay, here is an overlay plot of MBH vs. RCS, with RCS scaled to the 1900-1977 period of MBH, and with 95% confidence limits. This has been done for the 40-yr low-pass RCS data to be consistent with the low-pass MBH series you sent me. The 95% confidence limits of the RCS are also scaled appropriately. Since correlations with both instrumental and MBH are $O(0.95)$ after even 20-year smoothing because of the trend, the RCS limits are effectively based on the bootstrap 95% limits of the 14 chronologies. Assuming that the original RCS C.I.s are reasonably accurate (which I think they are), what is apparent (to me anyway) is that the confidence limits of MBH are uniformly narrower after AD 1600. Prior to that, they are comparable to RCS back to ca. AD 1200 where RCS C.I.s get bigger. Of course this is an odd comparison because the confidence limits are not derived the same way. However, I do think that they are somewhat informative nonetheless. What is also apparent is the much great amplitude of variability in the RCS estimates. This is consistent with the understanding that extratropical temperatures are more variable than tropical temperatures, which supports the idea that the MBH record does have more tropical temperature information in it. The other interesting thing about expressing the RCS data this way and overlaying it on MBH is the appearance that MBH is missing the LIA rather than the MWP, at least on multi-centennial timescales. This turns some of Broecker's criticism of the "hockey stick" on its head. I'm not sure where all this leads.

Any comments and further suggestions are welcome as long as they come in by tomorrow. I am definitely submitting the paper within a day or two.

Cheers,
Ed

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[1][http://www.cru.uea.ac.uk/cru/people/briffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/briffa[2]/)

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: INTAS
Date: Tue, 11 Sep 2001 17:03:28 +0500
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith,
below is the list of the Ekaterinburg team members with brief description.

=====

Stepan G. Shiyatov, Prof., Dr., head of the Laboratory of Dendrochronology, leader of Ekaterinburg team, took part in collecting subfossil wood in the Yamal Peninsula, cross-dating ring-width series, developing and analysing the multimillennial ring-width chronology. He has also carried out the work on evaluation of changes in composition and structure of forest-tundra ecosystems in Polar Urals.

Rashit M. Hantemirov, Dr., took part in collecting subfossil wood in the Yamal Peninsula, cross-dating ring-width series, developing and analysing the multimillennial ring-width chronology. He has also developed and analysed juniper chronology in Polar Urals.

Valery S. Mazepa, Dr., took part in treatment of individual ring-width series and analysing of the Yamal long chronology. He has also carried out the work on estimating of changes in woody biomass in Polar Urals.

Alexander Yu. Surkov, technician, took part in collecting, preparing and measuring the subfossil wood from Yamal Peninsula

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Finances(Eu):

| Labour | Overhead | travel/sub | equipment | consum | other | total |
|--------|----------|------------|-----------|--------|-------|-------|
| 12500 | 1250 | 7900 | 2950 | 400 | 0 | 25000 |

For any case - how many got each team member (Eu):

Shiyatov S.G. 4000
Mazepa V.S. 3800
Hantemirov R.M. 3700
Surkov A.Y. 1000

Best regards,
Stepan G. Shiyatov
Rashit M. Hantemirov

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Fax: +7 (3432) 29 41 61; phone: +7 (3432) 29 40 92

From: Keith Briffa <k.briffa@uea.ac.uk>
To: t.osborn@uea.ac.uk
Subject: [Fwd: Rapid Climate Change]
Date: Fri Sep 28 12:51:28 2001

Date: Thu, 27 Sep 2001 11:32:30 +0100
From: Simon Tett <simon.tett@metoffice.com>
Subject: [Fwd: Rapid Climate Change]
Sender: simon.tett@metoffice.com
To: k.briffa@uea.ac.uk, sandy.tudhope@ed.ac.uk
X-Mailer: Mozilla 4.75 [en] (X11; U; HP-UX B.11.00 9000/782)
X-Accept-Language: en

Dear Keith/Sandy,

please don't pass on or discuss further -- this is the email I got from Phil Newton. So with some reluctance I get to put up a strawman. I will go with what we discussed in London but some nice graphics (or any thoughts) would be helpful -- do you have any you can send me.

Simon

Date: Fri, 21 Sep 2001 16:02:14 +0100
From: Philip Newton <ppn@nerc.ac.uk>
Subject: Rapid Climate Change
To: sfbtett@email, a.j.watson@uea.ac.uk
Cc: Meric Srokosz <MAS@soc.soton.ac.uk>,
Catrin Yeomans <CVY.DST.Swindon@wpo.nerc.ac.uk>,
Judy Parker <JMP.DST.Swindon@wpo.nerc.ac.uk>,
Nigel Collins <NRC.DST.Swindon@wpo.nerc.ac.uk>,
Neville Hollingworth <NTH.DST.Swindon@wpo.nerc.ac.uk>
Message-id: <md5:867B0102E7BAE34BCAE86F2E32B8167E>
MIME-version: 1.0

Content-type: multipart/mixed; boundary="Boundary_(ID_5Sy4P7Icy2zVEqcBr4S8jA)"

Dear Simon, Andy,

Many thanks for agreeing to each give an informal presentation to the Steering Committee on the first afternoon of the meeting.

As I mentioned on the phone, what I'm after is for each of you to look at the Abrupt proposal and Prescient proposal/draft-science plan (attached as WORD documents), stand well back, and put forward some ideas for how one might combine them into a single coherent programme. The intention is to lay the foundation for some discussion, both Monday afternoon and evening, in advance of the formal Steering Committee meeting item that will deal with developing a single science plan. All SC members will have the attached documents in their papers.

I'll summarise the few constraints we have at the start of the Monday session, so you won't have to revisit the history; by the time we get to you, all will know that we have

the task of coming up with a single plan, and the events leading up to that circumstance.

The constraints as I see them are:

The Rapid Climate Change programme has a budget of £20m. The Abrupt proposal was written to £16.9m, and the STB decided to invest £17.0m in thermohaline-related rapid climate change. This proposal contained both palaeo and modelling components (as well as modern observational/process work), and a strong complementarity and close working relationship with Prescient was always envisaged by the writers. The Prescient proposal was written to £8m, and the Prescient draft science plan (following reduced award) was written to £4.5m. The STB did not have a discussion about how the science of the two programmes should be combined, but the nature and chronology of events/discussions imply that the STB decision to spend £17m on thermohaline-related work should be respected. I do not see that this has to be translated as an inexorable shackling of the £4.5m Prescient science aims, given that a good fraction of the Prescient draft science plan seems to be potentially relevant to thermohaline-related climate change, and that there is notionally £3m of the £20m that is not tied to thermohaline-related work, and there is a strong palaeo/modelling element to Abrupt.

So much for constraints. I do not want to give the impression that we are after a ring-fencing of Prescient and Abrupt monies and aims within Rapid. I would hope that there is scope for a much more integrated (in the sense of both palaeo/modern and obs/model) and coherent programme than that. One potential conflict, in the modelling context, seems to be the apparently regional approach of Abrupt cf the global approach of Prescient. I suspect (but may be wrong) that there is a scientific debate to be had as to whether an Atlantic-centric approach is sufficient to consider thermohaline-related climate change over NW Europe, or whether a more global treatment is required.

On practicalities, I've got you down for 20 minutes each, and have set aside half an hour for discussion straight afterwards. Please let Catrin Yeomans (cvy@nerc.ac.uk) know your audio-visual needs.

Get back to me if you need further clarification.

All the best,

Phil

Dr Philip Newton

Head of Marine Sciences Team

Science Programmes Directorate

Natural Environment Research Council

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa>[2]/

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "R K Pachauri" <pachauri@teri.res.in>
Subject: TERI launches TerraGreen, an e-magazine on the environment
Date: Wed, 28 Nov 2001 16:40:00 +0530

TERI is proud to announce the launch of TerraGreen (<http://www.teriin.org/terragreen/>), an e-magazine that will bring you news about energy, environment and sustainable development from India, once every two weeks.

TerraGreen was formally launched on Wednesday, November 28, 2001 by Mr. C. M. Vasudev, Secretary, Department of Economic Affairs, Government of India in New Delhi.

You are receiving TerraGreen because you have shown interest in TERI's research, multifarious activities or numerous publications over the years. Your address is saved in TERI's central database of e-mail addresses. If you should prefer not to receive this e-mail in future, please let us know. To do this, please scroll down to the end of this e-mail.

TERRAGREEN
News to Save the Earth

Issue 1, 15-30 November, 2001

Letter from the editor

Here is the first issue of TerraGreen, an e-magazine that will bring to you the most significant shakeouts in India's energy, environment and sustainable development scenarios. For concerned individuals across the world looking for reliable news and information in these fields from India has often been an uphill task. TERI has worked for over quarter of a century to disseminate information from these very fields. Taking that mandate forward, TerraGreen will bring you analytical, unbiased and straightforward reportage. In the wilderness of the Internet you will soon learn to rely on TerraGreen for news, views and information. So, welcome to the wilderness. Enjoy.

For full text click on:
<http://www.teriin.org/terragreen/issue1/letter.htm>

News of the fortnight

What's happening in our green horizons and elsewhere? TerraGreen's news updates bring you the latest in environment news.

This issue's headlines

Pepper and people power

Periyar Tiger Reserve, Kerala- The India Ecodevelopment Project brings a much-needed economic fillip to the lives of Mannan and Paliyan tribals through pepper cooperatives. Find out how it all happened at <http://www.teriin.org/terragreen/issuel/news.htm#pepper>

Sunny through the clouds

New Delhi- Anybody for the sun? Soft loans for setting up solar water heaters in group housing societies from IREDA. Visit <http://www.teriin.org/terragreen/issuel/news.htm#sunny> to also find out about sun-powered electricity in the high, cold reaches of the Himalayas, for villagers in Leh and Kargil.

Of Birds and War

Afghanistan- The terror of war and bombings in Afghanistan is spreading far. So hangs the fate of India's winged migratory friends -- the Siberian crane, shoveller ducks, the crested poacher and Arctic tern, to name a few. At <http://www.teriin.org/terragreen/issuel/news.htm#birds> read about these avian anxieties.

The Long Story

Let the Gentle Giants Be

Veraval, Gujarat- Fahmeeda Hanfee's first-hand report on the huge but vulnerable whale shark, and on a milestone that is something of a first in the official protection for marine life in India. Hanfee analyses the pros and cons at <http://www.teriin.org/terragreen/issuel/feature.htm#f1>

The Water Harvest

Kalakhoot-Madhya Pradesh, Sangani-Gujarat- Arnab Ray Ghatak's inspiring report of villagers (<http://www.teriin.org/terragreen/issuel/feature.htm#f2>) who looked beyond governmental apathy to drill water from parched lands on their own and are now reaping a golden harvest.

In Conversation

At a time when a lot of people across India are grappling with power shortages, Mr Suresh P. Prabhu, Union Minister for Power, talks to TerraGreen's executive editor in a one-on-one. <http://www.teriin.org/terragreen/issuel/interview.htm>

Centrepiece

No one Need Go Hungry

Dr. L. C. Jain, Chairman, Industrial Development Services, economist and Gandhian, Dr L C Jain, unfolds a simple blueprint to change the bizarre food security situation India faces today - of rotting foodgrains and starvation deaths. Read more about Jain's views at <http://www.teriin.org/terragreen/issuel/essay.htm>. He laments that if Gandhi were to be around today and learnt of this cruel irony, he would invite an assassin to end his life.

Reviews

Get the latest on your green reads. This week: Subhadra Menon reviews Brenda Cranney's *The Mountain Women of Himachal Pradesh*. Plus more short reviews at <http://www.teriin.org/terragreen/issuel/reviews.htm>

People in Action

Ever wondered how to reach people working at the grass-roots? To be able to make a difference? Let nothing stop you, contact them to work alongside, or just to help. Go to: <http://www.teriin.org/terragreen/issuel/people.htm>

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Check out our green calendar <http://www.teriin.org/terragreen/issuel/events.htm> for the fortnight.

Factfile

At <http://www.teriin.org/terragreen/issuel/facts.htm> check out some interesting facts about the environment around us.

CONTACT

Reach the executive editor of TerraGreen at <http://www.teriin.org/terragreen/contact.htm>

FEEDBACK

Need to reach us at TerraGreen with comments or suggestions? The second issue of TerraGreen is in the pipeline, do mail us at terragreen@teri.res.in or please fill the form at <http://www.teriin.org/terragreen/feedback.htm>

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Visit www.teriin.org/dsds/ for full audio and video coverage of the Delhi Sustainable Development Summit, 7-9 Feb. 2001.

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Myles Allen <m.allen1@physics.ox.ac.uk>
Subject: RE: RE: Tyndall proposal
Date: Wed Dec 12 09:29:29 2001

At 00:03 12/12/01, you wrote:

Hi Tim and Phil,
I'm afraid I missed their deadline -- I'm presenting at the Royal Society meeting on IPCC tomorrow, and that had to take priority. If Simon is interested enough to bend some rules quietly, I could certainly get him an outline proposal by Friday, but if not, it'll have to wait until their next call. It's frustrating, but it can't be helped. NERC just have too many calls. As Simon points out, the Tyndall Centre's style may be a more top-down, regulatory approach anyway, and good luck to them. Politically negotiated emission targets may work, but I have to confess to having doubts. Perhaps I have spent too much time talking to Dick Lindzen to believe in central planning any more.

Myles, by "Simon" do you mean Simon Shackley? I don't think he'd be able to bend the rules since the proposals have to go direct to the Tyndall Centre's administrator. As you say, they are being more directive (is that a word?) in what they want this time round, and since your idea isn't central to what they think they want I doubt whether they'd be prepared to bend the rules. Hope the Roy Soc goes well - I hear they're charging 100 quid to listen to you - a bargain!

Tim

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Ed Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Science paper
Date: Mon Dec 17 15:13:14 2001
Cc: esper@wsl.ch

Ed (and Jan)

Frankly I am a bit surprised at your and Jan's response to my letter.

I thought I had explained clearly what I was writing to Science and did only that. After some not too little experience in reviewing for Science and Nature , I returned what I considered to be a very positive response, one which I knew Science would interpret as a call to publish important results. This is precisely what they have done and no more could have been expected. As to the sentiments and opinions expressed , they are objective and , in my opinion still correct. They are to be interpreted as a request for re-thinking the logic and rationale of the presentation. I do not see why they require more than some re-phrasing. Though I will admit that they ask for some minor (entirely justifiable) work to include the correlations with summer seasonal data. I simply would not like to see you write a paper that puts out a confused message with regard to the global warming debate , leaving ambiguity as to your opinion on the validity of the Mann curve and implying that your series is a annual record , when I do not believe that you think it is. To get Science to consider a rewrite is surely what you would have hoped for , and satisfying my remarks , in small or large measure , will not be the determinant in getting this published. Indeed , it may well be that the tone of my letter could have convinced them that this was important work that should be published (though with some provisos) despite what other reviewers may have thought. What did the other reviewers say?

If you think I was too negative then I am sorry that we don't agree entirely - but that at least is the normal !

I would not like this affair to ruin my Christmas , as it surely will if it is the cause of our falling out . As for your message Jan , I prefer to think you were trying to calm troubled waters , though you seem peculiarly adept at doing the opposite where I am concerned, I prefer to ignore the remark about "not wanting to let this curve into Science" (a response might only injure the prospects of any further collaboration) but I will say that it goes without saying that Ed can have his opinion , just as even I can have. I would never consider myself stupid enough to imagine I could ever influence your response to Science by anything other than reasoned argument. If this is not accepted then ,at least Ed I am sure knows that, I would not let this stand in the way of this paper.

Ed, I am sorry to hear about your condition and I do know how debilitating it is. Useless as it is , you have all my sympathy and best wishes for a rapid recovery.

I am likely also guilty of short temper and extreme frustration at the moment because of conflicts between family and work , both sides demanding more time and both being increasingly ill served by me. Somewhere in the middle I feel increasingly suffocated of late and in moments of sane reflection can see that much of the trouble could perhaps be

lessened if one had time to be more considered in ones actions - but the moments of quiet reflection are invariably harder to find.

I am totally confident that after a day's rephrasing this paper can go back and be publishable to my satisfaction by Science. I am equally confident that this interchange was a waste of yours and my time . To the extent that I am culpable , I am truly sorry.

Keith

At 09:23 AM 12/17/01 -0500, you wrote:

Hi Keith,

First, I need to apologize a bit for what I wrote to you. It was a bit over the top and came out during some serious physical discomfort that I am experiencing now from a bout of shingles(? I'll find out from the doctor today). It is all rather painful and depressing. So while I still think that we have very real differences of opinion on the paper, I would hope that we can accept at least some of these differences as part of the scientific debate process and not let it affect us negatively or personally. Paul Krusic came by yesterday and brought with him several parcels from the lab, including the paper from Science. The editor will not accept the paper as submitted, but will consider it after revision. Obviously, this is as good as we should have expected. I will do whatever I can to satisfy the reviewers comments, including yours, but probably can not rewrite it in a way that will satisfy all of your concerns. At that point, it will be up to Science to decide how to proceed.

Regards,

Ed

=====
Dr. Edward R. Cook
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Palisades, New York 10964 USA
Email: drdendro@ldeo.columbia.edu
Phone: 845-365-8618
Fax: 845-365-8152
=====

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Professor Keith Briffa,
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Phone: +44-1603-593909

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[1][http://www.cru.uea.ac.uk/cru/people/briffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/briffa[2]/)

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Stepan Shiyatov <stepan@ipae.uran.ru>
Subject: Re: INTAS final money
Date: Tue Jan 22 15:42:27 2002

Stepan

I have the form , but it is not clear . Where I think I sign (page 1 bottom. under co-ordinator) it says I have to prove my identity in Brussels?

I will phone them to ask before sending the form back. Will Eugene need a similar signature?

Keith

At 12:15 PM 1/21/02 +0500, you wrote:

Dear Keith,

As I realized, our team must receive from INTAS the final sum of 737 EURO. I can get these money via Ekaterinburg Branch of VNESH TORGBANK, as we did earlier. I am sending to you "Payment request" for this sum, and you, as the coordinator, must sign it and send to Brussels. In that case I can receive money in Ekaterinburg.

Last two months I was very busy writing many reports for our activity in 2001. From that days I will begin to work with material obtained from the Polar Urals, mainly cartographic and photographic ones. WE intent to take part at PAGES meeting which will be in May in Moscow.

I wish you, your family and colleagues the best in New Year.

Best Regards,

Stepan stepan@ipae.uran.ru

--

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: tim Osborn <t.osborn@uea.ac.uk>
Subject: Fwd: Re: SCIENCE review
Date: Wed Feb 20 16:25:46 2002

Date: Fri, 30 Nov 2001 13:48:13 +0000
To: "Jesse Smith" <hjsmith@aaas.org>
From: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: SCIENCE review

Dear Jesse

I am sorry for messing you about with this but I really am leading a complicated life at the moment. I am attaching my comments on The Esper et al manuscript . You will see that I think the work is genuinely interesting and potentially of wide significance. The bottom line is that you should publish this but the way the authors have chosen to present their results smacks of a lack of clarity of thought (and a lot of fudging!) . I believe that they are more concerned with trying to temper their ideas so as not to "offend" Mann et al. They choose to present their work as a generalised demonstration of how to process a tree-ring data set merely to argue against an unjustified remark made by Broecker about tree-ring reconstructions in general. This simply devalues the significance of their work as this refutation is out there in the literature already if only Broecker bothered to check. By trying to skate around the real questions that Broecker was implying - i.e. is the methodology removing the true low-frequency variance in the Mann et al curve and is the magnitude of the Medieval warmth understated ? - Esper et al are obscuring the real message of their results - namely that Mann et al do most likely lose the low frequency variance in their reconstruction and they may very well be underestimating the Medieval warmth . To get at this the authors need to be honest about what their data represent (probably summer and certainly not hemispheric wide coverage) and is this really that different from what Mann et al actually represent (even though they believe their's is a mean annual Hemispheric record). I think the authors present a too-simplistic discussion of their curve and then gloss over these difficult but important issues.

So I really think they should be published , but they should think again about the interpretation and message .

At 09:25 AM 11/27/01 -0500, you wrote:

Dear Keith,

No, it is not too late, so please send your review. Thanks a million.

Sincerely,

Jesse

=====

Dr. Jesse Smith
Associate Editor

Science
1200 New York Avenue, NW
Washington, DC 20005
USA

(202) 326-6556
(202) 408-1256 (FAX)
hjsmith@aaas.org
=====

>>> Keith Briffa <k.briffa@uea.ac.uk> 11/27/01 09:17AM >>>
Is it too late for this or should I send a review by tomorrow?
Keith

--
Professor Keith Briffa,
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3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Raymond S. Bradley" <rbradley@geo.umass.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: questions

Date: Tue, 05 Mar 2002 23:26:18 -0500

<x-flowed>

I cut Hammer ref

I just thanked "ll those who provided data"

I was looking at Graybill & Shiyatov Fig 20.6, but you are right that the warmest period was after 1160....though some argue the MWP extends into the 14th century....certainly it shows a cold 11th century. So I'll cut that reference, as requested...

I leave it to you to contact Dave Fisher as I don't know what he sent you...so get back to me asap

Ray

>for the melt record (l) use .

>

>2. "Intercomparison of....techniques", Fisher and others.1996. Nato
>ASI Vol 141, "Climate variations and forcingmechanisms of the last
>2000 yrs", Springer Verlag etc. pp 297-328.

>Can not track down yet where the low re one came from (can you ask Dave
>directly)

>Other points are ok

>Did you track down the Hammer ref (some European conference) ?

>Do you need list of acknowledgements yet? Should include

>Mike Salmon for drawing the figure

>and Fisher, Black, Luterbacher, presumably Johnsson ,Bianchi,Kegwin,

>van Engelen,Keith Barber and Darrel.Maddy, for the data I used.

>I am really pushed , sorry about brief reponse- honest.

>Keith

>

>

>At 10:46 PM 3/4/02 -0500, you wrote:

>>yes--they do show a MWp in shiyatov and graybill 1992--but i added briffa

>>2000, too.

>>i still need a response to my last email

>>ray

>>

>>
>>Raymond S. Bradley
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>>
>>Tel: 413-545-2120
>>Fax: 413-545-1200
>>Climate System Research Center: 413-545-0659
>>Climate System Research Center Web Page:
>><<http://www.geo.umass.edu/climate/climate.html>>
>>Paleoclimatology Book Web Site (1999):
>><http://www.geo.umass.edu/climate/paleo/html>

>>
>
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Paleoclimatology Book Web Site (1999):
<http://www.geo.umass.edu/climate/paleo/html>

</x-flowed>

From: "Michael E. Mann" <mann@virginia.edu>

To: drdendro@ldeo.columbia.edu

Subject: Esper et al paper

Date: Thu, 21 Mar 2002 16:39:06 -0500

Cc: k.briffa@uea.ac.uk, p.jones@uea.ac.uk, Tom Crowley <tcrowley@duke.edu>, t.osborn@uea.ac.uk, rbradley@geo.umass.edu, Malcolm Hughes <mhughes@ltrr.arizona.edu>

Dear Ed,

I'm really sorry I couldn't be more supportive of the final version of the manuscript. I fully expected to be able to be more positive in my assessment. I was frankly very disappointed when I saw the final version--it is overwhelmingly different from the version you shared with us originally. Sadly, it seems to have suffered, and not benefited, from the review process--a very odd scenario. I fault the reviewers as much (in fact more) than I fault you for this. There are some really basic problems that they didn't seem to catch. I hope neither you nor your co-authors take this personally.

I'm trying to be as diplomatic as I can be in my discussions w/ reporters, etc. but I really wish you hadn't sprung this on us w/ no warning of the dramatic changes that were made. I'm forced to be somewhat critical, because the flaws in some of your conclusions need to be pointed out, or they will be exploited by those w/ ulterior motives. You certainly must have foreseen this, as must have the reviewers. I'm very disappointed, very disappointed indeed.

I'm sharing my comments w/ Keith, Phil, Tim, Tom, Ray, and Malcolm. I am resisting the temptation to write a letter of response to Science, although my better judgement dictates that I should...

Mike

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
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Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[1][http://www.evsc.virginia.edu/faculty/people/mann.\[2\].shtml](http://www.evsc.virginia.edu/faculty/people/mann.[2].shtml)

Attachment Converted: "c:\eudora\attach\treerings-comments.doc"

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

To: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, mhughes@ltr.arizona.edu, drdendro@ldeo.columbia.edu, rkerr@aaas.org, bhanson@aaas.org

Subject: Briffa & Osborn piece

Date: Fri, 22 Mar 2002 12:39:38 -0500

Keith and Tim,

Sadly, your piece on the Esper et al paper is more flawed than even the paper itself.

Ed, the AP release that appeared in the papers was even worse. Apparently you allowed yourself to be quoted saying things that are inconsistent with what you told me you had said.

You three all should have known better. Keith and Tim: Arguing you can scale the relationship between full Northern Hemisphere and extratropical Northern Hemisphere is *much* more problematic than even any of the seasonal issues you discuss, and this isn't even touched on in your piece. The evidence of course continues to mount (e.g., Hendy et al, Science, a couple weeks ago) that the tropical SST in the past centuries varied far more less in past centuries. Hendy et al specifically point out that there is little evidence of an LIA in the tropics in the data. The internal inconsistency here is remarkably ironic. The tropics play a very important part in our reconstruction, with half of the surface temperature estimate coming from latitudes below 30N. You know this, and in my opinion you have knowingly misrepresented our work in your piece.

This will be all be straightened out in due course. In the meantime, there is a lot of damage control that needs to be done and, in my opinion, you've done a disservice to the honest discussions we had all had in the past, because you've misrepresented the evidence. Many of us are very concerned with how Science dropped the ball as far as the review process on this paper was concerned. This never should have been published in Science, for the reason's I outlined before (and have attached for those of you who haven't seen them). I have to wonder why the functioning of the review process broke down so overtly here, Mike

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Attachment Converted: "c:\eudora\attach\treerings-comments1.doc"

References

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From: "drdendro@ldeo.columbia.edu" <drdendro@ldeo.columbia.edu>
To: "mann@multiproxy.evsc.virginia.edu"
<mann@multiproxy.evsc.virginia.edu>, "k.briffa@uea.ac.uk"
<k.briffa@uea.ac.uk>, "t.osborn@uea.ac.uk" <t.osborn@uea.ac.uk>,
"p.jones@uea.ac.uk" <p.jones@uea.ac.uk>, "tcrowley@duke.edu"
<tcrowley@duke.edu>, "rbradley@geo.umass.edu" <rbradley@geo.umass.edu>,
"mhughes@ltrr.arizona.edu" <mhughes@ltrr.arizona.edu>,
"drdendro@ldeo.columbia.edu" <drdendro@ldeo.columbia.edu>,
"rkerr@aaas.org" <rkerr@aaas.org>, "bhanson@aaas.org" <bhanson@aaas.org>
Subject: RE: Briffa & Osborn piece
Date: Fri, 22 Mar 2002 16:06:28 -0500
Reply-to: drdendro@ldeo.columbia.edu

Hi Mike and others,

I just read the AP release. As always, there is a bit of journalistic license that was applied to interpreting what I said. The opening statement in the release is utterly the words of the reporter. Some of the quotes are probably accurate, but of course do not include qualifiers, etc. I also talked with this journo before talking with you and would phrase things a bit more carefully now after hearing your concerns. So, I am not deceiving you in what I told you over the phone. I would not express things the same way as you in any case, because I do think that we have some legitimate differences of opinion on some issues, although I think we agree much more than we disagree. Be that as it may, talking over the phone to journalists in a rapid-fire manner is not the best way to convey ideas and information and I would have re-phrased or re-expressed some of what was written if I had seen it before it was released. This was not an option provided to me.

I think that it is a bit harse to say that the paper should not have been published. While I might wish to change some wording in the paper and express things a bit differently knowing what I know now, I don't think that the paper is fatally flawed, like you do. I should also point out that I have received a number of emails from respected scientists in global change research who do not appear to share your opinion. On the other hand, I have also received a couple of emails from certified nuts, which is what you are obviously most concerned about. I am not happy with such people, but I have also been savaged by similar nuts like John Daly in the past. So, I guess I can't win.

Finally, this whole global change debate totally sucks because it is so politicized. It reminds me too much of the ugly acid rain/forest decline debate that I was caught in the middle of years ago. I am quite happy to leave global change to others in the future.

Ed

Original Message:

From: Michael E. Mann mann@multiproxy.evsc.virginia.edu

Date: Fri, 22 Mar 2002 12:39:38 -0500
To: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, p.jones@uea.ac.uk,
tcrowley@duke.edu, rbradley@geo.umass.edu,
mhughes@ltrr.arizona.edu, drdendro@ldeo.columbia.edu,
rkerr@aaas.org, bhanson@aaas.org
Subject: Briffa & Osborn piece

Keith and Tim,

Sadly, your piece on the Esper et al paper is more flawed than even the paper itself.

Ed, the AP release that appeared in the papers was even worse.

Apparently

you allowed yourself to be quoted saying things that are inconsistent with what you told me you had said.

You three all should have known better. Keith and Tim: Arguing you can scale the relationship between full Northern Hemisphere and extratropical

Northern Hemisphere is *much* more problematic than even any of the seasonal issues you discuss, and this isn't even touched on in your piece.

The evidence of course continues to mount (e.g., Hendy et al, Science, a

couple weeks ago) that the tropical SST in the past centuries varied far more less in past centuries. Hendy et al specifically point out that there

is little evidence of an LIA in the tropics in the data. The internal inconsistency here is remarkably ironic. The tropics play a very important

part in our reconstruction, with half of the surface temperature estimate coming from latitudes below 30N. You know this, and in my opinion you have

knowingly misrepresented our work in your piece.

This will be all be straightened out in due course. In the meantime, there

is a lot of damage control that needs to be done and, in my opinion, you've

done a disservice to the honest discussions we had all had in the past, because you've misrepresented the evidence. Many of us are very concerned

with how Science dropped the ball as far as the review process on this paper was concerned. This never should have been published in Science, for

the reason's I outlined before (and have attached for those of you who haven't seen them). I have to wonder why the functioning of the review process broke down so overtly here,

Mike

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
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982-2137

<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

mail2web - Check your email from the web at
<http://mail2web.com/> .

From: "Raymond S. Bradley" <rbradley@geo.umass.edu>
To: drdendro@ldeo.columbia.edu, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk
Subject: Op-Ed
Date: Sat, 23 Mar 2002 10:19:00 -0500

<x-flowed>

Ed:

I just waded through all the correspondence with Mike re the Science paper and Keef's commentary. I wish to disassociate myself with Mike's comments, or at least the tone of them. I do not consider myself the final arbiter of what Science should publish, nor do I consider what you did to signify the end of civilization as we know it. Life goes on--now we have another working hypothesis to examine. Great...one of these days we'll really know what happened....until then, I find all these efforts to be really interesting. That's not to say I agree with everything you said or did, but then I don't suppose you are too enamoured of what I've done in the past either. C'est la vie.

Ray

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Climate System Research Center: 413-545-0659
Climate System Research Center Web Page:
<<http://www.geo.umass.edu/climate/climate.html>>
Paleoclimatology Book Web Site (1999):
<http://www.geo.umass.edu/climate/paleo/html>

</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, drdendro@ldeo.columbia.edu, rkerr@aaas.org, bhanson@aaas.org
Subject: Re: Briffa & Osborn piece
Date: Fri Apr 5 17:17:55 2002
Cc: Tim Osborn <t.osborn@uea.ac.uk>

Dear Mike, (and interested colleagues)

Given the list of people to whom you have chosen to circulate your message(s), we thought we should make a short, somewhat formal, response here. I am happy to reserve my informal response until we are face to face! We did not respond earlier because we had more pressing tasks to deal with. This is not the place to go into a long or over-detailed response to all of your comments but a few brief remarks might help to clear up a couple of misconceptions.

You consider our commentary on Ed and Jan's paper

"more flawed than even the paper itself"

on the basis that scaling the relationship between full Northern Hemisphere and extratropical Northern Hemisphere is *much* more problematic than even any of the seasonal issues we discuss. In fact we did not do this. The curve labelled Mann99 in our figure was, in fact, based on the average of only the land areas, north of 20 degrees N, extracted from your spatially-resolved reconstructions. We then scaled it by calibration against the instrumental annual temperatures from the same region. This is, just as you stress in your comments on the Esper et al. paper, what should have been done. We think that this single point addresses virtually of all your concerns. We can, of course, argue about what this means for the pre-1400 part of your reconstruction, when only 1 EOF was reconstructed, but the essential message is that we did our best to exclude the tropics (and the oceans too!) from your series so that it could more readily be compared with the other records.

The fact that we have used only the extra-tropical land from your data is not clear from the text, so we can see why you may not have appreciated this, but we think you will concede that this fact negates much of what you say and that we acted "more correctly" than you realised. Blame *Science* for being so mean with their space allocation if you want! Remember that this was an unrefereed piece and we felt justified in concentrating on one issue; that of the importance of the method of scaling and its effect on apparent "absolute" reconstruction levels. In our draft, we went on to say that this was crucial for issues of simple model sensitivity studies and climate detection, citing the work of Tom Crowley and Myles Allen, but this fell foul of the editor's knife.

You also express concerns about the calibration of Esper et al. (e.g., you say "if the authors had instead used the actual (unsmoothed) instrumental record for the extratropical northern hemisphere to scale their record, their reconstruction would be much closer to MBH99").

This point is wholly consistent with our discussion in the perspective piece, and indeed we show that in absolute terms the records are closer when Esper et al. is calibrated using unsmoothed data but since the variance is also reduced, the significance of the differences may be just as high.

Finally, we have to say that we do not feel constrained in what we say to the media or write in the scientific or popular press, by what the sceptics will say or do with our results. We can only strive to do our best and address the issues honestly. Some "sceptics" have their own dishonest agenda - we have no doubt of that. If you believe that I, or Tim, have any other objective but to be open and honest about the uncertainties in the climate change debate, then I am disappointed in you also.

Best regards

Keith (and Tim)

At 12:39 PM 3/22/02 -0500, Michael E. Mann wrote:

Keith and Tim,

Sadly, your piece on the Esper et al paper is more flawed than even the paper itself.

Ed, the AP release that appeared in the papers was even worse. Apparently you allowed yourself to be quoted saying things that are inconsistent with what you told me you had

said.

You three all should have known better. Keith and Tim: Arguing you can scale the relationship between full Northern Hemisphere and extratropical Northern Hemisphere is *much* more problematic than even any of the seasonal issues you discuss, and this isn't even touched on in your piece. The evidence of course continues to mount (e.g., Hendy et al, Science, a couple weeks ago) that the tropical SST in the past centuries varied far more less in past centuries. Hendy et al specifically point out that there is little evidence of an LIA in the tropics in the data. The internal inconsistency here is remarkably ironic. The tropics play a very important part in our reconstruction, with half of the surface temperature estimate coming from latitudes below 30N. You know this, and in my opinion you have knowingly misrepresented our work in your piece.

This will be all be straightened out in due course. In the meantime, there is a lot of damage control that needs to be done and, in my opinion, you've done a disservice to the honest discussions we had all had in the past, because you've misrepresented the evidence. Many of us are very concerned with how Science dropped the ball as far as the review process on this paper was concerned. This never should have been published in Science, for the reason's I outlined before (and have attached for those of you who haven't seen them). I have to wonder why the functioning of the review process broke down so overtly here,

Mike

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

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References

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3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

To: Edward Cook <drdendro@ldeo.columbia.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>

Subject: Re: Your letter to Science

Date: Thu, 11 Apr 2002 11:36:44 -0400

Cc: esper@wsl.ch, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu, srutherford@virginia.edu, mann@virginia.edu

Ed,

It will take some time to digest these comments, but my initial response is one of some disappointment. I will resist the temptation to make the letter to Science available to the others on this list, because of my fears of violating the embargo policy (I know examples of where doing so has led to Science retracting a piece from publication). So thanks for also resisting the temptation to do so...

But I must point out that the piece by Malcolm and me is very similar in its content to the letter of clarification that you and I originally crafted to send to Science some weeks ago, before your co-author objected to your involvement! If there is no objection on your part, I'd be happy to send that to everyone, because it is not under consideration in Science (a quite unfortunate development, as far as I'm concerned). The only real change from that version is the discussion of the use of RCS. That is in large part Malcolm's contribution, but I stand behind what Malcolm says. I think there are some real sins of omission with regard to the use of RCS too, and it would be an oversight on our part now to comment on these.

Finally, with regard to the scaling issues, let me simply attach a plot which speaks more loudly than several pages possibly could. The plot takes Esper et al (not smoothed, but the annual values) and scales it against the full Northern Hemisphere instrumental record 1856-1990 annual mean record, and compares against the entire 20th century instrumental record (1856-1999), as well as with MBH99 and its uncertainties.

Suppose that Esper et al is indeed representative of the full Northern Hemisphere annual mean, as MBH99 purports to be. To the extent that differences emerge between the two in assuming such a scaling, I interpret them as differences which exist due to the fact that the extratropical Northern Hemisphere series and full Northern Hemisphere series likely did not co-vary in the past the same way they co-vary in the 20th century (when both are driven predominantly, in a relative sense, by anthropogenic forcing, rather than natural forcing and internal variability). What the plot shows is quite remarkable. Scaled in this way, there is remarkably little difference between Esper et al and MBH99 in the first place (the two reconstructions are largely within the error estimates of MBH99!), but moreover, where they do differ, this could be explainable in terms of patterns of enhanced mid-latitude continental response that were discussed, for example, in Shindell et al (2001) in Science last December. So I think this plot says a lot. Its say that there are some statistically significant differences, but certainly no grounds to use Esper et al to contradict MBH99 or IPCC '2001 as, sadly, I believe at least one of the published pieces tacitly appears to want to do.

It is shame that such a plot, which I think is a far more meaningful comparison of the two records, was not shown in either Esper et al or the Briffa & Osborn commentary. I've always given the group of you adequate opportunity for commentary on anything we're about to publish in Nature or Science. I am saddened that many of my colleagues (and, I have always liked to think friends) didn't afford me the same opportunity before this all erupted in our face. It could have been easily avoided. But that's water under the bridge.

Finally, before any more back-and-forths on this, I want to make sure that everyone involved understands that none of this was in any way ever meant to be personal, at least

not on my part (and if it ever has, at least on my part, seemed that way, than I offer my apologies--it was never intended that way). This is completely about the "science". To the extent that I (and/or others) feel that the science has been mis-represented in places, however, I personally will work very hard to make sure that a more balanced view is available to the community. Especially because the implications are so great in this case. This is what I sought to do w/ the NYT piece and my NPR interview, and that is what I've sought to do (and Malcolm to, as far as I'm concerned) with the letter to Science. Being a bit sloppy w/ wording, and omission, etc. is something we're all guilty of at times. But I do consider it somewhat unforgivable when it is obvious how that sloppiness can be exploited. And you all know exactly what I'm talking about!

So, in short, I think are some fundamental issues over which we're in disagreement, and where those exist, I will not shy away from pointing them out. But I hope that is not mis-interpreted as in any way personal.

I hope that suffices,

Mike

p.s. It seemed like an omission to not cc in Peck and Scott Rutherford on this exchange, so I've done that. I hope nobody minds this addition...

At 10:57 AM 4/11/02 -0400, Edward Cook wrote:

Hi Mike and Malcolm,

I have received the letter that you sent to Science and will respond to it here first in some detail and later in edited and condensed form in Science. Since much of what you comment and criticize on has been disseminated to a number of people in your (Mike's) somewhat inflammatory earlier emails, I am also sending this lengthy reply out to everyone on that same email list, save those at Science. I hadn't responded in detail before, but do so now because your criticisms will soon be in the public domain.

However, I am not attaching your letter to Science to this email since that is not yet in the public domain. It is up to you to send out your submitted letter to everyone if you wish.

I must say at the beginning that some parts of your letter to Science are as "flawed" as your claims about Esper et al. (hereafter ECS). The Briffa/Osborn perspectives piece points out an important scaling issue that indeed needs further examination. However, to claim as you do that they show that the ECS 40-year low-pass temperature reconstruction is "flawed" begs the question: "flawed" by how much? It is not at all clear that scaling the annually resolved RCS chronology to annually resolved instrumental temperatures first before smoothing is the correct way to do it. The ECS series was never created to examine annual, or even decadal, time-scale temperature variability. Rather, as was clearly indicated in the paper, it was created to show how one can preserve multi-centennial climate variability in certain long tree-ring records, as a refutation of Broecker's truly "flawed" essay. As ECS showed in their paper (Table 1), the high-frequency correlations with NH mean annual temperatures after 20-year high-pass filtering is only 0.15. That result was expected and it makes no meaningful difference if one uses only extra-tropical NH temperature data. So, while the amplitude of the temperature-scaled 40-year low-pass ECS series might be on the high end (but still plausible given the gridded borehole temperature record shown in Briffa/Osborn), scaling on the annually resolved data first would probably have the opposite effect of excessively reducing the amplitude. I am willing to accept an intermediate value, but probably not low enough to satisfy you. Really, the more important result from ECS is the enhanced pattern of multi-centennial variability in the NH extra-tropics over the past 1100 years. We can argue about the amplitude later, but the enhanced

multi-centennial variability can not be easily dismissed. I should also point out, again, that you saw Fig. 3 in ECS BEFORE it was even submitted to Science and never pointed out the putative scaling "flaw" to me at that time.

With regards to the issue of the late 20th century warming, the fact that I did not include some reference to or plot of the up-to-date instrumental temperature data (cf. Briffa/Osborn) is what I regard as a "sin of omission". What I said was that the estimated temperatures during the MWP in ECS "approached" those in the 20th century portion of that record up to 1990. I don't consider the use of "approached" as an egregious overstatement. But I do agree with you that I should have been a bit more careful in my wording there. As you know, I have publicly stated that I never intended to imply that the MWP was as warm as the late 20th century (e.g., my New York Times interview). However, it is a bit of overkill to state twice in the closing sentences of the first two paragraphs of your letter that the ECS results do not refute the unprecedented late 20th century warming. I would suggest that once is enough. ECS were also very clear about the extra-tropical nature of their data. So, what you say in your letter about the reduced amplitude in your series coming from the tropics, while perhaps worth pointing out again, is beating a dead horse. However, I must say that the "sin of omission" in the Briffa/Osborn piece concerning the series shown in their plot is a bit worrying. As they say in the data file of series used in their plot (and in Keith's April 5 email response to you), Briffa/Osborn only used your land temperature estimates north of 20 degrees and recalibrated the mean of those estimates to the same domain of land-only instrumental temperatures using the same calibration period for all of the other non-borehole series in the same way. I would have preferred it if they had used your data north of 30N to make the comparisons a bit more one-to-one. However, I still think that their results are interesting. In particular, they reproduce much of the reduced multi-centennial temperature variability seen in your complete NH reconstruction. So, if the amplitude of scaled ECS multi-centennial variability is far too high (as you would apparently suggest), it appears that it is also too low in your estimates for the NH extra-tropics north of 20N. I think that we have to stop being so aggressive in defending our series and try to understand the strengths and weaknesses of each in order to improve them. That is the way that science is supposed to work.

I must admit to being really irritated over the criticism of the ECS tree-ring data standardized using the RCS method. First of all, ECS acknowledged up front the declining available data prior to 1200 and its possible effect on interpreting an MWP in the mean record. ECS also showed bootstrap confidence intervals for the mean of the RCS chronologies and showed where the chronologies drop out. Even allowing for the reduction in the number of represented sites before 1400 (ECS Fig. 2d), and the reduction in overall sample size (ECS Fig. 2b), there is still some evidence for significantly above average growth during two intervals that can be plausibly assigned to the MWP. Of course we would like to have had all 14 series cover the past 1000-1200 years. This doesn't mean that we can't usefully examine the data in the more weakly replicated intervals. In any case, the replication in the MWP of the ECS chronology is at least as good as in other published tree-ring estimates of large-scale temperatures (e.g., NH extra-tropical) covering the past 1000+ years. It also includes more long tree-ring records from the NH temperate latitudes than ever before. So to state that "this is a perilous basis for an estimate of temperature on such a large geographic scale" is disingenuous, especially when it is unclear how many millennia-long series are contributing the majority of the temperature information in the Mann/Bradley/Hughes (MBH) reconstruction prior to AD 1400. Let's be balanced here.

I basically agree with the closing paragraph of your letter. The ECS record was NEVER intended to refute MBH. It was intended, first and foremost, to refute Broecker's essay in Science that unfairly attacked tree rings. To this extent, ECS succeeded very well. The comparison of ECS with MBH was a logical thing to do given that it has been accepted by the IPCC as the benchmark reconstruction of NH annual temperature variability and change over the past millennium. Several other papers have made similar comparisons between MBH and other even more geographically restricted estimates of past temperature. So, I don't apologize in the slightest for doing so in ECS. The correlations in Table 2 between ECS and MBH were primarily intended to demonstrate the probable large-scale, low-frequency temperature signal in ECS independent of explicitly calibrating the individual RCS chronologies before aggregating them. The results should actually have pleased you because, for the 20-200 year band, ECS and MBH have correlations of 0.60 to 0.68, depending on the period used. Given that ECS is based on a great deal of new data not used in MBH, this result validates to a reasonable degree the temperature signal in MBH in the 20-200 year band over the past 1000 years. Given the incendiary and sometimes quite rude emails that came out at the time when ECS and Briffa/Osborn were published, I could also go into the whole complaint about how the review process at Science was "flawed". I will only say that this is a very dangerous game to get into and complaints of this kind can easily cut both ways. I will submit an appropriately edited and condensed version of this reply to Science.

Regards,
Ed

--

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[1][http://www.evsc.virginia.edu/faculty/people/mann.\[2\].shtml](http://www.evsc.virginia.edu/faculty/people/mann.[2].shtml)

Attachment Converted: "c:\eudora\attach\esper-scaledcompare1980.jpg"

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Tom Crowley <tcrowley@duke.edu>
To: Ed Cook <drdendro@ldeo.columbia.edu>
Subject: peace
Date: Fri, 12 Apr 2002 10:54:56 -0400
Cc: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, esper@wsl.ch, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu, srutherford@virginia.edu

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Dear friends,

I am concerned about the the stressed tone of some of the words being circulated lately. Such difficulties not only hamper collegiality (which I value greatly) but also the actual progress in our field.

I think you are all fine fellows and very good scientists and that it is time to smoke the peace pipe on all this and put a temporary moratorium on more email messages until tempers cool down a bit. After this maybe we can discuss things somewhere where each party comes to the meeting beforehand with a commitment to even-handed discussion and give and take.

I hope I have not offended anyone in this message -- it is of course a personal opinion. Maybe it is an illusion or prejudice on my part, but somehow I am not convinced that the "truth" is always worth reaching if it is at the cost of damaged personal relationships....

Best wishes, Tom

--
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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, Ed Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Your letter to Science
Date: Fri, 12 Apr 2002 12:32:33 -0400
Cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, esper@wsl.ch, t.osborn@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu, srutherford@virginia.edu

Whoaah...Please don't put words in my mouth Keith, especially such inflammatory word! I was not attributing the entirety of "spin" here (which is of a pretty massive scale) to you! And I said I think such "spin", where it has occurred, is EITHER sloppy OR disingenuous. You chose to assume I was talking about you in specific, and that I was attributing the latter rather than the former. My actual words don't bear this out. In the case of the Briffa & Osborn piece, I actually tend to believe that sloppiness was the main problem. In other cases of "spin" (e.g., the skeptics web pages of Daly and his ilk) it is most clearly disingenous...I don't equate you with Daly and those folks by any stretch of the imagination. Hopefully, you know that I respect you quite a bit as a scientist! But in this case, I think you were sloppy. And the sloppiness had a real cost... And as to whether or not your statements about IPCC are fair (I didn't use the word "disservice"!), I'll leave that to each to decide. But personally, I think they were unfair, because they opened up IPCC to criticism that is not merited by what is actually said or shown in the IPCC report. Other IPCC authors who have contacted me feel the same way, and perhaps there may be an official response on the part of IPCC authors. I don't know.

But I agree that any further discussion ought to take place in the peer-reviewed literature,

Mike

At 05:09 PM 4/12/02 +0100, Keith Briffa wrote:

I agree with the sentiments expressed by Tom . However, in his latest message Mike clearly says that our perspectives piece did the IPCC a disservice. He then accuses us of spinning the ECS paper to say that MBH is an underestimate of what it purports to be and that we have been sloppy and disingenuous. Frankly this is too much to take . I am not going to let this ruin my weekend so I wait until I have calmed down and find time next week to write a response. In the meantime I just wanted to note that I disagree with these comments. Perhaps the best place to continue this discussion is in the peer review literature.

Keith

At 11:11 AM 4/12/02 -0400, Michael E. Mann wrote:

Ed and others,

I thought I too should chime in here one last time...

I'll leave it to you, Malcolm, Keith and others to debate out the issue of any additional uncertainties, biases, etc. that might arise from RCS in the presence of limited samples. That is beyond my range of expertise. But since this is a new and relatively untested approach, and it is on the basis of this approach that other estimates are being argued to be "underestimates", we would indeed have been remiss now to point this out in our letter.

The wording "perilous" perhaps should be changed, by I very much stand by the overall sentiment expressed by Malcolm in our piece with regard to RCS.

One very important additional point that Malcolm makes in his message is that conservative estimates of uncertainties, appropriate additional caveats, etc. were indeed all provided in MBH99, and I have always been careful to interpret our results in the context of these uncertainties and caveats. IPCC '2001 was careful to do so to, and based its conclusions within the context of the uncertainties (hence the choice of the conservative term "likely" in describing the apparently unprecedented nature of late 20th century warmth) and, moreover, on the collective results of many independent reconstructions. Briffa & Osborn would have you believe that IPCC '2001's conclusions in this regard rested on MBH99 alone. Frankly, Keith and Tim, I believe that is unfair to the IPCC, whether or not one cares about being fair to MBH or not.

What is unfortunate here then is that Esper et al has been "spun" i to argue that MBH99 underestimates the quantity it purports to estimate, full Northern Hemisphere annual mean temperature. Given the readily acknowledged level of uncertainty in both estimates, combined with the "apples and oranges" nature of the comparison between the two (which

I have sought to clarify in my letter to Science, and in my messages to you all, and the comparison plot I provided). I believe it is either sloppy or disingenuous reasoning to argue that this is the case. The fact that this sloppiness also readily serves the interests of the skeptics is quite unfortunate, but it is indeed beside the point!

It would probably also be helpful for me to point out, without naming names, that many of our most prominent colleagues in the climate research community, as well government funding agency representatives, have personally contacted me over the past few weeks to express their dismay at the way they believe this study was spun. I won't get into the blame game, because there's more than enough of that to go around. But when the leaders of our scientific research community and our funding managers personally alert us that they believe the credibility of our field has been damaged, I think it is time for some serious reflection on this episode.

that's my final 2 cents,
Mike

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[1][http://www.evsc.virginia.edu/faculty/people/mann.\[2\].shtml](http://www.evsc.virginia.edu/faculty/people/mann.[2].shtml)

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

To: Ed Cook <drdendro@ldeo.columbia.edu>

Subject: Re: Your letter to Science

Date: Fri, 12 Apr 2002 17:35:33 -0400

Cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, esper@wsl.ch, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu, srutherford@virginia.edu

Dear Ed, Tom, Keith, etc.

In keeping w/ the spirit of Tom's and Keith's emails, I wanted to stress, before we all break for the weekend, that this is ultimately about the science, its not personal. If my comments seemed to assail e.g. Keith's motives or integrity, etc. I believe that they were misunderstood (as I tried to clarify that in my previous message), but I can see that there was a potential for misunderstanding of my message (precision in wording is very important) given the high levels of sensitivity in this debate. So I wanted to leave no uncertainty about that. And of course, I very much apologize to Keith (and Tim) if they took them my comments that way. They, again, were most decidedly not intended that way. I hope we can resolve the scientific issues objectively, and w/out injecting or any personal feelings into any of this. There are some substantial scientific differences here, lets let them play out the way they are supposed to, objectively, and in the peer reviewed literature.

Enjoy the weekend all.

cheers,

Mike

At 01:35 PM 4/12/02 -0400, Ed Cook wrote:

Hi Mike, Tom, etc.

Okay, I am quite happy to give this debate a rest, although I am sure that the issues brought up will still be grounds for scientific debate. I admit to getting a bit riled when I saw the ECS results on the MWP described as "perilous" because I regard that as being an unfair characterization of the work presented. Be that as it may, my reply to Science will be very carefully worded so as not to inflame the issues. Nuff said. Have a good weekend. I certainly intend to do so.

Ed

Ed and others,

I thought I too should chime in here one last time...

I'll leave it to you, Malcolm, Keith and others to debate out the issue of any additional uncertainties, biases, etc. that might arise from RCS in the presence of limited samples. That is beyond my range of expertise. But since this is a new and relatively untested approach, and it is on the basis of this approach that other estimates are being argued to be "underestimates", we would indeed have been remiss now to point this out in our letter.

The wording "perilous" perhaps should be changed, by I very much stand by the overall sentiment expressed by Malcolm in our piece with regard to RCS.

One very important additional point that Malcolm makes in his message is that conservative estimates of uncertainties, appropriate additional caveats, etc. were indeed all provided in MBH99, and I have always been careful to interpret our results in the context of these uncertainties and caveats. IPCC '2001 was careful to do so to, and based its conclusions within the context of the uncertainties (hence the choice of the conservative term "likely" in describing the apparently unprecedented nature of late 20th century warmth) and, moreover, on the collective results of many independent reconstructions. Briffa & Osborn would have you believe that IPCC '2001's conclusions in this regard rested on MBH99 alone. Frankly, Keith and Tim, I believe that is unfair to the IPCC, whether or not one cares about being fair to MBH or not.

What is unfortunate here then is that Esper et al has been "spun" i to argue that MBH99 underestimates the quantity it purports to estimate, full Northern Hemisphere annual mean temperature. Given the readily acknowledged level of uncertainty in both estimates, combined with the "apples and oranges" nature of the comparison between the two (which I have sought to clarify in my letter to Science, and in my messages to you all, and the comparison plot I provided), I believe it is either sloppy or disingenuous reasoning to argue that this is the case. The fact that this sloppiness also readily serves the interests of the skeptics is quite unfortunate, but it is indeed beside the point!

It would probably also be helpful for me to point out, without naming names, that many of our most prominent colleagues in the climate research community, as well government funding agency representatives, have personally contacted me over the past few weeks to express their dismay at the way they believe this study was spun. I won't get into the blame game, because there's more than enough of that to go around. But when the leaders of our scientific research community and our funding managers personally alert us that they believe the credibility of our field has been damaged, I think it is time for some

serious reflection on this episode.

that's my final 2 cents,

Mike

At 10:21 AM 4/12/02 -0400, Ed Cook wrote:

Just a few comments here and then I'm done.

Dear Ed and Mike and others,

All of our attempts, so far, to estimate hemisphere-scale temperatures for the period around 1000 years ago are based on far fewer data than any of us would like. None of the datasets used so far has anything like the geographical distribution that experience with recent centuries indicates we need, and no-one has yet found a convincing way of validating the lower-frequency components of them against independent data. As Ed wrote, in the tree-ring records that form the backbone of most of the published estimates, the problem of poor replication near the beginnings of records is particularly acute, and ubiquitous. I would suggest that this problem probably cuts in closer to 1600 than 1400 in the several published series. Therefore, I accept that everything we are doing is preliminary, and should be treated with considerable caution.

Therefore, I would guess that you would apply the word "perilous" to everyone's large-scale NH reconstructions covering the past 500-1000 years including those that you have been involved in. Why the sudden increase in caution now? It sounds very self-serving to me for you to call ECS "perilous" and not describe every other large-scale reconstruction in that way as well.

I differ from Ed, and his co-authors,

in believing that these problems have a special significance for the particular implementation of RCS they used, in the light of one of their conclusions that depends heavily on that implementation.

As I understand what Ed, Keith and Hal Fritts have written at various times about RCS, and from my own limited experience with the method, it is extremely important to have strong replication, and I don't see 50-70 samples probably from 25-35 trees as a big sample. For reference, most chronologies used in dendroclimatology are based on 10-40 trees, that is 20-80 samples at 2 cores per tree for a single "site", usually a few hectares.

Here are two passages from Briffa et al., 1992: page 114, column 1, last paragraph, "For a chronology composed of the same number of samples, one would therefore expect a larger statistical uncertainty using this approach than in a chronology produced using standardization curves fitted to the data from individual trees.....The RCS method therefore requires greater chronology depth (i.e. greater sample replication) to provide the same level of confidence in its representation of the hypothetical "true" chronology." ECS mention this issue.

As I said in my previous email, we hid nothing in terms of the uncertainty concerning the pre-1200 interval. Are you suggesting that we should not have even shown those results? If so, that is ridiculous.

page 114, column 1, third paragraph, there is a discussion of the problems arising from applying RCS when pith age is not known, "In the ring-width data, the final standardization curve probably slightly underestimates the width of young trees and could therefore impart a small positive bias to the standardized ring-width indices for young rings in a number of series. However, this effect will be insignificant when the biased indices are realigned according to calendar growth years and

averaged with many other series." The problem here is that this latter condition is not met (in my view), and the "small positive bias" that may be retained could turn out to be important to the most controversial conclusion of ECS (the Medieval question).

I can't speak for Jan here, but most of the data he used came from Schweingruber's lab. I believe that pains were taken to estimate the pith offset and that Jan used this information in his RCS analyses. Jan would be best to comment here. In any case, Jan has done a number of experiments in which he has artificially added large pith offset errors into the RCS analysis and the resulting bias is small. So, I do not believe that your "view" is correct.

I also suspect that Keith and colleagues underestimated both the size and variability of the loss of years at the beginning of records, but the point stands even if this is not so. So far as I can see, ECS do not mention this issue, at least in the context of a possible positive bias.

Are you claiming that the only possible bias is positive? I can show you examples of a probable negative bias using RCS.

The discussion of RCS in the supplementary materials seems to assume good replication.

It was a generic description of the method. The replication is clearly shown in the supplementary materials section as well as in the main paper. If you don't like the replication, that is your opinion. I would love to have more replication as well. Who wouldn't. But we did show the uncertainties, which you seem to ignore in your criticism. Ironically, the ECS estimates of warmth in the MWP are not that dissimilar to those seen in MBH, as ECS Fig. 3 shows. Are the MBH estimates of MWP warmth also similarly biased?

ECS, as Ed rightly points out, clearly indicate, in both words and diagrams at several points in their paper and in the supplementary materials, that the number of sites and number of samples they used decreases sharply before 1200. Even so, ECS gives prominence (second sentence of the abstract, for example) to the reconstruction in that very period, and makes a comparison with the magnitude of 20th-century warming. All the methods, and their realizations so far, have significant problems. In our letter (Mike and I) we draw attention to a specific problem with this implementation of RCS that has a special bearing on the reconstruction of a period to which ECS have drawn attention. Hence the strong note of caution about the ECS conclusion on the comparison between the 10th/11th and late 20th centuries.

I hope it's clear from this that I don't disagree with the general proposition that all existing reconstructions of hemisphere-scale temperatures 1000 years ago (or even for all the first half of the second millennium AD) should be viewed as very preliminary. If anyone is interested I attach a short note on the replication in the year AD 1000 of records used in MBH99 to give an idea of what we are up against.

There is obviously a lot more we can debate about here. I will simply stop here by saying that I stand by the results shown in ECS and will say so in my reply to your letter, pointing out that the use of the word "perilous" could be just as easily be applied to MBH.

We all have a lot to do. I see four important tasks - 1) more investigation of the strengths and limitations of methods like RCS and age-banding - for example, how many samples would have been enough in this case, does the RC change through time? and so on; 2) use of tree-ring records where the loss of low-frequency information

is least - those with long segments from open stands; 3) the search for tree-ring parameters without age/size related trend; 4) the development of completely independent proxies with intrinsically better low-frequency fidelity.

Cheers, Malcolm

The Briffa et al reference is to the 1992 paper, Climate Dynamics, 7:111-119

Hi Ed,

OK--thanks for your response. I'll let Malcolm respond to the technical issues regarding RC. I'm not really qualified to do so myself anyway. Your other points are well taken...

Cheers,

Mike

At 12:09 PM 4/11/02 -0400, Edward Cook wrote:

Hi Mike,

Thanks for the reply. I too do not want to see anything personal in our disagreements. It would be a shame if it got to that and it shouldn't. I don't think that the science we are talking about is sufficiently known yet to claim the "truth", which is why we are having some of our disagreements. I mainly wanted to clarify some issues relating to some criticisms of the ECS results that I thought were not totally fair. My biggest complaint is with Malcolm's contribution to your letter because it really isn't fair to use such words as "perilous". ECS did not hide anything and the uncertainties are clearly indicated in EGS

> Figs. 2 and 3. So, you can make your own judgement. However,

Malcolm's opinion does not invalidate the ECS record. If Malcolm's statement is correct, then ALL previous estimates of NH temperature over the past 1000 years are "perilous", especially before AD 1400 when the number of series available declines significantly in most records.

Ed

Ed,

It will take some time to digest these comments, but my initial response is one of some disappointment. I will resist the temptation to make the letter to Science available to the others on this list, because of my fears of violating the embargo policy (I know examples of where doing so has led to Science retracting a piece form publication). So thanks for also resisting the temptation to do so...

But I must point out that the piece by Malcolm and me is very similar in its content to the letter of clarification that you and I originally crafted to send to Science some weeks ago, before your co-author objected to your involvement! If there is no objection on your part, I'd be happy to send that to everyone, because it is not under consideration in Science (a quite unfortunate development, as far as I'm concerned). The only real change from that version is the discussion of the use of RCS. That is in large part Malcolm's contribution, but I stand behind what

> Malcolm says. I think there are some real sins of omission with

regard to the use of RCS too, and it would be an oversight on our part now to comment on these.

Finally, with regard to the scaling issues, let me simply attach a plot which speaks more loudly than several pages possibly could. The plot takes Esper et al (not smoothed, but the annual values) and scales it against the full Northern Hemisphere instrumental record 1856-1990 annual mean record, and compares against the entire 20th century instrumental record (1856-1999), as well as with MBH99 and its uncertainties.

Suppose that Esper et al is indeed representative of the fullNorthern Hemisphere annual mean, as MBH99 purports to be. To the extent that differences emerge

between the two in assuming such a scaling, I interpret them as differences which exist due to the fact that the extratropical Northern Hemisphere series and full Northern Hemisphere series likely did not co-vary in the past the same way they co-vary in the 20th century (when both are driven predominantly, in a relative sense, by anthropogenic forcing, rather than natural forcing and internal variability). What the plot shows is quite remarkable. Scaled in this way, there is remarkably little difference between Esper et al and MBH99 in the first place (the two reconstructions are largely within the error estimates of MBH99!), but moreover, where they do differ, this could be explainable in terms of patterns of enhanced mid-latitude continental response that were discussed, for example, in Shindell et al (2001) in Science last December. So I think this plot says a lot. Its say that there are some statistically significant differences, but certainly no grounds to use Esper et al to contradict MBH99 or IPCC '2001 as, sadly, I believe at least one of the published pieces tacitly appears to want to do.

It is shame that such a plot, which I think is a far more meaningful comparison of the two records, was not shown in either Esper et al or the Briffa & Osborn commentary. I've always given the group of you adequate opportunity for commentary on anything we're about to publish in Nature or Science. I am saddened that many of my colleagues (and, I have always liked to think friends) didn't affort me the same opportunity before this all erupted in our face. It could have been easily avoided. But that's water under the bridge.

>

Finally, before any more back-and-forths on this, I want to make sure that everyone involved understands that none of this was in any way ever meant to be personal, at least not on my part (and if it ever has, at least on my part, seemed that way, than I offer my apologies--it was never intended that way). This is completely about the "science". To the extent that I (and/or others) feel that the science has been mis-represented in places, however, I personally will work very hard to make sure that a more balanced view is available to the community. Especially because the implications are so great in this case. This is what I sought to do w/ the NYT piece and my NPR interview, and that is what I've sought to do (and Malcolm to, as far as I'm concerned) with the letter to Science. Being a bit sloppy w/ wording, and omission, etc. is something we're all guilty of at times. But I do consider it somewhat unforgivable when it is obvious how that sloppiness can be exploited. And you all know exactly what I'm talking about! So, in short, I think are some fundamental issues over which we're in disagreement, and where those exist, I will not shy away from pointing them out. But I hope that is not mis-interpreted as in any way personal. I hope that suffices,

>

Mike

p.s. It seemed like an omission to not cc in Peck and Scott Rutherford on this exchange, so I've done that. I hope nobody minds this addition...

At 10:57 AM 4/11/02 -0400, Edward Cook wrote:

Hi Mike and Malcolm,

I have received the letter that you sent to Science and will respond to it here first in some detail and later in edited and condensed form in Science.

Since much of what you comment and criticize on

has been disseminated to a number of people in your (Mike's) somewhat inflammatory earlier emails, I am also sending this lengthy reply out to everyone on that same email list, save those at Science. I hadn't responded in detail before, but do so now because your criticisms will soon be in the public domain. However, I am not attaching your letter to Science to this email since that is not yet in the public domain. It is up to you to send out your submitted letter to everyone if you wish.

I must say at the beginning that some parts of your letter to Science are as "flawed" as your claims about Esper et al. (hereafter ECS). The Briffa/Osborn perspectives piece points out an important scaling issue that indeed needs further examination. However, to claim as you do that they show that the ECS 40-year low-pass temperature reconstruction is "flawed" begs the question: "flawed" by how much? It is not at all clear that scaling the annually resolved RCS chronology to annually resolved instrumental temperatures first before smoothing is the correct way to do it. The ECS series was never created to examine annual, or even decadal, time-scale temperature variability. Rather, as was clearly indicated in the paper, it was created to show how one can preserve multi-centennial climate variability in certain long tree-ring records, as a refutation of Broecker's truly "flawed" essay. As ECS showed in their paper (Table 1), the high-frequency correlations with NH mean annual temperatures after 20-year high-pass filtering is only 0.15. That result was expected and it makes no meaningful difference if one uses only extra-tropical NH temperature data. So, while the amplitude of the temperature-scaled 40-year low-pass ECS series might be on the high end (but still plausible given the gridded borehole temperature record shown in Briffa/Osborn), scaling on the annually resolved data first would probably have the opposite effect of excessively

> reducing the amplitude. I am willing to accept an

intermediate value, but probably not low enough to satisfy you. Really, the more important result from ECS is the enhanced pattern of multi-centennial variability in the NH extra-tropics over the past 1100 years. We can argue about the amplitude later, but the enhanced multi-centennial variability can not be easily dismissed. I should also point out, again, that you saw Fig. 3 in ECS BEFORE it was even submitted to Science and never pointed out the putative scaling "flaw" to me at that time.

With regards to the issue of the late 20th century warming, the fact that I did not include some reference to or plot of the up-to-date instrumental temperature data (cf. Briffa/Osborn) is what I regard as a "sin of omission". What I said was that the estimated temperatures during the MWP in ECS "approached" those in the 20th century portion of that record up to 1990. I don't consider the use of "approached" as an egregious overstatement. But I do agree with you that I should have been a bit more careful in my wording there. As you know, I have publicly stated that I never intended to imply that the

MWP was as warm as the late 20th century (e.g.,

> my New York Times interview). However, it is a

bit of overkill to state twice in the closing sentences of the first two paragraphs of your letter that the ECS results do not refute the unprecedented late 20th century warming. I would suggest that once is enough.

ECS were also very clear about the extra-tropical nature of their data. So, what you say in your letter about the reduced amplitude in your series coming from the tropics, while perhaps worth pointing out again, is beating a dead horse.

However, I must say that the "sin of omission" in the Briffa/Osborn piece concerning the series shown in their plot is a bit worrying. As they say in the data file of series used in their plot (and in Keith's April 5 email response to you),

Briffa/Osborn only used your land temperature estimates north of 20 degrees and recalibrated the mean of those estimates to the same domain of land-only instrumental temperatures using the same calibration period for all of the other non-borehole series in the same way. I would have preferred it if they had used your data north of 30N to make the comparisons a bit more one-to-one. However, I still think that their results are interesting. In particular, they reproduce much of the reduced multi-centennial temperature

variability seen in your complete NH reconstruction. So, if the amplitude of scaled ECS multi-centennial variability is far too high (as you would apparently suggest), it appears that it is also too low in your estimates for the NH extra-tropics north of 20N. I think that we have to stop being so aggressive in defending our series and try to understand the strengths and weaknesses of each in order to improve them.

That is the way that science is supposed to work. I must admit to being really irritated over the criticism of the ECS tree-ring data standardized using the RCS method. First of all, ECS

acknowledged up front the declining available data prior to 1200 and its possible effect on interpreting an MWP in the mean record. ECS also showed bootstrap confidence intervals for the mean of the RCS chronologies and showed where the chronologies drop out. Even allowing for the reduction in the number of represented sites before 1400 (ECS Fig. 2d), and the reduction in overall sample size (ECS Fig. 2b), there is still some evidence for significantly above average growth during two intervals that can be plausibly assigned to the MWP. Of course

> we would like to have had all 14 series cover the

past 1000-1200 years. This doesn't mean that we can't usefully examine the data in the more weakly replicated intervals. In any case, the replication in the MWP of the ECS chronology is at least as good as in other published tree-ring estimates of large-scale temperatures (e.g., NH extra-tropical) covering the past 1000+ years. It also includes more long tree-ring records from the NH temperate latitudes than ever before. So to state that "this is a perilous basis for an estimate of temperature on such a large geographic scale"

is disingenuous, especially when it is unclear how many millennia-long series are contributing the majority of the temperature information in the Mann/Bradley/Hughes (MBH) reconstruction prior to AD 1400. Let's be balanced here. I basically agree with the closing paragraph of your letter. The ECS record was NEVER intended to refute MBH. It was intended, first and foremost, to refute Broecker's essay in Science that unfairly attacked tree rings. To this extent, ECS succeeded very well. The comparison of ECS with MBH was a logical thing to do given that it has been accepted by the

> IPCC as the benchmark reconstruction of NH

annual temperature variability and change over the past millennium. Several other papers have made similar comparisons between MBH and other even more geographically restricted estimates of past temperature. So, I don't apologize in the slightest for doing so in ECS. The correlations in Table 2 between ECS and MBH were primarily intended to demonstrate the probable large-scale, low-frequency temperature signal in ECS independent of explicitly calibrating the individual RCS chronologies before aggregating them. The results should actually have pleased you because, for the 20-200 year band, ECS and MBH have correlations of 0.60 to 0.68, depending on the period used. Given that ECS is based on a great deal of new data not used in MBH, this result validates to a reasonable degree the temperature signal in MBH in the 20-200 year band over the past 1000 years. Given the incendiary and sometimes quite rude emails that came out at the time when ECS and Briffa/Osborn were published, I could also go into the whole complaint about how the review process at Science was "flawed". I will only say that this is a very dangerous game to get into and complaints of this kind can easily cut both ways. I will submit an appropriately edited and condensed version of this reply to Science.

Regards,

Ed

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[1] <http://www.evsc.virginia.edu/faculty/people/mann.shtm>

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

To: Tim Osborn <t.osborn@uea.ac.uk>, Ed Cook <drendro@ldeo.columbia.edu>

Subject: Re: Your letter to Science

Date: Mon, 15 Apr 2002 12:44:53 -0400

Cc: Malcolm Hughes <mhughes@ltr.arizona.edu>, esper@wsl.ch, k.briffa@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu, srutherford@virginia.edu, mann@virginia.edu

Hi Tim,

Thanks for your message. Yes, you guys have us beat on the early monday end of things!

Your points are all taken. I think we all agree there is much work left to be done, more than enough for all of us to continue to be involved in constructive collaboration, etc.

Scott and I, for example, are almost done writing up the work based on your visit w/ us last year, and will send the initial draft on to you, Keith, and the others involved in the near future. It will be a good chance to try to address a lot of these questions in an article of adequate length to discuss the nuances that unfortunately cannot be addressed in a shorter piece.

I also appreciate your more detailed comments about the comparisons, etc. Your points are all reasonable ones. We can maintain an honest difference about how well those points were conveyed in the Science piece (for example, you can imagine how the statement in your piece

"This record has a smaller amplitude of century-to-century variability, and is consistently at or near the upper limit of alternate records produced by other researchers" might indeed have been interpreted as setting MBH99 apart as, in your words, an "outlier").

We have good reason to believe that our reconstruction *will* in fact underestimate extratropical temperature means but far less so full globe/hemisphere-means prior to the 18th century because the basis functions that primarily set the extratropics apart from the full hemispheric patterns (e.g., NAO type patterns and other anomaly patterns largely carried by EOFs #2 and #3) start to drop out from our basis set prior to the 18th century, while the pattern that best resolves the full global and/or hemispheric mean (with note from MBH98, particularly large loadings primarily in the tropics and subtropics) still remains. That is why we have never published an *extratropical* temperature reconstruction prior to the 18th century. I would be happy to discuss this point with you and Keith and others in more detail. Thus, I have compared Esper et al w/ our records in the manner described in my previous email, which I think allows us to diagnose the extent to which differing high-latitude and full-hemispheric patterns may, at times, explain the somewhat modest differences between the records when similarly scaled to the full hemispheric 1856-1990 mean, and always, within the context of the diagnosed uncertainties. There is no guarantee, as you say, that the uncertainties are correct, but I personally believe they'll stand up over time. You can call me on this 10 years from now, and somebody will owe somebody a beer...

In any case, I hope and fully expect we can all continue to all be engaged in constructive interaction & hopefully continued collaboration. It will require some sensitivity on all our part to the larger issues surrounding our work, and the way it gets presented to the broader community, but I don't think that should be all that difficult.

I look forward to these more constructive interactions. I'll do my best to foster them,

Mike

At 01:57 PM 4/15/02 +0100, Tim Osborn wrote:

Dear all,

well, the time zone may let you have the last word before the weekend, but we can get the first word in on a Monday morning!

At 22:35 12/04/02, Michael E. Mann wrote:

In keeping w/ the spirit of Tom's and Keith's emails, I wanted to stress, before we all break for the weekend, that this is ultimately about the science, its not personal. If my comments seemed to assail e.g. Keith's motives or integrity, etc. I believe that they were misunderstood (as I tried to clarify that in my previous message), but I can see that there was a potential for misunderstanding of my message (precision in wording is

very important) given the high levels of sensitivity in this debate. So I wanted to leave no uncertainty about that. And of course, I very much apologize to Keith (and Tim) if they took them my comments that way. They, again, were most decidedly not intended that way.

Thanks for clarifying that, Mike. I think that both Keith and I interpreted your earlier e-mail as being more critical of us than you actually meant it to be. Most issues surrounding the recent Esper et al. and Briffa & Osborn pieces seem to have been covered adequately already. There are just a couple of issues on which I'd like to add a few comments, hopefully clarifying the situation rather than opening up more avenues for debate.

The first relates to the purpose and style of the Briffa & Osborn piece. Perspectives are brief, non-technical and not peer-reviewed. Our instructions were: "The Perspective should provide an overview of recent research in the field and explain to the general reader why the work is particularly exciting." Is it any surprise then that we should focus on the new insights provided by the Esper et al. work, and that it suggests a different climate history than earlier work? And that the constraints of the perspectives format (in terms of length, audience and style) prevented us from listing ALL the caveats and uncertainties related to this and earlier reconstructions and that might be of relevance to their intercomparison? I don't think it is surprising, nor do I think we should be criticised for it.

Moreover, despite the constraints of the perspectives format, I think we were very careful with our wording to avoid misleading the reader. The reference to the IPCC, for example, was not at all sloppy - the opposite, in fact, since it was very carefully worded: the IPCC Synthesis Report is referred to, rather than the full TAR, and it is quite true that there is a focus on the reconstruction of Mann et al. in the former. As Mike says, IPCC conclusions were based on other work too. But I'd guess that many of the readers of our perspective won't have read the full IPCC report, so we thought it valid to focus on the difference between the new work and that shown in the Synthesis Report (which more will have seen). To do this is certainly not unfair to the IPCC. It would only have been unfair if we had implied that the IPCC had ignored this new work - but of course we weren't doing that, because how could one expect the TAR to consider work that is published a year after the TAR itself? We were similarly careful with our wording in our brief mention of the MWP, by saying it is "more pronounced" in Esper et al. - this doesn't mean it is warmer than the others (and thus has no implications for the IPCC conclusion of recent unusual warmth), rather it is pronounced because it is followed by stronger cooling.

The second issue is our re-calibration of the reconstructions. While it hasn't been explicitly stated, I get the impression that this is considered by some to be a poor thing to do. The particular re-calibration we do has a number of effects, including making the Mann et al. reconstruction appear more consistently at the top of the range of alternatives. But please let me assure you (Mike, Ray and Malcolm) that the reason for re-calibrating the records is definitely *not* to make your record appear as an outlier, and I hope you believe me. Indeed, in Jones, Osborn & Briffa (2001: Science 292, 662-667) we showed various NH records *without* applying our re-calibration. We produced our first comparison of records for an earlier Science perspectives piece in 1999 (Briffa & Osborn, 1999) and thought it would be useful to do a re-calibration to remove some of the reasons for inter-reconstruction differences (which can be due to: different proxy data, different statistical methods, different calibration target and different calibration period). The latter two reasons were removed by re-calibrating against a common target series and over a common period. We updated this in Briffa et al. (2001) and acknowledged that the target series (in terms of its spatial and seasonal definition) may not be optimal in all cases. Indeed, it may be especially sub-optimal for Mann et al., because their reconstruction approach combines the proxy records to optimally reconstruct full NH, annual mean T (whereas we have selected land north of 20N, warm-season T as our target for the recalibration). Despite this, we felt justified in doing the recalibration because the Mann et al. series still outperformed

the others in terms of its correlation with the instrumental record over the calibration period! In our latest piece, we have updated the intercomparison in two ways (as well as including new series): (i) we took the spatially-resolved gridded reconstructions of Mann et al. and extracted only land boxes north of 20N; and (ii) we used annual, not warm-season, temperature as the target. The first of these (as explained by Keith and I in an earlier e-mail, which is repeated below because it didn't get sent to all of you first time round) deals with all the points raised by Mike about tropical versus extratropical differences. I would again argue that we were not sloppy, because these changes to our intercomparison were carefully thought out.

So that explains what we have done and why. There is some sensitivity, clearly, to calibration choices, which implies to me that the true uncertainty ranges are probably larger than those estimated solely from the statistical properties of calibration residuals (as used by Briffa et al., and [I think] by Mann et al.). There is clearly more progress to be made!

Best regards to you all

Tim

Date: Fri, 05 Apr 2002 17:17:55 +0100

To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, drdendro@ldeo.columbia.edu, rkerr@aaas.org, bhanson@aaas.org

From: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: Briffa & Osborn piece

Cc: Tim Osborn <t.osborn@uea.ac.uk>

Dear Mike, (and interested colleagues)

Given the list of people to whom you have chosen to circulate your message(s), we thought we should make a short, somewhat formal, response here. I am happy to reserve my informal response until we are face to face! We did not respond earlier because we had more pressing tasks to deal with. This is not the place to go into a long or over-detailed response to all of your comments but a few brief remarks might help to clear up a couple of misconceptions.

You consider our commentary on Ed and Jan's paper

"more flawed than even the paper itself"

on the basis that scaling the relationship between full Northern Hemisphere and extratropical Northern Hemisphere is *much* more problematic than even any of the seasonal issues we discuss. In fact we did not do this. The curve labelled Mann99 in our figure was, in fact, based on the average of only the land areas, north of 20 degrees N, extracted from your spatially-resolved reconstructions. We then scaled it by calibration against the instrumental annual temperatures from the same region. This is, just as you stress in your comments on the Esper et al. paper, what should have been done. We think that this single point addresses virtually of all your concerns. We can, of course, argue about what this means for the pre-1400 part of your reconstruction, when only 1 EOF was reconstructed, but the essential message is that we did our best to exclude the tropics (and the oceans too!) from your series so that it could more readily be compared with the other records.

The fact that we have used only the extra-tropical land from your data is not clear from the text, so we can see why you may not have appreciated this, but we think you will concede that this fact negates much of what you say and that we acted "more correctly" than you realised. Blame *Science* for being so mean with their space allocation if you want! Remember that this was an unrefereed piece and we felt justified in concentrating on one issue; that of the importance of the method of scaling and its effect on apparent "absolute" reconstruction levels. In our draft, we went on to say that this was crucial for issues of simple model sensitivity studies and climate detection, citing the work of Tom Crowley and Myles Allen, but this fell foul of the editor's knife.

You also express concerns about the calibration of Esper et al. (e.g., you say "if the authors had instead used the actual (unsmoothed) instrumental record for the

extratropical northern hemisphere to scale their record, their reconstruction would be much closer to MBH99").

This point is wholly consistent with our discussion in the perspective piece, and indeed we show that in absolute terms the records are closer when Esper et al. is calibrated using unsmoothed data but since the variance is also reduced, the significance of the differences may be just as high.

Finally, we have to say that we do not feel constrained in what we say to the media or write in the scientific or popular press, by what the sceptics will say or do with our results. We can only strive to do our best and address the issues honestly. Some "sceptics" have their own dishonest agenda - we have no doubt of that. If you believe that I, or Tim, have any other objective but to be open and honest about the uncertainties in the climate change debate, then I am disappointed in you also.

Best regards

Keith (and Tim)

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4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Tim Osborn <t.osborn@uea.ac.uk>

To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, Ed Cook <drdendro@ldeo.columbia.edu>

Subject: Re: Your letter to Science

Date: Mon Apr 15 13:57:54 2002

Cc: Malcolm Hughes <mhughes@ltr.arizona.edu>, esper@wsl.ch, k.briffa@uea.ac.uk, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, jto@u.arizona.edu, srutherford@virginia.edu

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Most issues surrounding the recent Esper et al. and Briffa & Osborn pieces seem to have been covered adequately already. There are just a couple of issues on which I'd like to add a few comments, hopefully clarifying the situation rather than opening up more avenues for debate.

The first relates to the purpose and style of the Briffa & Osborn piece. Perspectives are brief, non-technical and not peer-reviewed. Our instructions were: "The Perspective should provide an overview of recent research in the field and explain to the general reader why the work is particularly exciting." Is it any surprise then that we should focus on the new insights provided by the Esper et al. work, and that it suggests a different climate history than earlier work? And that the constraints of the perspectives format (in terms of length, audience and style) prevented us from listing ALL the caveats and uncertainties related to this and earlier reconstructions and that might be of relevance to their intercomparison? I don't think it is surprising, nor do I think we should be criticised for it.

Moreover, despite the constraints of the perspectives format, I think we were very careful with our wording to avoid misleading the reader. The reference to the IPCC, for example, was not at all sloppy - the opposite, in fact, since it was very carefully worded: the IPCC Synthesis Report is referred to, rather than the full TAR, and it is quite true that there is a focus on the reconstruction of Mann et al. in the former. As Mike says, IPCC conclusions were based on other work too. But I'd guess that many of the readers of our perspective won't have read the full IPCC report, so we thought it valid to focus on the difference between the new work and that shown in the Synthesis Report (which more will have seen). To do this is certainly not unfair to the IPCC. It would only have been unfair if we had implied that the IPCC had ignored this new work - but of course we weren't doing that, because how could one expect the TAR to consider work that is published a year after the TAR itself? We were similarly careful with our wording in our brief mention of the MWP, by saying it is "more pronounced" in Esper et al. - this doesn't mean it is warmer

than the others (and thus has no implications for the IPCC conclusion of recent unusual warmth), rather it is pronounced because it is followed by stronger cooling.

The second issue is our re-calibration of the reconstructions. While it hasn't been explicitly stated, I get the impression that this is considered by some to be a poor thing to do. The particular re-calibration we do has a number of effects, including making the Mann et al. reconstruction appear more consistently at the top of the range of alternatives. But please let me assure you (Mike, Ray and Malcolm) that the reason for re-calibrating the records is definitely **not** to make your record appear as an outlier, and I hope you believe me. Indeed, in Jones, Osborn & Briffa (2001: Science 292, 662-667) we showed various NH records **without** applying our re-calibration.

We produced our first comparison of records for an earlier Science perspectives piece in 1999 (Briffa & Osborn, 1999) and thought it would be useful to do a re-calibration to remove some of the reasons for inter-reconstruction differences (which can be due to: different proxy data, different statistical methods, different calibration target and different calibration period). The latter two reasons were removed by re-calibrating against a common target series and over a common period. We updated this in Briffa et al. (2001) and acknowledged that the target series (in terms of its spatial and seasonal definition) may not be optimal in all cases. Indeed, it may be especially sub-optimal for Mann et al., because their reconstruction approach combines the proxy records to optimally reconstruct full NH, annual mean T (whereas we have selected land north of 20N, warm-season T as our target for the recalibration). Despite this, we felt justified in doing the recalibration because the Mann et al. series still outperformed the others in terms of its correlation with the instrumental record over the calibration period! In our latest piece, we have updated the intercomparison in two ways (as well as including new series): (i) we took the spatially-resolved gridded reconstructions of Mann et al. and extracted only land boxes north of 20N; and (ii) we used annual, not warm-season, temperature as the target. The first of these (as explained by Keith and I in an earlier e-mail, which is repeated below because it didn't get sent to all of you first time round) deals with all the points raised by Mike about tropical versus extratropical differences. I would again argue that we were not sloppy, because these changes to our intercomparison were carefully thought out.

So that explains what we have done and why. There is some sensitivity, clearly, to calibration choices, which implies to me that the true uncertainty ranges are probably larger than those estimated solely from the statistical properties of calibration residuals (as used by Briffa et al., and [I think] by Mann et al.). There is clearly more progress to be made!

Best regards to you all

Tim

Date: Fri, 05 Apr 2002 17:17:55 +0100

To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, p.jones@uea.ac.uk, tcrowley@duke.edu, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, drdendro@ldeo.columbia.edu, rkerr@aaas.org, bhanson@aaas.org

From: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: Briffa & Osborn piece

Cc: Tim Osborn <t.osborn@uea.ac.uk>

Dear Mike, (and interested colleagues)

Given the list of people to whom you have chosen to circulate your message(s), we thought we should make a short, somewhat formal, response here. I am happy to reserve my informal response until we are face to face! We did not respond earlier because we

had more pressing tasks to deal with. This is not the place to go into a long or over-detailed response to all of your comments but a few brief remarks might help to clear up a couple of misconceptions.

You consider our commentary on Ed and Jan's paper "more flawed than even the paper itself"

on the basis that scaling the relationship between full Northern Hemisphere and extratropical Northern Hemisphere is *much* more problematic than even any of the seasonal issues we discuss. In fact we did not do this. The curve labelled Mann99 in our figure was, in fact, based on the average of only the land areas, north of 20 degrees N, extracted from your spatially-resolved reconstructions. We then scaled it by calibration against the instrumental annual temperatures from the same region. This is, just as you stress in your comments on the Esper et al. paper, what should have been done. We think that this single point addresses virtually of all your concerns. We can, of course, argue about what this means for the pre-1400 part of your reconstruction, when only 1 EOF was reconstructed, but the essential message is that we did our best to exclude the tropics (and the oceans too!) from your series so that it could more readily be compared with the other records.

The fact that we have used only the extra-tropical land from your data is not clear from the text, so we can see why you may not have appreciated this, but we think you will concede that this fact negates much of what you say and that we acted "more correctly" than you realised. Blame *Science* for being so mean with their space allocation if you want! Remember that this was an unrefereed piece and we felt justified in concentrating on one issue; that of the importance of the method of scaling and its effect on apparent "absolute" reconstruction levels. In our draft, we went on to say that this was crucial for issues of simple model sensitivity studies and climate detection, citing the work of Tom Crowley and Myles Allen, but this fell foul of the editor's knife.

You also express concerns about the calibration of Esper et al. (e.g., you say "if the authors had instead used the actual (unsmoothed) instrumental record for the extratropical northern hemisphere to scale their record, their reconstruction would be much closer to MBH99").

This point is wholly consistent with our discussion in the perspective piece, and indeed we show that in absolute terms the records are closer when Esper et al. is calibrated using unsmoothed data but since the variance is also reduced, the significance of the differences may be just as high.

Finally, we have to say that we do not feel constrained in what we say to the media or write in the scientific or popular press, by what the sceptics will say or do with our results. We can only strive to do our best and address the issues honestly. Some "sceptics" have their own dishonest agenda - we have no doubt of that. If you believe that I, or Tim, have any other objective but to be open and honest about the uncertainties in the climate change debate, then I am disappointed in you also.

Best regards

Keith (and Tim)

From: Mike Hulme <m.hulme@uea.ac.uk>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: [Fwd: SSI Alert: IPCC Chair Vote]
Date: Mon Apr 22 18:14:44 2002
Cc: s.raper

Phil,

I can't quite see what all the fuss is about Watson - why should he be re-nominated anyway? Why should not an Indian scientist chair IPCC? One could argue the CC issue is more important for the South than for the North. Watson has perhaps thrown his weight about too much in the past. The science is well covered by Susan Solomon in WGI, so why not get an engineer/economist since many of the issues now raised by CC are more to do with energy and money, than natural science.

If the issue is that Exxon have lobbied and pressured Bush, then OK, this is regrettable but to be honest is anyone really surprised? All these decisions about IPCC chairs and co-chairs are deeply political (witness DEFRA's support of Martin Parry for getting the WGII nomination).

Mike

At 07:17 20/04/02 +0100, you wrote:

There is more on the BBC Sci/Tech web site.

Phil

Date: Fri, 19 Apr 2002 18:24:58 -0600
From: Tom Wigley <wigley@ucar.edu>
X-Mailer: Mozilla 4.76 [en] (Windows NT 5.0; U)
X-Accept-Language: en
To: Phil Jones <p.jones@uea.ac.uk>, Sarah Raper <s.raper@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>

Subject: [Fwd: SSI Alert: IPCC Chair Vote]
You may not have seen this latest piece of politicalization by the Bushies.

Tom.

----- Original Message -----

Subject: SSI Alert: IPCC Chair Vote
Date: Fri, 19 Apr 2002 18:00:59 -0400
From: "SSI Mailbox" <ssi@ucsusa.org>

***** EXECUTIVE SUMMARY *****

ISSUE: Today - April 19, 2002, the Intergovernmental Panel on Climate Change (IPCC) plenary voted for Dr. Rajendra Pachauri as the sole chair of the IPCC. Dr. Pachauri, an

economist and engineer, will replace Dr. Robert Watson, an atmospheric chemist, as chair of the IPCC. This outcome was actively sought by the Bush Administration at the behest of the most conservative elements of the fossil fuel industry. This development threatens to undermine the scientific credibility and integrity of the IPCC and may weaken the job this extraordinary body has done to bring the world's attention to one of the most pressing environmental problems.

ACTION: Monitor your local paper and respond to news stories with a letter-to-the-editor.

MAIN MESSAGE: Given the Bush Administration's consistent opposition to climate change mitigation, it is especially imperative at this time that the scientific community and Dr. Pachauri work together to ensure that the IPCC remains a strong and credible scientific process.

DEADLINE: As soon as possible after the story runs in your paper -- preferably the same day but no later than a day or two after.

*** THE ISSUE ***

According to a report by Associated Press today (appended below), Dr. Rajendra Pachauri was elected as Chair of the IPCC at a plenary meeting in Geneva. As you would be aware from our earlier SSI alerts of the past several weeks, this follows on from intense lobbying of the US government by the fossil fuel industry to remove Dr. Robert Watson as Chair. Although reports from Geneva are still sketchy, our sources on the ground tell us that there was intense behind-the-scenes lobbying by Saudi Arabia, with assistance from Don Pearlman -- a well known oil and gas lobbyist with strong connections to industry-backed organizations opposed to climate change mitigation. Through their maneuvering, the co-chair compromise approach -- comprised of former chair Dr. Robert Watson and Dr. Pachauri -- was not considered. As a result of this election, there is considerable concern in the climate science and environmental communities -- reinforced by the intensive lobbying from fossil fuel interests on this decision -- that the Bush Administration's lack of support for former IPCC Chair Dr. Robert Watson signals a more general lack of support for the IPCC as a credible international scientific assessment process that provides governments with sound information on climate

science, impacts, and solutions.

By supporting Dr. Pachauri for primarily political purposes, the Bush Administration has seriously threatened the scientific credibility of the IPCC process. The conservative fossil fuel interests should be exposed for their role in influencing the US government's stance on this issue, and the IPCC process must remain a scientifically credible and non-politicized process.

The next IPCC Climate Change Assessment is due out in five years, and it is the chair's role to oversee this complex process. The scientific community's voice is important in this issue to ensure that the IPCC process remains strong under the leadership of Dr. Pachauri and that the Bush Administration does not erode the effectiveness of this important international body.

*** THE ACTION ***

-- Monitor your local paper and respond to news stories with a letter-to-the-editor.

Information on how and to whom to submit a LTE is usually found right on the Letters Page in your paper. Many papers now accept letters via email. If you can't find the information you need, simply call the paper and ask how to go about submitting a letter in response to a recently published article.

To increase the chances that your letter will be published, do the following:

- keep it under 200 words and stay focused on one or two main points you'd like to make;
- focus on a local angle, if possible, that adds something new to the story that appeared in your paper;
- be sure to include your name, address, and daytime phone number; the paper will contact you before printing your letter; and
- submit the letter on the same day the story appears, if possible.

[For additional help with writing an effective letter to the editor, you may turn to the reference guide on the SSI member page at <[1]<http://www.ucsusa.org/ssimembers/index.html> >.]

-- MAIN MESSAGE: Given the Bush Administration's consistent opposition to climate change mitigation, it is especially imperative at this time that the scientific community and Dr. Pachauri work together to ensure that the IPCC remains a strong and credible scientific process.

-- TIMING: Your letter to the editor should reach your paper within a few days of the publication of the story to increase the chances of it being published.

-- SPECIAL NOTE: If your paper did not carry the story at all yet, send an LTE describing the story and emphasizing that this issue is of great interest to the paper's subscribers.

*** SUPPORTING MESSAGES ***

-- [Be sure to include a description of your scientific expertise, your involvement with the IPCC process, or the importance of the climate issue to your community.]

-- For the past 10 years, the IPCC's science has been the foundation for sound policymaking on the climate issue. The IPCC's unique intergovernmental approach to scientific consensus has worked amazingly well but is now threatened.

-- It is disturbing that the Bush Administration sought and received advice from the fossil fuel industry on the leadership of an important scientific body such as the IPCC. A politicized IPCC threatens the integrity and credibility of the scientific process.

-- There are fears that it will now be easier for the US to distance itself from the IPCC process. You may point out that the US already rejected the Kyoto protocol last year.

-- It is vital that the scientific process for the next Assessment Report (due out in another five years) not be compromised so that the IPCC continues to produce sound science on climate change.

-- The credibility of the IPCC's Third Assessment Report (TAR) findings were strongly affirmed by the US National Academy of Sciences (NAS), which published its supportive report in response to President Bush's request for an independent assessment on the state of climate science.

*** SUPPLEMENTAL INFORMATION ***

-- Dr Rajendra K. Pachauri is an Indian engineer and economist. Pachauri, formerly one of the five vice chairs of the IPCC, is highly regarded but will be the first non-atmospheric chemist as chair of the IPCC.

-- For more information on the ExxonMobil memo urging the Bush Administration to remove Dr. Watson from his position as IPCC Chair, please see

< [2]<http://www.nrdc.org/media/docs/020403.pdf> >.

-- For information on the Saudi/Pearlman connection, see the summary by Jeremy Leggett, author of "The Carbon War", at

< [3]<http://www.carbonwar.com/ccchrono.html> >.

-- IPCC - Intergovernmental Panel on Climate Change: The Intergovernmental Panel on Climate Change (IPCC) was established in 1988 under the auspices of the United Nations Environment Programme and the World Meteorological Organization for the purpose of assessing "the scientific, technical and socioeconomic information relevant for the understanding of the risk of human-induced climate change." To date, the IPCC has issued three comprehensive assessments. The first assessment report (FAR) was released in 1990, the second assessment report (SAR) was released in 1996, and the third assessment report (TAR) was released in 2001. These assessments are based on "published and peer reviewed scientific technical literature"

For more information see < [4]<http://www.ipcc.ch> >

NOTE: Please send us an email message that tells us what action you took. If you actually send a letter, please send us a "blind copy." (A blind copy simply means that you do not indicate anywhere on your letter that you are sending a copy to us.) Send to: ssi@ucsusa.org or UCS, 2 Brattle Square, Cambridge, MA 02238-9105 (attn. Jason Mathers). CHANGE OF EMAIL ADDRESS: Help us keep you posted! If your email address will soon change, or if you'd like us to use a different address, please let us know by sending a message to ssi@ucsusa.org with your new address. Thanks!

Associated Press

Fri Apr 19, 1:18 PM ET

U.S. scientist voted off international climate panel

By JONATHAN FOWLER, Associated Press Writer

GENEVA - A U.S. scientist was voted off an international climate panel Friday following what campaigners claimed was pressure from the oil industry and Washington.

Atmospheric scientist Robert Watson was seeking re-election as head of the Intergovernmental Panel on Climate Change. World Meteorological Organization (news - web sites) spokeswoman Mo Lagarde said Watson was defeated by Indian challenger Rajendra Pachauri. Some 76 countries supported Pachauri, while 49 voted for Watson in the secret ballot, she said.

Seven nations voted for Jose Goldemberg, a Brazilian (news - web sites) who entered the race this week.

The WMO and the U.N. Environment Program jointly host the IPCC's offices and organized the Geneva meeting.

Environmental groups have accused the administration of President George W. Bush (news - web sites) of caving in to a request from Exxon Mobil that it try to remove Watson, a leading expert on global warming (news - web sites), because he had consistently warned governments of the dangers of climate change.

"The fossil fuel industry and the U.S. government will be celebrating their success in kicking out Bob Watson, an experienced scientist who understood that urgent action is needed to tackle global climate change," said Kate Hampton, international climate co-ordinator for British-based Friends of the Earth (news - web sites). "The Bush administration and its friends would rather shoot the messenger than listen to the message," Hampton said in a statement.

The Swiss-based Worldwide Fund for Nature said it was worried by the "apparent politicization" of the IPCC.

"WWF is concerned that oil and gas interests had too much to say in the removal of Dr. Watson as chairman of what should be an impartial, scientific body," said Jennifer Morgan, Director of WWF's Climate Program.

But, Morgan said, the "IPCC is a vibrant group of scientists and WWF looks forward to working closely with Dr. Pachauri to protect the integrity of the IPCC and ensure that it continues to produce sound science on climate change."

The U.S. State Department said earlier this month that it would support Pachauri, who was the Indian government's nominee, to become the next chair.

Two weeks ago, the Natural Resources Defense Council, a Washington, D.C.-based environmental group, said the White House's Council on Environmental Quality received a memo from Exxon Mobil in February 2001 that asked, "Can Watson be replaced now at the request of the U.S.?"

The memo, which the group said it obtained through the Freedom of Information Act, also recommended that the administration "restructure the U.S. attendance at upcoming IPCC meetings to assure none of the Clinton/Gore proponents are involved in any decisional activities."

U.S. officials were unavailable for comment.

Watson has been an outspoken proponent of the idea that fossil fuel emissions contribute to rising global temperatures. He has led the panel since 1996 and is also

the chief scientist of the World Bank (news - web sites).
Pachauri is an engineer and an economist and is the director
of the Tata Energy Research Institute in New Delhi, India.

Prof. Phil Jones

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NR4 7TJ
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References

1. <http://www.ucsusa.org/ssimembers/index.html>
2. <http://www.nrdc.org/media/docs/020403.pdf>
3. <http://www.carbonwar.com/ccchrono.html%A0>
4. <http://www.ipcc.ch/>

From: Mike Hulme <m.hulme@uea.ac.uk>

To: s.torok

Subject: In Tyndall

Date: Sat May 18 17:25:51 2002

Simon,

A version of this for In Tyndall please - you should add the relevant EPSRC web site if you can track it down.

Mike

EPSRC invests in adventurous ideas

EPSRC is to establish an adventurous research fund. A total of £4.5 million has been earmarked for research projects that include a mixture of disciplines and as such may face barriers to selection under EPSRCs core research programmes. The pilot initiative will be launched with a call for outline proposals at the end of May. The closing date will be at end of July. Those successful at the outline stage will be asked to submit full proposals by December. The new funds principal novelty is an emphasis on funding people to work in other disciplines or between disciplines. EPSRC will fund any research project that falls within its centre of gravity. We are happy for it to be 49 per cent in another research council remit, so long as the majority is in the EPSRC remit, says Hylton. Equally, EPSRC has not capped how much money people can apply for. Another key difference is the way in which the proposals will be evaluated. It will be a two-stage process with outline proposals followed by full proposals. The outline stages of applying to the adventure fund are to be assessed anonymously. In addition, the initiative will have its own bespoke outline application form, proposal form and referees assessment form. EPSRC also hopes the initiative will go some way to changing UK research culture.

From: Ed Cook <drdendro@ldeo.columbia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Esper et al. and Mike Mann
Date: Mon, 17 Jun 2002 13:20:40 -0400

<x-flowed>

Hi Keith,

Of course, I agree with you. We both know the probable flaws in Mike's recon, particularly as it relates to the tropical stuff. Your response is also why I chose not to read the published version of his letter. It would be too aggravating. The only way to deal with this whole issue is to show in a detailed study that his estimates are clearly deficient in multi-centennial power, something that you actually did in your Perspectives piece, even if it was not clearly stated because of editorial cuts. It is puzzling to me that a guy as bright as Mike would be so unwilling to evaluate his own work a bit more objectively.

Ed

>I have just read this lettter - and I think it is crap. I am sick to
>death of Mann stating his reconstruction represents the tropical
>area just because it contains a few (poorly temperature
>representative) tropical series. He is just as capable of
>regressing these data again any other "target" series , such as the
>increasing trend of self-opinionated verbage he has produced over
>the last few years , and ... (better say no more)

>Keith

>

--

=====
Dr. Edward R. Cook
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Tree-Ring Laboratory
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=====

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>

To: "Michael E. Mann" <mann@virginia.edu>, rbradley@geo.umass.edu, k.briffa@uea.ac.uk, mhughes@ltr.arizona.edu, t.osborn@uea.ac.uk, srutherford@virginia.edu, mann@virginia.edu

Subject: Re: AGU abstract

Date: Tue, 13 Aug 2002 10:16:42 +0100

Mike,

Checked with Keith and Tim. The abstract is like one we would write - leaves all options open as to what will be presented. At least AGU and EGS don't charge to get abstracts printed. AMS have so many missing now with their charges that the book of abstracts is ridiculous. Fine for all three of us to be there and we look forward to seeing some results in the autumn. This will be when the real action begins.

The CCDD meeting in early Nov. might be at a good time to discuss some results.

Add an 'of' between choice and actual on the third line.

Cheers

Phil

At 19:56 12/08/02 -0400, Michael E. Mann wrote:

Dear all,

The following is an abstract for a talk I've been invited to give at the winter AGU meeting in a session on "Climate of the Past 2000 Years". I would like to summarize the collaborative work that was begun by Scott, Tim and myself a couple summers ago during Tim's visit here. Scott is working on finalizing the results of our analyses now, and a draft should be available for review shortly that compares reconstructions based on our covariance-based reconstruction method, using (i) multiproxy, (ii) MXD, and (ii) combined multiproxy+MXD datasets for different (cold, warm, annual) target seasonal windows. I'd like to invite everyone listed below to be authors on both this abstract, and the paper that we're in the process of drafting, describing the results. I've kept the abstract intentionally vague, so that we can work out an interpretation of the results that we're all comfortable with in the months ahead, prior to the talk, and submission of the paper.

I look forward to confirmation of your interest in being a co-author, and any feedback you have. I'd like to submit this by the end of the week, which will be my last opportunity to do so prior to the AGU abstract deadline, owing to my travel schedule. thanks in advance for getting back to me ASAP.

best regards,

Mike

Progress in Proxy-Based Reconstruction of Surface Temperature Variations in Past Centuries

Michael E. Mann

Raymond Bradley

Keith Briffa

Malcolm Hughes

Philip Jones

Timothy Osborn

Scott Rutherford

Results are presented from a set experiments designed to control for the various factors that may influence reconstructions of large-scale temperature patterns in past centuries, including (a) the choice actual proxy data used, (b) the reconstruction methodology, (c) the spatial domain of the reconstruction and (d) the seasonal window targeted. These experiments compare results based both on the global multiproxy data set used by Mann and coworkers and the extratropical Northern Hemisphere maximum latewood tree-ring density set used by Briffa and coworkers. Estimates of hemispheric mean temperature trends are formed both through averaging of large-scale patterns reconstructed from full proxy data network, and through simple compositing of regional temperature reconstructions. Northern hemisphere mean estimates are compared for the full Northern hemisphere (tropics and extratropics, land and ocean), and extratropical continents only, and using various (cold-season half year, warm-season half year, and annual mean) seasonal targets for the reconstructions. Implications of these experiments for the robustness of proxy-based reconstructions of past large-scale temperature trends are discussed.

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
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Charlottesville, VA 22903

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Yamal paper for The Holocene special issue
Date: Wed, 21 Aug 2002 17:56:18 +0500
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith,
thank you very much for editing our paper.
It's a pity you strike your name off the list of authors, you
make an important contribution to writing paper. Your corrections
and additions surely improve paper.

I would only notice the next sentence (page 8):

'The low interannual variability and the minimum occurrence of
cold extremes during the 20th century, argue that the most recent
decades of this long summer record represent the most favourable
climate conditions for tree growth within the last four
millennia.'

I'm not sure that this statement follows unambiguously from results
presented in this paper. Because mean temperatures during last
decades, according to presented reconstruction, are not exceptional.
Besides, e.g. period about 1700 BC, according to this
reconstruction, represent probably the same conditions taking
into account low variability, low occurrence of extremes and high
mean temperature.

Maybe to soften this statement and replace 'the most favourable'
with something like 'highly favourably' or 'probably the most
favourable'?

Thank you once more for invaluable assistance.

Best regards,
Rashit M. Hantemirov

(I'm sorry for the late answer, I just come back from the trip to
the north.)

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8 Marta St., 202

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<http://ipae.uran.ru/8personalies/dendro.html#3>

From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Iain Brown (UKCIP)" <iain.brown@ukcip.org.uk>
Subject: Re: temporal interpolation for UKCIP scenarios
Date: Wed Sep 11 12:39:26 2002
Cc: geoff.jenkins@metoffice.com,x.lu.j.turnpenny

Iain (and Geoff),

Definitive explanations are always dangerous! The reasoning behind this is as follows:

- the report only analysed and pictured seasonal and annual data (DJF,MAM, etc.) [in fact, nearly all published maps of climate model outputs show changes in seasonal - 3-month - averages]. This applying a uniform filter over 90 or 360 days.
- the requested datasets are at monthly time-steps. The default option for this is in effect applying a uniform 30-day filter. [one might also conceive of weekly or daily time-step files - e.g. changes in Week 13 for the 2050s for precip. for Medium-High or changes for Julian day number 256 for the 2080s for Tmin for Low].
- these are all arbitrary choices of course, dictated by convention. But the important point it seems to me is again a signal to noise issue - the shorter the time-averaging period, the weaker the S/N ratio [i.e., we have more confidence that averaged over a year, Tmin in the UK will increase by, say, 2.7degC for certain scenario, than that for the same scenarios Tmin on 13 June will increase - on average - by 2.6degC and on 14 June only by 2.3degC - is this difference between 2.6 on 13 June and 2.3 on 14 June really meaningful? No - it is most likely due to noise - natural variability].
- this reasoning suggests that as the time-averaging period decreases, one should pay less attention to small differences between adjacent time-averaged periods, e.g. if June precip. goes down by 10%, is the fact that July precip. goes down by 20% and August by 5% really meaningful?

-

At 10:13 11/09/02 +0100, Iain Brown (UKCIP) wrote:

Mike,

For the UKCIP Scenarios datasets - both 98 and 02 - temporal interpolation was applied to the raw model data in the form of a 1-2-1 filter. This had the effect of smoothing out monthly values so that there are not as abrupt transitions between adjacent months.

Can you provide us with the definitive explanation for the interpolation?

Some users (eg. in the recent London study) have noted that there are differences between the maps they have derived from the data and the maps in the UKCIP02 report.

best wishes,

Iain

Dr. Iain Brown

UK Climate Impacts Programme

12 St. Michael's St.

Oxford

OX1 2DU

From: Tom Wigley <wigley@ucar.edu>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: Hadley Centre request for MAGICC
Date: Fri, 13 Sep 2002 09:27:20 -0600
Cc: Gareth Jones <gareth.s.jones@metoffice.com>, s.raper@uea.ucar.edu, wigley@ncar.ucar.edu, Ben Santer <santer1@lnl.gov>

Gareth,

It seems to me, from reading your email, that you do not realize that this is precisely what MAGICC/SCENGEN already does -- i.e., it uses the scaling method that Ben Santer and I 'invented' in the late 1980s to get time dependent patterns of future climate change. I am attaching a description of the method as we employ it.

The current CDROM version uses only a SAR version of the UD-EBM. Of course, there is a TAR version that Sarah used for the TAR, developed by me and Sarah -- but mainly Sarah. This has not yet been put into MAGICC/SCENGEN, although I am in the process of doing so (along with making a number of other changes to the software). We do not normally give the code for TAR/MAGICC to others unless it is as part of a collaborative project. As Mike Hulme noted, what we can do for/with you will have to be a joint decision with me and Sarah.

The issue of how well scaling works compared with a full AOGCM is both important and of considerable interest to me (and Ben Santer). It is something we have looked at in the past, cursorily, and which we were planning to investigate more fully with the suite of PCM runs that we have here. There are some tricky issues that need to be addressed.

So, perhaps we should pool our intellectual, modelling and data resources?

Anyhow, check out the attached and get back to me with your views.

The 'new and improved version' of MAGICC/SCENGEN should be available in beta-test form in about a month. It will have around 30 models in its data base, and it does a lot of new things that I can tell you about later.

Tom.

+++++

Mike Hulme wrote:

>
> Gareth,
>
> Thank you for endowing me with the grand title of co-ordinator of magic!!
>
> Such a position does not really exist here. The model developers are Sarah
> Raper and Tom Wigley, to whom I am copying this reply, and it is the two of
> them that really need to grant your request.

>
> My role is more specifically in relation to the availability and
> distribution of the public domain version of MAGICC/SCENGEN Version 2.4 on
> CD-ROM and the accompanying manual. However, your request is really for
> the TAR version of MAGICC and even the source code and that request I
> cannot grant.
>
> I would hope that either/or Sarah and Tom will reply to you directly.
>
> Best wishes,
>
> Mike
>
> At 11:54 13/09/02 +0100, you wrote:
>>Dear Dr Hulme,
>> I believe that you are the MAGICC co-ordinator in the Climatic
>> Research
>>Unit. I hope you can assist me with the following request.
>>
>> I would like to obtain a version of the Magicc model that would allow
>>the input of climate forcings (rather than emission scenerios).
>>
>>I am in the detection and attribution group within the Hadley Centre, Met
>>Office. I am working with Dr Peter Stott and Dr John Mitchell on a project
>>that
>>requires an EBM.
>>
>>What we want to use the EBM for is to simulate global mean temperatures for
>>different forcings which we can then multiply with equilibrium temperature
>>spatial patterns for the same forcings to create surrogate transient time
>>varying climate patterns. If the surrogate patterns compare favourably
>>with our
>>HadCM3 simulations, we will then want to investigate how the detection and
>>attribution of climate change (for the detection schemes we use) will be
>>affected by uncertainties in the forcings we use. We would like to use
>>Magicc
>>as it has been tuned already to the HadCM3 anthropogenic emissions scenerios,
>>and as a model used extensively in the recent IPCC TAR would be most
>>appropriate
>>for our work.
>>
>>Would it be possible to obtain a copy of MAGICC or can you tell me how I
>>could
>>go about obtaining the model?
>>
>>Thanks in advance
>>Gareth
>>
>>--
>>Dr Gareth S. Jones Climate Research Scientist
>>Met Office, Hadley Centre for Climate Prediction and Research,
>>London Road, Bracknell, RG12 2SY, UK <http://www.metoffice.com>
>>Tel/Fax: +44(0)1344 85 6903/4898 email:gareth.s.jones@metoffice.comContent-Type: x-msword;

name="MAG-SG.doc"

Content-Disposition: inline;

filename="MAG-SG.doc"

Attachment Converted: "c:\eudora\attach\MAG-SG1.doc"

Content-Type: x-msword;

name="SGFlowchart.doc"

Content-Disposition: inline;

filename="SGFlowchart.doc"

Attachment Converted: "c:\eudora\attach\SGFlowchart1.doc"

From: Martin Welp <martin.welp@pik-potsdam.de>

To: gberz@munichre.com, ccarraro@unive.it, baldur.eliasson@ch.abb.com, juergen.engelhard@rheinbraun.de, bhare@ams.greenpeace.org, klaus.hasselmann@dkrz.de, hourcade@centre-cired.fr, m.hulme@uea.ac.uk, SSinger@wwfepo.org, carlo.jaeger@pik-potsdam.de, martin.welp@pik-potsdam.de

Subject: ECF: Monthly telephone conference (7 October)

Date: Wed, 02 Oct 2002 19:00:02 +0200

Cc: tloster@munichre.com, anders.h.nordstrom@se.abb.com, e.l.jones@uea.ac.uk, Ottmar.Edenhofer@pik-potsdam.de

Dear member of the extended board

The next ECF telephone conference takes place on Monday, 7 October 2002 at 17-18 CET (Central European Time). The participants are:

Gerhard Berz 089-3891 5290

Carlo Carraro +39-335-6170 775

Baldur Eliasson +41-58-586-8031

Jürgen Engelhard 0221-480 1460

Bill Hare 0331-288 2412

Klaus Hasselmann 04121-508 849

Jean-Charles Hourcade +33-1-43 94 73 63

Mike Hulme +44-1603-593162

Stephan Singer +32-2-74 38817

Carlo Jaeger 0331-288 2601

Martin Welp 0331-288 2619

Please check that your number is correct. If you want to be called at another number please inform me by the end of this week. In case there are technical problems at the beginning or during the conference please call the Deutsche Telekom at +49-(0)69-90922723.

The agenda is as follows (it may be modified at the beginning of the meeting):

1 Minutes of the previous telephone conference (5 Min.)

2 Working groups (10 Min.)

3 Meetings & Events (15 Min.)

- Report of the meeting with IEA (International Energy Agency)

- Report of the meeting with Vivendi Environnement Institute

- ECF general assembly (13 November)

- ECF conference in Berlin (14-15 November)

- Workshop of the Technology Group in Oldenburg (12-13 December)

4 Next steps (15 Min.)

5 Varia (15 Min.)

Best regards,

Martin Welp

--
NOTE NEW FAX NUMBER

Dr. Martin Welp

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<http://www.pik-potsdam.de/~welp/index.html>

<http://www.European-Climate-Forum.net/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Mike Salmon <m.salmon@uea.ac.uk>
Subject: Fwd: Re: Polar Urals data
Date: Fri Oct 11 09:08:25 2002

I am forwarding this to stimulate you (no it's not one of those emails!) to hassle me to check and update the tree-ring and my stuff on the web. Cheers
Keith

Date: Thu, 10 Oct 2002 11:22:37 -0400
From: Leonid Polyak <polyak.1@osu.edu>
Subject: Re: Polar Urals data
X-Sender: lpolyak@pop.service.ohio-state.edu
To: Keith Briffa <k.briffa@uea.ac.uk>
X-Mailer: QUALCOMM Windows Eudora Pro Version 3.0.3 (32)
Got it! Note that there appears to be an error in the explanation for the data file: Polar Ural data are f2, not f1 (as far as I can judge).

Thank you,

Leonid

>
>Leonid
>see [1]<http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/>
>The data (and other possibly interesting data are available there) .
>Best wishes
>Keith

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784

[2][http://www.cru.uea.ac.uk/cru/people/briffa\[3\]/](http://www.cru.uea.ac.uk/cru/people/briffa[3]/)

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <>wigley@ucar.edu>
Subject: Re: T data
Date: Mon, 28 Oct 2002 15:50:07 +0000
Cc: Ben Santer <santer1@llnl.gov>,t.osborn@uea.ac.uk

<x-flowed>

Tom,

Talked to Tim re the SD field. Can you read the following (J. Climate 10, 2548-2568)

before you come so you know how Tim infilled the SD field ? HadCM2 data was used.

This would seem to bias any model validation to this model. Also it would seem odd to

validate any model in a region where there is no data - in a region that had to be infilled.

I can see that global fields make things simpler, but they will need to be constructed in

the best possible way. In 1997 we thought the best way was to use a model, but our aim then was different from yours.

Cheers

Phil

At 06:04 28/10/02 -0700, Tom Wigley wrote:

>Phil,

>

>Thanx. I need to see if CMIP has the height fields for models ---

>Ben????

>

>Tom.

>

>_____

>

>Phil Jones wrote:

>>

>> Tom,

>> Here's the file that you should have got back in September. It is

>> 1981-2000 where this
>> could be calculated and 1961-90 elsewhere. The other fields (already
>> sent) enable you to
>> know where the 1961-90 field has been used.
>> All you need to overcome the problem of this being surface
>> temperatures is to get a
>> 5 by 5 degree average height field. I have emailed Mark New to see if he
>> has a 1 by 1 degree
>> height field, which could then be averaged. Mark must have had this at
>> some stage - he
>> has a 10 minute height field for the world, which I'm sure he has
>> degraded to 1 degree. I
>> have a land/sea mask at 1 by 1 degree, so am hoping Mark has the heights.
>> With this
>> all you will need is the model height fields.
>> As for the SD's it would be possible to produce this for a period
>> like 1981-2000 or 1961-90
>> but both would have gaps - probably exactly the same as in the
>> climatology. The options
>> to consider here are:
>>
>> 1. Period 1981-2000 or 1961-90?
>> 2. How many years in each needed to get an SD?
>> 3. How to infill the gaps?
>>
>> Tim Osborn must have infilled the gaps for the errors paper in 1997 as we
>> needed a complete
>> field of variances. He did this by blending some model data
>> (HadCM2/ECHAM3 probably)
>> with the real observations. Most areas get infilled easily - big problem
>> is the Southern Oceans
>> and the Antarctic (also central Arctic). I will talk to Tim.
>>
>> We can discuss this more when you come.
>>
>> Cheers
>> Phil
>>
>> PS I should have some results from Anders by the time you come. He is
>> comparing means/
>> SDs and extremes etc of HadRM3 with real world data from 200 sites across
>> Europe. Only
>> temperature variables in the first part. Clearly shows that for

>> islands/coasts comparisons
>> must be with land points in the model. We've had to 'move' some stations
>> to be on model
>> land to get better comparisons. Islands that are not in the model have
>> poor comparisons.
>> It is possible to see country outlines in some comparisons with either
>> max or min
>> temperatures. Corrections for elevation are needed to get over large
>> elevational differences
>> between stations and the model, but the Alps are still visible. Lapse
>> rates work best only
>> in some seasons - not very good in summer. Max temps produce consistent
>> difference maps
>> (model-obs) over Europe, but mins are more erratic/random. Min error is
>> overall small but
>> with a large variability while max has a larger error but low
>> variability. Due to mins being more
>> affected by local environment.
>>
>> At 09:13 27/10/02 -0700, Tom Wigley wrote:
>>>Phil,
>>>
>>>Re my last email
>>>
>>>I have looked at the data you sent. It would be very nice to have a
>>>gapless 1981-2000 T climatology to match the Xie/Arkin precip
>>>climatology. However, this means somehow filling in the gaps in the
>>>61-90 minus 81-00 differences, a nontrivial task. So my choice in the
>>>absence of this is either a gappy 81-00, or a full 61-90. I have chosen
>>>the latter -- perhaps we can discuss how to produce a gapless 81-00
>>>climatology when I am at CRU?
>>>
>>>A problem with the 61-90 is that it is surface, and that observed
>>>surface is not equal to model surface. I'm sure you have thought about
>>>this (in the model validation context) already, so this is another item
>>>to discuss.
>>>
>>>For precip, I also have the inter-annual S.D. climatology, so I can
>>>validate both the mean climate and the variability. Very interesting. It
>>>would be nice to be able to do this with temperature (especially since
>>>the mean climate for temperature in the models is pretty darn good --
>>>but how good is variability?) Is there an S.D. climatology for
>>>temperature that you can send me?

>>>
>>>Cheers, Tom.
>>
>> Prof. Phil Jones
>> Climatic Research Unit Telephone +44 (0) 1603 592090
>> School of Environmental Sciences Fax +44 (0) 1603 507784
>> University of East Anglia
>> Norwich Email p.jones@uea.ac.uk
>> NR4 7TJ
>> UK

> -----
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>> -----
>> Name: newabsref8100.out
>> newabsref8100.out Type: Plain Text (text/plain)
>> Encoding: base64

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NR4 7TJ
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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: k.briffa@uea.ac.uk
Subject: Fwd: Re: paleo data
Date: Fri, 01 Nov 2002 15:28:05 +0000

X-Sender: hegerl@mail-he.acpub.duke.edu

Date: Fri, 1 Nov 2002 09:56:45 -0500

To: Phil Jones <p.jones@uea.ac.uk>

From: Gabi Hegerl <hegerl@duke.edu>

Subject: Re: paleo data

No worries, I can wait till next week!

It would be great to hear from you next week particularly if you feel I have overlooked something, I am planning to submit a little GRL paper on the detection results based on paleodata soon, and so a warning if I am doing something wrong would be great.

Its not surprising that the detection results are stable, since other than volcanic forcing is mainly driven by the low-f component anyway.

But it looks to me like the volcanic response is not smaller or even a bit larger in the annual JGR data (except for one real real big peak in the 1998 data).

Greetings, have a good weekend and good luck for Keith's back
Gabi

Gabi,

I have printed the files, but I do not know the answer. Keith is off today with a bad back -

seeing a chiropractor. I need to talk to him before we can reply. I will be away Mon/Tues

next week, so we will not be able to reply until later next week.

Cheers

Phil

At 11:27 31/10/02 -0500, Gabi Hegerl wrote:

Dear Keith and Phil,

I checked and found that we did indeed use the JGR 2001 data (by reloading them from your JGR data file). I also got the

1998 data from the volcano paper, and did some checking. My detection results appear quite unimpressed by if I filter the 2001 data to focus on lower frequencies or not (the estimated amplitudes of solar, volcanic and ghg signals are virtually identical, volcanism gets a bit tougher to detect if you remove the high-frequency component).

Then I redid the Epoch analysis comparing the

response of your data old and new to volcanism, and find somewhat bigger volcanic signals on average (using 50 eruptions between 1400 and 1940) in the JGR paper record. I high-passed both datasets and get somewhat more variability in the JGR record, not the 1998 record.

I am wondering is there something I am overlooking?

I append a figure of the high-passed (var > ca 10 yrs removed) records, and the volcanic response in both datasets (averaging years 1-20 after the eruption, and removing the best-estimate solar and ghg signal before the analysis).

The analysis omits years with another volcanic eruption within the 20 yrs.

I also append one version of the figure where the upper 95%ile of the ghg signal (which appears underestimated in Briffa 98 data) is removed rather than the best estimate, in that case, the volcanic signals in both data appear nearly identical.

Greetings, and please let me know if I am doing something wrong with your data!

Also, what is the best reference to a discussion on the difference between both datasets?

Thanks in advance

Gabi

Dear Tom

after a little detective work we have deduced that the data sent to you constitute a version of Northern Hemisphere Land temperatures (april- sept) produced by PCA regression using regional average density chronologies (ie the JGR paper you refereed I believe). It is true that high frequency component is not in my opinion optimal in describing the relative magnitude of extreme inter-annual extremes. This is to do with the unpredictable weighting ascribed to certain areas (tree-density series) in the averaging of the original raw data (this is boring and I won't go into it unless you really want me to). The relative differences in year-to-year values are likely better represented in the N.Hemisphere series produced by averaging regional series produced using a different approach in which the initial data are high-pass filtered and then merged in a more straight forward way. This is more equivalent to the series on volcanic signals described in our Nature paper, though the low-frequency component in this series is definitely not represented. There is another series , that one could consider a good compromise . That is a composite of the Age-Banding approach (JGR) low-frequency variance added to the earlier (Nature) high-frequency component. We did this for Figure 6 in the JGR paper , but did not provide the data on our web site I now realize. However this composite series is VERY highly correlated with the "better" high frequency data - see the correlations (Table 1 and related text in

[1]<http://www.cru.uea.ac.uk/cru/people/briffa/jgr2001/Briffa2001.pdf>

There are many possible ways of producing a "Northern Hemisphere" average , involving different prior regionalisation and secondary weighting (in space and through time) of the constituent series) . Non can be considered "correct". If you would like us to dig out the composite series or discuss specific aspects of the logic or uncertainties

associated with the different large averages let me know. Perhaps it would be better to discuss this on the phone? As for longer series, we can provide the 2000 year N.Eurasian data (a composite of ring width chronologies in N.Sweden, The Yamal peninsula, and Taimyr). I will soon be able to provide a 4000-year version, that is now being worked on.

or a similar Northern tree-ring chronology incorporating more data eg see

[2]<http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/>

We do not have the bristlecone data - but they are available I presume from the International Tree-Ring Data bank, part of the NGDC holdings?

At 02:29 PM 10/1/02 +0100, Phil Jones wrote:

Tom,

Been away and going again tomorrow. Had a chat with Keith and Tim and one of them will send a reply and data later this week.

Cheers

Phil

At 11:28 26/09/02 -0400, Tom Crowley wrote:

Hi Phil,

thanks for all your help on the bams paper

DOE is being exceedingly slow in processing the paperwork for our new round - I will keep you posted.

I am also wondering whether we can get some data from you:

Gabi is comparing our 2d ebm run with the briffa et al 2001 jgr time series in order to compare the model prediction of - I think you mentioned at one point something to the effect that, although this series is good for estimating low resolution temperature variability, it may dampen high frequency variability. if my memory is correct in this case, would you please send gabi the record you consider best for comparing with the model predicted interannual response to volcanic eruptions?

on another matter we are extending our runs back in time - I have now compiled a record of global volcanism back to 4000 BP for both hemispheres - extended back to 8000 BP for 30-90N. we are therefore trying to compile paleo records older than AD 1000 to at least get some reconstruction we can compare with.

I seem to recall that Keith or you may have published some longer reconstruction but cannot recall where it is? if so, would you be so kind as to send it to me? also I am trying to find a long record from the eastern California for the bristlecone pine - for some reason I am having difficulty finding one. if you have a long record - even going back beyond 2000 BP, it would be very much appreciated.

thanks for any help you can give us on this and best wishes, Tom

--

Thomas J. Crowley

Nicholas Professor of Earth Systems Science

Dept. of Earth and Ocean Sciences

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--

~~~~~  
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email: hegerl@duke.edu, [4]<http://www.env.duke.edu/faculty/bios/hegerl.html>

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~~~~~  
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References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/jgr2001/Briffa2001.pdf>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>
4. <http://www.env.duke.edu/faculty/bios/hegerl.html>
5. <http://www.env.duke.edu/faculty/bios/hegerl.html>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Leonid Polyak <polyak.1@osu.edu>
Subject: Re: Polar Urals data
Date: Wed Nov 6 08:58:06 2002

The delay again is simply because I was away for 2 days. Attached are the data you want. First number is number of years of record, followed by (in first column) year A.D. and (in second column) the numbers you want . Ignore other columns. Cheers

Keith

At 02:58 PM 11/5/02 -0500, you wrote:

Keith,

To keep you informed about the use of your Salekhard data, I attach the MS which I'm submitting to The Holocene. I've referred to your papers of 1995 and 2000. If you'd like me to add more acknowledgement of your data, let me know and I'll gladly do that.

Sincerely,

Leonid

Leonid Polyak

Byrd Polar Research Center

Ohio State University

1090 Carmack Rd., Columbus, OH 43210

614-292-2602, fax 614-292-4697

[1]<http://polarmet.mps.ohio-state.edu/GeologyGroup/polyak.htm>

>Leonid

>see [2]<http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/>

>The data (and other possibly interesting data are available there) .

>Best wishes

>Keith

--

Professor Keith Briffa,
Climatic Research Unit
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Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784

[3][http://www.cru.uea.ac.uk/cru/people/briffa\[4\]/](http://www.cru.uea.ac.uk/cru/people/briffa[4]/)

References

1. <http://polarmet.mps.ohio-state.edu/GeologyGroup/polyak.htm>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/qsr1999/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>
4. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Ronald M. Lanner" <pinetree30@EARTHLINK.NET>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: The Great Controversy
Date: Wed, 13 Nov 2002 21:36:16 -00
Reply-to: grissino@UTKUX.UTCC.UTK.EDU

Dear Forumites -- Since I am neither a dendrochronologist nor a tree physiologist, I have a different take on this little brushfire we have going. Ideally, tree phys people should be producing information (among other things) that dendrochronologists find useful. And dendrochronologists should use the information within its limits and with enough understanding to get it right. I don't think either of those things is occurring with as much frequency as we would all like. I can understand Rod's annoyance at the massaging of numerical data that dendrochronologists do. I am basically a non-mathematical biologist mystified by such stuff, and I prefer handling measurements to deriving indices, or whatever. When I run up against such derived data, I generally turn skeptical, because I cannot verify the results from my own experience or intuition. On the other hand, when I read papers by cambial physiologists like Rod I also get annoyed. That's because my biology wants to integrate upwards, and all I get from cambial labs is biochemistry. So I'm in the middle, where it gets lonely. I try not to get mad at anybody, though I do wish I didn't find myself alone on the margins.

I find it frustrating that some dendrochronologists stubbornly see tree ring characteristics as being affected by climate. They are not. They are affected by cambial activity. Cambial activity is affected by internalities of tree behavior, mainly hormonal and nutrient fluxes in the crown. Those things are largely influenced by climatic factors. So there is quite a bit of slack between the climatic factor and the ring characteristic. Is this just negligible static? I doubt it. I see this as an oversight by dendrochronologists that weakens their credibility a tad among those knowledgeable about tree growth. I also have a quarrel with the dogma of dendrochology that the cambium changes as the tree becomes senescent. I know of no data that trees senesce -- that is, that they undergo changes due solely to aging. This started as forestry dogma, and was accepted by tree-ringers, who then corrected for it. I'm practically the only one who has systematically looked for evidence of senescence (with a Ph.D. student), and we could not find any in young to ancient bristlecones. But tree physiologists do not generally look at such issues because they have become progressively more reductionist. Nor do they try to produce a theory of tree growth based, as it must be, on evolutionary theory. Such a theory would be simple and general, and it would allow tree-ringers to approach rings with more sympathy and understanding. That might not get you further, but it would improve your character, I'm certain. And it would put all that assorted mishmash of tree phys data that have accumulated since 19th century Germany into a context at last, and maybe liberate the minds of all those tense physiologists out there with their ever-increasing inventories of electronic sensors and analyzers. The world would be a better place with more people having

fun in the woods.

---Ronald M. Lanner

--- [1]pinetree30@earthlink.net

--- EarthLink: It's your Internet.

References

1. <mailto:pinetree30@earthlink.net>

From: John Ogden <j.ogden@AUCKLAND.AC.NZ>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Re: Fwd: History and trees
Date: Fri, 15 Nov 2002 16:15:25 +1300
Reply-to: grissino@UTKUX.UTCC.UTK.EDU

Dear Professor Savidge, Hal Fritts's comments were, as always, to the point and gracious. I have much less patience with your ignorance and arrogance. The sampling and statistical procedures involved in the production of a cross-dated chronology are of course quite different to those used in a randomised experiment, but they are none-the-less logical, rigorous, science. We have been through all those arguments so many times - you are wasting everyone's time.
John Ogden.

On Wed, 13 Nov 2002 13:16:20 -0700 "Harold C. Fritts" <hfritts@LTRR.ARIZONA.EDU> wrote:

> Dear Ron,
> I respectfully disagree with you. We have reached out to you many times
> and find little but judgmental response. I have worked with this group
> for many years now and they are just as exact scientists as you. They
> are interested in what the tree tells us about the earth and its history
> and not as interested and experienced as you in how the tree works. I
> agree with you to the extent that we must understand how the tree works
> but I fear you have "created the reality that dendrochronologists are
> stupid and beneath your greatness" and that it will not ever change.
>
> People like you in the past such as Waldo Glock and Sampson at Berkley,
> CA made similar statements. When I was a young man, I set out trying to
> examine their criticism objectively with both physiological
> investigations and statistical analysis. I found that these criticisms
> could be met with data from solid physiological tests and even though
> those practicing the science at that time were astronomers, not
> physiologists. There are talented and insightful people in other
> sciences outside of plant physiology.
>
> I am sorry for all of our sakes. as the future holds many possibilities
> with many experts contributing to the future of science. If you could
> only get outside the judgmental ideas that you hold about us, I think
> you might be very surprised and pleased.

>
> Yes, I think many in this group oversimplify the response of the tree,
> but in the same way you oversimplify the practice of dendrochronology.
> We all have much to learn from each other, but calling each other names
> doesn't further anyone's science.

>
> I believe science is embarking on a course of greater cooperation among
> different disciplines. This implies respect and cooperation in both
> directions. We welcome your interest in dendrochronology but are
> saddened that you have so little respect for our integrity and honesty.
> It would be more appreciated if we could together work for a better
> future, not just quarrel, call each other names and delve on what is
> wrong with the past.

>
> Sincerely, Regretfully and Lovingly,
> Hal Fritts

>
> P.S.

> One other comment to my fellow scientists. I agree with Frank that I
> have made only a start at understanding the basis for tree ring
> formation. It will take much more work in physiology and modeling. In
> current discussions and debates on the importance of physiology and
> process modeling in dendrochronology, understanding plant processes
> often takes secondary impotence in the eyes of many
> dendrochronologists. I think this will change because I believe in the
> integrity of my colleagues, but I sometimes wonder how long this will
> take. I had at one time hoped that I might see it happen. We can
> answer such criticism, but not until we investigate further how the tree
> responds to its environment and how the tree lays down layers of cells
> we call the tree ring. Physiologists outside dendrochronology have
> little inclination to do it for us as this message reveals. We can and
> must do it ourselves by including, welcoming and funding physiological
> investigation in tree-ring research.

> HCF

>
>
> Rod Savidge wrote:

>>
>> To the Editor, New York Times

>>
> Indeed, its activities
>> include subjective interpretations of what does and what does not
>> constitute an annual ring, statistical manipulation of data to fulfill

>> subjective expectations, and discarding of perfectly good data sets when
>> they contradict other data sets that have already been accepted. Such
>> massaging of data cannot by any stretch of the imagination be considered
>> science; it merely demonstrates a total lack of rigor attending so-called
>> dendrochronology "research".

>>
>> I would add that it is the exceptionally rare dendrochronologist who has
>> ever shown any inclination to understand the fundamental biology of wood
>> formation, either as regulated intrinsically or influenced by extrinsic
>> factors. The science of tree physiology will readily admit that our
>> understanding of how trees make wood remains at quite a rudimentary state
>> (despite several centuries of research). On the other hand, there are many
>> hundreds, if not thousands, of publications by dendrochronologists
>> implicitly claiming that they do understand the biology of wood formation,
>> as they have used their data to imagine when past regimes of water,
>> temperature, pollutants, CO2, soil nutrients, and so forth existed. Note
>> that all of the counts and measurements on tree rings in the world cannot
>> substantiate anything unequivocally; they are merely observations. It
>> would be a major step forward if dendrochronology could embrace the
>> scientific method.

>>
>> sincerely,
>> RA Savidge, PhD
>> Professor, Tree Physiology/Biochemistry
>> Forestry & Environmental Management
>> University of New Brunswick
>> Fredericton, NB E3B 6C2

>>
>>>X-Sieve: cmu-sieve 2.0
>>>X-Mailer: Microsoft Outlook, Build 10.0.4024
>>>Importance: Normal
>>>Date: Tue, 12 Nov 2002 23:24:03 -0500
>>>Reply-To: grissino@UTKUX.UTCC.UTK.EDU
>>>Sender: ITRDB Dendrochronology Forum <ITRDBFOR@LISTSERV.ARIZONA.EDU>
>>>From: "David M. Lawrence" <dave@FUZZO.COM>
>>>Subject: History and trees
>>>Comments: To: scitimes@nytimes.com
>>>To: ITRDBFOR@LISTSERV.ARIZONA.EDU
>>>

>>>I was rather horrified by the inaccurate statements about tree-ring
>>>dating that you allowed to slip into print in the interview with Thomas
>>>Pakenham today. Tree-ring science is an exact science -- none of the
>>>data obtained from tree rings would be useful if the dates were

>>>inaccurate. Dendrochronologists don't say much these days about how old
>>>trees are because they are interested in more important questions --
>>>such as "What can the tree rings tell us about our planet's past?"
>>>
>>>You at The New York Times should know something about tree rings. A
>>>check on Lexis-Nexis shows that since 1980 you have run more than 100
>>>stories in which the words "tree rings" appear in full text. Some of
>>>the stories are irrelevant. But most are not, such as the July 13,
>>>2002, story in which you misspell the name of Neil Pederson at
>>>Lamont-Doherty Earth Observatory, or the March 26, 2002, story about a
>>>medieval climate warming detected in tree-ring data. I do not remember
>>>tree-ring dating being labeled an "inexact" science in stories like
>>>that.
>>>
>>>Did Walter Sullivan, who wrote a story about tree rings and drought on
>>>September 2, 1980, ever question the "exact" nature of tree-ring dating?
>>>He didn't seem to question it on June 7, 1994, when he wrote a story
>>>about ash from Santorini and said that the ash cloud may have "persisted
>>>long enough to stunt the growth of oak trees in Irish bogs and of
>>>bristlecone pines in the White Mountains of California, producing
>>>tightly packed tree rings." You really do have to know when those rings
>>>were laid down before you can associate them with a specific volcanic
>>>eruption.
>>>
>>>I tell you what. I am a member of the National Association of Science
>>>Writers as well as a working dendrochronologist and occasionally paid-up
>>>member of the Tree-Ring Society. If you feel the need for a refresher
>>>course on tree-ring dating, I'll be more than happy to try to introduce
>>>you to knowledgeable practioners in you neighborhood, such as Neil
>>>Pederson (not Peterson) at Lamont-Doherty Earth Observatory. (It's
>>>actually a local phone call for youse guys.)
>>>
>>>Sincerely,
>>>
>>>Dave Lawrence
>>>
>>>-----
>>> David M. Lawrence | Home: (804) 559-9786
>>> 7471 Brook Way Court | Fax: (804) 559-9787
>>> Mechanicsville, VA 23111 | Email: dave@fuzzo.com
>>> USA | http: http://fuzzo.com
>>>-----
>>>

>>>"We have met the enemy and he is us." -- Pogo

>>>

>>>"No trespassing

>>> 4/17 of a haiku" -- Richard Brautigan

>

> --

> Harold C. Fritts, Professor Emeritus, Lab. of Tree-Ring Research

> University of Arizona/ Owner of DendroPower

> 5703 N. Lady Lane, Tucson, AZ 85704-3905

> Ph Voice: (520) 887 7291

> <http://www.ltrr.arizona.edu/~hal>

John Ogden

j.ogden@auckland.ac.nz

From: Ben Santer <santer1@llnl.gov>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: CRU strategic review
Date: Tue, 19 Nov 2002 10:19:25 -0800

Dear Tim,

I'm really sorry I've been so slow in responding to your request for input to the CRU strategic review. Life has been rather hectic over the past few months. I hope to send you my response to your questionnaire by no later than the end of this month. Would that still be o.k?

Cheers,

Ben

=====

Tim Osborn wrote:

>
> Dear Ben,
>
> I've not had time to speak with Phil recently, so I don't know how things
> are with you at the moment, work-wise and home-wise. But I hope all is
> well. The (rather formal, sorry) message below is a follow-up to a
> letter/questionnaire that I sent in the summer. It would certainly be good
> to obtain your input, so if you have time...!
>
> Cheers
>
> Tim
>
> -----
> Dear Dr. Santer
>
> I wrote to you in the summer in my role as leader of the Climatic Research
> Unit's (CRU) strategic review team, as part of an exercise to obtain
> external input to our review process. This exercise was reasonably
> successful, with a 45% response rate. Despite this response rate, there
> are still some gaps in the "categories" that we hoped to obtain input
> from. We have analysed the responses, together with our own internal
> assessments, and are now looking to fill in some of the remaining gaps.
>
> I am contacting you again in the hope that you might be able to assist us

> in our review process, via the attached questionnaire. As stated in my
> original letter, we are aware that this process is primarily for our
> benefit, rather than yours, so we greatly appreciate any time that you
> could spend in assisting our review.
>
> Some respondents said that they would prefer to have received an electronic
> version of the questionnaire, and so I have decided to attach a Microsoft
> Word document containing the questionnaire that I sent to you in the summer.
>
> If you have any questions about the review process, or would prefer to
> provide your opinions over the telephone, then please phone me on 01603
> 592089. We will be grateful for whatever level of input you feel able to
> provide.

>
> Best regards

>
> Tim

>
> [Dr. Tim Osborn, Chair of Strategic Review Team]

>
> -----

> Name: questions for Santer.doc
> questions for Santer.doc Type: Microsoft Word Document (application/msword)
> Encoding: base64
>
> Part 1.3Type: Plain Text (text/plain)

--

PCMDI HAS MOVED TO A NEW BUILDING. NOTE CHANGE OF MAIL CODE!

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
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From: "L.B. Klyashtorin" <klyashtorin@mtu-net.ru>
To: "Keith Briffa" <k.briffa@uea.ac.uk>
Subject: Re: Fw: Fw: Reconstruction etc.
Date: Sat, 23 Nov 2002 00:01:30 +0300

Dear Keith,

Do not be embarrassed. This situation is very humorous and I am very glad to smile. It happens.

Thank you very much for your time series.

I would like to analyse spectra characteristics of summer temperatures (your series) and winter temperature series using Dansgaard's time series for the same period (since 550s). It seems to me the temperature data of Arctic basin is the most pronounced indices illustrating of long term climate oscillations.

Best wishes

Leonid

----- Original Message -----

From: [1]Keith Briffa

To: [2]L.B. Klyashtorin

Sent: Monday, November 18, 2002 11:01 PM

Subject: Re: Fw: Fw: Reconstruction etc.

I am very embarrassed as I have just realized I sent the data (a couple of weeks ago at least !) to the wrong person (someone called Leonid Polyak) by mistake. He wanted polar Urals data. I now attach the file with the Nature temperature reconstruction.

First number is the number of values , then subsequent lines contain the date in the first column (years AD) and the anomalies in the second (as described in the paper).

Sorry!!!!!!!!!!!!!!!!!!!!

Keith

At 10:45 PM 11/18/02 +0300, you wrote:

Dear Keith,

I apologise for persistens but I really need in the time series I requested from you and I will very grateful to you for these materials which you so kind promised send to me .

I hope receive it from you yet, although I have not reply from you to my two last messages.

Yours sincerely

Leonid Klyashtorin

----- Original Message -----

From: [3]L.B. Klyashtorin

To: [4]Keith Briffa

Sent: Sunday, October 27, 2002 1:45 PM

Subject: Re: Fw: Reconstruction etc.

Dear Keith,

I apologize for disturbing you but I did not received the data you promised to send me yet.

I would be very grateful to you for these time series.

Using your kind permission (from October 22) to remind you if these date do not arrive I hope to receive it from you....

Sorry for inconveniences and thank you in advance

Leonid

----- Original Message -----

From: [5]Keith Briffa
To: [6]L.B. Klyashtorin
Sent: Tuesday, October 22, 2002 5:08 PM
Subject: Re: Fw: Reconstruction etc.
Leonid

Sorry not to respond

I will search out the tree-ring series (ring width and density) and the numbers for the reconstruction and send them as soon as I can get to it. Remind me in a couple of days if they do not arrive. Cheers

Keith

At 02:17 PM 10/22/02 +0400, you wrote:

Dear Dr Briffa,

Unfortunately I did not receive reply on my first message sent to your address by October 8.

I apologize for disturbing you again but I will be very grateful to you for sending me the address of web site where I can find the data of tree ring reconstruction of the summer temperature.

I also very interested in receiving data published in one of your et al. old paper: "A 1400 year tree ring record of summer temperature in Fennoscandia,1990, Nature.vol 346, 2 August 1990."

The time series of Pinus silvestris published at Fig 2 a is very interesting for my work on the dynamics climate-linked fisheries of Northern Hemisphere.

I would be very grateful to you for your reply.

Best regards
Leonid Klyashtorin

----- Original Message -----

From: [7]L.B. Klyashtorin
To: [8]Briffa Keith R.
Sent: Tuesday, October 08, 2002 4:58 PM
Subject: Fw: Reconstruction etc.

I am Leonid Klyashtorin from Federal Institute for Fisheries and Oceanography (VNIRO),Moscow,Russia.

The last 6 monthes I was National Research Council Senior

Associate and worked as Visiting Scientist in the Pacific Fisheries Environmental Laboratory (PFEL), NOAA, National Marine Fisheries Service, Monterey, CA on the item "Climate and Fisheries".

My paper "Climate change and long-term fluctuations of commercial catches: the possibility of forecasting" published recently as a separate brochure, FAO Fisheries Technical Paper No 410,

pp 86, 2001, and is rather popular among fisheries specialists.

It gives insight of world major fisheries dynamics and contains forecast to the next 10-20 years. (The Abstract is attached, PDF file of all paper also is available)

I have read of your and T. Osborn very interesting and so useful paper "Blowing Hot and Cold.." in Science, v.295.,2002.

Your results clearly shows that main conception of IPCC experts about unicity of Global Warming events in 20-century is erroneous and now the additional data appear on the natural long term cyclic climate change at least for the last 2000 years.

My work on the "Climate - Fisheries" connected with questions of Climate Change and, naturely, touches of Global Warming Problem.

Me and my colleague from Institute of Physics of the Earth of Russian Academie of Science recently submitted our paper "On the coherence between dynamics of the world fuel consumption and global temperature anomaly". in the International Journal "Natural Hazards".

The paper is now under reviewing. (The Abstract is attached.)

Now me and a few my colleagues from US are in process of writing book dedicated of Climate- Fisheries problem and we would like use the data on the tree-rings analysis showing cyclic character of long-term climate changes.

I will be very grateful to you for receiving from you (if possible) the time series of annual reconstructed temperature anomaly from Figure (Esper02) and address of website, where these data are available.

Thank you in advance

Best regards
Leonid Klyashtorin

--

Professor Keith Briffa,
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--

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References

1. <mailto:k.briffa@uea.ac.uk>
2. <mailto:klyashtorin@mtu-net.ru>
3. <mailto:klyashtorin@mtu-net.ru>
4. <mailto:k.briffa@uea.ac.uk>
5. <mailto:k.briffa@uea.ac.uk>
6. <mailto:klyashtorin@mtu-net.ru>
7. <mailto:klyashtorin@mtu-net.ru>
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9. <http://www.cru.uea.ac.uk/cru/people/briffa/>

10. <http://www.cru.uea.ac.uk/cru/people/briffa/>

11. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Clare Goodess <C.Goodess@uea.ac.uk>

To: j.palutikof@uea.ac.uk,p.jones@uea.ac.uk,d.viner@uea.ac.uk, k.briffa@uea.ac.uk

Subject: UK Research Office - FP6 Proposal Writing for Researchers

Date: Tue, 26 Nov 2002 18:34:49 +0000

Cc: j.darch@uea.ac.uk

Dear all

I went to this meeting in London yesterday - which was useful. Julie will photocopy my notes/the overheads for you some time this week (if she doesnt have time, I'll do it when I get back next week). In the meantime, here are my main impressions/thoughts from the meeting. (Incidentally, Alex Haxeltine was due to go from UEA, but didnt turn up. Not sure who the other UEA people were! There was no list of participants.) Maybe we should get together (next week some time?) once you've had chance to look at some of this.

The Commission (EC) seems to be favouring smaller projects, e.g., typically 10 million Euro. Though it is up to proposers to define the necessary 'critical mass'.

UKRO seem quite wary of Networks of Excellence (NoE), e.g., warning of potential conflicts of interest with institutions. As with projects, smaller size seems to be in favour. An UKRO analysis suggests an NoE of 150-400 researchers would maximise the amount of money received per researcher.

Research activities can now be funded in NoE (the EC has changed its mind on this in the last month), but only if focused on integration.

The EC wont be proposing indicators of integration for NoE - the proposals should explain how this will be 'measured'.

Consortium quality seems to be an important concern for the EC, i.e., having the right people for the job and ensuring everyone has a clear role. In our rush to get a 'critical mass', I'm concerned that the GENIE consortium may appear too much as 'all our friends'. One possible strategy which UKRO seemed to think quite good for people, would be to put in a proposal from 6-8 key partners, indicating for which activities additional partners will be brought in at appropriate points. The EC will be providing formal procedures for these 'internal project' calls.

It is unlikely that the new online proposal preparation tool will be ready for the first call, but electronic submission (on CD) should be possible. Any paper submissions will be scanned.

Evaluation will be by electronic means initially, with possibility of proposers (and evaluators?) being invited to hearings in Brussels prior to panel meetings.

No signatures are required for the proposals (though a password/username will be required by co-ordinators to access the online system). Some institutions/consortia are apparently drawing up pre-consortia agreements or letters of intent/memorandum of understanding.

The guide for proposers is currently only in very rough draft.

There will be a second 'EOI' type exercise at the end of 2003/early 2004. This could lead to changes in the indicative themes for 2004.

UKRO is not keen on UK institutions using consultants for project management - we should be

building our own capacity.

Proposals should be written for the informed lay person. It is best if they are not obviously written by one person - better to show joint effort/co-ordination at an early stage.

Redundancy costs (i.e., costs of implementing the new fixed-term regulations) can be included for research staff.

The EC aims to audit all FP6 projects (because there will be fewer of them).

Recognition of the ERA and policy links will be important for the EC. (The ERA includes references to developing long-term careers for research staff and increasing the involvement of women - so maybe we should be thinking of some activities to address these issues.)

IPR will be an important issue in FP6 - need to get expert advice (e.g., what happens if consortium changes over course of project).

Consortium agreements will be compulsory.

The proposal forms (for IPs anyway) are relatively simple, e.g., only need to cost four different types of activity.

Clare

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Web: [1]<http://www.cru.uea.ac.uk/>

Editor "Climate Research" ([2]<http://www.int-res.com/journals/cr/>)

Southern Africa crisis appeal: [3]<http://dec.londonweb.net/appeal/>

References

1. <http://www.cru.uea.ac.uk/>
2. <http://www.int-res.com/journals/cr/>
3. <http://dec.londonweb.net/appeal/>

From: Eystein Jansen <Jansen@geol.uib.no>
To: Laurent Labeyrie <Laurent.Labeyrie@lsce.cnrs-gif.fr>, Keith Alverson <keith.alverson@pages.unibe.ch>, Keith Briffa <k.briffa@uea.ac.uk>, Rick Battarbee <r.battarbee@geog.ucl.ac.uk>, didier.paillard@lsce.cnrs-gif.fr, Dominique Raynaud <domraynaud@glaciog.obs.ujf-grenoble.fr>, jean jouzel <jouzel@lsce.saclay cea.fr>, Chappellaz Jerome <jerome@glaciog.obs.ujf-grenoble.fr>, Gerald Ganssen <gang@geo.vu.nl>, Jean Marc Barnola <barnola@glaciog.obs.ujf-grenoble.fr>, Ralph Schneider <rschneid@uni-bremen.de>
Subject: FP6 - NoE Dynamics of Climate Changes (DOCC)
Date: Mon, 2 Dec 2002 10:17:31 +0100
Cc: martin.miles@geol.uib.no, b.balino@uib.no

<x-flowed>
Dear friends,

I assume many of you have followed the development of the work programme for FP6, which have been quite dramatic at times for our field. The end result is not particularly good, and the whole area of Global Change has been cut by comparuison with FP5. I talked with Anver Ghazi last week, and what I know stems from this and from the Nov. 18 version of the work programme. The will be no opening for climate dynamics in the first call (Dec. 17). The second call due in June /July with a deadline in October 2003 will include some paleoclimate openings:

- STREPS for novel paleoreconstructions methods (i.e. a few of the normal projects of previous FPs) - but remember: 75% of funding goes to New Instruments: Integrated Projects and NoEs).
- Hot spots in the climate system, including the thermohaline circulation and the Arctic.

Brussels will not issue anything now about the thrird call, but according to Ghazi they plan to invite for either an NoE or an IP in climate dynamics with emphasis on past climate change at that point. Call will be in 2004. But things can change with this call. Thus we have quite some time to discuss if we shall go forward with DOCC or go for IP. The overall size of the IPs have been substantially reduced, so if we try an IP or an NoE either will need to be more focussed in terms of science and in terms of partnership than our Expression of interest.

Ceers,

Eystein
--

Eystein Jansen
Professor/Director
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Dep. of Geology, Univ. of Bergen
All@gaten 55
N-5007 Bergen
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The Bjerknes Training site offers 3-12 months fellowships to PhD students
More info at: www.bjerknes.uib.no/mcts

</x-flowed>

From: "Andy McLeod" <Andy.McLeod@ed.ac.uk>
To: "Mike Hulme" <m.hulme@uea.ac.uk>, <H.J.Schellnhuber@uea.ac.uk>
Subject: Climate Change Funding in Scotland
Date: Mon, 2 Dec 2002 15:09:24 -0000

Dear John and Mike

It was over two years ago that we first briefly discussed the opportunity to develop climate change research funding in Scotland using a grant to HEI's from the Scottish Higher Education Funding Council (SHEFC). My Centre, CECS, has been successful with such grants in the past. Last year there were no such grants but the opportunity has now arisen again. The funding is quite large (0.5 - 1.5 million over up to 4 years). With support from the three main agencies in Scotland I am keen to develop such a research proposal and will be entering the internal competition (within the University) shortly.

I am keen to develop a strong link/cooperation with the Tyndall Centre and I would like to explore ways in which this might be achieved. Last week I believe that you were busy with your Advisory Board. I would be very keen to talk with you on the phone about this as soon as possible. Please let me know if there is a suitable time when I might phone or feel free to contact me.

Best wishes

Andy

E-mail from:

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From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, Scott Rutherford <srutherford@gso.uri.edu>
Subject: Re: RegEM manuscript
Date: Mon, 06 Jan 2003 09:13:24 -0500
Cc: k.briffa@uea.ac.uk, Phil Jones <p.jones@uea.ac.uk>, Ray Bradley <rbradley@geo.umass.edu>, mhughes@ltrr.arizona.edu, mann@virginia.edu

Thanks very much Tim,

Your comments are extremely helpful.

I'm open to eliminating the comparison w/ Esper et al --but lets see if there is a consensus of the group as to what to do here. We're anxiously awaiting comments from the others...

thanks again,

mike

p.s. Scott can be reached at either U.Va or U.RI email equally well (I believe the former is forwarded to the latter)..

At 12:16 PM 1/6/2003 +0000, Tim Osborn wrote:

Dear Scott and Mike,

Over the Christmas break I (finally!) had time to read the RegEM manuscript in detail. Phil had already read and annotated a copy - so I've added my annotations to that and will mail it to you today. Mike asked for comments to go to Scott, so please tell me which address I should use (Rhode Island or Virginia?).

I spoke to Keith and he has partly read it too, and will provide separate comments soon.

Overall, I think the paper is a very nice piece of work and I'm pleased to be involved with it. The results regarding robustness with respect to proxy data, method, region and season are definitely good to publish.

Among the many comments annotated on the manuscript, a few are repeated here so that all authors may respond if they wish:

(1) Given the overwhelming number of values in the Tables, I suggest halving them by dropping all the CE values (keeping just RE values). As the paper points out, getting the verification period mean right is rewarded by RE but not by CE. Since we are interested in changes in the mean, I don't think that's a problem. CE is fine in addition, but dropping it would provide benefits of reducing manuscript size - and especially the size of the tables.

(2) The "mixed-hybrid" approach sounds dubious to me - more justification/explanation of why it is needed (and hence why it captures more variance than the simpler splitting into high- and low-frequency components method).

(3) It is not clear to me that the paragraph and figure on the comparison with Esper et al. are either correct or necessary. They also are problematic because it would appear that we (Briffa & Osborn) were contradicting our earlier paper when in fact we aren't. The paper is already long and to remove these parts would therefore be helpful anyway. The comparison with Esper et al. is important - but much better dealt with in a separate paper where it could be developed in more detail and with more room to explain the approach and its implications.

(4) I still hope to write up some more detailed comparisons of the reconstructions using just the MXD data but different methods and will let Mike/Scott know my plans on this soon.

Happy new year to you all.

Tim

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Mike Hulme <m.hulme@uea.ac.uk>
To: Timothy Carter <tim.carter@ymparisto.fi>, t.mitchell@uea.ac.uk
Subject: Re: Pattern scaling document for the TG CIA
Date: Sat, 18 Jan 2003 21:05:49 +0000

<x-flowed>

Tim,

As promised some comments on the paper.

General: It is very good, just what is needed and puts the last 4 years of debate into the right context.

General: why consistently 'climate changes' rather than the more usual 'climate change'?

Abstract, line 10: why only quote as high as 0.99 and not the lowest correlation (which actually is more to the point - it is still very good after the 2020s, even for precip).

Abstract, lines 12-13: as worded this does not quite follow, although I see from later that the ellipses used are at 95% confidence. Just because they fall outside natural variability does not *in itself* prove they are stat. sig.

p.2, lines 17-19 (and also several places on p.4): impacts are mentioned, but nothing said about adaptation. It is really adaptation actions/decisions that are crucial, impacts are only one way to get there. Alter the focus.

p.2, line -10: add 'necessarily' between 'not' and 'be'. AOGCMs may actually do not so bad a job on occasions about climate change (relative changes for example), so don't completely dismiss this one.

p.5, section 2: general point: there is no list or table or statement about exactly what these 17 experiments are. The models are listed, but not the experiments. e.g. which SRES scenarios did which modelling group and how many ensembles? For the lay person this is not obvious.

p.7, top line: you should perhaps make the point that simple bias indices such as these may partly be explained by elevation offsets (model height vs. real height). It is to my mind a mitigating factor than can work in a model's favour (not always). It should be mentioned, because the biases

may not be due to just dumb models, but due to simple resolution issues that can be adjusted easily. A similar point perhaps applies in the next para. about ocean/land boundaries. OK, you could say this just shows how bad models are, but it perhaps gives people a poorer view of the model physics and credibility than is truly needed. Another point to mention in this para about precip. is the obvious point about decadal natural variability. It's a tall order to expect the models to get the 1961-90 monthly mean precip. exactly right, owing to internal variability. Indeed, give such variability can be plus/minus 10-20% or more it would be astonishing if they matched. Be generous to models I say.

p.9, middle - interesting point about ECHAM4 and NCAR masks!!

p.15, para 2: didn't you have A1FI available from Hadley? Surely it could have been used to test this? Last sentence in this para: why 'evidently conform'?

p.16, last line: interesting point here: if you claim the pattern-scaling didn't work for the 2020s because of nat var (S/N ratios) then why actually should we go with the raw model results anyway - certainly if it is the signal we are interested in (and not the noise), it suggests the raw 2020s models results are misleading us! This is a rather circular argument I realise but the bottom line point again comes back to S/N ratios and the role of nat decadal variability, esp. for precip. Are we going to recommend adaptations to noise or to signals - and why?

p.17, middle para: what about mentioning climate sensitivity here? I know its out of vogue now, but PCM and NIES differences are explained by overall model sensitivity aren't they.

p.17, para 4: this point about where agreement occurs between models is important. Some people - I heard Wigley do it recently - write models off at regional scales re. precip changes because they all disagree. They do for some regions, but not all and where we think we have physical grounds to accept agreement as legit. (e.g. UK; cf. UKCIP02 scenario methodology) then we should be confident to say so.

p.17, line -7: why use 'forecasting' here? Could confuse some people. The old argument about terms I guess. And again top line on p.18 is dangerous - we can "predict" nat. variability in a stochastic sense using ensembles. Change the wording.

p.18, line 9: not only are they difficult to foresee, they are simply

unforseeable to a significant extent because it is we who determine them; I prefer to make the distinction between different types of prediction problem more explicit.

p.18, lines 19-20: I don't like the use of 'truth' and 'precise' here. It implies a strong natural science view prediction and the competence of science (modellers!) which I think should be softened.

p.18, para 4: the inter-model differences bit being as large as the inter-scenario differences. Again at least mention the role of nat var here - some of these inter-model differences **must** be due to nat var, not simply models not able to agree with each other.

p.19, para 1: I think the stabilisation case should be mentioned here. What about pattern-scaling stab scenarios? As I hear it from DEFRA and Hadley here in UK this was a big issue at the TGCIA meeting. Make a comment at least; I think in principle p-scaling is probably OK (within some limits) even here. I think you should make reference to some of Tim Mitchell's work here (and/or elsewhere) since he has looked at some of these things too. His thesis or his CC paper perhaps.

And finally, w/o sounding as self-serving as Tom Wigley, it would be nice if you could reference (perhaps in section 3.3) the Hulme/Brown (1998) paper in CR which was the first time I published scatter plots in this form for GCMs results - and possible the first time this form of presentation had been used anywhere (but I stand corrected of course; maybe I simply picked it up from someone else).

So there it is: a great piece of work and a good write up. I don't know Kimmo but pass on my congratulations to him. I'll look out for it on the web site.

Best wishes,

Mike

At 13:42 13/01/03 +0200, Timothy Carter wrote:

>Dear Mike and Tim,

>

>I know that you are not now involved in the TGCIA, but there is still some
>old baggage from the days of Mike's tenure that you may have some interest
>to comment on concerning regional pattern-scaling work.

>

>I attach a paper that we have prepared and distributed at the latest TGCIA
>meeting for comment (last week). If you have any comments, I would be very
>appreciative. I need comments if possible by the end of this week.

>

>The 96 pages of scatter plots are currently enormous files, and I can't
>possibly attach these for you to see. I am working on a way to get these
>substantially reduced in size. I have attached one example so you can see
>what to expect.

>

>Any feedback would be much appreciated. We intend to post this document,
>or something like it, on the DDC.

>

>Tim - have you published any of your Ph.D. results yet?

>

>Best regards and Happy New Year,

>

>Tim

>

>

>

>*****

>Timothy Carter

>Research Professor

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>Web: <http://www.ymparisto.fi/eng/research/projects/finsken/welcome.html>

>*****

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

To: Ulrich Cubasch <cubasch@zedat.fu-berlin.de>

Subject: Re: multiproxy

Date: Tue, 28 Jan 2003 12:33:35 -0500

Cc: Tim Osborne <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Irina Fast <f14@zedat.fu-berlin.de>, Scott Rutherford <srutherford@gso.uri.edu>, mann@virginia.edu

Dear Ulrich,

That's fine--you can go ahead and use it. But I have to issue a number of caveats first.

This is a version we gave Tim Osborne when he was visiting here, and since Tim hasn't used it, and we haven't compared results from that code w/ our published results, I can't vouch for it--it may or may not be the exact same version we ultimately used, and it may or may not run properly on platforms other than the one I was using (Sun running ultrix). Scott Rutherford (whom I've cc'd on this email) has worked with the code more frequently. The code is not very user friendly unfortunately. For example, the determination of the optimal subset of PCs to retain is based on application of the criterion described in our paper, which involves running the code many times w/ different choices. So the "iterative" process has to be performed by brute force.

The method, as outlined, is quite straightforward and others have implemented it themselves. SO you might prefer to code it yourself. That would be my suggestion. But you are, of course, free to use our code.

That having been said, we have essentially abandoned that method now in favor of a somewhat more sophisticated version of the approach, which makes use of the RegEM method for imputing missing values of a field described by Schneider (J. Climate, 2000).

Some initial results are described here:

Mann, M.E., Rutherford, S., Climate Reconstruction Using 'Pseudoproxies', Geophysical Research Letters, 29 (10), 1501, doi: 10.1029/2001GL014554

[1][ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.\[2\].pdf](ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.[2].pdf)

and in a paper in press in Journal of Climate.

Rutherford, S., Mann, M.E., Delworth, T.L., Stouffer, R., The Performance of Covariance-Based Methods of Climate Field Reconstruction Under Stationary and Nonstationary Forcing, J. Climate, in press, 2003.

(I don't have the preprint--Scott Rutherford can provide you with one however).

In our view, this is a preferable approach on a number of levels, though the results obtained are generally quite similar.

I will be in Nice, and looking forward to seeing you there,

Mike

At 04:59 PM 1/28/03 +0100, Ulrich Cubasch wrote:

Dear Michael,

as you might know we (Briffa, Wanner, v. Storch, Tett ...) have an European project called SOAP, which aims at combining multy proxi and model data.

more under [3]<http://www.cru.uea.ac.uk/cru/projects/soap>

In the workpackage I am coordinating we would like to use your multi-proxy program for some

temperature reconstructions. The colleagues in Norwich have got your program already, but I would like to implement it here in Berlin. I therefore would like to ask you if you can grant me the permission to use it.

I will probably copy it then from Keith and Tim directly.

I will keep you informed about the results we obtain with it.

regards

Ulrich Cubasch

P. S.

Are you coming to Nice?

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[4][http://www.evsc.virginia.edu/faculty/people/mann.\[5\].shtml](http://www.evsc.virginia.edu/faculty/people/mann.[5].shtml)

References

1. <ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.pdf>
2. <ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.pdf>
3. <http://www.cru.uea.ac.uk/cru/projects/soap>
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
5. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>

To: f14@zedat.fu-berlin.de

Subject: Re: program code

Date: Wed, 05 Feb 2003 13:19:29 -0500

Cc: Scott Rutherford <srutherford@gso.uri.edu>, Zhang <zz9t@virginia.edu>, mann@virginia.edu, Tim Osborne <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Irina Fast <f14@zedat.fu-berlin.de>, mhughes@ltr.arizona.edu, rbradley@geo.umass.edu

Dear Irina,

The code we used in Mann/Bradley/Hughes 1998 was not changed or "improved", but there may be different versions of the code floating around, and in a previous email to Uli Cubasch, I indicated that I was not sure the version you have (from Tim Osborn), is identical to the version we used in our original paper (it would require some work on my part to insure it gives precisely the same results, and I don't have the time to do that). I suspect, however, that the code is the same as the one we used in our paper and any differences, if they exist, should be minor (as long as the code compiles and runs correctly on the platform you have--the possible platform-dependence of fortran is a potential cause for concern here).

Numerous people have coded up our method independently, including Ed Zorita, w/ whom I believe your group has a close collaboration, and my graduate student Zhang has successfully coded this up independently in Matlab (its a short script, which didn't take Zhang long to write anyway). I'm copying this message to Zhang, so that he can provide you with his matlab version of the code if you are interested. Because Zhang's version is in Matlab, it should run correctly, independently of the particular platform (an advantage over the fortran code) [As an aside, on a pedagogical note, I would still encourage you to code this up yourself].

As I indicated in a previous email to Uli, the selection of the optimal subset of EOFs to retain is not automated in the code, and you need to do that yourself...The methodology we used is described in detail in our publications.

We have tested this method against the approach our group now uses for climate field reconstruction (Schneider RegEM approach), and find that the results are similar, but the cross-validation statistics improve slightly w/ the RegEM approach, which we now favor and use in place of the old, Mann et al approach.

Details of this latter approach are described in these two manuscripts (as well as the original paper by Schneider referenced within):

Mann, M.E., Rutherford, S., Climate Reconstruction Using 'Pseudoproxies', Geophysical Research Letters, 29 (10), 1501, doi: 10.1029/2001GL014554, 2002.

available at:

[1][ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.\[2\].pdf](ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.[2].pdf)

Rutherford, S., Mann, M.E., Delworth, T.L., Stouffer, R., Climate Field Reconstruction Under Stationary and Nonstationary Forcing, Journal of Climate, 16, 462-479, 2003.

available at:

[3]<ftp://holocene.evsc.virginia.edu/pub/mann/Rutherfordetal-Jclim03.pdf>

The RegEM code is available over the web, and Scott Rutherford can provide you with the ftp side if you are interested. It, too, is available only in matlab.

I hope you find this information of help.

Best of luck w/ your research,

mike mann

At 06:10 PM 2/5/03 +0100, Irina Fast wrote:

Dear Michael,

I believe that you have not heard about me as yet. My name is Irina Fast.

Since the January 2003 I am a PhD student at the Free University in Berlin in

the framework of the EU-Project SOAP. My supervisor is Ulrich Cubasch.

At the SOAP's start-up meeting it was proposed to use your multiproxy calibration method (published in 1998) for the joint analysis of model simulations and proxydata.

Because your method was essential improved since 1998 I would like to know if you kann provide us with your program code.

We could try to code your approach ourselves, but we do not know if this kind of analysis will success in our case. In the case of failure we will have to search for other analyses methodes. And the timespan for the data processing is rather short. Naturally you will not miss our gratitude and acknowledgement.

I apologise for my mistakes in this letter.

Best regards

Irina Fast

--

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[4][http://www.evsc.virginia.edu/faculty/people/mann.\[5\].shtml](http://www.evsc.virginia.edu/faculty/people/mann.[5].shtml)

References

1. <ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.pdf>
2. <ftp://holocene.evsc.virginia.edu/pub/mann/Pseudoproxy02.pdf>
3. <ftp://holocene.evsc.virginia.edu/pub/mann/Rutherfordetal-Jclim03.pdf>
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
5. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Mike Hulme <m.hulme@uea.ac.uk>

To: "Kabat, dr. P." <P.Kabat@Alterra.wag-ur.nl>, "Schellnhuber (E-mail)" <h.j.schellnhuber@uea.ac.uk>

Subject: Re: Letter of Support

Date: Wed Feb 12 15:45:03 2003

Cc: "Alex Haxeltine (E-mail)" <Alex.Haxeltine@uea.ac.uk>

Pavel

I will certainly make sure a letter reaches you for Friday. And Good Luck!

Mike

At 14:07 12/02/03 +0100, Kabat, dr. P. wrote:

Dear Mike, John, Alex:

referring to our tel. conversation yesterday with Alex, hereby our request for a letter of support/recommendation on behalf of Tyndall for our national Global Change Initiative programme proposal called "Climate changes the spatial planning", ("Climate for Spatial Planning Spatial Planning for Climate); unofficially known to you I guess as "Netherlands Tyndall-like initiative...)

After we have successfully passed the first round of the selection last year with the Dutch Government, we are now in final stages of submitting the final proposal/business plan (deadline 17/2/03 - next Monday).

The proposed programme has a total budget of 100 million Euro, of which 49 million is requested from the Government, rest contribution of public and private institutions. As a part of this programme we are aiming to set up Netherlands Centre of Excellence (partly virtual) institute, modelled after Tyndall. Leading parties in this effort are all well known to you:

Wageningen (kabat)

VU Amsterdam (vellinga)

RIVM (metz)

KNMI (Komen)

ICIS (Rotmans)

ECN (Bruggink)

plus another almost 50 parties.

Could you pls send us a short letter of support, in which you indicate the importance of this initiative for advancing this type global change science, European dimension, UK - NL collaboration, etc, etc?

We need to receive this by Friday, so send also by fax pls (apologies for the rush). Letter is to be addressed as follows:

Prof. Dr Pavel Kabat

Science Director

Netherlands National Research Initiative "Climate changes the spatial planning", (ICES KIS 3)

Postal address: PO Box 47, 6700 AA Wageningen

Visiting address: Lawickse Allee 11, IAC building, room 156

Voice +31 317 474314/74713 (office), +31 653489378 (mobile), +31 264463567
(home);

Fax: +31 317495590

I attach 3 documents as background of our proposal

Many thanks for your kind help!

Pavel, Pier en colleagues

<<BPDraft2.3NoFigures.doc>> <<OrganisatieSchema.doc>> <<Overview budget
131.xls>>

From: "Alex Haxeltine" <Alex.Haxeltine@uea.ac.uk>
To: "Armin Haas" <haas@pik-potsdam.de>, "Alexander Wokaun" <wokaun@psi.ch>, "Anco Lankreijer" <lana@geo.vu.nl>, "Andrew Jordan" <a.jordan@uea.ac.uk>, "Antoni Rosell" <antoni.rosell@uab.es>, "Antonio Navarra" <navarra@ingv.it>, Asbjørn Torvanger <asbjorn.torvanger@cicero.uio.no>, <baldur.eliasson@ch.abb.com>, Benito Müller <benito.mueller@philosophy.oxford.ac.uk>, "Bert Metz" <bert.metz@rivm.nl>, <bhare@ams.greenpeace.org>, "Brian O'Neill" <oneill@iiasa.ac.at>, "Carlo Carraro" <ccarraro@unive.it>, "Carlo Jaeger" <carlo.jaeger@pik-potsdam.de>, "Catherine Boemare" <boemare@centre-cired.fr>, "Christian Azar" <ftrca@fy.chalmers.se>, "Christian Flachsland" <christian.flachsland@pik-potsdam.de>, "Christos Giannakopoulos" <cgianak@meteo.noa.gr>, "Claudia Kemfert" <kemfert@uni-oldenburg.de>, "Daniel Droste" <d.droste@consultants.mvv.de>, "Eberhard Jochem" <eberhard.jochem@isi.fhg.de>, "Eberhard Jochem" <jochem@cepe.mavt.ethz.ch>, "Elas Hunfeld" <els.hunfeld@falw.vu.nl>, "Felicity Thomas" <ft@ier.uni-stuttgart.de>, "Ferenc Toth" <toth@iiasa.ac.at>, "Francis Johnson" <francis.johnson@sei.se>, "Frank Thomalla" <frank.thomalla@pik-potsdam.de>, "Fred Langeweg" <Fred.Langeweg@rivm.nl>, "Gary Yohe" <gyohe@wesleyan.edu>, <gberz@munichre.com>, "Gernot Klepper" <gklepper@ifw.uni-kiel.de>, "HALLEGATTE Stephane" <Stephane.Hallegatte@lmd.jussieu.fr>, "Harald Bradke" <hb@isi.fhg.de>, "Heike Zimmermann-Timm" <heike.zimmermann-timm@pik-potsdam.de>, "Helga Kromp-Kolb" <kromp-ko@tornado.boku.ac.at>, "Henning Jappe" <h.jappe@consultants.mvv.de>, "Henning Niemeyer" <h.niemeyer@consultants.mvv.de>, "Henry Neufeldt" <neufeldt@ife-le.de>, "Herve Le Treut" <letreut@lmd.ens.fr>, "Jaap C. Jansen" <j.jansen@ecn.nl>, "Jan Rotmans" <j.Rotmans@icis.unimaas.nl>, "Jean Palutikof" <j.palutikof@uea.ac.uk>, "Jean-Charles Hourcade" <hourcade@centre-cired.fr>, "Jeroen van der Sluijs" <j.p.vandersluijs@chem.uu.nl>, "Joan David Tabara" <jdtabara@terra.es>, "John Schellnhuber" <h.j.schellnhuber@uea.ac.uk>, "John Turnpenny" <j.turnpenny@uea.ac.uk>, "Jon Hovi" <jon.hovi@stv.uio.no>, Jonathan Köhler <j.kohler@uea.ac.uk>, <juergen.engelhard@rwerheinbraun.com>, Jürgen Kurths <jkurths@agnild.uni-potsdam.de>, Jürgen Kurths <juergen@lenne.agnild.uni-potsdam.de>, "Katrin Gerlinger" <Katrin.Gerlinger@pik-potsdam.de>, Klaus Böswald <klaus.boeswald@factorag.ch>, "Klaus Hasselmann" <klaus.hasselmann@dkrz.de>, "Kornelis Blok" <K.Blok@chem.uu.nl>, "Leen Hordijk" <hordijk@iiasa.ac.at>, "Lennart Olsson" <lennart.olsson@miclu.lu.se>, "Liudmila Romaniuk" <Romaniuk@mail.lanck.net>, "Marco Berg" <marco.berg@factorag.ch>, "Marcus Lindner" <Marcus.Lindner@efi.fi>, "Marina Fischer-Kowalski" <marina.fischer-kowalski@univie.ac.at>, "Marjan Minnesma" <Marjan.Minnesma@ivm.vu.nl>, "Mark Rounsevell" <rounsevell@geog.ucl.ac.be>, "Martin Claussen" <Martin.Claussen@pik-potsdam.de>, "Martin Kaltschmitt" <kaltschmitt@ife-le.de>, "Martin Parry" <martin.parry@uea.ac.uk>, "martin.welp" <martin.welp@pik-potsdam.de>, "Mike Hulme" <m.hulme@uea.ac.uk>, "Monika Ritt" <Monika.ritt@falw.vu.nl>, "MVV C&E Berlin Tom Mansfield" <mansfield@euweb.de>, "MVV C&E Hanan Abdul-Rida" <h.abdulrida@consultants.mvv.de>, "Nakicenovic" <naki@iiasa.ac.at>, "Neil Adger" <n.adger@uea.ac.uk>, Niklas Höhne <n.hoehne@ecofys.de>, "Ola Johannessen" <ola.johannessen@nersc.no>, "Ottmar Edenhofer" <Ottmar.Edenhofer@pik-potsdam.de>, "Pal Prestrud"

<prestrud@cicero.uio.no>, Pål Prestrud <pal.prestrud@cicero.uio.no>, "Pavel Kabat" <P.Kabat@Alterra.wag-ur.nl>, "Philippe Ambrosi" <ambrosi@centre-cired.fr>, "Pier Vellinga" <pier.vellinga@falw.vu.nl>, "Pier Vellinga" <vell@geo.vu.nl>, "Pim Martens" <P.Martens@icis.unimaas.nl>, "Reinhard G. Budich" <budich@dkrz.de>, "Renaud Crassous" <crassous@centre-cired.fr>, "Richard Klein" <Richard.Klein@pik-potsdam.de>, "Rik Leemans" <rik.leemans@rivm.nl>, "Roger Kasperson" <roger.kasperson@sei.se>, "Rupert Klein" <Rupert.Klein@pik-potsdam.de>, "S.E. van der Leeuw" <vanderle@wanadoo.fr>, "S.E. van der Leeuw" <vanderle@mae.u-paris10.fr>, "Saleemul Huq" <saleemul.huq@iied.org>, "Sebastian Gallehr" <gallehr@e5.org>, "Simone Ullrich" <SU@ier.uni-stuttgart.de>, <SSinger@wwfepo.org>, "Stephane Hallegatte" <hallegatte@centre-cired.fr>, "Sybille van den Hove" <s.vandenhove@terra.es>, "Tim O'Riordan" <t.oriordan@uea.ac.uk>, "Tobias Kampet" <t.kampet@consultants.mvv.de>, "Tom Downing" <tom.downing@sei.se>, "Tom Kram" <Tom.Kram@rivm.nl>, "Tony Patt" <tonypatt@pik-potsdam.de>, "V.K. Dochenko" <donchenkovk@mail.ru>, "Wim Turkenburg" <W.C.Turkenburg@chem.uu.nl>, "Wolfgang Cramer" <Wolfgang.Cramer@pik-potsdam.de>, "Wolfgang Lucht" <Wolfgang.Lucht@pik-potsdam.de>
Subject: Re: AMS proposal
Date: Mon, 10 Mar 2003 17:36:46 -0000

Dear Colleagues,

In the email from Armin Haas (signed by Carlo and Klaus) on 5th March, we were informed that a strategy committee and a research committee had been formed; with the latter being primarily responsible for the preparation of the proposal.

WE NOW HAVE ONLY 20 WORKING DAYS LEFT UNTIL THE PROPOSAL HAS TO BE SUBMITTED!!!

And while I am aware and involved in a number of parallel activities addressing the writing of text for specific work domains and work packages, I have not received any formal communication about what role is expected of me as a member of the research committee (that has primary responsibility for the preparation of the proposal).

Needless to say I find this extremely worrying, and suggest that we URGENTLY need clarification about 1) exactly what the research committee should do; 2) how it should do it; 3) what responsibility for making decisions this committee will have/how it should liaise with the strategy committee.

It seems clear that in order to finalize an overall project structure we will need to meet face-to-face for at least 36 hours, and that this needs to happen with the utmost urgency.

I have made a provisional booking of a facility very near Stanstead airport in the UK for

next Monday and Tuesday (17th and 18th March), and offer this as a possible time and place

to meet; but am of course open to other suggestions. I would imagine that in addition to

the research committee assigned so far, we would need to co-opt the writers of several of

the work packages and the work domains leaders for the purpose of this meeting.

With warm regards and the utmost sense of urgency,

Alex Haxeltine

Dr Alexander Haxeltine
International Science Co-ordinator
Tyndall Centre for Climate Change Research
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ, UK

Tel: +44 1603 593902
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Website: [1]<http://www.tyndall.ac.uk>

References

1. <http://www.tyndall.ac.uk/>

From: "Michael E. Mann" <mann@virginia.edu>

To: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu, mhughes@ltr.arizona.edu, srutherford@gso.uri.edu, tcrowley@duke.edu

Subject: Re: Fwd: Soon & Baliunas

Date: Tue, 11 Mar 2003 08:14:49 -0500

Cc: k.briffa@uea.ac.uk, jto@u.arizona.edu, drdendro@ldeo.columbia.edu, keith.alverson@pages.unibe.ch, mmaccrac@comcast.net, jto@u.arizona.edu, mann@virginia.edu

Thanks Phil,

(Tom: Congrats again!)

The Soon & Baliunas paper couldn't have cleared a 'legitimate' peer review process anywhere. That leaves only one possibility--that the peer-review process at Climate Research has been hijacked by a few skeptics on the editorial board. And it isn't just De Frietas, unfortunately I think this group also includes a member of my own department...

The skeptics appear to have staged a 'coup' at "Climate Research" (it was a mediocre journal to begin with, but now its a mediocre journal with a definite 'purpose').

Folks might want to check out the editors and review editors:

[1]<http://www.int-res.com/journals/cr/crEditors.html>

In fact, Mike McCracken first pointed out this article to me, and he and I have discussed this a bit. I've cc'd Mike in on this as well, and I've included Peck too. I told Mike that I believed our only choice was to ignore this paper. They've already achieved what they wanted--the claim of a peer-reviewed paper. There is nothing we can do about that now, but the last thing we want to do is bring attention to this paper, which will be ignored by the community on the whole...

It is pretty clear that thee skeptics here have staged a bit of a coup, even in the presence of a number of reasonable folks on the editorial board (Whetton, Goodess, ...). My guess is that Von Storch is actually with them (frankly, he's an odd individual, and I'm not sure he isn't himself somewhat of a skeptic himself), and without Von Storch on their side, they would have a very forceful personality promoting their new vision.

There have been several papers by Pat Michaels, as well as the Soon & Baliunas paper, that couldn't get published in a reputable journal.

This was the danger of always criticising the skeptics for not publishing in the "peer-reviewed literature". Obviously, they found a solution to that--take over a journal! So what do we do about this? I think we have to stop considering "Climate Research" as a legitimate peer-reviewed journal. Perhaps we should encourage our colleagues in the climate research community to no longer submit to, or cite papers in, this journal. We would also need to consider what we tell or request of our more reasonable colleagues who currently sit on the editorial board...

What do others think?

mike

At 08:49 AM 3/11/2003 +0000, Phil Jones wrote:

Dear All,

Apologies for sending this again. I was expecting a stack of emails this morning
in

response, but I inadvertently left Mike off (mistake in pasting) and picked up Tom's old address. Tom is busy though with another offspring !

I looked briefly at the paper last night and it is appalling - worst word I can think of today without the mood pepper appearing on the email ! I'll have time to read more at the weekend as I'm coming to the US for the DoE CCPP meeting at Charleston. Added Ed, Peck and Keith A. onto this list as well. I would like to have time to rise to the bait, but I have so much else on at the moment. As a few of us will be at the EGS/AGU meet in Nice, we should consider what to do there.

The phrasing of the questions at the start of the paper determine the answer they get. They have no idea what multiproxy averaging does. By their logic, I could argue 1998 wasn't the warmest year globally, because it wasn't the warmest everywhere. With their LIA being 1300-1900 and their MWP 800-1300, there appears (at my quick first reading) no discussion of synchronicity of the cool/warm periods. Even with the instrumental record, the early and late 20th century warming periods are only significant locally at between 10-20% of grid boxes.

Writing this I am becoming more convinced we should do something - even if this is just to state once and for all what we mean by the LIA and MWP. I think the skeptics will use this paper to their own ends and it will set paleo back a number of years if it goes unchallenged.

I will be emailing the journal to tell them I'm having nothing more to do with it until they rid themselves of this troublesome editor. A CRU person is on the editorial board, but papers get dealt with by the editor assigned by Hans von Storch.

Cheers

Phil

Dear all,

Tim Osborn has just come across this. Best to ignore probably, so don't let it spoil your day. I've not looked at it yet. It results from this journal having a number of editors. The responsible one for this is a well-known skeptic in NZ. He has let a few papers through by

Michaels and Gray in the past. I've had words with Hans von Storch about this, but got nowhere.

Another thing to discuss in Nice !

Cheers
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X-Sender: f055@pop.uea.ac.uk
X-Mailer: QUALCOMM Windows Eudora Version 5.1
Date: Mon, 10 Mar 2003 14:32:14 +0000
To: p.jones@uea
From: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Soon & Baliunas
Dr Timothy J Osborn | phone: +44 1603 592089
Senior Research Associate | fax: +44 1603 507784
Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
School of Environmental Sciences | web-site:
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[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.int-res.com/journals/cr/crEditors.html>
2. <http://www.cru.uea.ac.uk/~timo/>
3. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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Subject: Fwd: Soon & Baliunas
Date: Tue, 11 Mar 2003 08:49:22 +0000
Cc: k.briffa@uea.ac.uk,jto@u.arizona.edu,drdendro@ldeo.columbia.edu, keith.alverson@pages.unibe.ch

<x-flowed>

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</x-flowed>

Attachment Converted: "c:\eudora\attach\Soon & Baliunas 20031.pdf"

From: "Michael E. Mann" <mann@virginia.edu>

To: Phil Jones <p.jones@uea.ac.uk>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Tom Crowley <tcrowley@duke.edu>

Subject: Re: Fwd: Soon & Baliunas

Date: Wed, 12 Mar 2003 08:12:56 -0500

Cc: rbradley@geo.umass.edu,mhughes@ltrr.arizona.edu,srutherford@gso.uri.edu, k.briffa@uea.ac.uk,t.osborn@uea.ac.uk,mann@virginia.edu

Dear All,

I like Phil's suggestion. I think such a piece would do a lot of good for the field. When something as full of half-truths/mis-truths as the S&B piece is put forth, it would be very useful to have a peer-reviewed review like this, which we all have endorsed through co-authorship, to point to in response. This way, when we get the inevitable "so what do you have to say about this" from our colleagues, we already have a self-contained, thorough rejoinder to point to. I'm sure we won't all agree on every detail, but there is enough commonality in our views on the big issues to make this worthwhile.

Perhaps Phil can go ahead and contact the editorial board at "Reviews of Geophysics" and see if they're interested. If so, Phil and I (and anyone else interested) could take the lead with this, and then we can entrain everyone else in as we proceed with a draft, etc.

mike

p.s. Keith: I hope you're feeling well, and that your recovery proceeds quickly!

At 10:02 AM 3/12/2003 +0000, Phil Jones wrote:

Dear All,

I agree with all the points being made and the multi-authored article would be a good idea, but how do we go about not letting it get buried somewhere. Can we not address the misconceptions by finally coming up with definitive dates for the LIA and MWP and redefining what we think the terms really mean? With all of us and more on the paper, it should carry a lot of weight. In a way we will be setting the agenda for what should be being done over the next few years.

We do want a reputable journal but is The Holocene the right vehicle. It is probably the best of its class of journals out there. Mike and I were asked to write an article for the EGS journal of Surveys of Geophysics. You've not heard of this - few have, so we declined. However, it got me thinking that we could try for Reviews of Geophysics. Need to contact the editorial board to see if this might be possible. Just a thought, but it certainly has a high profile.

What we want to write is NOT the scholarly review a la Jean Grove (bless her soul) that just reviews but doesn't come to anything firm. We want a critical review that enables agendas to be set. Ray's recent multi-authored piece goes a lot of the way so we need to build on this.

Cheers

Phil

At 12:55 11/03/03 -0500, Michael E. Mann wrote:

Hi Malcolm,

Thanks for the feedback--I largely concur. I do, though, think there is a particular problem with "Climate Research". This is where my colleague Pat Michaels now publishes exclusively, and his two closest colleagues are on the editorial board and review editor board. So I promise you, we'll see more of this there, and I personally think there *is* a bigger problem with the "messenger" in this case...

But the Soon and Baliunas paper is its own, separate issue too. I too like Tom's latter

idea, of a more hefty multi-authored piece in an appropriate journal (Paleoceanography? Holocene?) that seeks to correct a number of misconceptions out there, perhaps using Baliunas and Soon as a case study ('poster child?'), but taking on a slightly greater territory too.

Question is, who would take the lead role. I *know* we're all very busy,
mike

At 10:28 AM 3/11/03 -0700, Malcolm Hughes wrote:

I'm with Tom on this. In a way it comes back to a rant of mine to which some of you have already been victim. The general point is that there are two arms of climatology:

neoclimatology - what you do based on instrumental records and direct, systematic observations in networks - all set in a very Late Holocene/Anthropocene time with hourly to decadal interests.

paleoclimatology - stuff from rocks, etc., where major changes in the Earth system, including its climate, associated with major changes in boundary conditions, may be detected by examination of one or a handful of paleo records.

Between these two is what we do - "mesoclimatology" - dealing with many of the same phenomena as neoclimatology, using documentary and natural archives to look at phenomena on interannual to millennial time scales. Given relatively small changes in boundary conditions (until the last couple of centuries), mesoclimatology has to work in a way that is very similar to neoclimatology. Most notably, it depends on heavily replicated networks of precisely dated records capable of being either calibrated, or whose relationship to climate may be modeled accurately and precisely.

Because this distinction is not recognized by many (e.g. Sonnechkin, Broecker, Karlen) we see an accumulation of misguided attempts at describing the climate of recent millennia. It would be better to head this off in general, rather than draw attention to a bad paper. After all, as Tom rightly says, we could all nominate really bad papers that have been published in journals of outstanding reputation (although there could well be differences between our lists).

End of rant, Cheers, Malcolm

> Hi guys,

>

> junk gets published in lots of places. I think that what could be done is a short reply to the authors in Climate Research OR a SLIGHTLY longer note in a reputable journal entitled something like "Continuing Misconceptions About interpretation of past climate change." I kind of like the more pointed character of the latter and submitting it as a short note with a group authorship carries a heft that a reply to a paper, in no matter what journal, does not.

>

> Tom

>

>

>

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>>Attachment converted: Macintosh HD:Soon & Baliunas 2003.pdf (PDF
>>/CARO) (00016021)

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References

1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
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From: Tom Crowley <tcrowley@duke.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: Soon & Baliunas
Date: Wed, 12 Mar 2003 09:15:48 -0500
Cc: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Tom Crowley <tcrowley@duke.edu>, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, srutherford@gso.uri.edu, mann@virginia.edu, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

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> >>X-Mailer: QUALCOMM Windows Eudora Version 5.1
> >>Date: Mon, 10 Mar 2003 14:32:14 +0000
> >>To: p.jones@uea
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> >>Subject: Soon & Baliunas
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From: Scott Rutherford <srutherford@gso.uri.edu>

To: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: Soon & Baliunas

Date: Wed, 12 Mar 2003 10:53:07 -0500

Cc: Tom Crowley <tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>, Malcolm Hughes <mhughes@ltr.arizona.edu>, rbradley@geo.umass.edu, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

<x-rich>Dear All,

First, I'd be willing to handle the data and the plotting/mapping. Second, regarding Mike's suggestions, if we use different reference periods for the reconstructions and the models we need to be extremely careful about the differences. Not having seen what this will look like, I suggest that we start with the same instrumental reference period for both (1856-1960). If you are willing to send me your series please send the raw (i.e. unfiltered) series. That way I can treat them all the same. We can then decide how we want to display the results.

Finally, Tom's suggestion of Eos struck me as a great way to get a short, pointed story out to the most people (though I have no feel for the international distribution). My sense (being relatively new to this field compared to everyone else) is that within the neo- and mesoclimate research community there is a (relatively small?) group of people who don't or won't "get it" and there is nothing we can do about them aside from continuing to publish quality work in quality journals (or calling in a Mafia hit). Those (e.g. us) who are engrossed in the issues and are aware of all the literature should be able to distinguish between well done and poor work. Should then the intent of this proposed contribution be to education those who are not directly involved in MWP/LIA issues including those both on the periphery of the issue as well as those outside? If so, then the issue that Phil raised about not letting it get buried is significant and I think Eos is a great way to get people to see it.

Cheers,

Scott

On Wednesday, March 12, 2003, at 10:32 AM, Michael E. Mann wrote:

<excerpt>p.s. The idea of both a representative time-slice spatial plot emphasizing the spatial variability of e.g. the MWP or LIA, and

an EOF analysis of all the records is a great idea. I'd like to suggest a small modification of the latter:

I would suggest we show 2 curves, representing the 1st PC of two different groups, one of empirical reconstructions, the other of model simulations, rather than just one in the time plot.

Group #1 could include:

- 1) Crowley & Lowery
- 2) Mann et al 1999
- 3) Bradley and Jones 1995
- 4) Jones et al, 1998
- 5) Briffa et al 200X? [Keith/Tim to provide their preferred MXD reconstruction]
- 6) Esper et al [yes, no?--one series that differs from the others won't make much of a difference]

I would suggest we scale the resulting PC to the CRU 1856-1960 annual Northern Hemisphere mean instrumental record, which should overlap w/ all of the series, and which pre-dates the MXD decline issue...

Group #2 would include various model simulations using different forcings, and with slightly different sensitivities. This could include 6 or so simulation results:

- 1) 3 series from Crowley (2000) [based on different solar/volcanic reconstructions],
- 2) 2 series from Gerber et al (Bern modeling group result) [based on different assumed sensitivities]
- 1) Bauer et al series (Claussen group EMIC result) [includes 19th/20th century land use changes as a forcing].

I would suggest that the model's 20th century mean is aligned with the 20th century instrumental N.Hem mean for comparison (since this is

when we know the forcings best).

I'd like to nominate Scott R. as the collector of the time series and the performer of the EOF analyses, scaling, and plotting, since Scott already has many of the series and many of the appropriate analysis and plotting tools set up to do this.

We could each send our preferred versions of our respective time series to Scott as an ascii attachment, etc.

thoughts, comments?

thanks,

mike

At 10:08 AM 3/12/2003 -0500, Michael E. Mann wrote:

Thanks Tom,

Either would be good, but Eos is an especially good idea. Both Ellen M-T and Keith Alverson are on the editorial board there, so I think there would be some receptiveness to such a submission.t

I see this as complementary to other pieces that we have written or are currently writing (e.g. a review that Ray, Malcolm, and Henry Diaz are doing for Science on the MWP) and this should proceed entirely independently of that.

If there is group interest in taking this tack, I'd be happy to contact Ellen/Keith about the potential interest in Eos, or I'd be happy to let Tom or Phil to take the lead too...

Comments?

mike

At 09:15 AM 3/12/2003 -0500, Tom Crowley wrote:

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>>>X-Mailer: QUALCOMM Windows Eudora Version 5.1

>>>Date: Mon, 10 Mar 2003 14:32:14 +0000

>>>To: p.jones@uea

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<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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</x-rich>

From: "Michael E. Mann" <mann@virginia.edu>

To: Scott Rutherford <srutherford@gso.uri.edu>

Subject: Re: Soon & Baliunas

Date: Wed, 12 Mar 2003 11:07:43 -0500

Cc: Tom Crowley <tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, rbradley@geo.umass.edu, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, mann@virginia.edu

Thanks Scott,

I concur. We may want to try a few different alignment/scaling choices in the end, and

then just vote on which we like the best,

Anxious to here others' thoughts on all of this,

mike

At 10:53 AM 3/12/2003 -0500, Scott Rutherford wrote:

Dear All,

First, I'd be willing to handle the data and the plotting/mapping. Second, regarding Mike's suggestions, if we use different reference periods for the reconstructions and the models we need to be extremely careful about the differences. Not having seen what this will look like, I suggest that we start with the same instrumental reference period for both (1856-1960). If you are willing to send me your series please send the raw (i.e. unfiltered) series. That way I can treat them all the same. We can then decide how we want to display the results.

Finally, Tom's suggestion of Eos struck me as a great way to get a short, pointed story out to the most people (though I have no feel for the international distribution). My sense (being relatively new to this field compared to everyone else) is that within the neo- and mesoclimate research community there is a (relatively small?) group of people who don't or won't "get it" and there is nothing we can do about them aside from continuing to publish quality work in quality journals (or calling in a Mafia hit).

Those (e.g. us) who are engrossed in the issues and are aware of all the literature should be able to distinguish between well done and poor work. Should then the intent of this proposed contribution be to education those who are not directly involved in MWP/LIA issues including those both on the periphery of the issue as well as those outside? If so, then the issue that Phil raised about not letting it get buried is significant and I think Eos is a great way to get people to see it.

Cheers,

Scott

On Wednesday, March 12, 2003, at 10:32 AM, Michael E. Mann wrote:

p.s. The idea of both a representative time-slice spatial plot emphasizing the spatial variability of e.g. the MWP or LIA, and an EOF analysis of all the records is a great idea. I'd like to suggest a small modification of the latter:

I would suggest we show 2 curves, representing the 1st PC of two different groups, one of empirical reconstructions, the other of model simulations, rather than just one in the time plot.

Group #1 could include:

- 1) Crowley & Lowery
- 2) Mann et al 1999
- 3) Bradley and Jones 1995
- 4) Jones et al, 1998
- 5) Briffa et al 200X? [Keith/Tim to provide their preferred MXD reconstruction]
- 6) Esper et al [yes, no?--one series that differs from the others won't make much of a difference]

I would suggest we scale the resulting PC to the CRU 1856-1960 annual Northern

Hemisphere mean instrumental record, which should overlap w/ all of the series, and which pre-dates the MXD decline issue...

Group #2 would include various model simulations using different forcings, and with slightly different sensitivities. This could include 6 or so simulation results:

- 1) 3 series from Crowley (2000) [based on different solar/volcanic reconstructions],
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I would suggest that the model's 20th century mean is aligned with the 20th century instrumental N.Hem mean for comparison (since this is when we know the forcings best).

I'd like to nominate Scott R. as the collector of the time series and the performer of the EOF analyses, scaling, and plotting, since Scott already has many of the series and many of the appropriate analysis and plotting tools set up to do this.

We could each send our preferred versions of our respective time series to Scott as an ascii attachment, etc.

thoughts, comments?

thanks,

mike

At 10:08 AM 3/12/2003 -0500, Michael E. Mann wrote:

Thanks Tom,

Either would be good, but Eos is an especially good idea. Both Ellen M-T and Keith Alverson are on the editorial board there, so I think there would be some receptiveness to such a submission.t

I see this as complementary to other pieces that we have written or are currently writing (e.g. a review that Ray, Malcolm, and Henry Diaz are doing for Science on the MWP) and this should proceed entirely independently of that.

If there is group interest in taking this tack, I'd be happy to contact Ellen/Keith about the potential interest in Eos, or I'd be happy to let Tom or Phil to take the lead too...

Comments?

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At 09:15 AM 3/12/2003 -0500, Tom Crowley wrote:

Phil et al,

I suggest either BAMS or Eos - the latter would probably be better because it is shorter, quicker, has a wide distribution, and all the points that need to be made have been made before.

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I suggest two figures- one on time series and another showing the spatial array of temperatures at one point in the Middle Ages. I produced a few of those for the Ambio paper but already have one ready for the Greenland settlement period 965-995 showing the regional nature of the warmth in that figure. we could add a few new sites to it, but if people think otherwise we could of course go in some other direction.

rather than getting into the delicate question of which paleo reconstruction to use I suggest that we show a time series that is an eof of the different reconstructions - one that emphasizes the commonality of the message.

Tom

Dear All,

I agree with all the points being made and the multi-authored article would be a good idea, but how do we go about not letting it get buried somewhere. Can we not address the misconceptions by finally coming up with definitive dates for the LIA and MWP and redefining what we think the terms really mean? With all of us and more on the paper, it should carry a lot of weight. In a way we will be setting the agenda for what should be being done over the next few years.

We do want a reputable journal but is The Holocene the right vehicle. It is probably the best of its class of journals out there. Mike and I were asked to write an article for the EGS journal of Surveys of Geophysics. You've not heard of this - few have, so we declined. However, it got me thinking that we could try for Reviews of Geophysics. Need to contact the editorial board to see if this might be possible. Just a thought, but it certainly has a high profile.

What we want to write is NOT the scholarly review a la Jean Grove (bless her soul) that just reviews but doesn't come to anything firm. We want a critical review that enables agendas to be set. Ray's recent multi-authored piece goes a lot of the way so we need to build on this.

Cheers

Phil

At 12:55 11/03/03 -0500, Michael E. Mann wrote:

HI Malcolm,

Thanks for the feedback--I largely concur. I do, though, think there is a particular problem with "Climate Research". This is where my colleague Pat Michaels now publishes exclusively, and his two closest colleagues are on the editorial board and review editor board. So I promise you, we'll see more of this there, and I personally think there *is* a bigger problem with the "messenger" in this case...

But the Soon and Baliunas paper is its own, separate issue too. I too like Tom's latter idea, of a more hefty multi-authored piece in an appropriate journal (Paleoceanography? Holocene?) that seeks to correct a number of misconceptions out there, perhaps using Baliunas and Soon as a case study ('poster child?'), but taking on a slightly greater territory too.

Question is, who would take the lead role. I *know* we're all very busy, mike

At 10:28 AM 3/11/03 -0700, Malcolm Hughes wrote:

I'm with Tom on this. In a way it comes back to a rant of mine to which some of you have already been victim. The general point is that there are two arms of climatology: neoclimatology - what you do based on instrumental records and direct, systematic observations in networks - all set in a very Late Holocene/Anthropocene time with hourly to decadal interests. paleoclimatology - stuff from rocks, etc., where major changes

in the Earth system, including its climate, associated with major changes in boundary conditions, may be detected by examination of one or a handful of paleo records.

Between these two is what we do - "mesoclimatology" - dealing with many of the same phenomena as neoclimatology, using documentary and natural archives to look at phenomena on interannual to millennial time scales. Given relatively small changes in boundary conditions (until the last couple of centuries), mesoclimatology has to work in a way that is very similar to neoclimatology. Most notably, it depends on heavily replicated networks of precisely dated records capable of being either calibrated, or whose relationship to climate may be modeled accurately and precisely.

Because this distinction is not recognized by many (e.g. Sonnechkin, Broecker, Karlen) we see an accumulation of misguided attempts at describing the climate of recent millennia. It would be better to head this off in general, rather than draw attention to a bad paper. After all, as Tom rightly says, we could all nominate really bad papers that have been published in journals of outstanding reputation (although there could well be differences between our lists).

End of rant, Cheers, Malcolm

> Hi guys,

>

> junk gets published in lots of places. I think that what could be
> done is a short reply to the authors in Climate Research OR a SLIGHTLY
> longer note in a reputable journal entitled something like "Continuing
> Misconceptions About interpretation of past climate change." I kind
> of like the more pointed character of the latter and submitting it as
> a short note with a group authorship carries a heft that a reply to a
> paper, in no matter what journal, does not.

>

> Tom

>

>

>

>> Dear All,

>> Apologies for sending this again. I was expecting a stack of
>> emails this morning in

>> response, but I inadvertently left Mike off (mistake in pasting)

>> and picked up Tom's old

>> address. Tom is busy though with another offspring !

>> I looked briefly at the paper last night and it is appalling -

>> worst word I can think of today

>> without the mood pepper appearing on the email ! I'll have time to

>> read more at the weekend

>> as I'm coming to the US for the DoE CCPP meeting at Charleston.

>> Added Ed, Peck and Keith A.

>> onto this list as well. I would like to have time to rise to the

>> bait, but I have so much else on at

>> the moment. As a few of us will be at the EGS/AGU meet in Nice, we

>> should consider what

>> to do there.

>> The phrasing of the questions at the start of the paper
>>determine the answer they get. They
>> have no idea what multiproxy averaging does. By their logic, I
>>could argue 1998 wasn't the
>> warmest year globally, because it wasn't the warmest everywhere.
>>With their LIA being 1300-
>>1900 and their MWP 800-1300, there appears (at my quick first
>>reading) no discussion of
>> synchronicity of the cool/warm periods. Even with the instrumental
>>record, the early and late
>> 20th century warming periods are only significant locally at
>>between 10-20% of grid boxes.
>> Writing this I am becoming more convinced we should do
>>something - even if this is just
>> to state once and for all what we mean by the LIA and MWP. I think
>>the skeptics will use
>> this paper to their own ends and it will set paleo back a number of
>>
>>years if it goes
>> unchallenged.

>>
>> I will be emailing the journal to tell them I'm having
>>nothing more to do with it until they
>> rid themselves of this troublesome editor. A CRU person is on the
>>editorial board, but papers
>> get dealt with by the editor assigned by Hans von Storch.

>>
>> Cheers
>> Phil

>>
>> Dear all,
>> Tim Osborn has just come across this. Best to ignore
>>probably, so don't let it spoil your
>> day. I've not looked at it yet. It results from this journal
>>having a number of editors. The
>> responsible one for this is a well-known skeptic in NZ. He has let
>>
>>a few papers through by
>> Michaels and Gray in the past. I've had words with Hans von Storch
>>
>>about this, but got nowhere.

>> Another thing to discuss in Nice !

>>
>> Cheers
>> Phil

>>
>>>X-Sender: f055@pop.uea.ac.uk
>>>X-Mailer: QUALCOMM Windows Eudora Version 5.1
>>>Date: Mon, 10 Mar 2003 14:32:14 +0000
>>>To: p.jones@uea
>>>From: Tim Osborn <t.osborn@uea.ac.uk>
>>>Subject: Soon & Baliunas
>>>

>>>

>>>

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>>>School of Environmental Sciences | web-site: University of East
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>>>[2]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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>>Attachment converted: Macintosh HD:Soon & Baliunas 2003.pdf (PDF
>>/CARO) (00016021)

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References

1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/sunlock.htm>
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From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: Soon & Baliunas
Date: Wed, 12 Mar 2003 12:12:02 -0500
Cc: rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, srutherford@gso.uri.edu, k.briffa@uea.ac.uk, mann@virginia.edu

Dear Tim,

Thanks for your rapid replies and your help. This is all very useful.

Well, lets see what this gives...

There are some notable differences just between our relative comparisons of the different series which must have something to do with the relative scaling and aligning of the series. The position of Crowley and Lowery, in particular, is quite inconsistent between our respective comparisons. When we scale the various series to the full N. Hem instrumental annual mean CRU record 1856-1980, we get a a very different relative ordering of the different series, as shown in the attached figure from my Science perspective piece from last year

This should not, however, influence the EOF decomposition if all series are zero-mean and standardized prior to the EOF analysis, but the scaling and alignment of the result, in the end, will be sensitive to all of these various issues.

So, in short, lets see what we get, and then discuss any similarities/differences w/ your result, then make a decision as to what to show in the Eos piece. I'm sure we can come up w/ something we're all happy with...

Please do send us your & Keith's preferred version of the MXD reconstruction--we'll collect the others from the individual sources (most we already have, I think)...

mike

At 04:53 PM 3/12/2003 +0000, Tim Osborn wrote:

At 16:29 12/03/03, Michael E. Mann wrote:

but there are many variables here [not the least of which is the choice of scaling the series to an extratropical summer mean, which as we have argued before, we don't think is appropriate for a full N. Hem mean because of changes in meridional temperature gradient over time, and the choice of calibration period--I wonder if 1856-1960 or 1856-1980 gives a more stable result).

True, but as I indicated I have tried alternatives. The attached is what I get with annual mean temperature as the target series - still taken only from land >20N though [but I have extracted that domain from your spatial reconstructions to produce the time series that I used for "Mann et al." - which should make it reasonably appropriate back to 1400 at least]. I have also tried different calibration periods (including not calibrating against instrumental data at all!). All give qualitatively similar results - see attached .pdf and compare with the first one I sent.

The point is, that (I believe) the approach will introduce a *new* result and while that is interesting it wouldn't be appropriate for a short EOS piece - and having found this out, I was trying to save you the effort.

But, on reflection, it would be good if you went ahead and did this anyway, because the results might well be useful to publish in another paper, even if they weren't deemed suitable for the EOS piece.

I could provide the 7 series that I have used, but would prefer that you got them from

the original sources to ensure that you have the most up-to-date/correct versions.

Cheers

Tim

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Senior Research Associate | fax: +44 1603 507784

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School of Environmental Sciences | web-site:

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Attachment Converted: "c:\eudora\attach\mannpersp2002.gif"

References

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<x-flowed>

This is an excellent idea, Mike, IN PRINCIPLE at least. In practise, however, it raises some interesting results (as I have found when attempting this myself) that may be difficult to avoid getting bogged down with discussing.

The attached .pdf figure shows an example of what I have produced (NB. please don't circulate this further, as it is from work that is currently being finished off - however, I'm happy to use it here to illustrate my point).

I took 7 reconstructions and re-calibrated them over a common period and against an observed target series (in this case, land-only, Apr-Sep, >20N - BUT I GET SIMILAR RESULTS WITH OTHER CHOICES, and this re-calibration stage is not critical). You will have seen figures similar to this in stuff Keith and I have published. See the coloured lines in the attached figure.

In this example I then simply took an unweighted average of the calibrated series, but the weighted average obtained via an EOF approach can give similar results. The average is shown by the thin black line (I've ignored the potential problems of series covering different periods). This was all done with raw, unsmoothed data, even though 30-yr smoothed curves are plotted in the figure.

The thick black line is what I get when I re-calibrate the average record against my target observed series. **THIS IS THE IMPORTANT BIT.** The *re-calibrated* mean of the reconstructions is nowhere near the mean of the reconstructions. It has enhanced variability, because averaging the reconstructions results in a redder time series (there is less common variance between the reconstructions at the higher frequencies compared with the lower frequencies, so the former averages out to leave a smoother curve) and the re-calibration is then more of a case of fitting a trend (over my calibration period 1881-1960) to the observed trend. This results in enhanced variability, but also enhanced uncertainty (not shown here) due to fewer effective degrees of freedom during calibration.

Obviously there are questions about observed target series, which series to include/exclude etc., but the same issue will arise regardless: the

analysis will not likely lie near to the middle of the cloud of published series and explaining the reasons behind this etc. will obscure the message of a short EOS piece.

It is, of course, interesting - not least for the comparison with borehole-based estimates - but that is for a separate paper, I think.

My suggestion would be to stick with one of these options:

- (i) a single example reconstruction;
- (ii) a plot of a cloud of reconstructions;
- (iii) a plot of the "envelope" containing the cloud of reconstructions (perhaps also the envelope would encompass their uncertainty estimates), but without showing the individual reconstruction best guesses.

How many votes for each?

Cheers

Tim

At 15:32 12/03/03, Michael E. Mann wrote:

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>>mike

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>>>>Tom

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>>>> carry a lot of weight. In a way we will be setting the agenda for
>>>> what should be being done
>>>> over the next few years.
>>>> We do want a reputable journal but is The Holocene the right
>>>> vehicle. It is probably the
>>>> best of its class of journals out there. Mike and I were asked to
>>>> write an article for the EGS
>>>> journal of Surveys of Geophysics. You've not heard of this - few
>>>> have, so we declined. However,
>>>> it got me thinking that we could try for Reviews of Geophysics. Need
>>>> to contact the editorial
>>>> board to see if this might be possible. Just a thought, but it
>>>> certainly has a high profile.
>>>> What we want to write is NOT the scholarly review a la Jean Grove
>>>> (bless her soul) that
>>>> just reviews but doesn't come to anything firm. We want a critical
>>>> review that enables
>>>> agendas to be set. Ray's recent multi-authored piece goes a lot of
>>>> the way so we need
>>>> to build on this.

>>>>
>>>> Cheers
>>>> Phil

>>>>
>>>>
>>>>

>>>>>At 12:55 11/03/03 -0500, Michael E. Mann wrote:

>>>>>HI Malcolm,

>>>>>

>>>>>Thanks for the feedback--I largely concur. I do, though, think there
>>>>>is a particular problem with "Climate Research". This is where my
>>>>>colleague Pat Michaels now publishes exclusively, and his two closest
>>>>>colleagues are on the editorial board and review editor board. So I
>>>>>promise you, we'll see more of this there, and I personally think
>>>>>there *is* a bigger problem with the "messenger" in this case...

>>>>>

>>>>>But the Soon and Baliunas paper is its own, separate issue too. I too
>>>>>like Tom's latter idea, of a more hefty multi-authored piece in an
>>>>>appropriate journal (Paleoceanography? Holocene?) that seeks to

>>>>>correct a number of misconceptions out there, perhaps using Baliunas
>>>>>and Soon as a case study ('poster child?'), but taking on a slightly
>>>>>greater territory too.

>>>>>

>>>>>Question is, who would take the lead role. I *know* we're all very busy,

>>>>>

>>>>>mike

>>>>>

>>>>> At 10:28 AM 3/11/03 -0700, Malcolm Hughes wrote:

>>>>>>I'm with Tom on this. In a way it comes back to a rant of mine

>>>>>>to which some of you have already been victim. The general

>>>>>>point is that there are two arms of climatology:

>>>>>> neoclimatology - what you do based on instrumental records

>>>>>>and direct, systematic observations in networks - all set in a

>>>>>>very Late Holocene/Anthropocene time with hourly to decadal

>>>>>>interests.

>>>>>>paleoclimatology - stuff from rocks, etc., where major changes

>>>>>>in the Earth system, including its climate, associated with

>>>>>>major changes in boundary conditions, may be detected by

>>>>>>examination of one or a handful of paleo records.

>>>>>>Between these two is what we do - "mesoclimatology" -

>>>>>>dealing with many of the same phenomena as neoclimatology,

>>>>>>using documentary and natural archives to look at phenomena

>>>>>>on interannual to millennial time scales. Given relatively small

>>>>>>changes in boundary conditions (until the last couple of

>>>>>>centuries), mesoclimatology has to work in a way that is very

>>>>>>similar to neoclimatology. Most notably, it depends on heavily

>>>>>>replicated networks of precisely dated records capable of

>>>>>>being either calibrated, or whose relationship to climate may

>>>>>>be modeled accurately and precisely.

>>>>>>Because this distinction is not recognized by many (e.g.

>>>>>>Sonnechkin, Broecker, Karlen) we see an accumulation of

>>>>>>misguided attempts at describing the climate of recent

>>>>>>millennia. It would be better to head this off in general, rather

>>>>>>than draw attention to a bad paper. After all, as Tom rightly

>>>>>>says, we could all nominate really bad papers that have been

>>>>>>published in journals of outstanding reputation (although there

>>>>>>could well be differences between our lists).

>>>>>>End of rant, Cheers, Malcolm

>>>>>> > Hi guys,

>>>>>> >

>>>>>> > junk gets published in lots of places. I think that what could be

>>>>>> > done is a short reply to the authors in Climate Research OR a SLIGHTLY

>>>>>> > longer note in a reputable journal entitled something like "Continuing

>>>>>> > Misconceptions About interpretation of past climate change." I kind
>>>>>> > of like the more pointed character of the latter and submitting it as
>>>>>> > a short note with a group authorship carries a heft that a reply to a
>>>>>> > paper, in no matter what journal, does not.
>>>>>> >
>>>>>> > Tom
>>>>>> >
>>>>>> >
>>>>>> >
>>>>>> > Dear All,
>>>>>> > Apologies for sending this again. I was expecting a stack of
>>>>>> > emails this morning in
>>>>>> > response, but I inadvertently left Mike off (mistake in pasting)
>>>>>> > and picked up Tom's old
>>>>>> > address. Tom is busy though with another offspring !
>>>>>> > I looked briefly at the paper last night and it is appalling -
>>>>>> > worst word I can think of today
>>>>>> > without the mood pepper appearing on the email ! I'll have time to
>>>>>> > read more at the weekend
>>>>>> > as I'm coming to the US for the DoE CCPP meeting at Charleston.
>>>>>> > Added Ed, Peck and Keith A.
>>>>>> > onto this list as well. I would like to have time to rise to the
>>>>>> > bait, but I have so much else on at
>>>>>> > the moment. As a few of us will be at the EGS/AGU meet in Nice, we
>>>>>> > should consider what
>>>>>> > to do there.
>>>>>> > The phrasing of the questions at the start of the paper
>>>>>> > determine the answer they get. They
>>>>>> > have no idea what multiproxy averaging does. By their logic, I
>>>>>> > could argue 1998 wasn't the
>>>>>> > warmest year globally, because it wasn't the warmest everywhere.
>>>>>> > With their LIA being 1300-
>>>>>> > 1900 and their MWP 800-1300, there appears (at my quick first
>>>>>> > reading) no discussion of
>>>>>> > synchronicity of the cool/warm periods. Even with the instrumental
>>>>>> > record, the early and late
>>>>>> > 20th century warming periods are only significant locally at
>>>>>> > between 10-20% of grid boxes.
>>>>>> > Writing this I am becoming more convinced we should do
>>>>>> > something - even if this is just
>>>>>> > to state once and for all what we mean by the LIA and MWP. I think
>>>>>> > the skeptics will use
>>>>>> > this paper to their own ends and it will set paleo back a number of
>>>>>> >

>>>>>> > >years if it goes
>>>>>> > > unchallenged.
>>>>>> > >
>>>>>> > > I will be emailing the journal to tell them I'm having
>>>>>> > >nothing more to do with it until they
>>>>>> > > rid themselves of this troublesome editor. A CRU person is on the
>>>>>> > >editorial board, but papers
>>>>>> > > get dealt with by the editor assigned by Hans von Storch.
>>>>>> > >
>>>>>> > > Cheers
>>>>>> > > Phil
>>>>>> > >
>>>>>> > > Dear all,
>>>>>> > > Tim Osborn has just come across this. Best to ignore
>>>>>> > >probably, so don't let it spoil your
>>>>>> > > day. I've not looked at it yet. It results from this journal
>>>>>> > >having a number of editors. The
>>>>>> > > responsible one for this is a well-known skeptic in NZ. He has let
>>>>>> > >
>>>>>> > >a few papers through by
>>>>>> > > Michaels and Gray in the past. I've had words with Hans von Storch
>>>>>> > >
>>>>>> > >about this, but got nowhere.
>>>>>> > > Another thing to discuss in Nice !
>>>>>> > >
>>>>>> > > Cheers
>>>>>> > > Phil
>>>>>> > >
>>>>>> > >>X-Sender: f055@pop.uea.ac.uk
>>>>>> > >>X-Mailer: QUALCOMM Windows Eudora Version 5.1
>>>>>> > >>Date: Mon, 10 Mar 2003 14:32:14 +0000
>>>>>> > >>To: p.jones@uea
>>>>>> > >>From: Tim Osborn <t.osborn@uea.ac.uk>
>>>>>> > >>Subject: Soon & Baliunas
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>>>>

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Daer Armin,

I would like to confirm that RIVM is strongly committed to make a
substantial contribution to the AMS proposal, as was clear from our
active
involvement in the discussions so far (except the Paris meeting where we
unfortunately could not send a representative). We have been in touch
with
several other partners in developing ideas for the workpackage, but in
view
of the high pressure under which the proposal is being put together,
communication is not always easy. I therefore include a list of elements
we
would like to contribute to the respective parts of the proposal:.

WP1. Scenarios: involved with proposal Brian O'Neill (contact:
Detlef
van Vuuren). Important issues: delineation with scenarios in other
workpackages - no response so far.

WP 3.1. Possible contribution, depends on connection with WP1
3.3. Primarily through cooperation with Un.Utrecht - proposal sent
to
Wokaun but no response. Possible to add global context with IMAGE/TIMER
and
add non-energy emssion reductions not covered in original proposal by
Wokaun

3. 4. and 3.5: as for 3.3

WP 4.1. Suggested role for multi-gas stabilization profiles, burden sharing regimes and EU action with IMAGE-FAIR combination (building on work

we have done with other partners for the European Commission). Current proposal by Haxeltine, Leemans and Adger has 100% focus on impacts and adaptation and should be broadened. We are ready to contribute

4.2. Now contains the regimes that should go under 4.1

4.6. Involved actively: see proposal Olsson&Metz that went to John Schellhuber

WP 5.4. Strong interest, but no response from coordinator (C. Jaeger) and

WP coordinator Hasselmann refers back to CJ (!). We will put together proposal with Tyndall towards development of CIAS model.

Best regards,

Bert Metz

From: Earth Government <earthgov@shaw.ca>
Subject: Press release from Earth Government and April Newsletter
Date: Thu, 27 Mar 2003 16:05:07 -0800

Press release from Earth Government and April Newsletter
FOR IMMEDIATE RELEASE

This Press release from Earth Government is found at
[1]<http://members.shaw.ca/earthgov/HNewsPR05.htm>

Formation of Earth Government for the good of all

March 27th, 2003

To all Peoples of the Earth,

Earth has long been waiting for a truly global governing body based on universal values, human rights, global concepts and democracy. Earth Government might as well be created now, there is no longer any reason to wait. We are the Earth Community, and we will form the Earth Government. Earth management is a priority and is a duty by every responsible person. A democratically elected Earth Government will now be formed, and we want you to reflect on future effects of such an event on the history of humanity. Certainly one will expect extraordinary changes: a reorganizing of human activities all over the planet; participation by all societies on the planet in solving local and global problems; new alliances forming; north meeting with south (eradication of poverty will be the price to pay to get votes from the south) in order to gather more votes within the newly created Earth Government to satisfy power struggles between European, Asian and Western countries; adoption of democratic principles, human and Earth rights, global concepts, and universal values by every human being; expansion of consciousness; gathering and coordinating of forces to resolve social and political problems in a peaceful way (no more conflicts or wars); gathering and coordinating of forces (technologies, scientific research, exploration work, human resources, etc.) to resolve global problems such as global climate, environment, availability of resources, poverty, employment, etc. Thousands more changes!

Let your heart and mind reflect on 'the good' of a democratically elected Earth Government. Everyone is part of Earth Community by birth and therefore everyone has a right to vote. Everyone should be given a chance to vote. Decisions will be made democratically.

Earth Government is proposing that:

- a) different nations may require different political systems at different times
- b) a democratic system is not a "must have it" to be a responsible member nation of the Earth Government
- c) all democracies are to be upgraded, or improved upon, to be a responsible member nation of the Earth Government. The Scale of Human and Earth Rights and the Charter of the Earth

Government are the newly added requirements to all democratic systems of the world.

In today's Earth Government it is important for our survival to cooperate globally on several aspects such as peace, security, pollution in the air, water and land, drug trade, shelving the war industry, keeping the world healthy, enforcing global justice for all, eradicating poverty worldwide, replacing the Universal Declaration of Human Rights by the Scale of Human and Earth Rights, and entrenching the Charter of Earth Government as a way of life for the good of all.

Earth needs urgently a world system of governance. The United Nations fail to satisfy the needs of the people of the 21st Century. It has never improved upon the old ways and thinking of the middle of the 20th Century. Its voting system no longer satisfy the 6.157 billion people on Earth. The challenges are different and require a world organization up for dealing with the needs of all these people.

During the past several years, the Earth Government has been pleading the United Nations leaders to make changes in the UN organizational structure and ways of doing things. There has been an urgent need for fundamental changes in the United Nations organization. The decision of the United States Government to invade the Middle East nations and Afghanistan has shown to be a result of this incapacity for changes on the part of the United Nations. A lack of leadership at the United Nations is a major threat to the security of the world. The world wants a true democratic world organization. The UN is not!

The most fundamental requirement of a world organization is a democratic system of voting. Democracy must be a priority. The right that the greatest number of people has by virtue of its number (50% plus one) is a human right. It should be respected. The actual UN system of voting is undemocratic, unfair and noone likes it. It does not work! Earth Government has proposed a voting system based on democracy.

Of the 190 Member States of the United Nations, it takes only one of the five permanent members to overthrow any decision or proposal during a meeting. This means 1/189 or 0.5% of the membership is more powerful than the remaining 99.5%. If that is not a dictatorship, what is it? It does not say much about democracy at the UN. More like a dictatorship of the five permanent members. In the Preamble of the Charter of the United Nations, it says "WE THE PEOPLES OF THE UNITED NATIONS " but in fact it should say "WE THE FIVE PERMANENT MEMBERS".

The voting system for Earth Government is very simple and practical. One representative per million people. If all countries in the world had decided now to participate with this process we would have today 6,114 elected representatives to form Earth Government. They would form the Legislative body of Earth Government. They could actually all stay home to govern or from some place in their communities. Today communications are more than good enough to allow voting and discussing issues, etc. through the Internet and video conferencing. That would cut cost of governing down to a minimum, at least administrative costs. The Executive body would also govern in this way to cut cost down to a minimum. Ministers can administer their Ministries from where they live if they wish to. There will be a place for the Headquarters. We will show that it costs very little to administer Earth Government, and that we can achieve immense results. There is no limit to the good the Earth Government can achieve in the world. Think! What can do a unified 6.114 billion

people determined to make things work to keep Earth healthy?

For the first time in human history, and the first time this millennium, humanity has proposed a benchmark:

- * formation of Earth Government
- * formation of global ministries in all important aspects of our lives
- * the Scale of Human and Earth Rights as a replacement to the Universal Declaration of Human Rights
- * an evolved Democracy based on the Scale of Human and Earth Rights and the Charter of the Earth Government
- * a central organization for Earth management, the restoration of the planet and Earth governance: the Global Community Assessment Centre (GCAC)
- * the Earth Court of Justice to deal with all aspects of the Governance and Mangement of the Earth
- * a new impetus given to the way of doing business and trade
- * more new, diversified (geographical, economical, political, social, business, religious) symbiotical relationships between nations, communities, businesses, for the good and well-being of all
- * the event and formation of the human family and the Soul of Humanity
- * proposal to reform the United Nations, the World Trade Organization, the World Bank, the IMF, NAFTA, FTAA, and to centralize them under Earth Government, and these organizations will be asked to pay a global tax to be administered by Earth Government
- * the Peace Movement of the Earth Government and shelving of the war industry from humanity
- * a global regulatory framework for capitals and corporations that emphasizes global corporate ethics, corporate social responsibility, protection of human and Earth rights, the environment, community and family aspects, safe working conditions, fair wages and sustainable consumption aspects
- * the ruling by the Earth Court of Justice of the abolishment of the debt of the poor or developing nations as it is really a form of global tax to be paid annually by the rich or industrialized nations to the developing nations
- * establishing freshwater and clean air as primordial human rights

The political system of an individual country does not have to be a democracy. Political rights of a country belong to that country alone. Democracy is not to be enforced by anyone and to anyone or to any community. Every community can and should choose the political system of their choice with the understanding of the importance of such a right on the Scale of Human and Earth Rights. On the other hand, representatives to Earth Government must be elected democratically in every part of the world. An individual country may have any political system at home but the government of that country will have to ensure (and allow verification by Earth Government) that representatives to Earth Government have been elected democratically. This way, every person in the world can claim the birth right of electing a democratic government to manage Earth: the rights to vote and elect representatives to form the Earth Government.

In order to elect representatives to Earth Government it is proposed the following:

A. Each individual government in the world will administer the election of representatives to Earth Government with an NGO and/or members of Earth Government be allowed to verify all aspects of the process to the satisfaction of all parties involved.

B. Representatives be elected every five years to form a new Earth Government.

C. It is proposed here that there will be one elected representative per 1,000,000 people. A population of 100 million people will elect 100 representatives. This process will create a feeling of belonging and participating to the affairs of the Earth Community and Earth Government.

D. A typical community of a million people does not have to be bounded by a geographical or political border. It can be a million people living in many different locations all over the world. The Global Community is thus more fluid and dynamic. We need to let go the archaic ways of seeing a community as the street where I live and contained by a border. Many conflicts and wars will be avoided by seeing ourselves as people with a heart, a mind and a Soul, and as part of a community with the same.

E. Earth population is now 6.114 billion people. If all representatives had been elected this year there would be 6,114 representatives to form Earth Government. They would be the Legislative elected body of Earth Government. They would participate in some ways in choosing the Executive and Judiciary bodies of Earth Government.

Humanity has now a Vision of the Earth in the years to come and a sense of direction.

May the DIVINE WILL come into our lives and show us the way.

May our higher purpose in life bring us closer to the Soul of Humanity and God.

Germain Dufour, President

Earth Community Organization (ECO) and Earth Government

The Newsletter can be found at the following location:

April 2003 Newsletter

[2]<http://members.shaw.ca/earthgov/NewsA.htm>

There are no costs in reading our Newsletters

([3]<http://members.shaw.ca/earthgov/EarthGovernment.htm>).

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4.0 Letter to all Canadians concerning the total and global embargo on all US products, all goods and services

- 5.0 Letter to the Moslem and the Arab Peoples
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 - L) The Earth Court of Justice holds the people of the U.S.A. and Britain as criminals
 - M) Foundation for the new world order, Earth Government

Improved Democracy, Nonviolence, and Peace

Respect and Care for the Global Community of Life

Ecological Integrity

Social and Economic Justice

A new symbiotical relationship between that of spirituality and the protection of the global life-support systems

Scale of Human and Earth Right

Earth Court of Justice

Charter of Earth Government

May the DIVINE WILL come into our lives and show us the way.

May our higher purpose in life bring us closer to the Soul of Humanity and God.

Germain Dufour, President

[4]Earth Community Organization (ECO) and [5]Earth Government

Website of the Earth Community Organization and of Earth Government

[6]<http://www.telusplanet.net/public/gdufour/>

[7]<http://members.shaw.ca/earthgov>

Email addresses

[8]gdufour@globalcommunitywebnet.com

[9]gdufour@telusplanet.net

[10]earthgov@shaw.ca

References

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5. <http://members.shaw.ca/earthgov>
6. <http://www.telusplanet.net/public/gdufour/>
7. <http://members.shaw.ca/earthgov>
8. <mailto:gdufour@globalcommunitywebnet.com>
9. <mailto:gdufour@telusplanet.net>
10. <mailto:earthgov@shaw.ca>

From: "Eystein Jansen" <eystein.jansen@geo.uib.no>
To: "Keith Briffa" <k.briffa@uea.ac.uk>
Subject: Re: Re: Holclim follow up
Date: Mon 7 Apr 2003 16:04

Dear Keith.

I had a chat with Dominique Reynaud on this matter today here in Nice. His impression is the same, but added that he thinks Brussels would insist on a NoE rather than an IP. If we wish to have an IP it needs lobbying it seems. He told about the meeting in Brussels in June. I am not invited as far as I can tell. Dominique mentioned that Nick Shackleton would be there and I will talk with him. The key thing would be to sort out what the most exciting science our community can offer when we integrate the communities.

In terms of meetings it seems to depend a little of what comes out of the June meeting in Brussels.

Cheers

Eystein

>----- Original Message ---

>From: Keith Briffa <k.briffa@uea.ac.uk>

>To: Eystein Jansen <eystein.jansen@geo.uib.no>

>Subject: Re: Holclim follow up

>

>

>Eystein

>your point is exactly correct , that only one project (and I believe it=20

>should be an IP) will be allowed and with the shrinking general scale of=20

>these things, it likely needs to be very clearly focused (on integrating=20

>evidence and providing some state-of-the-art product on climate history and=

>=20

>its causes) . I am not in Nice (have to go to 2 other meetings in May) .

I=

>=20

>am still leaning towards your institute co-ordinating this . I have not=20

>discussed anything with the rest of the HOLIVAR committee.

>We do need some sort of meeting but only small - there is no chance of a 25=

>=20

>million Euro project and many people are likely to be disappointed . I have=

>=20

>to be in Brussels for a meeting with Brelen in June . What are you thinking=

>=20

>about , re. a meeting?

>Keith

>At 10:01 PM 4/3/03 +0200, you wrote:

>>Dear Keith,

>> I was just wondering whether you were coming the the EGS meeting in
Nice=
>=20
>> next week, in order for us to exchange some ideas about how to
proceed=20
>> for FP6. Recent rumors says that the palaeoclimate variability item is
in=
>=20
>> the books for the third call, and that the call will be issued by
the=20
>> turn of the year, thus we should start discussing how to proceed. So
far=
>=20
>> my DOCC initiative is dormant, and I am more inclined to develop or
take=
>=20
>> part in developing an IP if the call for proposals allow for one. But
the=
>=20
>> size of these IPs seems to be diminishing, hence a careful
focussing=20
>> needs to be undertaken in order for there to be resources for the
science=
>=20
>> teams. I would be happy to discuss idea with you on this in Nice or=20
>> sometime else if you=B4re not there.
>>
>>Cheers,
>>Eystein
>>
>>
>>
>>Eystein Jansen
>>prof/director
>>Bjerknes Centre for Climate Research
>>All=E9gaten 55, N5007 Bergen, Norway
>>tel: +4755583491/secr:+4755589803/fax:+4755584330
>>eystein.jansen@geo.uib.no, www.bjerknes.uib.no
>
>--
>Professor Keith Briffa,
>Climatic Research Unit
>University of East Anglia
>Norwich, NR4 7TJ, U.K.
>
>Phone: +44-1603-593909
>Fax: +44-1603-507784
>
><http://www.cru.uea.ac.uk/cru/people/briffa/>
>
>

From: Tom Wigley <wigley@ucar.edu>
To: Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, James Hansen <jhansen@giss.nasa.gov>, Danny Harvey <harvey@cirque.geog.utoronto.ca>, Ben Santer <santer1@llnl.gov>, Kevin Trenberth <trenbert@ucar.edu>, Robert Wilby <rob.wilby@kcl.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, Tom Karl <Thomas.R.Karl@noaa.gov>, Steve Schneider <shs@stanford.edu>, Tom Crowley <tcrowley@duke.edu>, jto <jto@u.arizona.edu>, "simon.shackley" <simon.shackley@umist.ac.uk>, "tim.carter" <tim.carter@vyh.fi>, "p.martens" <p.martens@icis.unimaas.nl>, "peter.whetton" <peter.whetton@dar.csiro.au>, "c.goodess" <c.goodess@uea.ucar.edu>, "a.minns" <a.minns@uea.ac.uk>, Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>, "j.salinger" <j.salinger@niwa.co.nz>, "simon.torok" <simon.torok@csiro.au>, Mark Eakin <mark.eakin@noaa.gov>, Scott Rutherford <srutherford@deschutes.geo.uri.edu>, Neville Nicholls <n.nicholls@bom.gov.au>, Ray Bradley <rbradley@geo.umass.edu>, Mike MacCracken <mmaccrac@comcast.net>, Barrie Pittock <Barrie.Pittock@csiro.au>, Ellen Mosley-Thompson <thompson4@osu.edu>, "pachauri@teri.res.in" <pachauri@teri.res.in>, "Greg.Ayers" <Greg.Ayers@csiro.au>
Subject: My turn
Date: Wed, 23 Apr 2003 23:53:38 -0600

Dear friends,

[Apologies to those I have missed who have been part of this email exchange -- although they may be glad to have been missed]

I think Barrie Pittock has the right idea -- although there are some unique things about this situation. Barrie says

- (1) There are lots of bad papers out there
- (2) The best response is probably to write a 'rebuttal'

to which I add

- (3) A published rebuttal will help IPCC authors in the 4AR.

Let me give you an example. There was a paper a few years ago by Legates and Davis in GRL (vol. 24, pp. 2319-1222, 1997) that was nothing more than a direct

and pointed criticism of some work by Santer and me -- yet neither of us was asked to review the paper. We complained, and GRL admitted it was poor judgment on the part of the editor. Eventually (> 2 years later) we wrote a response (GRL 27, 2973-2976, 2000). However, our response was more than just a rebuttal, it was an attempt to clarify some issues on detection. In doing things this way we tried to make it clear that the original Legates/Davis paper was an example of bad science (more bluntly, either sophomoric ignorance or deliberate misrepresentation).

Any rebuttal must point out very clearly the flaws in the original

paper. If some new science (or explanations) can be added -- as we did in the above example -- then this is an advantage.

There is some personal judgment involved in deciding whether to rebut. Correcting bad science is the first concern. Responding to unfair personal criticisms is next. Third is the possible misrepresentation of the results by persons with ideological or political agendas. On the basis of these I think the Baliunas paper should be rebutted by persons with appropriate expertise. Names like Mann, Crowley, Briffa, Bradley, Jones, Hughes come to mind. Are these people willing to spend time on this?

There are two other examples that I know of where I will probably be involved in writing a response.

The first is a paper by Douglass and Clader in GRL (vol. 29, no. 16, 10.1029/2002GL015345, 2002). I refereed a virtually identical paper for J. Climate, recommending rejection. All the other referees recommended rejection too. The paper is truly appalling -- but somehow it must have been poorly reviewed by GRL and slipped through the net. I have no reason to believe that this was anything more than chance. Nevertheless, my judgment is that the science is so bad that a response is necessary.

The second is the paper by Michaels et al. that was in Climate Research (vol. 23, pp. 109, 2002). Danny Harvey and I refereed this and said it should be rejected. We questioned the editor (deFreitas again!) and he responded saying

The MS was reviewed initially by five referees. ... The other three referees, all reputable atmospheric scientists, agreed it should be published subject to minor revision. Even then I used a sixth person to help me decide. I took his advice and that of the three other referees and sent the MS back for revision. It was later accepted for publication. The refereeing process was more rigorous than usual.

On the surface this looks to be above board -- although, as referees who advised rejection it is clear that Danny and I should have been kept in the loop and seen how our criticisms were responded to.

It is possible that Danny and I might write a response to this paper -- deFreitas has offered us this possibility.

This second case gets to the crux of the matter. I suspect that deFreitas deliberately chose other referees who are members of the skeptics camp. I also suspect that he has done this on other occasions. How to deal with this is unclear, since there are a number of individuals with bona fide scientific credentials who could be used by an unscrupulous editor to ensure that 'anti-greenhouse' science can get

through the peer review process (Legates, Balling, Lindzen, Baliunas, Soon, and so on).

The peer review process is being abused, but proving this would be difficult.

The best response is, I strongly believe, to rebut the bad science that does get through.

Jim Salinger raises the more personal issue of deFreitas. He is clearly giving good science a bad name, but I do not think a barrage of ad hominem attacks or letters is the best way to counter this.

If Jim wishes to write a letter with multiple authors, I may be willing to sign it, but I would not write such a letter myself.

In this case, deFreitas is such a poor scientist that he may simply disappear. I saw some work from his PhD, and it was awful (Pat Michaels' PhD is at the same level).

Best wishes to all,
Tom.

From: Tom Wigley <wigley@ucar.edu>
To: Timothy Carter <tim.carter@ymparisto.fi>
Subject: Re: Java climate model
Date: Thu, 24 Apr 2003 09:17:29 -0600
Cc: Mike Hulme <m.hulme@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>

Tim,

I know about what Matthews has done. He did so without contacting Sarah or me. He uses a statistical emulation method that can never account for the full range of uncertainties. I would not trust it outside the calibration zone -- so I doubt that it can work well for (e.g.) stabilization cases. As far as I know it has not been peer reviewed. Furthermore, unless he has illegally got hold of the TAR version of the model, what he has done can only be an emulation of the SAR version.

Personally, I regard this as junk science (i.e., not science at all).

Matthews is doing the community a considerable disservice.

Tom.

PS Re CR, I do not know the best way to handle the specifics of the editing. Hans von Storch is partly to blame -- he encourages the publication of crap science 'in order to stimulate debate'. One approach is to go direct to the publishers and point out the fact that their journal is perceived as being a medium for disseminating misinformation under the guise of refereed work. I use the word 'perceived' here, since whether it is true or not is not what the publishers care about -- it is how the journal is seen by the community that counts.

I think we could get a large group of highly credentialed scientists to sign such a letter -- 50+ people.

Note that I am copying this view only to Mike Hulme and Phil Jones. Mike's idea to get editorial board members to resign will probably not work -- must get rid of von Storch too, otherwise holes will eventually fill up with people like Legates, Balling, Lindzen, Michaels, Singer, etc. I have heard that the publishers are not happy with von Storch, so the above approach might remove that hurdle too.

Timothy Carter wrote:

>
> Dear Tom,
>
> Since you were online yesterday contributing to the "Climate Research"
> discussion, I figured that you might be in town to give your views on the
> Java Climate Model which, I understand, is based in large part on MAGICC:
>
> <http://chooseclimate.org/jcm/>
>
> and seems to be getting considerable exposure amongst the policy community
> now that Ben Matthews (was he a student of yours at UEA?) has made this
> available online.
>
> I wondered if this has been subjected to "peer review" by the people whose
> models it is based on or anyone else, since I have Ministry people here in
> Finland asking me if this type of tool is something they should think of
> using during the negotiating process!
>
> It's certainly a smart piece of software, though it seems to have
> irritating bugs, like returning to the default state when any little thing
> is adjusted. What is critically important, though, is that it can do what
> it is advertising. If it can't, then the careful work done offline by
> people such as yourself, could be undermined.
>
> Any thoughts?
>
> Best regards from a sunny though cool Helsinki.
>
> Tim
>
> P.S. On the CR issue, I agree that a rebuttal seems to be the only method
> of addressing the problem (I communicated this to Mike yesterday morning),
> and I wonder if a review of the refereeing policy is in order. The only way
> I can think of would be for all papers to go through two Editors rather
> than one, the former to have overall responsibility, the latter to provide
> a second opinion on a paper and reviewers' comments prior to publication. A
> General Editor would be needed to adjudicate in the event of disagreement.
> Of course, this could then slow down the review process enormously.
> However, without an editorial board to vote someone off, how can suspect

> Editors be removed except by the Publisher (in this case, Inter-Research).

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: mark.eakin@noaa.gov
Subject: Re: My turn
Date: Thu, 24 Apr 2003 12:39:14 -0400
Cc: Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, James Hansen <jhansen@giss.nasa.gov>, Danny Harvey <harvey@cirque.geog.utoronto.ca>, Ben Santer <santer1@llnl.gov>, Kevin Trenberth <trenbert@ucar.edu>, Robert Wilby <rob.wilby@kcl.ac.uk>, Tom Karl <Thomas.R.Karl@noaa.gov>, Steve Schneider <shs@stanford.edu>, Tom Crowley <tcrowley@duke.edu>, jto <jto@u.arizona.edu>, "simon.shackley" <simon.shackley@umist.ac.uk>, "tim.carter" <tim.carter@vyh.fi>, "p.martens" <p.martens@icis.unimaas.nl>, "peter.whetton" <peter.whetton@dar.csiro.au>, "c.goodess" <c.goodess@uea.ac.uk>, "a.minns" <a.minns@uea.ac.uk>, Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>, "j.salinger" <j.salinger@niwa.co.nz>, "simon.torok" <simon.torok@csiro.au>, Scott Rutherford <srutherford@deschutes.gso.uri.edu>, Neville Nicholls <n.nicholls@bom.gov.au>, Ray Bradley <rbradley@geo.umass.edu>, Mike MacCracken <mmaccrac@comcast.net>, Barrie Pittock <Barrie.Pittock@csiro.au>, Ellen Mosley-Thompson <thompson.4@osu.edu>, "pachauri@teri.res.in" <pachauri@teri.res.in>, "Greg.Ayers" <Greg.Ayers@csiro.au>, wuebbles@atmos.uiuc.edu, christopher.d.miller@noaa.gov, mann@virginia.edu

<x-flowed>
HI Mark,

Thanks for your comments, and sorry to any of you who don't wish to receive these correspondances...

Indeed, I have provided David Halpern with a written set of comments on the offending paper(s) for internal use, so that he was armed w/ specifics as he confronts the issue within OSTP. He may have gotten additional comments from other individuals as well--I'm not sure. I believe that the matter is in good hands with Dave, but we have to wait and see what happens. In any case, I'd be happy to provide my comments to anyone who is interested.

I think that a response to "Climate Research" is not a good idea. Phil and I discussed this, and agreed that it would be largely unread, and would tend to legitimize a paper which many of us don't view as having passed peer review in a legitimate manner. On the other hand, the in prep. review articles by Jones and Mann (Rev. Geophys.), and Bradley/Hughes/Diaz (Science) should go along way towards clarification of the issues (and, at least tangentially, refutation of the worst of the claims of Baliunas and co). Both should be good resources for the FAR as well...

cheers,

mike

p.s. note the corrections to some of the emails in the original distribution list.

At 09:27 AM 4/24/03 -0600, Mark Eakin wrote:

>At this point the question is what to do about the Soon and Baliunas
>paper. Would Bradley, Mann, Hughes et al. be willing to develop and
>appropriate rebuttal? If so, the question at hand is where it would be
>best to direct such a response. Some options are:

>

>1) A rebuttal in Climate Research

>2) A rebuttal article in a journal of higher reputation

>3) A letter to OSTP

>

>The first is a good approach, as it keeps the argument to the level of
the

>current publication. The second would be appropriate if the Soon and
>Baliunas paper were gaining attention at a more general level, but it is
>not. Therefore, a rebuttal someplace like Science or Nature would
>probably do the opposite of what is desired here by raising the
attention

>to the paper. The best way to take care of getting better science out in
a

>widely read journal is the piece that Bradley et al. are preparing for
>Nature. This leaves the idea of a rebuttal in Climate Research as the
>best published approach.

>

>A letter to OSTP is probably in order here. Since the White House has
>shown interest in this paper, OSTP really does need to receive a
measured,

>critical discussion of flaws in Soon and Baliunas' methods. I agree
with

>Tom that a noted group from the detection and attribution effort such as
>Mann, Crowley, Briffa, Bradley, Jones and Hughes should spearhead such a
>letter. Many others of us could sign on in support.

>This would provide Dave Halpern with the ammunition he needs to provide
>the White House with the needed documentation that hopefully will
dismiss

>this paper for the slipshod work that it is. Such a letter could be
>developed in parallel with a rebuttal article.

>

>I have not received all of the earlier e-mails, so my apologies if I am
>rehashing parts of the discussion that might have taken place elsewhere.

>

>Cheers,

>Mark

>

>

>

>Michael E. Mann wrote:

>

>>Dear Tom et al,
>>
>>Thanks for comments--I see we've built up an impressive distribution
list
>>here!
>>
>>This seemed like an appropriate point for me to chime in here. By in
>>large, I agree w/ Tom's comments (and those of Barrie's as well). A
>>number of us have written reviews and overviews of this topic during
the
>>past couple years. There has been a lot of significant scientific
process
>>in this area (both with regard to empirical "climate reconstruction"
and
>>in the area of model/data comparison), including, in fact, detection
>>studies along the lines of what Barrie Pittock asked about in a
previous
>>email (see. e.g. Tom Crowley's Science article from 2000). Phil Jones
and
>>I are in the process of writing a review article for /Reviews of
>>Geophysics/ which will, among other things, dispel the most severe of
the
>>myths that some of these folks are perpetuating regarding past climate
>>change in past centuries. My understanding is that Ray Bradley, Malcolm
>>Hughes, and Henry Diaz are working, independently, on a solicited piece
>>for /Science/ on the "Medieval Warm Period".
>>Many have simply dismissed the Baliunas et al pieces because, from a
>>scientific point of view, they are awful--that is certainly true. For
>>example, Neville has pointed out in a previous email, that the
standard
>>they applied for finding "a Medieval Warm Period" was that a particular
>>proxy record exhibit a 50 year interval during the period AD 800-1300
>>that was anomalously *warm*, *wet*, or *dry* relative to the "20th
>>century" (many of the proxy records don't really even resolve the late
>>20th century!) could be used to define an "MWP" anywhere one might like
>>to find one. This was the basis for their press release arguing for a
>>"MWP" that was "warmer than the 20th century" (a non-sequitur even from
>>their awful paper!) and for their bashing of IPCC and scientists who
>>contributed to IPCC (which, I understand, has been particularly
viscious
>>and ad hominem inside closed rooms in Washington DC where their words
>>don't make it into the public record). This might all seem laughable,
it
>>weren't the case that they've gotten the (Bush) White House Office of
>>Science & Technology taking it as a serious matter (fortunately, Dave
>>Halpern is in charge of this project, and he is likely to handle this
>>appropriately, but without some external pressure).
>>
>>So while our careful efforts to debunk the myths perpetuated by these
>>folks may be useful in the FAR, they will be of limited use in
fighting
>>the disinformation campaign that is already underway in Washington DC.
>>Here, I tend to concur at least in spirit w/ Jim Salinger, that other

>>approaches may be necessary. I would emphasize that there are indeed, as
>>Tom notes, some unique aspects of this latest assault by the skeptics
>>which are cause for special concern. This latest assault uses a
>>compromised peer-review process as a vehicle for launching a scientific
>>disinformation campaign (often viscious and ad hominem) under the guise
>>of apparently legitimately reviewed science, allowing them to make use
of
>>the "Harvard" moniker in the process. Fortunately, the mainstream media
>>never touched the story (mostly it has appeared in papers owned by
>>Murdoch and his crowd, and dubious fringe on-line outlets). Much like
a
>>server which has been compromised as a launching point for computer
>>viruses, I fear that "Climate Research" has become a hopelessly
>>compromised vehicle in the skeptics' (can we find a better word?)
>>disinformation campaign, and some of the discussion that I've seen
(e.g.
>>a potential threat of mass resignation among the legitimate members of
>>the CR editorial board) seems, in my opinion, to have some potential
merit.
>>
>>This should be justified not on the basis of the publication of science
>>we may not like of course, but based on the evidence (e.g. as provided
by
>>Tom and Danny Harvey and I'm sure there is much more) that a legitimate
>>peer-review process has not been followed by at least one particular
>>editor. Incidentally, the problems alluded to at GRL are of a different
>>nature--there are simply too many papers, and too few editors w/
>>appropriate disciplinary expertise, to get many of the papers submitted
>>there properly reviewed. Its simply hit or miss with respect to whom
the
>>chosen editor is. While it was easy to make sure that the worst
papers,
>>perhaps including certain ones Tom refers to, didn't see the light of
the
>>day at /J. Climate/, it was inevitable that such papers might slip
>>through the cracks at e.g. GRL--there is probably little that can be
done
>>here, other than making sure that some qualified and responsible
climate
>>scientists step up to the plate and take on editorial positions at GRL.
>>
>>best regards,
>>
>>Mike
>>
>>At 11:53 PM 4/23/2003 -0600, Tom Wigley wrote:
>>
>>>Dear friends,
>>>
>>>[Apologies to those I have missed who have been part of this email
>>>exchange -- although they may be glad to have been missed]
>>>
>>>I think Barrie Pittock has the right idea -- although there are some

>>>unique things about this situation. Barrie says
>>>
>>>(1) There are lots of bad papers out there
>>>(2) The best response is probably to write a 'rebuttal'
>>>
>>>to which I add
>>>
>>>(3) A published rebuttal will help IPCC authors in the 4AR.
>>>
>>>_____

>>>
>>>Let me give you an example. There was a paper a few years ago by Legates and Davis in GRL (vol. 24, pp. 2319-1222, 1997) that was nothing more than a direct and pointed criticism of some work by Santer and me -- yet neither of us was asked to review the paper. We complained, and GRL admitted it was poor judgment on the part of the editor. Eventually (> 2 years later) we wrote a response (GRL 27, 2973-2976, 2000). However, our response was more than just a rebuttal, it was an attempt to clarify some issues on detection. In doing things this way we tried to make it clear that the original Legates/Davis paper was an example of bad science (more bluntly, either sophomoric ignorance or deliberate misrepresentation).
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>>>Any rebuttal must point out very clearly the flaws in the original paper. If some new science (or explanations) can be added -- as we did in the above example -- then this is an advantage.
>>>
>>>_____

>>>
>>>There is some personal judgment involved in deciding whether to rebut. Correcting bad science is the first concern. Responding to unfair personal criticisms is next. Third is the possible misrepresentation of the results by persons with ideological or political agendas. On the basis of these I think the Baliunas paper should be rebutted by persons with appropriate expertise. Names like Mann, Crowley, Briffa, Bradley, Jones, Hughes come to mind. Are these people willing to spend time on this?
>>>
>>>_____

>>>
>>>There are two other examples that I know of where I will probably be involved in writing a response.
>>>
>>>The first is a paper by Douglass and Clader in GRL (vol. 29, no. 16, 10.1029/2002GL015345, 2002). I refereed a virtually identical paper for J. Climate, recommending rejection. All the other referees recommended rejection too. The paper is truly appalling -- but somehow it must have

>>>been poorly reviewed by GRL and slipped through the net. I have no
>>>reason to believe that this was anything more than chance.
Nevertheless,
>>>my judgment is that the science is so bad that a response is
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>>>
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Research
>>>(vol. 23, pp. 19, 2002). Danny Harvey and I refereed this and said it
>>>should be rejected. We questioned the editor (deFreitas again!) and he
>>>responded saying

>>>
>>>The MS was reviewed initially by five referees. ... The other three
>>>referees, all reputable atmospheric scientists, agreed it should be
>>>published subject to minor revision. Even then I used a sixth person
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>>>
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From: j.salinger@niwa.co.nz
To: Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Mike Hulme <m.hulme@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, James Hansen <jhansen@giss.nasa.gov>, Danny Harvey <harvey@cirque.geog.utoronto.ca>, Ben Santer <santer1@llnl.gov>, Kevin Trenberth <trenbert@ucar.edu>, Robert Wilby <rob.wilby@kcl.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, Tom Karl <Thomas.R.Karl@noaa.gov>, Steve Schneider <shs@stanford.edu>, Tom Crowley <tcrowley@duke.edu>, jto <jto@u.arizona.edu>, "simon.shackley" <simon.shackley@umist.ac.uk>, "tim.carter" <tim.carter@vyh.fi>, "p.martens" <p.martens@icis.unimaas.nl>, "peter.whetton" <peter.whetton@dar.csiro.au>, "c.goodess" <c.goodess@uea.ucar.edu>, "a.minns" <a.minns@uea.ac.uk>, Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>, "j.salinger" <j.salinger@niwa.co.nz>, "simon.torok" <simon.torok@csiro.au>, Mark Eakin <mark.eakin@noaa.gov>, Scott Rutherford <srutherford@deschutes.geo.uri.edu>, Neville Nicholls <n.nicholls@bom.gov.au>, Ray Bradley <rbradley@geo.umass.edu>, Mike MacCracken <mmaccrac@comcast.net>, Barrie Pittock <Barrie.Pittock@csiro.au>, Ellen Mosley-Thompson <thompson4@osu.edu>, "pachauri@teri.res.in" <pachauri@teri.res.in>, "Greg.Ayers" <Greg.Ayers@csiro.au>, Tom Wigley <wigley@ucar.edu>
Subject: And again from the south!
Date: Thu, 24 Apr 2003 20:28:20 +1200

Dear friends and colleagues

This will be the last from me for the moment and I believe we are all arriving at a consensus voiced by Tom, Barrie, Neville et al., from excellent discussions.

Firstly both Danny and Tom have complained to de Freitas about his editorial decision, which does not uphold the principles of good science. Tom has shared the response. I would be curious to find out who the other four cited are - but a rebuttal would be excellent.

Ignoring bad science eventually reinforces the apparent 'truth' of that bad science in the public mind, if it is not corrected. As importantly, the 'bad science' published by CR is used by the sceptics' lobbies to 'prove' that there is no need for concern over climate change. Since the IPCC makes it quite clear that there are substantial grounds for concern about climate change, is it not partially the responsibility of climate science to make sure only satisfactorily peer-reviewed science appears in scientific publications? - and to refute any inadequately reviewed and wrong articles that do make their way through the peer review process?

I can understand the weariness which the ongoing sceptics' onslaught would induce in anyone, scientist or not. But that's no excuse for ignoring bad science. It won't go away, and the more we ignore it the more traction it will gain in the minds of the general public, and the UNFCCC negotiators. If science doesn't uphold the purity of science, who will?

We Australasians (including Tom as an ex pat) have suggested

some courses of action. Over to you now in the north to assess the success of your initiatives, the various discussions and suggestions and arrive on a path ahead. I am happy to be part of it.

Warm wishes to all

Jim

On 23 Apr 2003, at 23:53, Tom Wigley wrote:

> Dear friends,
>
> [Apologies to those I have missed who have been part of this email
> exchange -- although they may be glad to have been missed]
>
> I think Barrie Pittock has the right idea -- although there are some
> unique things about this situation. Barrie says
>
> (1) There are lots of bad papers out there
> (2) The best response is probably to write a 'rebuttal'
>
> to which I add
>
> (3) A published rebuttal will help IPCC authors in the 4AR.
>
> _____
>
> Let me give you an example. There was a paper a few years ago by
> Legates and Davis in GRL (vol. 24, pp. 2319-1222, 1997) that was
> nothing more than a direct and pointed criticism of some work by
> Santer and me -- yet neither of us was asked to review the paper. We
> complained, and GRL admitted it was poor judgment on the part of the
> editor. Eventually (> 2 years later) we wrote a response (GRL 27,
> 2973-2976, 2000). However, our response was more than just a rebuttal,
> it was an attempt to clarify some issues on detection. In doing things
> this way we tried to make it clear that the original Legates/Davis
> paper was an example of bad science (more bluntly, either sophomoric
> ignorance or deliberate misrepresentation).
>
> Any rebuttal must point out very clearly the flaws in the original
> paper. If some new science (or explanations) can be added -- as we did
> in the above example -- then this is an advantage.
>
> _____
>
> There is some personal judgment involved in deciding whether to rebut.
> Correcting bad science is the first concern. Responding to unfair
> personal criticisms is next. Third is the possible misrepresentation
> of the results by persons with ideological or political agendas. On
> the basis of these I think the Baliunas paper should be rebutted by
> persons with appropriate expertise. Names like Mann, Crowley, Briffa,
> Bradley, Jones, Hughes come to mind. Are these people willing to spend
> time on this?

>
> _____
>
> There are two other examples that I know of where I will probably be
> involved in writing a response.
>
> The first is a paper by Douglass and Clader in GRL (vol. 29, no. 16,
> 10.1029/2002GL015345, 2002). I refereed a virtually identical paper
> for J. Climate, recommending rejection. All the other referees
> recommended rejection too. The paper is truly appalling -- but somehow
> it must have been poorly reviewed by GRL and slipped through the net.
> I have no reason to believe that this was anything more than chance.
> Nevertheless, my judgment is that the science is so bad that a
> response is necessary.
>
> The second is the paper by Michaels et al. that was in Climate
> Research (vol. 23, pp. 109, 2002). Danny Harvey and I refereed this
> and said it should be rejected. We questioned the editor (deFreitas
> again!) and he responded saying

>
> The MS was reviewed initially by five referees. ... The other three
> referees, all reputable atmospheric scientists, agreed it should be
> published subject to minor revision. Even then I used a sixth person
> to help me decide. I took his advice and that of the three other
> referees and sent the MS back for revision. It was later accepted for
> publication. The refereeing process was more rigorous than usual.
>
> On the surface this looks to be above board -- although, as referees
> who advised rejection it is clear that Danny and I should have been
> kept in the loop and seen how our criticisms were responded to.
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> -- deFreitas has offered us this possibility.
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> deFreitas deliberately chose other referees who are members of the
> skeptics camp. I also suspect that he has done this on other
> occasions. How to deal with this is unclear, since there are a number
> of individuals with bona fide scientific credentials who could be used
> by an unscrupulous editor to ensure that 'anti-greenhouse' science can
> get through the peer review process (Legates, Balling, Lindzen,
> Baliunas, Soon, and so on).
>
> The peer review process is being abused, but proving this would be
> difficult.
>
> The best response is, I strongly believe, to rebut the bad science
> that does get through.
>
> _____
>
> Jim Salinger raises the more personal issue of deFreitas. He is

> clearly giving good science a bad name, but I do not think a barrage
> of ad hominem attacks or letters is the best way to counter this.
>
> If Jim wishes to write a letter with multiple authors, I may be
> willing to sign it, but I would not write such a letter myself.
>
> In this case, deFreitas is such a poor scientist that he may simply
> disappear. I saw some work from his PhD, and it was awful (Pat
> Michaels' PhD is at the same level).
>
> _____
>
> Best wishes to all,
> Tom.
>

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Tel + 64 9 375 2053 Fax + 64 9 375 2051
e-mail: j.salinger@niwa.co.nz

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Edward Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Review- confidential
Date: Tue Apr 29 13:55:38 2003

Thanks Ed

Can I just say that I am not in the MBH camp - if that be characterized by an unshakable "belief" one way or the other, regarding the absolute magnitude of the global MWP. I certainly believe the "medieval" period was warmer than the 18th century - the equivalence of the warmth in the post 1900 period, and the post 1980s, compared to the circa Medieval times is very much still an area for much better resolution. I think that the geographic / seasonal biases and dating/response time issues still cloud the picture of when and how warm the Medieval period was. On present evidence, even with such uncertainties I would still come out favouring the "likely unprecedented recent warmth" opinion - but our motivation is to further explore the degree of certainty in this belief - based on the realistic interpretation of available data. Point re Jan well taken and I will inform him
At 07:59 AM 4/29/03 -0400, you wrote:

Hi Keith,

I will start out by sending you the chronologies that I sent Bradley, i.e. all but Mongolia. If you can talk Gordon out of the latter, you'll be the first from outside this lab. The chronologies are in tabbed column format and Tucson index format. The latter have sample size included. It doesn't take a rocket scientist (or even Bradley after I warned him about small sample size problems) to realize that some of the chronologies are down to only 1 series in their earliest parts. Perhaps I should have truncated them before using them, but I just took what Jan gave me and worked with the chronologies as best I could. My suspicion is that most of the pre-1200 divergence is due to low replication and a reduced number of available chronologies. I should also say that the column data have had their means normalized to approximately 1.0, which is not the case for the chronologies straight out of ARSTAN. That is because the site-level RCS-detrended data were simply averaged to produce these chronologies, without concern for their long-term means. Hence the "RAW" tag at the end of each line of indices. Bradley still regards the MWP as "mysterious" and "very incoherent" (his latest pronouncement to me) based on the available data. Of course he and other members of the MBH camp have a fundamental dislike for the very concept of the MWP, so I tend to view their evaluations as starting out from a somewhat biased perspective, i.e. the cup is not only "half-empty"; it is demonstrably "broken". I come more from the "cup half-full" camp when it comes to the MWP, maybe yes, maybe no, but it is too early to say what it is. Being a natural skeptic, I guess you might lean more towards the MBH camp, which is fine as long as one is honest and open about evaluating the evidence (I have my doubts about the MBH camp). We can always politely(?) disagree given the same admittedly equivocal evidence.

I should say that Jan should at least be made aware of this reanalysis of his data. Admittedly, all of the Schweingruber data are in the public domain I believe, so that should not be an issue with those data. I just don't want to get into an open critique of the Esper data because it would just add fuel to the MBH attack squad. They tend to work in their own somewhat agenda-filled ways. We should also work on this stuff on our own, but I do not think that we have an agenda per se, other than trying to objectively understand what is going on.

Cheers,
Ed

Ed

thanks for this - and it is intriguing , not least because of the degree of coherence in these series between 1200 and 1900 - more than can be accounted for by either replication of data between the series (of which there is still some) or artifact of the standardisation method (with the use of RCS curves which are possibly inappropriate for all the data to which each is applied) . Having then got some not insubstantial confidence in the likelihood of a real temperature signal in this period - the question of why the extreme divergence in the series pre-1200 and post 1900? A real geographic difference in the forcing , replication and standardisation problems? - both are likely. We would like the raw cores for each site: the RCS indices upon which you base the chronologies ; the site chronologies (which I think you sent to Ray?). At first we will simply plot the site chronologies , correlate each with local climate and come back to you again. We will also plot each "set" of indices and compare site RCS curves and reconsider the validity of the classification into linear and non-linear growth patterns. I know you have done all this but we need to get a feel for these data and do some comparisons with my early produce ring-width RCS chronologies for certain sites and compare the TRW series with the same site MXD chronologies - all a bit suck and see at first. I am talking with Tim later today about the review idea and I will email/phone before 16.00 my time today.

Thanks

Keith

At 10:01 AM 4/28/03 -0400, you wrote:

Hi Keith,

Here is the new Esper plot with three different forms of regionalization: linear vs. nonlinear (as in the original paper), north vs. south as defined in the legend, and east vs. west (i.e. eastern hemisphere vs. western hemisphere). All of the series have been smoothed with a 50-yr spline after first averaging the annual values. The number of cores/chronologies are given in the legend in parentheses. Not surprisingly, the north and south chronologies deviate most in the post-1950 period. Before 1950 and back to about 1200 the series are remarkably similar (to me anyway). Prior to 1200 there is more chaos, perhaps because the number of chronologies have declined along with the within-chronology replication. However, there is still some evidence for spatially

coherent above-average growth. I showed this plot at the Duke meeting. Karl Taylor actually told me that he thought it looked fairly convincing, i.e. that the low-frequency structure in the Esper series was not an artefact of the RCS method.

Cheers,
Ed

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References

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From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: belated thanks for review and questions
Date: Fri, 02 May 2003 18:46:41 -0400

Hi Keith,

No problem, I know how hectic the past couple months have been for you, so no apologies necessary whatsoever!

Call me old fashioned, but I still tend to prefer the "blind" reviewer convention, so I'd prefer to remain anonymous unless you think that revealing my identity would be help in any particular way.

I agree w/ your take on this--a journal like GRL is probably more appropriate, or even "Climatic Change" because a number of similar papers have been published there in the past (by folks like Nychka, Bloomfield, and others). I'm not sure if Steve Schneider is sick and tired of those papers though...

Please don't hesitate to let me know if I can be of any additional help w/ this.

Looking forward to seeing you one of these days,
mike

At 02:36 PM 5/2/2003 +0100, you wrote:

Mike

in hassling another reviewer , I realised that I did not thank you properly for the review you did of the manuscript by Gil-Alana (fractionally integrated techniques used to show increased persistence in global temperature record in 20th century). So this is by way of thanks and to ask whether you wish me to reveal your name to the reviewer (considering you make some very helpful suggestions for further analysis)? I would otherwise assume no. As it happens I can not get a response from the other reviewer - but rather than prolong the wait for the submitter , I am tempted (on the basis of my reading also) to just send your comments and reject the manuscript as it is - I suppose they could resubmit a major rework following your suggestions - but I tend to the opinion that it would be better suited to another journal anyway - GRL comes to mind. What do you think

Cheers

Keith

--

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References

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: Edward Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Review- confidential
Date: Mon May 12 17:26:29 2003

Ed

just back from really sunny Austria and very pleasant south of France. Have talked at length with Jan and he says it is fine to send the raw and detrended cores series (segmented for each site if possible). Do you also have a convenient Table with the Lats and Longs you used to plot the sites map? This would mean I don't have to look them all up. I will phone to report on our discussions and ask several things that arose from these. Just have to do essential other stuff first - so probably tuesday afternoon (my time) Do you have that review yet?

love and kisses

Keith

At 07:59 AM 4/29/03 -0400, you wrote:

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actually told me that he thought it looked fairly convincing, i.e. that the low-frequency structure in the Esper series was not an artefact of the RCS method.

Cheers,
Ed

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fwd: Clivar Conference 2004
Date: Tue May 20 14:57:55 2003

Mike

Lennart has managed to confuse me with his latest message. At one point he mentioned that you and I would do a joint overview paper . Now he suggests we choose 5-10 co-authors but also refers to "other people in our section" who he has apparently already informed , need "to consult with you (ie us) as required" (my emphasis).

As for my opinion of the theme or content of our section , I suggest it be "quantifying Natural and Anthropogenic influences on the course of Global climate during recent millennia" or some such . This allows for the review , redefinition of Global climate history (Southern as well as Northern , and moisture as well as Temperature). Importantly , it also incorporates the issue of forcing history(ies) and work quantifying the influence of these histories - using simple empirical techniques or using them in conjunction with models of different complexity to attribute causes of this change.

I am happy to go with the "usual suspects" in the overview paper , but would be happy if we considered others who are also running controlled model/data comparisons (examples are Von Storch , Simon Tett , Caspar Ammann). We need first to clarify whether we will present one large , multi-author presentation/paper or whether it is just me and you and the others divided into other papers/presentations/posters. Should we copy this message to Lennart or contact him directly with specific questions?

Keith

At 09:49 PM 5/18/03 -0400, you wrote:

Hi Keith,

I hope all is well.

Apparently, we're supposed to choose 5-10 additional "co-authors"? I guess the obvious ones would be Phil, Tim, Ray, Malcolm, perhaps Ed Cook, Scott Rutherford,...any other suggestions?

As I understand it, the co-authors would be invited to attend and present in the poster session; I assume they are listed separately from you and I who will jointly present the oral overview. As for the theme, I'm assuming "climate changes of the past couple/few millennia" or something like that. As we have 45 minutes total between the two of us, I would suggest we each take about 20 minutes, and then we'll have 5 minutes left for questions.

Any suggestions, thoughts would be greatly appreciated.

thanks,

mike

X-Sender: m214001@regen.dkrz.de

Date: Sun, 18 May 2003 22:53:58 +0200

To: k.briffa@uea.ac.uk, mann@virginia.edu

From: "Prof. Dr. Lennart Bengtsson" <bengtsson@dkrz.de>

Subject: Clivar Conference 2004

Cc: bengtsson@dkrz.de, kornelia.mueller@dkrz.de

--

Dear Dr. Mann,

Dear Dr. Briffa,

The preparation of the Clivar conference is progressing well and all invited speakers have now agreed (See attached draft program). As I have informed you previously Journal of Climate will have a special issue devoted to the Conference and I expect you would be willing to prepare a paper to be ready at the time of the conference. I have made arrangements with the chief editor to make a flexible interpretation of the content of the papers so to agree with the objective of the conference and the draft program.

We would now like you to come up with a suitable theme for your presentation at the conference as well a list of names which you have selected as co-authors. As we anticipate a broad and forward-looking contribution I believe some 5-10 people seems appropriate. It was our intention that the first person listed should be the lead author but you can arrange this otherwise if you prefer to do so. I have informed the other speakers in your section to consult with you as required.

For the conference I expect a rather wide audience in addition to a broad scientific community including representatives from different agencies such as the meteorological services, as well as media representatives. For the media we intend to provide a special set of information. In view of the societal importance of the CLIVAR program and the considerable progress in extended range forecasts and climate change assessment and prediction I believe there will be an excellent opportunity to bring the scientific progress and associated applications of CLIVAR to the participants of the conference. It would be very helpful if you could let me know the status of your arrangements not later than June 15. If you see any particular difficulties please let me know as soon as possible.

As you can see from the attached program each part of the conference will have poster sessions. The poster sessions will be an important part of the conference and I anticipate that some of your co-authors will prepare such posters. We also plan to have the poster contents on a CD ROM prior to the conference.

The practical planning of the conference as a whole is proceeding well. The arrangements in Baltimore are quite excellent with the nearby Baltimore inner harbor as a particular attractive focal point. There are all reasons that the conference will be a success both scientifically and socially. See further the Clivar Conference website:

[1] <http://www.clivar2004.org>.

We are presently exploring the possibilities for financial support of selected participants. However, any support you may manage to obtain from national funds would be most helpful.

With my very best regards

Lennart Bengtsson

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1. <http://www.clivar2004.org/>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>
4. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>, Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu
Subject: Re: Soon et al. paper
Date: Tue May 20 16:07:41 2003
Cc: Jerry Meehl <meehl@ucar.edu>, Caspar Ammann <ammann@ucar.edu>, mann@virginia.edu

Mike and Tom and others

My silence to do with the specific issue of the Soon and Baliunas conveys general strong agreement with all the general remarks (and restatement of many in various forms) by Tom Crowley, Mike Mann, Neville Nichols and now Tom Wigley regarding the scientific value of the paper and its obvious methodological flaws.

I have to say that I tended towards the "who cares" camp , in as much as those who are concerned about the science should see through it anyway . I also admit to thinking that some of you seem a little paranoid (especially in the implication that Climate Research is a pro sceptic journal) but I am changing my mind regarding the way the "meaning" of the BS paper is being presented to the wider public - in response to some very poor recent reporting in the British press and several requests from the US that indicate that those of you who work there can not simply rely on the weight of good science eventually showing through as regards the public perception . As Tom W. states , there are uncertainties and "difficulties" with our current knowledge of Hemispheric temperature histories and valid criticisms or shortcomings in much of our work. This is the nature of the beast - and I have been loathe to become embroiled in polarised debates that force too simplistic a presentation of the state of the art or "consensus view". Having read Tom W's and Mike's latest statements I now agree about the need to make some public comment on BS . (I too have given my personal view of the work to David Appell who I assume is writing a balanced view of this paper for Scientific American). I see little need to get involved in a over detailed critic of all the points in the paper , because I am not sure what audience would benefit from it, but the points made by those I listed above could usefully be fashioned into a simple letter to Climate Research, signed by those who wish. This would then go on record as a simple statement of refutation of the method employed and corresponding limitation of the work for informing the "global warming " debate . This could be quickly citable when talking to the media.

The one additional point I would make that seems to have been overlooked in the discussions up to now , is the invalidity of assuming that the existence of a global Medieval Warm period , even if shown to be as warm as the current climate , somehow negates the possibility of enhanced greenhouse warming. The business of constructing a reliable climate history is only one part of establishing the relative roles of natural and anthropogenic forcings, now and in the future. Without reference to the roles of natural forcings in recent and past times , comparisons with other periods are of very limited value anyway. So I agree with Tom and Mike that something needs to go "on record" . The various papers apparently in production, regardless of their individual emphasis or approaches, will find their way in to the literature and the next IPCC can sift and present their message(s) as it wishes., but in the meantime , why not a simple statement of the shortcomings of the BS paper as they have been listed in these messages and why not in Climate Research?

Keith

At 05:04 PM 5/16/03 -0400, Michael E. Mann wrote:

Tom,

Thanks for your response, which I will maintain as confidential within the small group of the original recipients (other than Ray whom I've included in as well), given the sensitivity of some of the comments made.

Whether or not their comments are ad hominem or potentially libelous is probably immaterial here (some people who have read them think they might be--in certain places, ulterior motives are implied on the part of individually named scientists in the discussion of scientific methodologies).

However, the real issue, as you point out, is whether or not their arguments and criticisms are valid. I would argue that very few of them are--I have prepared (and have

attached) a draft of replies to some of the specifics in their two papers--this is rough, and I'm working on preparing a refined version of this for use by those who are trying to combat the disinformation that the Baliunas and co. supporters are working at spreading within the beltway, with the full support of industry, and perhaps the administration. By necessity this is brief and focus on the most salient points--a point-by-point rebuttal would take a very long time.

In the meantime, Phil and I, and Ray/Malcolm/Henry D are independently working on review pieces (ours for R.O.G., Ray et al's for Science) that will also correct in more detail some of the most egregious untruths put forward by the Baliunas/Soon pieces (what one colleague of mine aptly chooses to abbreviate as "BS").

The most fundamental criticism, of course, is that the hypothesis, methods, and assumptions are absolutely nonsensical by construction--as you already pointed out. One could demonstrate that with an example, but then again, why do so when it is self evident that defining an anomaly of either wetter or dryer (what does that leave out?) relative to the 20th century (a comparison which is itself also ill-defined by the authors, since they don't use a uniform 20th century reference period for defining their qualitative anomalies, and discuss proxy records with variable resolution and temporal sampling of the 20th century) was "warmer than the 20th century" is nonsense at the most fundamental level. It defies the most elementary logic, and thus is difficult to reply to other than noting that it is nonsense by its very nature.

Would we be compelled to provide a counterexample to disprove the authors if they had asserted that "1=2"? What they have done isn't that much different...

So its one thing to throw out a bunch of criticisms, very few of which are valid. But to then turn around and present a fundamentally ill-posed, supposed "analysis" which doesn't even attempt to provide a quantitative "alternative" to past studies, to claim to have disproven those past studies, and to supposedly support the non-sequitor conclusion that the "MWP was warmer than the 20th century" is irresponsible, deceptive, dishonest, and a violation of the very essence of the scientific approach in my view. One or two people can't fight that alone, certainly not with the "artillery" (funding and political organization) that has been lined up on the other side. In my view, it is the responsibility of our entire community to fight this intentional disinformation campaign, which represents an affront to everything we do and believe in. I'm doing everything I can to do so, but I can't do it alone--and if I'm left to, we'll lose this battle,

mike

At 02:18 PM 5/16/2003 -0600, Tom Wigley wrote:

Dear folks,

I have just read the Soon et al. paper in E&E. Here are some comments, and a request.

Mike said in an email that he thought the paper contained possibly 'legally actionable' ad hominem attacks on him and others. I do not agree that there are ad hominem attacks. There are numerous criticisms, usually justified (although not all the justifications are valid). I did not notice any intemperate language.

While many of the criticisms are invalid, and some are irrelevant, there are a number that seem to me to be quite valid. Probably, most of these can be rebutted, and perhaps some of these are already covered in the literature. In my view, however, there a small number of points that are valid criticisms.

[Off the record, the most telling criticisms apply to Tom Crowley's work -- which I do not hold in very high regard.]

The real issue that the press (to a limited extent) and the politicians (to a greater extent) have taken up is the conclusions of the paper's original research.

First, Soon et al. come down clearly in favor of the existence of a MWE and a LIA. I think many of us would agree that there was a global-scale cool period that can be identified with a LIA. The MWE is more equivocal. There are real problems in identifying both of these 'events' with certainty due to (1) data coverage, (2) uncertainty in transfer functions, and (3) the noise of internally generated variability on the century time scale. [My paper on the latter point is continually ignored by the paleo

community, but it is still valid.]

So, we would probably say: there was a LIA; but the case for *or against* a MWE is not proven. There is no strong disagreement with Soon et al. here.

The main disagreements are with the methods used by Soon et al. to draw their LIA/MWE conclusion, and their conclusion re the anomalousness/uniqueness of the 20th century (a conclusion that is based on the same methods).

So what is their method? I need to read the paper again carefully to check on this, but it seems that they say the MWE [LIA] was warm [cold] if at a particular site there is a 50+ year period that was warm, wet, dry [cold, dry, wet] somewhere in the interval 800-1300 [1300-1900], where warm/cold, wet, dry are defined relative to the 20th century.

The problems with this are

- (1) Natural internally generated variability alone virtually guarantees that these criteria will be met at every site.
- (2) As Nev Nicholls pointed out, almost any period would be identified as a MWE or LIA by these criteria -- and, as a corollary, their MWE period could equally well have been identified as a LIA (or vice versa)
- (3) If the identified warm blips in their MWE were are different times for different locations (as they are) then there would be no global-mean signal.
- (4) The reason for including precip 'data' at all (let alone both wet and dry periods in both the MWE and LIA) is never stated -- and cannot be justified. [I suspect that if they found a wet period in the MWE, for example, they would search for a dry period in the LIA -- allowing both in both the MWE and LIA seems too stupid to be true.]
- (5) For the uniqueness of the 20th century, item (1) also applies.

So, their methods are silly. They seem also to have ignored the fact that what we are searching is a signal in global-mean temperature.

The issue now is what to do about this. I do not think it is enough to bury criticisms of this work in other papers. The people who have noticed the Soon et al paper, or have had it pointed out to them, will never see or become aware of such rebuttals/responses. Furthermore, I do not think that a direct response will give the work credibility. It is already 'credible' since it is in the peer reviewed literature (and E&E, by the way, is peer reviewed). A response that says this paper is a load of crap for the following reasons is *not* going to give the original work credibility -- just the opposite.

How then does one comprehensively and concisely demolish this work? There are two issues here. The first is the point by point response to their criticisms of the literature. To do this would be tedious, but straightforward. There will be at least some residual criticisms that must be accepted as valid, and this must be admitted. Cross-referencing to other review papers would be legitimate here.

The second is to demolish the method. I have done this qualitatively (following Nev mainly) above, but this is not enough. What is needed is a counter example that uses the method of *reductio ad absurdum*. This would be clear and would be appropriate since it avoids us having to point out in words that their methods are absurd. I have some ideas how to do this, but I will let you think about it more before going further.

You will see from this email that I am urging you to produce a response. I am happy to join you in this, and perhaps a few others could add their weight too. I am copying this to Jerry since he has to give some congressional testimony next week and questions about the Soon et al work are definitely going to be raised. I am also copying this to Caspar, since the last millenium runs that he is doing with paleo-CSM are relevant.

Best wishes,
Tom.

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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[2][http://www.cru.uea.ac.uk/cru/people/briffa\[3\]/](http://www.cru.uea.ac.uk/cru/people/briffa[3]/)

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>

To: craig.wallace@uea.ac.uk

Subject: Fwd: Re: reminder

Date: Thu May 22 09:34:54 2003

Date: Wed, 21 May 2003 13:38:24 -0400

To: Keith Briffa <k.briffa@uea.ac.uk>

From: Edward Cook <drdendro@ldeo.columbia.edu>

Subject: Re: reminder

Hi Keith,

Busy, busy, busy as usual. Here are the lats and lons.

| LAT | LON | SITE | COORDINATES IN DECIMAL DEGREES |
|--------|---------|---------------|--------------------------------|
| 52.220 | -117.23 | ATHABASCA | |
| 36.000 | -118.33 | BOREAL | |
| 68.160 | -133.20 | CAMPBILL | |
| 57.000 | 18.500 | GOTLAND | |
| 63.500 | 13.500 | JAEMTLAND | |
| 66.680 | 82.300 | MANGAZEJA | |
| 48.280 | 98.920 | MONGOLIA | |
| 66.830 | 65.670 | POLAR URALS | |
| 57.500 | -76.000 | QUEBEC | |
| 72.000 | 102.00 | TAYMIR | |
| 47.000 | 11.000 | TIROL | |
| 68.220 | 19.720 | TORNETRASK | |
| 37.000 | -118.42 | UPPER WRIGHT | |
| 67.450 | 142.62 | ZHASCHIVIERSK | |

I will get the data to you next week. I have to off to Rob Wilson's thesis defense now.

Cheers,

Ed

.. about the review and the data (or at least accurate lats and longs while waiting)

cheers

Keith

--

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3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Mike Hulme <m.hulme@uea.ac.uk>

To: simon.shackley@umist.ac.uk, mgrc@ceh.ac.uk

Subject: Re: thresholds and CO2 leakage

Date: Thu May 22 11:18:31 2003

Cc: tlent@ceh.ac.uk, tim.cockerill@sunderland.ac.uk, shol@bgs.ac.uk, kevin.anderson@umist.ac.uk

Simon,

Some comments to your questions below

At 13:46 20/05/2003 +0100, Simon J Shackley wrote:

dear Melvin, Tim, Mike, Tim, Sam and Kevin

For our analysis of acceptable leakage rates of carbon dioxide from geological storage sites, we can use the data provided in Lenton & Cannell CC paper I think. In particular, we could use your finding that to limit warming to under 0.2oC per decade, rate of increase of fossil fuel emissions has to be limited to under 0.03 GtC/yr/yr.

This would seem sufficient to avoid the peak warming which occurs in about 2250 under the IS92a emissions scenario (figure 1(c)). Is the 0.2oc / decade threshold widely accepted in the science community however?

This threshold (0.2/decade; 2degC absolute by 2100) is the most commonly cited in science-policy circles. The EU have formally adopted it as a preferred target. It's origin however is less than obvious and it's adequacy difficult to establish. And of course it also depends whether this is carried out to 2200 - the impacts of 4degC by 2200 is not the equivalent of impacts of 2degC by 2100.

My personal view is that there is much circular argument here. The first GCM experiments in the 1980s were 2xCO2 equilibrium, i.e., 550ppmv (cf. 275ppmv pre-industrial). Thus much early work used these scenarios. 550ppmv is also a commonly cited target for no other reason than this. A 60% reduction in CO2 is broadly commensurate with 550ppm stabilisation (admittedly, the range is wide coz of C cycle uncertainty; but 60% is mid-range). And (again mid-range) 550ppm leads to about a 2degC global warming, which by 2100 is 0.2degC/decade. Independent arguments for 0.2deg/decade exist for sure - e.g. rate of ecosystem migration - but as we all know (and have pointed out in our paper on external and internal definitions of dangerous climate change), no single metric is adequate.

My feeling is that the 2degC (0.2deg/decade) mantra is as much related to the early mind-set of 2xCO2 GCM experiments as it is rooted in any more substantive reasoning. One might also point out of course that the world has been warming at about 0.15degC/decade now for three decades (since the 1970s) - has this been acceptable/dangerous?

Should we also be looking at a 0.1oC / decade threshold as well?

I would regard this threshold as a very conservative (or radical - depending on how you look at it) one

Since we are only looking at the UK we will need to translate the 0.03 GtC figure into allowable rate of increase (presumably decrease) of European emissions and then pro-rata to the UK. IPCC SRES Emissions scenarios would provide some basis for doing these calculations and i'll have a look at the data they provide. Alternatively / in addition, we could use the Contraction and Convergence model of the GCI to calculate 'acceptable' rates of change (decreasing) of UK emissions into the next millenium. In Lenton & Cannell, the authors argue that: 'Early consideration should be given to leaving a fraction of fossil carbon unused, and/or to carbon capture and storage'. One implication of the work on leakage from geological storage sites is that the suggestion to use CCS to lessen eventual warming might not hold on longer timescales, depending on the rate of leakage. So does any one have any idea on what fraction of fossil carbon should be left in the ground so as to provide a cap on the eventual warming on long time scales (3000 years say)? Is there an 'accepted' threshold for eventual warming which is 'safe' and to which society can adapt? If so, what does this threshold tell us about how much carbon has to be left in the ground? A simpler way forward for us might again be to use Contraction & Convergence to provide us with an acceptable absolute level of emissions from the UK on long millenial timescales and to work backwards from that figure to calculate acceptable leakage rates for the UK.

Thanks for any help you can provide

Simon

From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Pritchard, Norah" <norah.pritchard@metoffice.com>
Subject: Re: IPCC WG2 AR4 draft outlines - WGII outline & Chapters 2 and 13
Date: Mon Jun 2 13:49:07 2003

Dear Osvaldo and Martin,

It is very difficult to make considered input into this process at such short notice. I received the emails Wednesday afternoon, just before being away from the office for 48 hours. I also am not fully aware of the process into which this is fitting and it is the first time I have seen the WGII outline. I do however make some comments on the following:

The WGII outline

Chapter 2 on data etc.

Chapter 13 on critical damage etc.

WGII outline

Key Questions: there is, in analytical terms, very little difference between the 2nd and 4th key question you pose. The impacts under unmitigated CC (Q2) are not in any fundamental way different from the impacts under mitigated CC (Q4). 2degC warming, for example, will give broadly the same impacts whether this occurs because of strong CC policy intervention or whether it occurs because of low carbon development paths. What matters more for impacts is the rate of CC and what matters more for how important those impacts are is the development path pursued. I think this distinction between mitigated and unmitigated CC is tenuous and unhelpful. This has a bearing on the later discussions about stabilisation (where "stabilisation" is usually assumed to be, indeed often synonymous with, the result of mitigative action; actually (quasi-) stabilisation, at different levels, can occur in a world with relatively little direct CC mitigation policy).

The progression through the sections follows a rather linear and reductionist model - observed impacts, future impacts, adaptation, regions. I would have liked to have seen an early opening chapter on the nature of the dynamic relationship between climate and society (before we even start talking about climate change), this being able to bring out notions of vulnerability and adaptation - both fundamental to put on the table before we start thinking about future climate change and how important it is. This could also point out that "critical" damage is already being caused by climate and climate variability.

Under your structure, the observed impacts section (II) should surely parallel the later future impacts section (III) in terms of sectors/themes. There are only 4 themes in section II, yet 6 (different) themes in section III. Why for example is nothing said about observed impacts on urban infrastructure or on coasts? The asymmetry between these section sub-themes is itself perhaps revealing.

It seems odd that adaptation is to be addressed in all the thematic chapters in Section III *as well as* in a separate later chapter on adaptation. This situation is ripe for overlap and redundancy. Our understanding of adaptation in any case should be brought in right at the beginning (see above).

The avoiding critical damage chapter suffers from the same problem identified above - what matters is whether and how such exceedance rates can be identified, not whether they result from either a mitigated or an unmitigated scenario - this academic distinction cannot be sustained in the real world.

The regional section is in danger of repeating the mistake in the TAR, again leading to dispersion of effort and redundancy. My suggestion would be *not* to assess all new regional knowledge (again; very turgid), but instead to produce a much more streamlined section focusing on a few regional/local case studies that illustrate sharply many of the (integrating) themes introduced earlier - vulnerability, adaptation, criticality, impacts. Deliberately seek to be selective and not comprehensive.

I also do not see how the WGII chapters will be co-ordinated with the 5 cross-cutting papers identified here - again, there seems much scope for duplicitous effort and redundancy or even contradiction. And since the cross-cutting papers are really the interesting and useful ones, this suggests to me that the old traditional WG structure of IPCC is now deeply flawed (as I have said more than once before in public).

Chapter 2 - Assumptions, etc.

First question to raise is what is WGI doing in this regard? I cannot comment sensibly without knowing how WGI will tackle questions of scenarios and future projections. In section 2.3, 4th bullet: how relevant really are these "Stabilisation scenarios (mitigation)"? At the very least IPCC must clear up this issue about whether stabilisation is a short-hand for mitigation (as implied here). This is potentially misleading, since stabilisation can occur in many different worlds, by no means all of them worlds with strong CC mitigation policies. Continuation of this thinking means reality is being forced to accommodate the arbitrary thinking of the UNFCCC rather than UNFCCC being forced to take account of reality.

Also in this bullet is "Impacts of extreme climate events". Why are impacts being looked at here? Surely this is totally misplaced. What is important are scenarios - of whatever origin and methodology - that embed within them changes in the character of "extreme" weather and how we describe such changes. We should not separate this out as a separate issue surely.

Section 2.4 (the second appearance) confuses me. Much of this material appears earlier in 2.3, thus characterisations of future conditions is what 2.3 is about and also the projected changes in key drivers is what the scenarios part of 2.3 is all about. Do you mean to differentiate between methodology (2.3) and outcomes (2.4b)? And as always you will run into the problem of summarising what scenarios actually *are* assumed in this report - is there to be an IPCC 4AR standard scenario(s) that all should use? I suspect not. Resolving this problem gets to the heart of the structural problem with IPCC.

Different people will use different assumptions.

Chapter 13 - Critical Damage ...

This outline was almost unintelligible to me! For example having read the opening aims and scope statement several times, I am still not clear about the approach this chapter is

taking. Sections 13.2 and 13.3 are also extremely unclear as is section 13.4.

I think someone needs to do some clearer thinking about this chapter before sending it out for people to comment on. I have my own views on this, but at such short notice and without knowing the agreed IPCC process I'm not going to write the chapter outline for you.

Inter alia, the chapter should address the following:

- different paradigms for defining "critical"; will vary by sector, culture, etc.
- distinction between external (pronounced) definitions of critical and internal (experienced/perceived) definitions
- relationship between adaptive capacity and "critical" rates of change
- dependence of critical thresholds on sector and spatial scale
- reversibility (or not) of critical damage

... and if the use of "critical" is a euphemism for "dangerous" then it is not very subtle

- people will see through this. What is the difference between critical and dangerous?

Professor Mike Hulme

Tyndall Centre

At 14:32 28/05/2003 +0100, you wrote:

Dear Mike

We are now developing chapter outlines for the Fourth Assessment Report of the IPCC and we write to ask if you will help us in this task. Enclosed is a one-page outline of the proposed chapter on Assumptions, Data and Scenarios, which we would like you to adjust and expand (but not to more than one and a half pages in all, please). The overall list of proposed topics to be covered in the assessment is also attached.

We would like to make the next revision to the outline in a few days so could you please return your outline to Norah Pritchard << ipccwg2@metoffice.com >> at the WGII Technical Support Unit at the UK Met Office's Hadley Centre not later than 2nd June?

The process of designing the Fourth Assessment and selecting authors is different from previously. This time the authors will not be nominated by governments and then selected until *after* the outline has been approved by IPCC Plenary this November. The outlines are therefore being widely commented on between now and mid-September, when they will be finalised. We consider your input at this time to be most important.

We appreciate that you are busy, but urge that you give a few minutes to this crucial task.

In another message we will be writing for your suggestions regarding other experts to consult in the fields of Assumptions, Data and Scenarios.

We look forward to hearing from you

With thanks and kind regards,

Osvaldo Canziani and Martin Parry

Co-Chairs, IPCC Working Group II (Vulnerability, Impacts and Adaptation)

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<<AR4_outline27May_2scen_v1.doc>> <<AR4 WG2 summary final.doc>>

From: Scott Rutherford <srutherford@gso.uri.edu>

To: Malcolm Hughes <mhughes@lrr.arizona.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>

Subject: revised NH comparison manuscript

Date: Tue, 3 Jun 2003 14:51:09 -0400

Cc: Mike Mann <mann@virginia.edu>

<x-flowed>

Attached to this e-mail is a revision of the northern hemisphere comparison manuscript. First some general comments. I tried as best as possible to incorporate everyone's suggestions. Typically this meant adding/deleting or clarifying text. There were cases where we disagreed with the suggested changes and tried to clarify in the text why.

In this next round of changes I encourage everyone to make specific suggestions in terms of wording and references (e.g. Rutherford et al. GRL 1967 instead of "see my GRL paper"). I also encourage everyone to make suggestions directly in the file in coloured text or by using Microsquish Word's "Track Changes" function (this will save me deciphering cryptic penmanship; although I confess, my writing is worse than anyone's). If you would prefer to use the editing functions in Adobe Acrobat let me know and I will send a PDF file. If you still feel strongly that I have not adequately addressed an issue please say so. I will incorporate the suggestions from this upcoming round into a manuscript to be submitted. After review, everyone will get a crack at it again.

I will not detail every change made (if anyone wants the file with the changes tracked I can send it). Here are the major changes:

- 1) removal of mixed-hybrid approach and revised discussions/figures
- 2) removal of CE scores from the verification tables
- 3) downscaling of the Esper comparison to a single figure panel and one paragraph.
- 4) revised discussion of spatial maps and revised figure (figure 8).
- 5) seasonal comparisons have been revised

Several suggestions have been made for where to submit. These are listed on page 1 of the manuscript. Please indicate your preference ASAP and I will tally the votes.

I would like to submit by late July, so if you could please get me comments by say July 15 that would be great. I will send out a reminder in early July. If I don't hear from you by July 15 I will assume that you are comfortable with the manuscript.

Please let me know if you have difficulty with the file or would prefer a different format.

Regards,

Scott

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Attachment Converted: "c:\eudora\attach\nhcomparison_v7_1.doc"

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Scott Rutherford

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</x-flowed>

From: "Michael E. Mann" <mann@virginia.edu>

To: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu, Tom Wigley <wigley@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Keith Briffa <k.briffa@uea.ac.uk>, trenbert@cgd.ucar.edu, Michael Oppenheimer <omichael@princeton.edu>, Jonathan Overpeck <jto@u.arizona.edu>

Subject: Re: Prospective Eos piece?

Date: Wed, 04 Jun 2003 10:17:57 -0400

Cc: mann@virginia.edu, Scott Rutherford <srutherford@gso.uri.edu>

Thanks Phil, and Thanks Tom W and Keith for your willingness to help/sign on. This certainly gives us a "quorum" pending even a few possible additional signatories I'm waiting to hear back from.

In response to the queries, I will work on a draft today w/ references and two suggested figures, and will try to send on by this evening (east coast USA). Tom W indicated that he wouldn't be able to look at a draft until Thursday anyway, so why doesn't everyone just take a day then to digest what I've provided and then get back to me with comments/changes (using word "track changes" if you like).

I'd like to tentatively propose to pass this along to Phil as the "official keeper" of the draft to finalize and submit IF it isn't in satisfactory shape by the time I have to leave (July 11--If I hadn't mentioned, I'm getting married, and then honeymoon, prior to IUGG in Sapporo--gone for about 1 month total). Phil, does that sound ok to you?

Re Figures, what I had in mind were the following two figures:

1) A plot of various of the most reliable (in terms of strength of temperature signal and reliability of millennial-scale variability) regional proxy temperature reconstructions around the Northern Hemisphere that are available over the past 1-2 thousand years to convey the important point that warm and cold periods were highly regionally variable.

Phil and Ray are probably in the best position to prepare this (?). Phil and I have recently submitted a paper using about a dozen NH records that fit this category, and many of which are available nearly 2K back--I think that trying to adopt a timeframe of 2K, rather than the usual 1K, addresses a good earlier point that Peck made w/ regard to the memo, that it would be nice to try to "contain" the putative "MWP", even if we don't yet have a hemispheric mean reconstruction available that far back [Phil and I have one in review--not sure it is kosher to show that yet though--I've put in an inquiry to Judy Jacobs at AGU about this]. If we wanted to be fancy, we could do this the way certain plots were presented in one of the past IPCC reports (was it 1990?) in which a spatial map was provided in the center (this would show the locations of the proxies), with "rays" radiating out to the top, sides, and bottom attached to rectangles showing the different timeseries. Its a bit of work, but would be a great way to convey both the spatial and temporal information at the same time.

2) A version of the now-familiar "spaghetti plot" showing the various reconstructions as well as model simulations for the NH over the past 1 (or maybe 2K). To give you an idea of what I have in mind, I'm attaching a Science piece I wrote last year that contains the same sort of plot.

However, what I'd like to do different here is:

In addition to the "multiproxy" reconstructions, I'd like to Add Keith's maximum latewood density-based series, since it is entirely independent of the multiproxy series, but conveys the same basic message. I would also like to try to extend the scope of the plot back to nearly 2K. This would be either w/ the Mann and Jones extension (in review in GRL) or, if that is deemed not kosher, the Briffa et al Eurasian tree-ring composite that extends back about 2K, and, based on Phil and my results, appears alone to give a reasonably accurate picture of the full hemispheric trend.

Thoughts, comments on any of this?

thanks all for the help,

mike

At 09:25 AM 6/4/2003 +0100, Phil Jones wrote:

Mike,

This is definitely worth doing and I hope you have the time before the 11th, or can pass it on to one of us at that time. As you know I'm away for a couple of days but back Friday.

So count me in. I've forwarded you all the email comments I've sent to reporters/fellow scientists, so you're fully aware of my views, which are essentially the same as all of the list

and many others in paleo. EOS would get to most fellow scientists. As I said to you the other

day, it is amazing how far and wide the SB pieces have managed to percolate. When it comes

out I would hope that AGU/EOS 'publicity machine' will shout the message from rooftops everywhere. As many of us need to be available when it comes out.

There is still no firm news on what Climate Research will do, although they will likely

have two editors for potentially controversial papers, and the editors will consult when papers

get different reviews. All standard practice I'd have thought. At present the editors

get no
guidance whatsoever. It would seem that if they don't know what standard practice is
then
they shouldn't be doing the job !
Cheers
Phil

At 22:34 03/06/03 -0400, Michael E. Mann wrote:

Dear Colleagues,
Eos has invited me (and prospective co-authors) to write a 'forum' piece (see below).
This was at Ellen Mosely-Thompson's suggestion, upon my sending her a copy of the
attached memo that Michael Oppenheimer and I jointly wrote. Michael and I wrote this to
assist colleagues who had been requesting more background information to help counter
the spurious claims (with which I believe you're all now familiar) of the latest
Baliunas & Soon pieces.

The idea I have in mind would be to use what Michael and I have drafted as an initial
starting point for a slightly expanded piece, that would address the same basic issues
and, as indicated below, could include some references and figures. As indicated in
Judy Jacobs' letter below, the piece would be rewritten in such a way as to be less
explicitly (though perhaps not less implicitly) directed at the Baliunas/Soon claims,
criticisms, and attacks.

Phil, Ray, and Peck have already indicated tentative interest in being co-authors. I'm
sending this to the rest of you (Tom C, Keith, Tom W, Kevin) in the hopes of broadening
the list of co-authors. I strongly believe that a piece of this sort co-authored by 9
or so prominent members of the climate research community (with background and/or
interest in paleoclimate) will go a long way in helping to counter these attacks, which
are being used, in turn, to launch attacks against IPCC.

AGU has offered to expedite the process considerably, which is necessary because I'll be
travelling for about a month beginning June 11th. So I'm going to work hard to get
something together ASAP. I'd would therefore greatly appreciate a quick response from
each of you as to whether or not you would potentially be willing to be involved as a
co-author. If you're unable or unwilling given other current commitments, I'll
understand.

Thanks in advance for getting back to me on this,
mike

Date: Tue, 03 Jun 2003 20:19:08 -0400
From: Ellen Mosley-Thompson <thompson.4@osu.edu>
Subject: Re: position paper by Mann,
Bradley et al that is a refutation to Soon et al
X-Sender: ethomps@pop.service.ohio-state.edu
To: Judy Jacobs <JJacobs@agu.org>, "Michael E. Mann" <mann@virginia.edu>
X-Mailer: QUALCOMM Windows Eudora Version 4.3

Judy and Mike -
This sounds outstanding.
Am I right in assuming that Fred reviews and approves the Forum pieces?
If so, can you hint about expediting this. Timing is very critical here.
Judy, thanks for taking the bull by the horns and getting the ball rolling.

Best regards,
Ellen

At 07:33 PM 06/03/2003 -0400, Judy Jacobs wrote:

Dear Dr. Mann,
Thanks for the prompt reply.
Based on what you have said, it sounds to me as if Mann, Bradley, et al. will not be in
violation of AGU's prohibition on duplicate publication.
The attachment to your e-mail definitely has the look and feel of something that would
be published in Eos under the "FORUM" column header. FORUM pieces are usually comments
on articles of any description that have been published in previous issues of Eos; or
they can be articles on purely scientific or science policy-related issues around which
there is some controversy or difference of opinion; or articles on current public issues
that are of interest to the geosciences; or on issues--science or broader policy
ones--on which there is an official AGU Position Statement. In this last category, I
offer, for example, the teaching of creationism in public schools, either alongside
evolution, or to the exclusion of evolution.
AGU has an official Position Statement, "Climate Change and Greenhouse Gases," which
states, among other things, that there is a high probability that man-made gases
primarily from the burning of fossil fuels is contributing to a gradual rise in mean
global temperatures. In this context, your proto-article--in the form of the attachment
you sent me-- would seem right on target for a Forum piece. However, since the Soon et
al. article wasn't actually published in Eos, anything that you and Dr. Bradley craft
will have to minimize reference to the specific article or articles, and concentrate on
"the science" that is set forth in these papers. Presumably this problem could be

solved by simply referencing these papers.

A Forum piece can be as long as 1500 words, or approximately 6 double-spaced pages. A maximum of two figures is permitted. A maximum of 10 references is encouraged, but if the number doesn't exceed 10 too outrageously, I don't make a fuss, and neither will Ellen.

Authors are now asked to submit their manuscripts and figures electronically via AGU's Internet-based Geophysical Electronic Manuscript System (GEMS), which makes it possible for the entire submission-review process to be conducted online.

If you have never used GEMS before, you can register for a login and password, and get initial instructions, by going to

[1]<http://eos-submit.agu.org/>

If you would like to have a set of step-by-step instructions for first-time GEMS users, please ask me.

Ellen indicated that she/you would like to get something published sooner rather than later. The Eos staff can certainly expedite the editorial process for anything you and your colleagues submit.

Don't hesitate to contact me with any further questions.

Best regards,

Judy Jacobs

Michael E. Mann wrote:

Dear Judy,

Thanks very much for getting back to me on this. Ellen had mentioned this possibility, and I have been looking forward to hearing back about this.

Michael Oppenheimer and I drafted an informal memo that we passed along to colleagues who needed some more background information so that they could comment on the Soon et al papers in response to various inquiries they were receiving from the press, etc. I've attached a copy of this memo.

It has not been our intention for this memo to appear in print, and it has not been submitted anywhere for publication. On the other hand, when Ellen mentioned the possibility of publishing something *like* this in e.g. the "Eos" forum, that seemed like an excellent idea to me, and several of my colleagues that I have discussed the possibility with.

What we had in mind was to produce a revised version of the basic memo that I've attached, modifying it where necessary, and perhaps expanding it a bit, seeking broader co-authorship by about 9 or so other leading climate scientists. So far, Phil Jones of the University of East Anglia, Ray Bradley of the University of Massachusetts, and Jonathan Overpeck of the University of Arizona, have all indicated their interest in co-authoring such a piece. We suspect that a few other individuals would be interested in being co-authors as well. I didn't want to pursue this further, however, until I knew whether or not an Eos piece was a possibility.

So pending further word from you, I would indeed be interested in preparing a multi-authored "position" paper for Eos in collaboration with these co-authors, based loosely on the memo that I have attached.

I look forward to further word from you on this.

best regards,

mike mann

At 04:59 PM 6/3/2003 -0400, you wrote:

Dear Dr. Mann,

I am the managing editor for Eos, the weekly newspaper of the American Geophysical Union.

Late last week, the Eos editor for atmospheric sciences, Ellen Mosley-Thompson, asked me if Eos would publish what she called "a position paper" by you, Phillip Bradley, et al that would, in effect, be a refutation to a paper by Soon et al. that was published in a British journal, Energy & Environment a few weeks ago. This Energy & Environment article was subsequently picked up by the Discovery Channel and other print and electronic media that reach the general public.

Before I can answer this question, I need to ask if you and your colleagues intend for this position paper to be published simultaneously in outlets other than Eos. If this is the case, I'm afraid it being published in Eos is a moot point, because of AGU's no duplicate publication policy: if the material has been published elsewhere first, AGU will not publish it.

I look forward to your response.

Best regrds,

Judy Jacobs

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\eudora\attach\MannPersp20021.pdf"

References

1. <http://eos-submit.agu.org/>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Edward Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Review- confidential REALLY URGENT
Date: Wed Jun 4 13:42:54 2003

I am really sorry but I have to nag about that review - Confidentially I now need a hard and if required extensive case for rejecting - to support Dave Stahle's and really as soon as you can. Please

Keith

At 08:00 AM 5/28/03 -0400, you wrote:

Hi Keith,

Okay, here is a zipped archive containing Jan's ring-width measurement series. The directory names are:

random

all

slope

flat

"All" contains files with "all" series; "slope" has those series Jan reckoned had curvilinear growth trends; "flat" has those series with linear growth trends; "random" are those series that Jan chose not to use. Note that I had to pull out the Mongolia data set. I would love to give you it, but Gordon would go nuts if he found out. I don't know any way around this problem.

The file names are:

01ath Athabasca

02bor Boreal

03cam Camphill

04que Quebec

05upp Upper Wright

06got Gotland

07jae Jaemtland

08lau Lauenen (site not used in paper)

09tir Tirol

10tor Tornestrask

11man Mangazeja

13pol Polar Urals

14tay Taymir

15zha Zhaschiviersk

I can't put my hands on the derived RCS indices for these sites just now, but I can find them if you want them. This at least gives you the basic data and how it was partitioned by Jan. I did not participate in this stage of the analysis, so any questions about it should be directed to Jan.

Cheers,

Ed

--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa>[2]/

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Edward Cook <drendro@ldeo.columbia.edu>
Subject: Re: Review- confidential REALLY URGENT
Date: Wed Jun 4 16:02:09 2003

Hi Big Boy

You just caught me as I was about to slope off after a brutal day - we spent all day yesterday interviewing for a job we have and then someone accepted it - and now Janice tells us we don't have the money to pay at the rate the job was advertised for! This attack sounds like the last straw- from what you say it is a waste of time my looking at it but send a copy anyway. The file you have is an old version of a reconstruction output for one Tornetrask reconstruction - if it was labelled something like 990 it is the original Nature one , but 997 (i Think//1) would make it the Climate Dynamics one . Trouble is I will have to go back and find out which . Please ring if I haven't my tomorrow to remind me - and concentrate on the review for now. I will also talk about an extended nearby data set (temp) that might allow a longer more rigorous validation . Kirsten has just done Math GCSE and Amy her driving test so I have to go and pick them up. I will look at the file and be ready with an answer by midday my time. the best and a beer til then

Keith

At 09:50 AM 6/4/03 -0400, you wrote:

Hi Keith,

Okay, today. Promise! Now something to ask from you. Actually somewhat important too. I got a paper to review (submitted to the Journal of Agricultural, Biological, and Environmental Sciences), written by a Korean guy and someone from Berkeley, that claims that the method of reconstruction that we use in dendroclimatology (reverse regression) is wrong, biased, lousy, horrible, etc. They use your Tornetrask recon as the main whipping boy. I have a file that you gave me in 1993 that comes from your 1992 paper. Below is part of that file. Is this the right one? Also, is it possible to resurrect the column headings? I would like to play with it in an effort to refute their claims. If published as is, this paper could really do some damage. It is also an ugly paper to review because it is rather mathematical, with a lot of Box-Jenkins stuff in it. It won't be easy to dismiss out of hand as the math appears to be correct theoretically, but it suffers from the classic problem of pointing out theoretical deficiencies, without showing that their improved inverse regression method is actually better in a practical sense. So they do lots of monte carlo stuff that shows the superiority of their method and the deficiencies of our way of doing things, but NEVER actually show how their method would change the Tornetrask reconstruction from what you produced. Your assistance here is greatly appreciated. Otherwise, I will let Tornetrask sink into the melting permafrost of northern Sweden (just kidding of course).

Cheers,

Ed

TORNETRASK RECONSTRUCTION

| | | | | | | |
|-----|-------|-------|------|------|------|------|
| 500 | 1.24 | -9.99 | 0.00 | 0.16 | 0.81 | 0.31 |
| 501 | 0.38 | -9.99 | 0.00 | 0.25 | 0.81 | 0.39 |
| 502 | 0.51 | -9.99 | 0.00 | 0.08 | 0.81 | 0.25 |
| 503 | 0.14 | -9.99 | 0.00 | 0.19 | 0.81 | 0.34 |
| 504 | -1.32 | -9.99 | 0.00 | 0.19 | 0.81 | 0.34 |
| 505 | -0.65 | -9.99 | 0.00 | 0.08 | 0.81 | 0.25 |
| 506 | -0.19 | -9.99 | 0.00 | 0.07 | 0.81 | 0.24 |
| 507 | 0.55 | -9.99 | 0.00 | 0.19 | 0.81 | 0.33 |
| 508 | 0.54 | -9.99 | 0.00 | 0.16 | 0.81 | 0.31 |
| 509 | 0.93 | -9.99 | 0.00 | 0.11 | 0.81 | 0.27 |
| 510 | 0.02 | -9.99 | 0.00 | 0.14 | 0.81 | 0.29 |
| 511 | -1.62 | -9.99 | 0.00 | 0.20 | 0.81 | 0.35 |
| 512 | -0.01 | -9.99 | 0.00 | 0.13 | 0.81 | 0.28 |
| 513 | 1.00 | -9.99 | 0.00 | 0.11 | 0.81 | 0.27 |
| 514 | 0.10 | -9.99 | 0.00 | 0.14 | 0.81 | 0.29 |
| 515 | -0.96 | -9.99 | 0.00 | 0.11 | 0.81 | 0.26 |
| 516 | -0.08 | -9.99 | 0.00 | 0.12 | 0.81 | 0.27 |
| 517 | 0.35 | -9.99 | 0.00 | 0.09 | 0.85 | 0.25 |
| 518 | 0.30 | -9.99 | 0.00 | 0.10 | 0.85 | 0.26 |
| 519 | 0.55 | -9.99 | 0.00 | 0.10 | 0.85 | 0.26 |
| 520 | -0.19 | -9.99 | 0.00 | 0.10 | 0.85 | 0.26 |
| 521 | -0.84 | -9.99 | 0.00 | 0.23 | 0.85 | 0.38 |
| 522 | -0.83 | -9.99 | 0.00 | 0.23 | 0.85 | 0.37 |
| 523 | 0.05 | -9.99 | 0.00 | 0.07 | 0.85 | 0.24 |
| 524 | -0.27 | -9.99 | 0.00 | 0.08 | 0.85 | 0.25 |
| 525 | 0.14 | -9.99 | 0.00 | 0.07 | 0.85 | 0.24 |
| 526 | 0.01 | -9.99 | 0.00 | 0.10 | 0.85 | 0.25 |
| 527 | -0.31 | -9.99 | 0.00 | 0.13 | 0.85 | 0.28 |
| 528 | 0.46 | -9.99 | 0.00 | 0.09 | 0.85 | 0.25 |
| 529 | 0.01 | -9.99 | 0.00 | 0.09 | 0.85 | 0.25 |

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|------|-------|-------|------|------|------|------|-------|------|-------|-------|
| 1848 | 0.10 | -9.99 | 0.00 | 0.09 | 1.00 | 0.24 | | | | |
| 1849 | -0.39 | -9.99 | 0.00 | 0.14 | 1.00 | 0.28 | | | | |
| 1850 | 0.55 | -9.99 | 0.00 | 0.16 | 1.00 | 0.29 | | | | |
| 1851 | 0.04 | -9.99 | 0.00 | 0.13 | 1.00 | 0.27 | 1.92 | 0.96 | -1.98 | -1.24 |
| | -1.41 | -0.35 | | | | | | | | |
| 1852 | 0.68 | -9.99 | 0.00 | 0.12 | 1.00 | 0.26 | -2.82 | 0.59 | 1.66 | 1.95 |
| | 2.12 | 0.70 | | | | | | | | |
| 1853 | 0.67 | -9.99 | 0.00 | 0.14 | 1.00 | 0.28 | -2.23 | 0.24 | 2.27 | 1.64 |
| | -0.33 | 0.32 | | | | | | | | |
| 1854 | 1.13 | -9.99 | 0.00 | 0.14 | 1.00 | 0.27 | 0.21 | 1.57 | 0.89 | 2.47 |

| | | | | | | | | | | | |
|-------|-------|-------|------|------|------|------|-------|-------|-------|-------|--|
| 2.11 | 1.45 | | | | | | | | | | |
| 1855 | 0.05 | -9.99 | 0.00 | 0.15 | 1.00 | 0.29 | -0.74 | -0.80 | 0.24 | 4.19 | |
| -0.16 | 0.55 | | | | | | | | | | |
| 1856 | -1.41 | -9.99 | 0.00 | 0.19 | 1.00 | 0.33 | -0.48 | -1.24 | -1.37 | -0.34 | |
| -2.55 | -1.20 | | | | | | | | | | |
| 1857 | -0.30 | -9.99 | 0.00 | 0.19 | 1.00 | 0.32 | -1.13 | -0.78 | -1.39 | -0.23 | |
| 2.44 | -0.22 | | | | | | | | | | |
| 1858 | 0.81 | -9.99 | 0.00 | 0.15 | 1.00 | 0.28 | -0.63 | 0.48 | 1.37 | 2.74 | |
| 2.72 | 1.34 | | | | | | | | | | |
| 1859 | -0.60 | -9.99 | 0.00 | 0.10 | 1.00 | 0.25 | -1.28 | 0.73 | 1.04 | 0.10 | |
| 0.16 | 0.15 | | | | | | | | | | |
| 1860 | 0.49 | -9.99 | 0.00 | 0.10 | 1.00 | 0.24 | -0.41 | -1.37 | 0.62 | 0.42 | |
| 0.17 | -0.11 | | | | | | | | | | |
| 1861 | 0.73 | -9.99 | 0.00 | 0.10 | 1.00 | 0.24 | -1.19 | -2.59 | 1.54 | 2.27 | |
| 0.33 | 0.07 | | | | | | | | | | |
| 1862 | -0.15 | -9.99 | 0.00 | 0.06 | 1.00 | 0.22 | -0.06 | 0.50 | -1.16 | -2.08 | |
| -1.95 | -0.95 | | | | | | | | | | |
| 1863 | 0.03 | -9.99 | 0.00 | 0.08 | 1.00 | 0.23 | 1.00 | -0.79 | 0.18 | -1.72 | |
| -0.60 | -0.39 | | | | | | | | | | |
| 1864 | -0.50 | -9.99 | 0.00 | 0.11 | 1.00 | 0.25 | -0.49 | -3.34 | 0.26 | 0.74 | |
| -2.40 | -1.05 | | | | | | | | | | |
| 1865 | -0.32 | -9.99 | 0.00 | 0.07 | 1.00 | 0.22 | 0.10 | 0.14 | -2.96 | 1.61 | |
| -1.31 | -0.48 | | | | | | | | | | |
| 1866 | -0.37 | -9.99 | 0.00 | 0.10 | 1.00 | 0.24 | 0.29 | -1.99 | 0.67 | -1.17 | |
| 0.67 | -0.31 | | | | | | | | | | |
| 1867 | -1.03 | -9.99 | 0.00 | 0.12 | 1.00 | 0.26 | -2.83 | -5.37 | -2.59 | -0.62 | |
| -0.31 | -2.34 | | | | | | | | | | |
| 1868 | -0.28 | -9.99 | 0.00 | 0.16 | 1.00 | 0.29 | -0.02 | 1.04 | -0.36 | 1.72 | |
| 2.78 | 1.03 | | | | | | | | | | |
| 1869 | -0.84 | -9.99 | 0.00 | 0.10 | 1.00 | 0.25 | 1.21 | -1.14 | -1.40 | 0.53 | |
| -0.63 | -0.29 | | | | | | | | | | |
| 1870 | -0.25 | -9.99 | 0.00 | 0.12 | 1.00 | 0.26 | 1.33 | -0.70 | -0.27 | 1.12 | |
| -0.36 | 0.22 | | | | | | | | | | |
| 1871 | -0.59 | -9.99 | 0.00 | 0.10 | 1.00 | 0.24 | -2.34 | -2.32 | -2.34 | 1.12 | |
| -0.09 | -1.19 | | | | | | | | | | |
| 1872 | 0.44 | -9.99 | 0.00 | 0.10 | 1.00 | 0.25 | 0.80 | 0.57 | 1.16 | 1.32 | |
| -0.34 | 0.70 | | | | | | | | | | |
| 1873 | 0.52 | -9.99 | 0.00 | 0.14 | 1.00 | 0.28 | -1.97 | -2.50 | 0.82 | 1.38 | |
| 0.12 | -0.43 | | | | | | | | | | |
| 1874 | -0.54 | -9.99 | 0.00 | 0.11 | 1.00 | 0.25 | 0.25 | -2.24 | -1.15 | 0.15 | |
| -1.06 | -0.81 | | | | | | | | | | |
| 1875 | 0.36 | -9.99 | 0.00 | 0.09 | 1.00 | 0.24 | -1.96 | 0.36 | 0.00 | 0.87 | |
| -0.33 | -0.21 | | | | | | | | | | |

| | | | | | | | | | | |
|-------|-------|-------|-------|------|------|------|-------|-------|-------|-------|
| 1876 | 0.46 | -0.15 | 0.61 | 0.12 | 1.00 | 0.25 | -0.70 | -3.06 | 1.93 | 0.74 |
| 0.34 | -0.15 | | | | | | | | | |
| 1877 | -0.98 | -1.74 | 0.76 | 0.14 | 1.00 | 0.28 | -3.31 | -2.70 | -1.18 | 0.26 |
| -1.76 | -1.74 | | | | | | | | | |
| 1878 | -0.04 | -0.19 | 0.15 | 0.08 | 1.00 | 0.23 | 1.02 | -0.30 | 0.16 | -1.71 |
| -0.12 | -0.19 | | | | | | | | | |
| 1879 | 0.20 | -0.41 | 0.62 | 0.10 | 1.00 | 0.25 | -1.24 | -0.19 | -1.09 | -0.64 |
| 1.09 | -0.41 | | | | | | | | | |
| 1880 | -1.05 | 0.14 | -1.19 | 0.17 | 1.00 | 0.31 | 0.17 | -0.53 | -0.70 | -0.20 |
| 1.94 | 0.14 | | | | | | | | | |
| 1881 | -1.34 | -1.88 | 0.54 | 0.17 | 1.00 | 0.30 | -3.66 | -2.02 | -1.35 | -1.07 |
| -1.32 | -1.88 | | | | | | | | | |
| 1882 | 0.30 | 0.37 | -0.08 | 0.16 | 1.00 | 0.30 | -0.32 | 0.21 | -0.36 | 0.56 |
| 1.78 | 0.37 | | | | | | | | | |
| 1883 | 1.13 | 0.24 | 0.89 | 0.13 | 1.00 | 0.26 | 0.49 | -0.08 | 0.99 | 0.52 |
| -0.70 | 0.24 | | | | | | | | | |
| 1884 | 0.00 | -0.80 | 0.80 | 0.14 | 1.00 | 0.27 | -0.80 | -1.99 | -1.15 | 0.32 |
| -0.39 | -0.80 | | | | | | | | | |
| 1885 | -1.26 | -1.25 | -0.01 | 0.14 | 1.00 | 0.28 | -0.29 | -2.26 | -2.34 | 0.42 |
| -1.76 | -1.25 | | | | | | | | | |
| 1886 | -0.24 | 0.10 | -0.34 | 0.15 | 1.00 | 0.28 | 0.69 | -0.55 | -0.01 | 0.13 |
| 0.24 | 0.10 | | | | | | | | | |
| 1887 | -0.83 | -0.40 | -0.43 | 0.14 | 1.00 | 0.27 | -0.10 | 0.23 | -1.01 | -0.12 |
| -1.02 | -0.40 | | | | | | | | | |
| 1888 | -0.79 | -1.69 | 0.90 | 0.12 | 1.00 | 0.26 | -2.95 | -1.85 | -1.37 | -1.05 |
| -1.25 | -1.69 | | | | | | | | | |
| 1889 | 0.28 | 0.71 | -0.43 | 0.08 | 1.00 | 0.23 | -0.46 | 2.98 | 2.28 | -0.40 |
| -0.84 | 0.71 | | | | | | | | | |
| 1890 | 0.47 | 0.22 | 0.25 | 0.08 | 1.00 | 0.23 | 1.06 | 2.04 | -0.58 | -1.18 |
| -0.26 | 0.22 | | | | | | | | | |
| 1891 | -0.55 | -0.49 | -0.06 | 0.16 | 1.00 | 0.30 | -0.43 | -0.38 | -1.74 | 1.24 |
| -1.12 | -0.49 | | | | | | | | | |
| 1892 | -1.58 | -1.46 | -0.12 | 0.16 | 1.00 | 0.29 | -0.95 | -1.55 | -2.20 | -1.24 |
| -1.36 | -1.46 | | | | | | | | | |
| 1893 | -0.61 | -0.60 | -0.01 | 0.10 | 1.00 | 0.24 | -0.46 | -1.17 | -0.48 | -0.07 |
| -0.80 | -0.60 | | | | | | | | | |
| 1894 | 0.53 | 0.79 | -0.26 | 0.09 | 1.00 | 0.24 | 2.61 | 0.07 | 0.50 | 1.18 |
| -0.40 | 0.79 | | | | | | | | | |
| 1895 | 0.68 | 0.38 | 0.30 | 0.09 | 1.00 | 0.24 | -0.15 | 2.19 | 0.78 | -0.66 |
| -0.24 | 0.38 | | | | | | | | | |
| 1896 | 0.06 | 0.47 | -0.41 | 0.11 | 1.00 | 0.25 | -0.04 | -0.30 | 1.40 | 2.02 |
| -0.73 | 0.47 | | | | | | | | | |
| 1897 | 0.71 | 1.01 | -0.30 | 0.13 | 1.00 | 0.27 | 0.90 | 2.20 | -0.20 | 1.10 |

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|-------|-------|-------|-------|------|------|------|-------|-------|-------|-------|--|
| 1.05 | 1.01 | | | | | | | | | | |
| 1898 | 0.10 | -0.61 | 0.71 | 0.12 | 1.00 | 0.25 | -1.06 | -0.20 | -0.16 | -1.03 | |
| -0.60 | -0.61 | | | | | | | | | | |
| 1899 | -1.36 | -0.84 | -0.53 | 0.17 | 1.00 | 0.31 | -0.98 | -1.95 | -1.85 | 2.38 | |
| -1.79 | -0.84 | | | | | | | | | | |
| 1900 | -0.38 | -0.89 | 0.51 | 0.18 | 1.00 | 0.31 | -1.31 | -2.02 | -0.02 | -1.11 | |
| -0.01 | -0.89 | | | | | | | | | | |
| 1901 | 0.85 | 1.32 | -0.47 | 0.17 | 1.00 | 0.30 | 0.76 | 0.56 | 1.05 | 3.24 | |
| 1.00 | 1.32 | | | | | | | | | | |
| 1902 | -1.59 | -2.44 | 0.85 | 0.19 | 1.00 | 0.33 | -2.71 | -2.33 | -2.44 | -2.52 | |
| -2.22 | -2.44 | | | | | | | | | | |
| 1903 | -1.27 | -0.42 | -0.85 | 0.20 | 1.00 | 0.33 | 0.36 | 0.14 | -0.37 | -1.02 | |
| -1.22 | -0.42 | | | | | | | | | | |
| 1904 | -1.52 | -1.11 | -0.42 | 0.15 | 1.00 | 0.29 | 0.77 | -1.61 | -1.73 | -1.64 | |
| -1.32 | -1.11 | | | | | | | | | | |
| 1905 | -0.45 | -0.06 | -0.39 | 0.08 | 1.00 | 0.23 | -1.29 | 0.69 | 1.41 | 0.05 | |
| -1.16 | -0.06 | | | | | | | | | | |
| 1906 | -0.44 | 0.55 | -0.98 | 0.08 | 1.00 | 0.23 | 1.44 | 1.74 | 0.34 | 0.69 | |
| -1.47 | 0.55 | | | | | | | | | | |
| 1907 | -0.40 | -1.10 | 0.69 | 0.07 | 1.00 | 0.23 | 0.24 | -2.05 | -0.31 | -0.70 | |
| -2.67 | -1.10 | | | | | | | | | | |
| 1908 | -0.15 | -0.55 | 0.41 | 0.11 | 1.00 | 0.25 | 0.36 | -1.22 | -1.31 | -0.22 | |
| -0.38 | -0.55 | | | | | | | | | | |
| 1909 | -0.77 | -1.71 | 0.94 | 0.09 | 1.00 | 0.24 | -2.54 | -3.21 | -1.26 | -0.51 | |
| -1.03 | -1.71 | | | | | | | | | | |
| 1910 | -0.16 | 0.00 | -0.16 | 0.09 | 1.00 | 0.24 | 1.18 | 0.91 | -0.19 | -0.60 | |
| -1.32 | 0.00 | | | | | | | | | | |
| 1911 | -0.38 | 0.02 | -0.40 | 0.09 | 1.00 | 0.24 | -0.37 | 1.25 | -1.34 | -0.55 | |
| 1.12 | 0.02 | | | | | | | | | | |
| 1912 | 0.06 | -0.23 | 0.29 | 0.06 | 1.00 | 0.22 | -1.32 | -0.99 | 0.16 | 0.79 | |
| 0.20 | -0.23 | | | | | | | | | | |
| 1913 | 0.08 | 0.29 | -0.21 | 0.07 | 1.00 | 0.22 | 1.68 | 0.02 | -1.15 | 0.99 | |
| -0.07 | 0.29 | | | | | | | | | | |
| 1914 | 0.09 | 0.84 | -0.75 | 0.07 | 1.00 | 0.22 | 1.51 | -0.37 | 0.47 | 3.50 | |
| -0.93 | 0.84 | | | | | | | | | | |
| 1915 | 0.11 | -0.91 | 1.01 | 0.06 | 1.00 | 0.22 | -0.20 | -1.59 | -2.40 | 0.61 | |
| -0.95 | -0.91 | | | | | | | | | | |
| 1916 | -0.35 | -0.51 | 0.16 | 0.13 | 1.00 | 0.26 | 0.46 | -1.26 | -1.37 | 1.65 | |
| -2.04 | -0.51 | | | | | | | | | | |
| 1917 | 0.18 | -0.02 | 0.20 | 0.11 | 1.00 | 0.25 | -1.95 | -1.60 | 1.89 | -0.78 | |
| 2.35 | -0.02 | | | | | | | | | | |
| 1918 | 0.71 | -0.39 | 1.10 | 0.10 | 1.00 | 0.24 | 1.11 | -0.49 | -1.73 | 0.68 | |
| -1.52 | -0.39 | | | | | | | | | | |

| | | | | | | | | | | |
|-------|-------|-------|-------|------|------|------|-------|-------|-------|-------|
| 1919 | -0.09 | 0.12 | -0.21 | 0.07 | 1.00 | 0.22 | -0.88 | 1.29 | 0.09 | 1.87 |
| -1.79 | 0.12 | | | | | | | | | |
| 1920 | 0.33 | 0.85 | -0.52 | 0.07 | 1.00 | 0.22 | 2.05 | 2.16 | -0.36 | 0.93 |
| -0.51 | 0.85 | | | | | | | | | |
| 1921 | 0.29 | 0.75 | -0.46 | 0.10 | 1.00 | 0.24 | 3.97 | 2.43 | -0.68 | -1.35 |
| -0.62 | 0.75 | | | | | | | | | |
| 1922 | 0.66 | -0.23 | 0.89 | 0.12 | 1.00 | 0.26 | -0.60 | 0.22 | 0.00 | 0.12 |
| -0.88 | -0.23 | | | | | | | | | |
| 1923 | -0.66 | -1.84 | 1.19 | 0.12 | 1.00 | 0.26 | -1.53 | -1.74 | -3.76 | 0.02 |
| -2.20 | -1.84 | | | | | | | | | |
| 1924 | 0.49 | -0.46 | 0.95 | 0.08 | 1.00 | 0.23 | -1.60 | -0.68 | -1.93 | 0.64 |
| 1.25 | -0.46 | | | | | | | | | |
| 1925 | 0.30 | 1.10 | -0.80 | 0.12 | 1.00 | 0.26 | 1.66 | 0.70 | -0.63 | 3.49 |
| 0.30 | 1.10 | | | | | | | | | |
| 1926 | 0.47 | 0.06 | 0.41 | 0.10 | 1.00 | 0.24 | -0.06 | -0.51 | 0.02 | 0.75 |
| 0.12 | 0.06 | | | | | | | | | |
| 1927 | 0.23 | 0.10 | 0.14 | 0.11 | 1.00 | 0.25 | -0.58 | -2.17 | -1.54 | 3.18 |
| 1.60 | 0.10 | | | | | | | | | |
| 1928 | -0.82 | -1.21 | 0.39 | 0.11 | 1.00 | 0.25 | 0.42 | -0.20 | -3.05 | -2.14 |
| -1.09 | -1.21 | | | | | | | | | |
| 1929 | 0.00 | -1.25 | 1.26 | 0.15 | 1.00 | 0.28 | -3.24 | 0.57 | -1.51 | -1.02 |
| -1.06 | -1.25 | | | | | | | | | |
| 1930 | 1.00 | 1.42 | -0.42 | 0.16 | 1.00 | 0.29 | 1.78 | 1.81 | 0.59 | 1.58 |
| 1.34 | 1.42 | | | | | | | | | |
| 1931 | -0.67 | -0.21 | -0.46 | 0.08 | 1.00 | 0.23 | -0.29 | 1.18 | -2.95 | 1.21 |
| -0.20 | -0.21 | | | | | | | | | |
| 1932 | -0.32 | 0.27 | -0.59 | 0.08 | 1.00 | 0.23 | 0.54 | 0.03 | -1.68 | 1.74 |
| 0.74 | 0.27 | | | | | | | | | |
| 1933 | 0.65 | 0.36 | 0.29 | 0.12 | 1.00 | 0.26 | -0.33 | -0.86 | 1.64 | 1.77 |
| -0.43 | 0.36 | | | | | | | | | |
| 1934 | 0.56 | 0.98 | -0.42 | 0.12 | 1.00 | 0.26 | 0.37 | 1.88 | -0.48 | 1.88 |
| 1.27 | 0.98 | | | | | | | | | |
| 1935 | -0.56 | -0.37 | -0.20 | 0.09 | 1.00 | 0.24 | 0.30 | -1.94 | 0.11 | -0.05 |
| -0.25 | -0.37 | | | | | | | | | |
| 1936 | -0.09 | 1.48 | -1.57 | 0.19 | 1.00 | 0.33 | 0.03 | 1.84 | 2.96 | 1.86 |
| 0.71 | 1.48 | | | | | | | | | |
| 1937 | 1.77 | 2.39 | -0.62 | 0.19 | 1.00 | 0.32 | 2.82 | 2.55 | 1.32 | 2.26 |
| 3.01 | 2.39 | | | | | | | | | |
| 1938 | 0.58 | 0.91 | -0.33 | 0.09 | 1.00 | 0.24 | 0.59 | -0.07 | -0.60 | 2.49 |
| 2.14 | 0.91 | | | | | | | | | |
| 1939 | 0.31 | 0.71 | -0.40 | 0.08 | 1.00 | 0.23 | -0.22 | -0.15 | 0.04 | 0.99 |
| 2.88 | 0.71 | | | | | | | | | |
| 1940 | 0.20 | 0.42 | -0.22 | 0.15 | 1.00 | 0.28 | -0.95 | 2.26 | 0.72 | 0.67 |

| | | | | | | | | | | | |
|-------|-------|-------|-------|------|------|------|-------|-------|-------|-------|--|
| -0.60 | 0.42 | | | | | | | | | | |
| 1941 | -0.03 | -0.20 | 0.17 | 0.14 | 1.00 | 0.28 | -2.00 | -1.34 | -1.20 | 3.70 | |
| -0.17 | -0.20 | | | | | | | | | | |
| 1942 | 0.11 | -0.50 | 0.61 | 0.08 | 1.00 | 0.23 | 0.14 | -1.04 | -1.47 | -0.32 | |
| 0.20 | -0.50 | | | | | | | | | | |
| 1943 | 0.36 | 0.69 | -0.33 | 0.07 | 1.00 | 0.22 | 1.55 | 0.88 | 0.99 | 0.69 | |
| -0.64 | 0.69 | | | | | | | | | | |
| 1944 | 0.12 | -0.50 | 0.62 | 0.10 | 1.00 | 0.24 | -1.67 | -1.25 | -1.58 | 1.18 | |
| 0.83 | -0.50 | | | | | | | | | | |
| 1945 | 0.57 | 0.71 | -0.14 | 0.10 | 1.00 | 0.25 | 1.21 | -0.53 | -0.86 | 1.81 | |
| 1.91 | 0.71 | | | | | | | | | | |
| 1946 | 0.48 | 0.64 | -0.16 | 0.09 | 1.00 | 0.24 | 1.17 | 0.28 | -0.18 | 1.62 | |
| 0.31 | 0.64 | | | | | | | | | | |
| 1947 | 0.69 | 1.20 | -0.51 | 0.10 | 1.00 | 0.24 | 0.18 | 1.48 | 1.69 | 1.43 | |
| 1.20 | 1.20 | | | | | | | | | | |
| 1948 | 0.00 | 0.67 | -0.67 | 0.08 | 1.00 | 0.23 | 2.10 | 1.66 | 0.03 | 0.74 | |
| -1.18 | 0.67 | | | | | | | | | | |
| 1949 | -0.21 | 0.11 | -0.32 | 0.14 | 1.00 | 0.27 | 1.26 | 1.76 | -1.34 | -0.14 | |
| -1.01 | 0.11 | | | | | | | | | | |
| 1950 | 0.83 | 0.73 | 0.09 | 0.10 | 1.00 | 0.24 | 2.24 | 0.91 | -0.14 | -0.52 | |
| 1.18 | 0.73 | | | | | | | | | | |
| 1951 | -0.13 | -0.34 | 0.21 | 0.07 | 1.00 | 0.22 | 0.78 | -1.83 | -1.25 | -1.23 | |
| 1.84 | -0.34 | | | | | | | | | | |
| 1952 | -0.13 | -0.38 | 0.25 | 0.12 | 1.00 | 0.26 | 1.78 | -0.91 | -1.17 | -0.26 | |
| -1.34 | -0.38 | | | | | | | | | | |
| 1953 | 0.95 | 1.11 | -0.16 | 0.11 | 1.00 | 0.25 | 1.80 | 0.21 | 3.01 | 0.37 | |
| 0.16 | 1.11 | | | | | | | | | | |
| 1954 | 0.12 | 0.32 | -0.20 | 0.10 | 1.00 | 0.24 | -0.60 | 2.11 | -0.57 | 0.70 | |
| -0.05 | 0.32 | | | | | | | | | | |
| 1955 | 0.02 | -0.76 | 0.77 | 0.09 | 1.00 | 0.24 | -2.65 | -2.42 | -2.22 | 1.42 | |
| 2.09 | -0.76 | | | | | | | | | | |
| 1956 | -0.26 | -0.94 | 0.68 | 0.07 | 1.00 | 0.22 | -2.32 | 0.39 | 0.12 | -0.73 | |
| -2.15 | -0.94 | | | | | | | | | | |
| 1957 | -0.15 | -0.31 | 0.16 | 0.07 | 1.00 | 0.22 | -0.09 | -0.53 | -2.06 | 1.32 | |
| -0.19 | -0.31 | | | | | | | | | | |
| 1958 | -0.08 | -0.90 | 0.82 | 0.09 | 1.00 | 0.24 | -1.29 | -1.07 | -1.05 | -0.77 | |
| -0.31 | -0.90 | | | | | | | | | | |
| 1959 | 0.83 | 0.98 | -0.16 | 0.15 | 1.00 | 0.28 | 1.03 | 0.66 | 0.44 | 1.32 | |
| 1.47 | 0.98 | | | | | | | | | | |
| 1960 | 1.13 | 1.02 | 0.11 | 0.13 | 1.00 | 0.27 | 0.63 | 1.88 | 0.92 | 1.39 | |
| 0.29 | 1.02 | | | | | | | | | | |
| 1961 | 0.05 | 0.17 | -0.11 | 0.10 | 1.00 | 0.25 | -0.12 | 0.10 | 1.47 | 0.19 | |
| -0.81 | 0.17 | | | | | | | | | | |

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|-------|-------|-------|-------|------|------|------|-------|-------|-------|-------|
| 1962 | -0.45 | -1.01 | 0.56 | 0.09 | 1.00 | 0.24 | 1.27 | -0.52 | -2.15 | -1.65 |
| -2.00 | -1.01 | | | | | | | | | |
| 1963 | 0.11 | 0.79 | -0.68 | 0.18 | 1.00 | 0.31 | 0.43 | 3.15 | -0.33 | -0.07 |
| 0.77 | 0.79 | | | | | | | | | |
| 1964 | -0.21 | -0.09 | -0.13 | 0.15 | 1.00 | 0.28 | 0.64 | 1.02 | -0.78 | -0.42 |
| -0.90 | -0.09 | | | | | | | | | |
| 1965 | -0.82 | -0.82 | 0.00 | 0.10 | 1.00 | 0.24 | 0.62 | -1.64 | -0.03 | -1.74 |
| -1.30 | -0.82 | | | | | | | | | |
| 1966 | 0.07 | -0.13 | 0.20 | 0.06 | 1.00 | 0.22 | -2.47 | 0.26 | 1.97 | 0.46 |
| -0.87 | -0.13 | | | | | | | | | |
| 1967 | -0.22 | 0.21 | -0.44 | 0.08 | 1.00 | 0.23 | 0.69 | 0.29 | -0.80 | 0.13 |
| 0.75 | 0.21 | | | | | | | | | |
| 1968 | -0.57 | 0.10 | -0.67 | 0.13 | 1.00 | 0.27 | 1.18 | -1.20 | 1.37 | -1.07 |
| 0.22 | 0.10 | | | | | | | | | |
| 1969 | 0.55 | 0.54 | 0.01 | 0.08 | 1.00 | 0.23 | 0.21 | -0.61 | 0.90 | 0.37 |
| 1.82 | 0.54 | | | | | | | | | |
| 1970 | 0.37 | 0.40 | -0.04 | 0.10 | 1.00 | 0.24 | -1.25 | 0.51 | 2.27 | 0.05 |
| 0.44 | 0.40 | | | | | | | | | |
| 1971 | -0.31 | -0.12 | -0.19 | 0.07 | 1.00 | 0.22 | -0.71 | 0.81 | -0.64 | 0.03 |
| -0.07 | -0.12 | | | | | | | | | |
| 1972 | 0.25 | 1.18 | -0.94 | 0.08 | 1.00 | 0.23 | 0.18 | 0.44 | 1.62 | 3.00 |
| 0.68 | 1.18 | | | | | | | | | |
| 1973 | 0.30 | 0.85 | -0.55 | 0.10 | 0.99 | 0.25 | -0.02 | 0.76 | 1.31 | 2.85 |
| -0.66 | 0.85 | | | | | | | | | |
| 1974 | 0.07 | 0.12 | -0.05 | 0.11 | 0.99 | 0.25 | 0.86 | -0.41 | 0.62 | -0.30 |
| -0.18 | 0.12 | | | | | | | | | |
| 1975 | -0.49 | 0.51 | -1.00 | 0.08 | 0.99 | 0.23 | 0.45 | 1.72 | -1.09 | 0.62 |
| 0.84 | 0.51 | | | | | | | | | |
| 1976 | 0.08 | -9.99 | 0.00 | 0.07 | 0.99 | 0.22 | -0.28 | 1.72 | -1.36 | -0.23 |
| 0.05 | -0.02 | | | | | | | | | |
| 1977 | -0.33 | -9.99 | 0.00 | 0.08 | 0.99 | 0.23 | -1.05 | -0.01 | -0.50 | -0.90 |
| -0.65 | -0.62 | | | | | | | | | |
| 1978 | -0.30 | -9.99 | 0.00 | 0.07 | 0.96 | 0.23 | -0.98 | 0.92 | 0.14 | -0.48 |
| -1.07 | -0.29 | | | | | | | | | |
| 1979 | 0.06 | -9.99 | 0.00 | 0.12 | 0.95 | 0.26 | -0.73 | 0.75 | 1.02 | -0.83 |
| 0.07 | 0.06 | | | | | | | | | |
| 1980 | 0.93 | -9.99 | 0.00 | 0.13 | 0.95 | 0.26 | 1.42 | -0.37 | 1.23 | 1.02 |
| -0.36 | 0.59 | | | | | | | | | |

I am really sorry but I have to nag about that review - Confidentially I now need a hard and if required extensive case for rejecting - to support Dave Stahle's and really as soon as you can. Please
Keith

At 08:00 AM 5/28/03 -0400, you wrote:

Hi Keith,

Okay, here is a zipped archive containing Jan's ring-width measurement series. The directory names are:

random

all

slope

flat

"All" contains files with "all" series; "slope" has those series Jan reckoned had curvilinear growth trends; "flat" has those series with linear growth trends; "random" are those series that Jan chose not to use. Note that I had to pull out the Mongolia data set. I would love to give you it, but Gordon would go nuts if he found out. I don't know any way around this problem.

The file names are:

01ath Athabasca

02bor Boreal

03cam Camphill

04que Quebec

05upp Upper Wright

06got Gotland

07jae Jaemtland

08lau Lauenen (site not used in paper)

09tir Tirol

10tor Tornestrask

11man Mangazeja

13pol Polar Urals

14tay Taymir

15zha Zhaschiviersk

I can't put my hands on the derived RCS indices for these sites just now, but I can find them if you want them. This at least gives you the basic data and how it was partitioned by Jan. I did not participate in this stage of the analysis, so any questions about it should be directed to Jan.

Cheers,

Ed

--

=====
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[2]<http://www.cru.uea.ac.uk/cru/people/briffa>[3]/

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Michael E. Mann" <mann@virginia.edu>

To: rbradley@geo.umass.edu, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>

Subject: Fwd: Re: Prospective Eos piece?

Date: Wed, 04 Jun 2003 16:12:06 -0400

Cc: mann@virginia.edu, Scott Rutherford <srutherford@gso.uri.edu>

Dear All,

I've attached a draft (attached word document), incorporating many of the suggestions, wording, etc. I've already recieved from various of you. Some specific comments/inquiries/requests for help indicated in yellow highlighting. Waiting to hear back from Peck and Tom C (guys: if you're out there, can you give a holler, to let me know your disposition? thanks). Otherwise everyone else has indicated they're on board.

I've been in touch w/ Judy Jacobs at AGU to clarify the ground rules. Apparently we *can* refer, where necessary, to press releases, parenthetically in the piece. I think this is important in our case because there is a subtle, but important, distinction between what the papers actual purport to show, and what the authors (and their promoters) have *claimed* they show (e.g. in the Harvard-Smithsonian press release). We need to draw out this distinction-I sent Judy my paragraph on that, and she said it looks fine--so apparently its kosher.

I've avoided any reference to unpublished work however (e.g. Mann and Jones), because this opens up a can of worms. We can nicely make use of work that Keith has already done to provide a suggestion of the longer-term (past 2K) changes, for greater context...

Re, references--we necessarily have to go well over the normal 10 or so, because part of the strength of our piece is the wealth of recent studies supporting our basic conclusions.

Judy said that's ok too--especially since our text is short (by about 100 words) relative to the official (1200 word) limit. So we should try to keep it that way..ie, we need to play a zero-sum game, as much as possible, with any suggested revisions.

Re figures, Scott Rutherford has generously offered to help prepare a draft of figure 1 which I'll send on to everyone once its available.

I've also described, in the figure caption, my concept of Figure 2--clearly it would be helpful if Phil and Ray could collaborate on the preparation of this one (guys?).

Looking forward to comments, and suggested revisions. I'll just accumulate these from everyone in whatever form you prefer to provide them (emailed comments, word file w/ track changes or highlighting of changes used, etc) and try to prepare a revised draft once I've heard back from everyone.

Thanks again to everyone for their willingness to help with this and to be involved with this,

mike

Date: Wed, 04 Jun 2003 10:17:57 -0400

To: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu, Tom Wigley <wigley@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Keith Briffa <k.briffa@uea.ac.uk>, trenbert@cgd.ucar.edu, Michael Oppenheimer <omichael@princeton.edu>, Jonathan Overpeck <jto@u.arizona.edu>

From: "Michael E. Mann" <mann@virginia.edu>

Subject: Re: Prospective Eos piece?

Cc: mann@virginia.edu, Scott Rutherford <srutherford@gso.uri.edu>

Thanks Phil, and Thanks Tom W and Keith for your willingness to help/sign on. This certainly gives us a "quorum" pending even a few possible additional signatories I'm waiting to hear back from.

In response to the queries, I will work on a draft today w/ references and two suggested figures, and will try to send on by this evening (east coast USA). Tom W indicated that he wouldn't be able look at a draft until Thursday anyway, so why doesn't everyone just take a day then to digest what I've provided and then get back to me with comments/changes (using word "track changes" if you like).

I'd like to tentatively propose to pass this along to Phil as the "official keeper" of the draft to finalize and submit IF it isn't in satisfactory shape by the time I have to leave (July 11--If I hadn't mentioned, I'm getting married, and then honeymoon, prior to IUGG in Sapporo--gone for about 1 month total). Phil, does that sound ok to you?

Re Figures, what I had in mind were the following two figures:

1) A plot of various of the most reliable (in terms of strength of temperature signal and reliability of millennial-scale variability) regional proxy temperature reconstructions around the Northern Hemisphere that are available over the past 1-2 thousand years to convey the important point that warm and cold periods where highly regionally variable. Phil and Ray are probably in the best position to prepare this (?). Phil and I have recently submitted a paper using about a dozen NH records that fit this category, and many of which are available nearly 2K back--I think that trying to adopt a timeframe of 2K, rather than the usual 1K, addresses a good earlier point that Peck made w/ regard to the memo, that it would be nice to try to "contain" the putative "MWP", even if we don't yet have a hemispheric mean reconstruction available that far back [Phil and I have one in review--not sure it is kosher to show that yet though--I've put in an inquiry to Judy Jacobs at AGU about this]. If we wanted to be fancy, we could do this the way certain plots were presented in one of the past IPCC reports (was it 1990?) in which a spatial map was provided in the center (this would show the locations of the proxies), with "rays" radiating out to the top, sides, and bottom attached to rectangles showing the different timeseries. Its a bit of work, but would be a great way to convey both the spatial and temporal information at the same time.

2) A version of the now-familiar "spaghetti plot" showing the various reconstructions as well as model simulations for the NH over the past 1 (or maybe 2K). To give you an idea of what I have in mind, I'm attaching a Science piece I wrote last year that contains the same sort of plot.

However, what I'd like to do different here is:

In addition to the "multiproxy" reconstructions, I'd like to Add Keith's maximum latewood density-based series, since it is entirely independent of the multiproxy series, but conveys the same basic message. I would also like to try to extend the scope of the plot back to nearly 2K. This would be either w/ the Mann and Jones extension (in review in GRL) or, if that is deemed not kosher, the Briffa et al Eurasian tree-ring composite that extends back about 2K, and, based on Phil and my results, appears alone to give a reasonably accurate picture of the full hemispheric trend.

Thoughts, comments on any of this?

thanks all for the help,

mike

At 09:25 AM 6/4/2003 +0100, Phil Jones wrote:

Mike,

This is definitely worth doing and I hope you have the time before the 11th, or can pass it on to one of us at that time. As you know I'm away for a couple of days but back Friday.

So count me in. I've forwarded you all the email comments I've sent to reporters/fellow scientists, so you're fully aware of my views, which are essentially the same as all of the list

and many others in paleo. EOS would get to most fellow scientists. As I said to you the other

day, it is amazing how far and wide the SB pieces have managed to percolate. When it comes

out I would hope that AGU/EOS 'publicity machine' will shout the message from rooftops everywhere. As many of us need to be available when it comes out.

There is still no firm news on what Climate Research will do, although they will likely

have two editors for potentially controversial papers, and the editors will consult when papers

get different reviews. All standard practice I'd have thought. At present the editors get no

guidance whatsoever. It would seem that if they don't know what standard practice is then

they shouldn't be doing the job !

Cheers

Phil

At 22:34 03/06/03 -0400, Michael E. Mann wrote:

Dear Colleagues,

Eos has invited me (and prospective co-authors) to write a 'forum' piece (see below).

This was at Ellen Mosely-Thompson's suggestion, upon my sending her a copy of the attached memo that Michael Oppenheimer and I jointly wrote. Michael and I wrote this to assist colleagues who had been requesting more background information to help counter the spurious claims (with which I believe you're all now familiar) of the latest Baliunas & Soon pieces.

The idea I have in mind would be to use what Michael and I have drafted as an initial starting point for a slightly expanded piece, that would address the same basic issues and, as indicated below, could include some references and figures. As indicated in Judy Jacobs' letter below, the piece would be rewritten in such a way as to be less explicitly (though perhaps not less implicitly) directed at the Baliunas/Soon claims, criticisms, and attacks.

Phil, Ray, and Peck have already indicated tentative interest in being co-authors. I'm sending this to the rest of you (Tom C, Keith, Tom W, Kevin) in the hopes of broadening the list of co-authors. I strongly believe that a piece of this sort co-authored by 9 or so prominent members of the climate research community (with background and/or interest in paleoclimate) will go a long way in helping to counter these attacks, which are being used, in turn, to launch attacks against IPCC.

AGU has offered to expedite the process considerably, which is necessary because I'll be travelling for about a month beginning June 11th. So I'm going to work hard to get something together ASAP. I'd would therefore greatly appreciate a quick response from each of you as to whether or not you would potentially be willing to be involved as a co-author. If you're unable or unwilling given other current commitments, I'll understand.

Thanks in advance for getting back to me on this,

mike

Date: Tue, 03 Jun 2003 20:19:08 -0400

From: Ellen Mosley-Thompson <thompson.4@osu.edu>

Subject: Re: position paper by Mann,

Bradley et al that is a refutation to Soon et al

X-Sender: ethompo@pop.service.ohio-state.edu

To: Judy Jacobs <JJacobs@agu.org>, "Michael E. Mann" <mann@virginia.edu>

X-Mailer: QUALCOMM Windows Eudora Version 4.3

Judy and Mike -

This sounds outstanding.

Am I right in assuming that Fred reviews and approves the Forum pieces?

If so, can you hint about expediting this. Timing is very critical here.

Judy, thanks for taking the bull by the horns and getting the ball rolling.

Best regards,

Ellen

At 07:33 PM 06/03/2003 -0400, Judy Jacobs wrote:

Dear Dr. Mann,

Thanks for the prompt reply.

Based on what you have said, it sounds to me as if Mann, Bradley, et al. will not be in violation of AGU's prohibition on duplicate publication.

The attachment to your e-mail definitely has the look and feel of something that would be published in Eos under the "FORUM" column header. FORUM pieces are usually comments on articles of any description that have been published in previous issues of Eos; or they can be articles on purely scientific or science policy-related issues around which there is some controversy or difference of opinion; or articles on current public issues that are of interest to the geosciences; or on issues--science or broader policy ones--On which there is an official AGU Position Statement. In this last category, I offer, for example, the teaching of creationism in public schools, either alongside evolution, or to the exclusion of evolution.

AGU has an official Position Statement, "Climate Change and Greenhouse Gases," which states, among other things, that there is a high probability that man-made gases primarily from the burning of fossil fuels is contributing to a gradual rise in mean globab temperatures. In this context, your proto-article--in the form of the attachment you sent me-- would seem right on target for a Forum piece. However, since the Soon et al. article wasn't actually published in Eos, anything that you and Dr. Bradley craft will have to minimize reference to the specific article or articles, and concentrate on "the science" that is set forth in these papers. Presumably this problem could be solved by simply referencing these papers.

A Forum piece can be as long as 1500 words, or approximately 6 double-spaced pages. A maximum of two figures is permitted. A maximum of 10 references is encouraged, but if the number doesn't exceed 10 too outrageously, I don't make a fuss, and neither will Ellen.

Authors are now asked to submit their manuscripts and figures electronically via AGU's Internet-based Geophysical Electronic Manuscript System (GEMS), which makes it possible for the entire submission-review process to be conducted online.

If you have never used GEMS before, you can register for a login and password, and get initial instructions, by going to

[1]<http://eos-submit.agu.org/>

If you would like to have a set of step-by-step instructions for first-time GEMS users, please ask me.

Ellen indicated that she/you would like to get something published sooner rather than later. The Eos staff can certainly expedite the editorial process for anything you and your colleagues submit.

Don't hesitate to contact me with any further questions.

Best regards,

Judy Jacobs

Michael E. Mann wrote:

Dear Judy,

Thanks very much for getting back to me on this. Ellen had mentioned this possibility,

and I have been looking forward to hearing back about this.

Michael Oppenheimer and I drafted an informal memo that we passed along to colleagues who needed some more background information so that they could comment on the Soon et al papers in response to various inquiries they were receiving from the press, etc. I've attached a copy of this memo.

It has not been our intention for this memo to appear in print, and it has not been submitted anywhere for publication. On the other hand, when Ellen mentioned the possibility of publishing something *like* this in e.g. the "Eos" forum, that seemed like an excellent idea to me, and several of my colleagues that I have discussed the possibility with.

What we had in mind was to produce a revised version of the basic memo that I've attached, modifying it where necessary, and perhaps expanding it a bit, seeking broader co-authorship by about 9 or so other leading climate scientists. So far, Phil Jones of the University of East Anglia, Ray Bradley of the University of Massachusetts, and Jonathan Overpeck of the University of Arizona, have all indicated their interest in co-authoring such a piece. We suspect that a few other individuals would be interested in being co-authors as well. I didn't want to pursue this further, however, until I knew whether or not an Eos piece was a possibility.

So pending further word from you, I would indeed be interested in preparing a multi-authored "position" paper for Eos in collaboration with these co-authors, based loosely on the memo that I have attached.

I look forward to further word from you on this.

best regards,

mike mann

At 04:59 PM 6/3/2003 -0400, you wrote:

Dear Dr. Mann,
I am the managing editor for Eos, the weekly newspaper of the American Geophysical Union.
Late last week, the Eos editor for atmospheric sciences, Ellen Mosley-Thompson, asked me if Eos would publish what she called "a position paper" by you, Phillip Bradley, et al that would, in effect, be a refutation to a paper by Soon et al. that was published in a British journal, Energy & Environment a few weeks ago. This Energy & Environment article was subsequently picked up by the Discovery Channel and other print and electronic media that reach the general public.
Before I can answer this question, I need to ask if you and your colleagues intend for this position paper to be published simultaneously in outlets other than Eos. If this is the case, I'm afraid it being published in Eos is a moot point, because of AGU's no duplicate publication policy: if the material has been published elsewhere first, AGU will not publish it.
I look forward to your response.
Best regards,
Judy Jacobs

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\eudora\attach\EosForum.doc"

References

1. <http://eos-submit.agu.org/>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
5. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: Re: Revised Version!
Date: Sat, 07 Jun 2003 12:40:12 -0400
Cc: "Raymond S. Bradley" <rbradley@geo.umass.edu>, Keith Briffa
<k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Caspar Ammann
<ammann@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Michael Oppenheimer
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<srutherford@gso.uri.edu>, mann@virginia.edu

Thanks Kevin,

Those are helpful--Tom C. has returned from travels and will be providing comments shortly.

Will incorporate those and any others I receive into a revised version, which I hope to send out (w/ Figure 1 included) tonight or tomorrow,
mike

p.s. Tom W is taking the lead on preparing a companion, more targeted commentary, to be submitted to "Climate Research". Any one else interested should contact Tom...

At 05:16 PM 6/6/2003 -0600, Kevin Trenberth wrote:

Good job. I am attaching marked up copy with few suggestions.
Kevin
Michael E. Mann wrote:

Dear all,
Here is my best attempt to incorporate everyone's suggestions, views, etc. One major change you'll notice is that the final item (the one on co2 increase and recent warming) was eliminated, because it seemed to open a can of warms, and also distract from the central message. Note that, with the number of references we have, we are currently just about at the word limit for the piece. We shouldn't go over 1400 words, which puts some tight constraint on any additions, etc.
I hope to forward a draft of Figure 1 later on this afternoon. I'm assuming that Phil can take care of Figure 2 (Phil?--Scott has graciously indicated his willingness to help if necessary), but its pretty clear what this figure will show, so I don't think its that essential that we have that figure done to try to finalize the draft.
I'll attempt one final(?) revision of the text based on any remaining comments you may have--please try, if possible, to keep the suggested changes minimal at this point. I'll assume that anyone we haven't yet heard back from in the author list over the next day

or so is unable to be a co-author, and will respectfully drop them from the author list any related future emailings.

Thanks all for your help. Its rare to have every single co-author make substantial contributions to improving the draft, and that was clearly the case here...

mike

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[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <mailto:mann@virginia.edu>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <mailto:trenbert@ucar.edu>
4. <http://www.cgd.ucar.edu/cas/>

5. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>, Scott Rutherford <srutherford@gso.uri.edu>
Subject: Re: Figure 1
Date: Tue, 10 Jun 2003 11:18:17 -0400
Cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

Sounds great on all counts.

Kevin's comments are all good ones,
mike

At 04:09 PM 6/10/2003 +0100, Phil Jones wrote:

Scott,

Seems OK. we will send both figures and the text for one last look through today.
Trying now to incorporate Kevin's comments.

Cheers

Phil

At 10:48 10/06/03 -0400, Scott Rutherford wrote:

Phil and others,

Here is a revised figure. What do you think?

Scott

On Tuesday, June 10, 2003, at 07:21 AM, Phil Jones wrote:

Scott (and Mike if he's still there),

The three of us have been through the text, Fig 1 and decided what to put in Fig 2.

Tim is doing Fig 2 (9 long series - we'll send when we have it). I'm modifying the text

slightly - adding in refs that are missing (mostly with Fig 2) and generally tidying up.

Keith is working on the final sentence of the penultimate para. We all agree with this,

but it could be misinterpreted - so trying to avoid this.

WRT Fig 1.

There are quite a few changes we think would improve things and make it more consistent, all to the labelling.

1. Add et al to Bauer and Gerber (twice).
2. Years only in for Mann et al., so this is the only one where refs would be ambiguous.
3. So, Briffa et al 2000 becomes Briffa and Osborn 1999
4. Briffa et al, 2001 becomes Briffa et al .

5 Remove Long instrumental - the orange line from the plot and key.

It isn't explained in the caption, nor in the text.

6. As the grey line may not be seen under the grey shading, we think that all lines should

be as thin as the grey one. Some are thicker than others - can all be the same thinness.

7. Back to key, change Optimal borehole (Mann et al, 2003) to Mann et al. 2003 (Optimal

borehole) for consistency with the others.

8 . Most important is the SCALING. Needs to be clear which are scaled (to annual) and which

aren't. Text in caption is ambiguous. So can you tell us which is scaled (to annual) and

which aren't. If they are scaled then key should say - scaled 1856-1980 as with Jones et al .

Does this apply to Briffa and Osborn and to Briffa et al (the grey and orange lines).

9. Whilst on scaling are all scaled or regressed? Scaling we think of as giving the same

mean and variance. Regression does this also but which has been used.

10. Finally, Figure would look good with a thin black line along the zero line from 0 to 2000.

Call me or Tim if anything you don't follow. Try Mike as well. I sent him an email earlier

today and he'd already put his reply message up for the next 4-5 weeks.

Cheers

Phil

At 12:25 09/06/03 -0400, Scott Rutherford wrote:

Mike and Phil,

Attached is figure 1. The format is Adobe Illustrator with an embedded PDF. You can view it in Acrobat. Let me know if you have questions.

Regards,

Scott

Scott Rutherford

Marine Research Scientist

Graduate School of Oceanography

University of Rhode Island

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>

To: Scott Rutherford <srutherford@gso.uri.edu>

Subject: Re: EOS text

Date: Tue, 10 Jun 2003 14:26:07 -0400

Cc: phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

Hi Scott,

I concur w/ your assessment--keeping the figure the way it is now is preferable in my opinion...

mike

At 02:23 PM 6/10/2003 -0400, Scott Rutherford wrote:

Dear All,

I agree that figure 1 is very busy, but I'm not sure that is a bad thing in this case because we aren't trying to highlight differences between reconstructions/models or single out one or two from the rest. I think the current figure illustrates the range of reconstructions, the range of models and how well they agree (similar to one of our original ideas of a "cloud of reconstructions").

If we put the models into a separate panel we will need a curve common to both panels that people can use as a reference. If we go with the two panel figure I suggest that the second panel include the models, the Mann et al. 1999 reconstruction with uncertainties and the instrumental record.

I'll leave it to the group to decide.

-Scott

On Tuesday, June 10, 2003, at 01:16 PM, Michael E. Mann wrote:

I don't really like the idea of changing the figure dramatically at this point.

If we have to, I suggest the following options:

- 1) Take out one of the model simulation results--e.g. Gerber et al w/ the lower sensitivity
- 2) If we want to adopt Kevin's two panel strategy, then show the model results along w/ the gray-shaded uncertainty region from the top (reconstructions) panel. And show the instrumental record in both panels.

Anyway, up to you guys...

mike

At 10:59 AM 6/10/2003 -0600, you wrote:

Phil

Thanks for the great work.

Some reactions.

- 1) Fig. 1 is very busy and perhaps unduly crowded. My reaction is to take the model results out and put them in a separate panel. The separate panel would fit along side the key. But better below the main figure.

Can we change "gridded and arealy weighted" to "gridded, area-weighted..")

What is "optimal borehole",? Should "optimal" be in quotes?

2) Fig. 2: Can we please add a country to each name for those that don't have them?

Increased spacing between them would be nice.

Thanks

Kevin

Phil Jones wrote:

Dear All,

Keith, Tim and I have been at this for part of the day. Scott has also redrawn Fig 1.

Attached is the latest draft, which includes Kevin's from about 1 hour ago, but not Ray's

latest email.

Fig 1 from Scott is OK to us here. Fig 2 is a draft. Tim needs to space the series

out a little. To use all these we've needed to add a load of references. Getting these and

making the captions OK has taken most time and the drawing of Fig 2.

Hopefully we can all agree to this in the next day or so, then I'll submit on say

Thursday UK morning time, so you've all got all day today and tomorrow.

We've been through the text carefully and all happy with it.

Apologies - no time to make Fig 2 pdf. Hope all can see postscript. We still need to work

on the captions and tidy the refs a little more.

We'll be back at 8.30 tomorrow UK time. Peck - you've got 2 days to say yes/no !

Cheers

Phil

Prof. Phil Jones

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</blockquote></x-html>

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.cgd.ucar.edu/cas/>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: possible rewording of section of letter?
Date: Tue Jun 10 15:23:53 2003

thanks and all now ok

Keith

At 10:30 AM 6/10/03 -0400, you wrote:

Hi Keith,

no problem...Responses below. let me know what you think...

thanks,

mike

At 03:01 PM 6/10/2003 +0100, Keith Briffa wrote:

thanks for that Mike - sorry but just a few more questions

the reference to "agree remarkably well with the proxy-based reconstructions (Figure 1)

" [later part of paragraph] . Unfortunately , the Bauer et al curve clearly does not -

at least from AD 1100 to 1400!

Again some qualifyer is needed - perhaps "for the most part , agree well " ?

Yes, "remarkably" is an overstatement given that, as you say, Bauer et al does stray some bit.

How about simply:

"Agree with the proxy-based reconstructions within estimated uncertainties (Figure 1)".

and later [middle of the 6th paragraph],

"relative hemispheric warmth during the 10th to 12th centuries" is ambiguous and we

prefer "relative hemispheric warmth during much of the the 10th,11th and 12th centuries"

yep, better...

but also , where we say [just below] "the specific periods of cold and warm apparent for Europe differ significantly from those for the Northern Hemisphere as a whole." , to what evidence of European anomalies are we referring?

ahh--I left that open-ended, for Phil and you guys to deal with as you see best. I was anticipating that Figure 2 would include an appropriate proxy series or two for Europe (CET, Fennoscandia?) that would make this point. But why don't you guys revise the wording, as necessary, based on Figure 2?

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[2]<http://www.cru.uea.ac.uk/cru/people/briffa>[3]/

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Kevin Trenberth <trenbert@cgd.ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: EOS text
Date: Fri, 13 Jun 2003 09:55:59 -0600
Cc: Tom Wigley <wigley@ucar.edu>, "Michael E. Mann" <mann@virginia.edu>, "Raymond S. Bradley" <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>, Michael Oppenheimer <omichael@princeton.edu>, Tom Crowley <tcrowley@duke.edu>, Scott Rutherford <srutherford@gso.uri.edu>, t.osborn@uea.ac.uk, jto@u.arizona.edu

<x-flowed>

Hi all

On isotopes, see the paper by Werner et al (briefly discussed in our Science perspectives) showing that isotopes don't sample the deep winter well as there is inadequate precip then in Greenland during the past. I had to send this as I have been getting 2 of everything and I so I adjusted the cc list.

Kevin

Phil Jones wrote:

>
> Tom,
> The W. Greenland series is based on a stack of 6 isotope series -
> see chapter by
> Fisher et al in book from 1996 by Jones, Bradley and Jouzel.
> Correlation of this series
> with Greenland Annual temps is 0.58 on annual timescale over 1901-80.
> It is one of the
> better ones of the series in Fig 2. Others are better with different
> seasons, but this one
> is good for annual. The averaging of the 6 sites improves it a lot.
>
> Cheers
> Phil

> At 08:51 13/06/03 -0600, Tom Wigley wrote:

>> Phil,
>>
>> If W Greenland is based on isotopes, I note that the correlation
>> between these and temperature is very low. Do we really want to
>> perpetuate the myth that ice core isotopes are a good proxy for
>> temperature?
>>
>> Tom.

>> _____
>>

>> Phil Jones wrote:

>>
>>>

>>>>
>>>> Dear All,
>>>
>>> Keith, Tim and I have been at this for part of the day.
>>> Scott has also redrawn Fig 1.
>>> Attached is the latest draft, which includes Kevin's from about 1
>>> hour ago, but not Ray's
>>> latest email.
>>> Fig 1 from Scott is OK to us here. Fig 2 is a draft. Tim
>>> needs to space the series
>>> out a little. To use all these we've needed to add a load of
>>> references. Getting these and
>>> making the captions OK has taken most time and the drawing of Fig 2.
>>> Hopefully we can all agree to this in the next day or so,
>>> then I'll submit on say
>>> Thursday UK morning time, so you've all got all day today and
>>> tomorrow.
>>> We've been through the text carefully and all happy with it.
>>> Apologies - no time to make Fig 2 pdf. Hope all can see
postscript.
>>> We still need to work
>>> on the captions and tidy the refs a little more.
>>> We'll be back at 8.30 tomorrow UK time. Peck - you've got 2
>>> days to say yes/no !
>>> Cheers
>>> Phil
>>>
>>>
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</x-flowed>

From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: VERY VERY IMPORTANT
Date: Fri, 20 Jun 2003 14:19:20 -0400
Cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, mann@virginia.edu

Hi Phil et al,

Re, Malcolm co-authorship--big oversight on my part. Can you ask Ellen if we can add his name (i.e., just say it was 'accidentally left off'), where it belongs alphabetically in the list.

I've talked to Malcolm on the phone. The PC #1 **is** the right one--but Malcolm has raised the valid point that we need to cover our behinds on what was done here, lest we be vulnerable to the snipings of the Idsos and co (i.e., that non-climatic influences on recent growth were nominally dealt w/, as in MBH99).

Malcolm is supposed to be sending some text to Phil.

So, can we incorporate his small bit of text, and add his name, and then resubmit to AGU ASAP?

Thanks all for all the help here. Now, I better get back to my newlywed wife!
mike

At 05:25 PM 6/20/2003 +0100, Phil Jones wrote:

Mike,

Malcolm has just called Keith. He's been with Ray. Apart from probably being a little

miffed off he's not on the article, he says that the W. US series in Figure 2 is wrong.

He says

it looks the first PC (which I said it was), but that this isn't the corrected one (for CO2 growth

effects). Can you check whether it is the right one? Malcolm says that Idso (who was on

E&E) will say that the increase in that series is not climatic but due to fertilization. This

would not look good obviously. Idso was on a paper with Don Graybill re fertilisation effects

on bristlecones.

If you need to send a revised series for this top series in Fig 2 then send it to Tim.

Tim has done this plot so can make the alterations if another series is needed. If you think

that the series is OK then we'll leave it. If you do change it will affect Fig 2 of the GRL also

but probably not to any noticeable effect - at least at the size the plot will be.

Tim will send round the copyright forms to all and reprint forms. Tell Tim if you want any.

Seems like the pdf will do.

Cheers

Phil

PS Tell Lorraine I'm not always emailing you - but Malcolm thought the above was important.

I assumed you would have sent the corrected one you used in GRL in 1999.

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Ellen Mosley-Thompson <thompson.4@osu.edu>, Phil Jones <p.jones@uea.ac.uk>
Subject: Re: 2003ES000354 Decision Letter
Date: Tue, 24 Jun 2003 03:33:46 -0400
Cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, mann@virginia.edu

Hi Ellen,

I'm still travelling, and have only intermittent email access. I'm pretty sure Phil is travelling now too, so I'm hoping Keith or Tim can help out here.

I think we actually discussed two small changes from the final version Phil sent you. This involved adding Malcolm Hughes as a co-author (his name was accidentally left off the list), and changing the wording of one sentence slightly. I believe that Tim and Keith have these changes, and hopefully they can submit this via GEMS? If not, will have to wait until Phil or I have a solid internet connection to do this (that will likely be at IUGG in Sapporo in about 2 weeks).

Thanks for bringing this to our attention. Phil--if you're reading email, any way you can help out here?

thanks all,

mike

At 04:36 PM 6/23/2003 -0400, Ellen Mosley-Thompson wrote:

Phil,

I just learned from AGU that you did not submit the revised version back to AGU via the GEMS system. Can you or Mike do this as soon as possible? I would like to get this paper moving through AGU. Fred Spilhaus still has to approve it - he approves all Forum pieces - so this adds a layer that will cost us time.

Thanks

Ellen

P.S. I have copied everyone who might be able to handle this in your and Mike's absence. Thanks

At 05:13 PM 06/20/2003 +0100, you wrote:

Dear Ellen,

I'm off on Sunday, but I've managed to get the revisions done. The revised pdf is attached. This contains a reduced size manuscript by about 10 lines and we've reduced the

references to the absolute minimum. This is still 30. If we go any lower we have to change the

figures. As we are commenting on a paper we need to specifically reference all the series we use.

Thanks for going through so quickly.

If further changes are required I won't be here so can you email either Keith Briffa

or Tim Osborn (k.briffa@uea.ac.uk, t.osborn@uea.ac.uk) .

I will ask Keith and Tim to get the copyright forms rolling.

Cheers

Phil

At 13:50 18/06/03 -0400, eos@agu.org wrote:

Dear Dr. Mann: (copy to Phil Jones)

I am pleased to accept "On Past Temperatures and Anomalous late-20th Century Warmth" for publication in Eos with the provision that in your final submission you modify to the first paragraph slightly so that it is fully consistent with the text of the AGU statement on climate change and greenhouse gases:

[1]http://www.agu.org/sci_soc/policy/climate_change_position.html

Note that first sentence of your paper indicates that the AGU statement includes the inference that there is a high probability I cannot find the words high probability in the AGU statement (unlike IPCC that does state "high probability."). It is critical that the introductory paragraph is carefully constructed so as not to diminish any of the points you make in the Forum piece. I suggest a modification of your first paragraph - please feel free to further modify this.

Evidence from Gases," that there is a compelling basis for concern over future climate changes, including increases in global mean surface temperatures, due to increased concentrations of greenhouse gases, primarily from fossil fuel burning. If this is too long, you might wish to break it into two sentences. This says the same thing as your original intro sentence but is fully consistent with the text of the AGU statement.

Also in the first paragraph would you agree to this change?

... such anomalous warm cannot be fully explained natural factors (Added the word "fully" to indicate that some but not all of the anomalous warming can be explained by natural factors.)

Another suggestion is to remove the second reference to the AGU policy (second paragraph). What about ... these claims in light of the fact that they have

The content of the Forum piece is just fine, but I did find a few minor problems that you need to fix in the final submission.

1) 3rd paragraph line 8 - reference to Jones et al. (1998) - this date occurs in several places in the paper and should be Jones et al. 1999; e.g., point (2) line 3

2) page 2 - the second (2) point

last 3 lines: remove double period after U.S.; also that sentence reads awkwardly - try a comma after the word 'cancelling'.

3) the second paragraph of point 2 (2); last three lines: this is awkward; the word "apparent" is out of place; I think this should this read apparent coldness and warmth differ

4) point 3) last line of first paragraph - change ... insight to (Remove in from

into)

5) references - the Jones et al. 1999 reference is formatted differently than the rest (put date at end).

Finally - everywhere throughout the text et al should be corrected to et al (The period is consistently absent)

Before publication, your article will be edited to reflect the Eos newspaper style, including a possible change in the headline. We will send the edited version to you for review and final approval before the article is published.

Please note that before we can proceed with production work on your submission, a copyright transfer agreement and reprint order form must be completed and returned to AGU. These forms may be printed* from the AGU web site:

[2]http://www.agu.org/pubs/journal_forms/EosCopyright.pdf

[3]http://www.agu.org/pubs/journal_forms/EosReprint_orders.pdf.

For information on the production process, please contact Shermonta Grant, Eos Production Coordinator, at +202.777.7533 or sgrant@agu.org.

In the absence of information from you to the contrary, I am assuming that all authors listed on the manuscript concur with publication in its final accepted form and that neither this manuscript nor any of its essential components have been published previously or submitted to another journal. The AGU Guidelines for Publication emphasize that: "It is unethical for an author to publish manuscripts describing essentially the same research in more than one journal of primary publication."

Thank you for your contribution to Eos.

Sincerely,

Ellen Mosley-Thompson

Editor, Eos

*If you need Adobe Acrobat Reader, it is freely available at:

[4]<http://www.adobe.com/prodindex/acrobat/readstep.html>

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[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. http://www.agu.org/sci_soc/policy/climate_change_position.html
2. http://www.agu.org/pubs/journal_forms/EosCopyright.pdf
3. http://www.agu.org/pubs/journal_forms/EosReprint_orders.pdf
4. <http://www.adobe.com/prodindex/acrobat/readstep.html>
5. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <>wigley@ucar.edu>
Subject: Re: bradley comment
Date: Tue, 24 Jun 2003 14:01:50 -0400
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "Raymond S. Bradley" <rbradley@geo.umass.edu>, mann@virginia.edu

Tim,

I suggest we let Eos size the figures, etc. Then, in the end, we can simply substitute a version of Figure 2 w/ the correlations added at the proof stage. Anything else will slow down the publication of the manuscript unnecessarily, in my opinion.

Phil and I have already discussed--we agree that the low weight given to the record in the Mann and Jones composite treats the record appropriately...

mike

At 02:37 PM 6/24/2003 +0100, Tim Osborn wrote:

Hi Tom,

In Phil's absence I was just now looked at his PC because I needed some files/emails for a separate matter, and I noticed that you had emailed Phil/Ray/Mike concurring with Ray's concerns. Until I saw that, I hadn't realised that anyone else had commented on Yang et al.

Keith and I discussed exactly this issue this morning, and though Keith also had concerns about the record (I haven't read their paper, so can't comment) we decided to leave things as they were because: (i) Mike suggested adding correlations to the figure at the proof stage rather than now; (ii) I wasn't sure how to word a caveat about Yang et al. without making it seem odd that we were including a doubtful record and odd that we hadn't added caveats about some of the other records.

The current status is that the version I circulated has been submitted back to EOS (because of the reasons given above), and Ellen Mosley-Thompson has approved it. It needs to be reviewed internally at AGU by either Fred Spilhaus or an Associate Editor. It will then be edited to reflect the Eos newspaper style.

I've cc'd this to Mike and Phil to see what they want to do. I/we can put a hold on the processing of the current submission and then submit a new version with revised figure and caption. Alternatively we could wait and see what it's like after EOS have edited it, and then make any final modifications at that stage.

Over to you/Mike/Phil.

Cheers

Tim

At 14:00 24/06/2003, you wrote:

Tim,

I think it is **extremely** important to cover Ray's point about Yang et al. and Mike Mann's response about weighting. This requires a small addition to the Figure caption.

Tom.

Dr Timothy J Osborn
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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "Raymond S. Bradley" <rbradley@geo.umass.edu>
Subject: Re: ice cores/China series (FYI)
Date: Tue, 24 Jun 2003 14:06:25 -0400
Cc: mann@virginia.edu

Thanks Keith,

I just read your email after reading the others. We actually eliminate records with negative correlations (this is mentioned briefly in the GRL article,), and we investigated a variety of weighting schemes to assure the basic robustness of the composite--but I certainly endorse your broader point here. Many of these records have some significant uncertainties or possible sources of bias, and this isn't the place to get into that. The uncertainties get at this, at some level, and other places (e.g. the Reviews of Geophysics paper Phil and I are drafting) will provide an opportunity to discuss these kinds of issues in more detail--we will certainly be seeking advice (either officially or unofficially) from each of you once we have finalized the draft of that...

Now back to my honeymoon...

mike

At 02:38 PM 6/24/2003 +0100, Keith Briffa wrote:

To keep you informed , here is a reply to Tom Wigley re his request to "deal with Ray's Comments" re the China series in EOS piece

Tom

Tim has just told me of your message expressing concern about the China series , and your statement of the necessity to "deal with Ray's comment" and add in the "small adjustment to the Figure Caption" . .

We (I and Tim) decided to get this off as soon as possible to Ellen (AGU) , as we had been asked to do (and as requested by Ellen). Hence it went off earlier today (and before your message arrived). Mike was aware of Ray's comment and was happy to leave any amendment to the text "until the proof stage" .

In my opinion it is not practical (or desirable) to try to "qualify " any one record in this limited format. It was a majority decision to leave the Mann and Jones 2000-year series in the Figure 1 (as it was to remove the Briffa and Osborn tree-ring based one) , and the details of the logic used to derive the Mann and Jones series is to be found in the (cited) text of their paper. Signing on to this letter , in my mind. implies agreement with the text and not individual endorsement of all curves by each author. I too have expressed my concern to Phil (and Ray) over the logic that you leave all series you want in but just weight them according to some (sometimes low) correlation (in this case based on decadal values). I also believe some of the series that make up the Chinese record are dubious or obscure , but the same is true of other records Mann and Jones have used (e.g. how do you handle a series in New Zealand that has a -0.25 correlation?) . Further serious problems are still (see my and Tim's Science comment on the Mann 1999 paper) lurking with the correction applied to the Western US tree-ring PC amplitude series used (and shown in Figure 2). There are problems (and limitations) with ALL series used. At this stage , singling out individual records for added (and unavoidably cursory added description) is not practical. We were told to cut the text and References significantly - and further cuts are implied by Ellen's messages to us. If you wish to open this up to general discussion , it may be best to wait 'til the proof stage and then we can all consider the balance of emphasis - but we had also better guard against too "selective" a choice of data to present? If you want to get a

somewhat wider discussion of this point going in the meantime , feel free to forward this to whoever you wish along with your disagreement , while we wait on the response from AGU.

Best wishes

Keith

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Asher Minns" <A.Minns@uea.ac.uk>
Subject: Re: From Prof. Pachauri
Date: Thu Jun 26 15:04:29 2003

Asher,

Spoke with Sinclair-Wilson from Earthscan yesterday about this and we agreed one or two things. We should take next steps on this after the Assembly business has died down.

Mike

At 07:51 19/06/2003 +0100, you wrote:

Mike, this message below is fresh-in from RK Pachauri. He seems keen, and we have been given a direct contact at TERI. He has made a few interesting suggestions on content, though nothing on funding as of yet.

Asher

Mr Asher Minns
Communication Manager
Tyndall Centre for Climate Change Research
[1]www.tyndall.ac.uk
Mob: 07880 547 843
Tel: +44 0 1603 593906

----- Original Message -----

From: "R K Pachauri" <pachauri@teri.res.in>
To: <tyndall@uea.ac.uk>
Cc: "Ulka Kelkar" <ulkak@teri.res.in>
Sent: Thursday, June 19, 2003 7:34 AM
Subject: Dear Prof. Hulme

Dear Prof. Hulme,

Thank you for your letter proposing that the Tyndall Centre and TERI jointly produce a series of yearbooks on climate change. May I congratulate you on this excellent idea! I am convinced that a market exists for precisely such a publication, and am delighted that you thought of TERI as a partner in this venture.

I am putting down some initial thoughts on the proposed publication and the suggested contents that you had sent.

While there is a lot of information and related data available on climate change, it is scattered. On the one hand we have the IPCC assessment on the state of knowledge about climate change, and on the other the WMO's annual bulletins. Similarly, the UNFCCC compiles GHG inventory information from periodically submitted National Communications, while the IEA presents annual fuel combustion emission statistics. In such a scenario, the metier

of our Yearbook would be to synthesise the current knowledge on climate change. As mentioned in your note, it would present this information in a clear and visually appealing manner. Moreover, it would go into climate change issues in more detail than say, the annual World Resources brought out by WRI.

The Foreword - and perhaps an Emerging Issues section at the end of the book - could comment on scientific and political issues, which are otherwise not discussed in either the IPCC Reports or in the types of publications mentioned above.

In the draft table of contents, there are two sections that are slightly different in character from the others. In the chapter on national policies, we may choose between alternative structures:

1 By Annex I country

2 By type of policy/instrument (e.g. CDM, international trading regimes, taxation, etc)

The proposed chapter on Social Change and Adaptation is important to complete the set of topics/issues covered in the Yearbook, but is probably the most complex in terms of scope/structure. One option that we could discuss is to cover adaptation policies not in chapter 7, but in chapter 9, and to highlight studies of community and local government level implementation.

With such a scope, the media would also be an important part of the audience for this yearbook

I do appreciate that producing this Yearbook would involve significant commitment in terms of time and effort if all relevant literature is to be reviewed. However, by teaming up authors from our two organisations, I am confident that we will provide an impartial yet balanced North-South perspective to the Yearbook. For specialised subjects, like the chapter on business, we may even think of invited chapters, by say the WBCSD.

You may also be interested to know that TERI also brings out a yearbook focusing on India, called the TERI Energy Directory, Database, and Yearbook (TEDDY). This publication has a readership of 15000-20000, reaching out to government, corporates, individual researchers, and libraries in India and overseas.

These are just some initial thoughts, and my colleagues can be in touch with your team to develop this outline further. Ms Ulka Kelkar (ulkak@teri.res.in) will coordinate this effort on behalf of TERI.

We look forward to working with you on this Yearbook.

With kind regards,

Yours sincerely,

R.K. Pachauri

References

1. <http://www.tyndall.ac.uk/>

From: Jenny Duckmanton <jmd4@york.ac.uk>
To: Mick Kelly <m.kelly@uea.ac.uk>
Subject: Re: Tiempo final invoice
Date: Mon, 30 Jun 2003 11:22:28 +0100
Cc: "Duckmanton, Jenny" <jmd4@york.ac.uk>, "Kuylenstierna, Johan" <jck1@york.ac.uk>

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Ciao Mick

Just back from Tuscany and still ploughing through accumulated emails. Where the UEA invoice is concerned, I just opened an invoice from UEA for SEK 71,074.09 and would be most obliged if you could let me know if this is the correct amount, so I can get it paid?

Please give my regards to Sarah and let her know that Tuscany is still as beautiful as ever, but a bit more expensive than before but still cheaper than the UK. We also went to spend a few days in Umbria where some friends of ours had rented a lovely villa with magnificent views, gardens, pool, etc.

Best regards
Jenny

Mick Kelly wrote:

> Jenny
> UEA should send the final invoice on the old contract within a day or two. I
> am trying to see it before it goes to check it is for the right amount. In
> case I fail and it's not the right amount, please let me know asap!
> Thanks
> Mick
>
> _____
>
> Mick Kelly Climatic Research Unit
> School of Environmental Sciences
> University of East Anglia
> Norwich NR4 7TJ United Kingdom
> Tel: 44-1603-592091 Fax: 44-1603-507784
> Email: m.kelly@uea.ac.uk
> Web: <http://www.cru.uea.ac.uk/tiempo/>
> _____

--

From: "Kuylenstierna, J.C." <jck1@york.ac.uk>
To: Mick Kelly <m.kelly@uea.ac.uk>
Subject: New tiempo cpsts
Date: Mon, 30 Jun 2003 18:25:29 +0100

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Hi Mick,

Sara has suggested that with the timetable given, that we ought to plan on the extension until end February 2004. I have then started to change the budget to add some more time. As we have already used the funds for one (June) issue of the three planned, I thought we would just add some days as follows:

Mick 5
Sarah 10
Mike Salmon 2.5
Gerry 4
Johan 4
Jenny 2

This would increase the total funds to 1,315,813 from 1,178,000, an increase of 137813 SEK (about £10,000). The publication cost for March 2003 would be in the new proposal, but all the work will have been done in Jan/Feb.

Does that sound OK?

Johan
--
Johan Kuylenstierna
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+44 1904 432897 (general)
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Email.: jck1@york.ac.uk

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From: "Mick Kelly" <m.kelly@uea.ac.uk>
To: 'dean.env@uea.ac.uk'
Subject: Museum of Climate Change
Date: Wed, 02 Jul 2003 13:17:11 +0000

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Trevor

A quick update:

1. I'm arranging a meeting between our team and the Museums Service (including I hope the director) late July to discuss next stage. I'll consult Chris Flack about possible dates. They are ready to push ahead with the next stage.

2. N County Council now appear well and truly behind the project and want to bring development responsibility into their Economic Development Unit. Good news in terms of political will, but some concern about loss of control and transformation into a tourism project.

Think we need to resolve how best this initiative might relate to the linking CRED initiative, as discussed, and reach understanding with Museums Service sooner rather than later? Unless it's premature?

Finally, Melissa Burgan, ex MSc student, now with NCC transport division is very impressed with way CRED has been taken seriously by county council politicians. I assume her assessment is accurate!

Mick

Mick Kelly Climatic Research Unit
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United Kingdom
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Email: m.kelly@uea.ac.uk
Web: <http://www.cru.uea.ac.uk/tiempo/>

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HBh

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: FP6-news?
Date: Fri, 4 Jul 2003 21:29:43 +0200

<x-flowed>

Dear Keith, thanks for the update. I think I am reading much the same message as you do. I also agree that we need focus, and not too many groups involved. In terms of where the focus should be I agree that DOCC is too wide, and my feeling now is to dissolve it and reorganise under another heading with fewer groups, perhaps as an IP if Brussels allows. I do not have any preconceived notions as to where the co-ordinations should lie.

I agree with you that integration with biogeochemistry is not straight forward with Holocene climate variability except for the vegetation feedback which may be important.

I also know of one other palaeo-based initiative, ICON, dealing with the thermohaline circulation, coordinated by Rainer Zahn. We are involved. This will be submitted for the call just launched under the hot spots in the climate system heading, but may be brought over to the next call if unsuccessful (probably). We are involved there with a number of modelling centres and many of the palaeoceanography labs.

I guess we should discuss a bit further after summer has passed what to do. I am very keen on the science of Holclim and hope to be able to develop this initiative with you and others.

Last thing - any idea of when the conference Brussels wants is going to happen?.

I am away for two weeks on the Greek islands, but then I am back again.

Cheers,
Eystein

>Eystein

>I seem to keep getting distracted this week so I have not phoned
>again. I can say the basics here though. I went to the meeting that
>was also attended by Berger, Raynaud, Shackleton, Starkel and
>Zorita
>(in place of Von Storch). The rationale for the meeting was nothing
>more than The EC (Hans Brelen) felt that they ought to be organising
>a palaeoclimate conference, but there was some hinting that this

>might signal the new call (in Sept 04) but not imply any weighting
>in the appraisal of proposals. It seems definite that there will be
>money for a single (new instrument) project only , as we supposed .
>Some at the meeting spoke about a range of time scales and possible
>subject foci for the conference (and by implication also for the
>call) but I still feel strongly , on the evidence of other projects
>that I have heard are to be funded , that the need is for a sharper
>focus than was involved in our DOCC concept , and that the HOLIVAR
>approach is the optimum way forward. The problem will be scale of
>initiative (15-20 million seems a maximum likely request , with
>perhaps 12-15 a likely maximum award). The unified data / modelling
>route, as outlined in the HOLCLIM NoI seems the most likely
>candidate still. Obviously there remain difficulties even with this
>, such as geographic focus , use of the integrated data for defining
>future climate probabilities and links with socio-economic (impacts)
>community. This is also likely to clash with the direct interests of
>some major palaeoclimate scientists who focus on longer time scales
>and stronger climate and response signals. It is easier to think of
>climate forcings and the interaction of bio-geochemical cycles at
>glacial /interglacial time scales , but I am not convinced that this
>type of work would be a practical inclusion in this call. This is
>still my opinion , but an admittedly (unashamedly) biased one.
>Keith

>
>

>At 07:34 PM 6/19/03 +0200, you wrote:

>>Dear Keith,

>>I wonder if there are any news around the meeting with Brelen on
>>FP6 that can be used. Lots of rumors around and not much specific
>>knowledge, so if you have an update I'd appreciate it.

>>Cheers,

>>Eystein

>>

>>På mandag, 7. april 2003, kl. 10:46, skrev Keith Briffa:

>>

>>>Eystein

>>>your point is exactly correct , that only one project (and I
>>>believe it should be an IP) will be allowed and with the shrinking
>>>general scale of these things, it likely needs to be very clearly
>>>focused (on integrating evidence and providing some
>>>state-of-the-art product on climate history and its causes) . I am
>>>not in Nice (have to go to 2 other meetings in May) . I am still
>>>leaning towards your institute co-ordinating this . I have not

>>>discussed anything with the rest of the HOLIVAR committee.
>>>We do need some sort of meeting but only small - there is no
>>>chance of a 25 million Euro project and many people are likely to
>>>be disappointed . I have to be in Brussels for a meeting with
>>>Brelen in June . What are you thinking about , re. a meeting?

>>>Keith

>>>At 10:01 PM 4/3/03 +0200, you wrote:

>>>>Dear Keith,

>>>> I was just wondering whether you were coming the the EGS meeting
>>>>in Nice next week, in order for us to exchange some ideas about
>>>>how to proceed for FP6. Recent rumors says that the palaeoclimate
>>>>variability item is in the books for the third call, and that the
>>>>call will be issued by the turn of the year, thus we should start
>>>>discussing how to proceed. So far my DOCC initiative is dormant,
>>>>and I am more inclined to develop or take part in developing an
>>>>IP if the call for proposals allow for one. But the size of these
>>>>IPs seems to be diminishing, hence a careful focussing needs to
>>>>be undertaken in order for there to be resources for the science
>>>>teams. I would be happy to discuss idea with you on this in Nice
>>>>or sometime else if you're not there.

>>>>

>>>>Cheers,

>>>>Eystein

>>>>

>>>>

>>>>

>>>>Eystein Jansen

>>>>prof/director

>>>>Bjerknes Centre for Climate Research

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>>>>tel: +4755583491/secr:+4755589803/fax:+4755584330

>>>>eystein.jansen@geo.uib.no, www.bjerknes.uib.no

>>>>

>>>>--

>>>>Professor Keith Briffa,

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>>>>Norwich, NR4 7TJ, U.K.

>>>>

>>>>Phone: +44-1603-593909

>>>>Fax: +44-1603-507784

>>>>

>>>><http://www.cru.uea.ac.uk/cru/people/briffa/>

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>>eystein.jansen@geo.uib.no, www.bjerknes.uib.no

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The Bjerknes Training site offers 3-12 months fellowships to PhD students
More info at: www.bjerknes.uib.no/mcts

</x-flowed>

From: Keith Alverson <keith.alverson@pages.unibe.ch>
To: Rick Battarbee <r.battarbee@geog.ucl.ac.uk>, Eystein Jansen <eystein.jansen@geol.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: fp6
Date: Mon, 07 Jul 2003 09:57:05 +0200

Dear Rick, Keith and Eystein,

It is certainly good news that FP6 will have a climate change and paleo related call. My personal feeling is that whatever paleo proposal(s) eventually do go in that it would be a good thing to specifically include the PAGES office in Bern as a participant in the network. This would, I believe, help the network by providing an international context and the many PAGES resources for outreach within Europe, and inclusion of non-europeans. On the other side of the coin, PAGES is currently seeking to broaden our support base beyond USA and Switzerland and participation in an EU framework proposal would be an ideal way to do this, given the strong representation of European scientists within the PAGES community. If, however, you have reason to believe that explicit inclusion of the PAGES office in the list of partner organizations would reduce the chance of success of such a proposal, then of course don't do it. Basically, I would much appreciate being kept in the loop with your plans and am happy to participate, and offer the help of PAGES, in any way I that you deem useful.

Keith

on 07/04/2003 08:08 PM, Rick Battarbee at r.battarbee@geog.ucl.ac.uk wrote:

> Dear all,
>
> We have just come to the end of a very rewarding and successful HOLIVAR
> training course here with a very good bunch of young scientists from across
> Europe all involved in some aspect of high resolution Holocene change and
> embracing climate modelling, and climate reconstruction both from marine
> and continental records. We shall be putting details on the HOLIVAR
> website soon. (I should also say that Andy Lotter's workshop in April on
> age modelling was also very successful, and details are now on the web)
>
> I will produce a more detailed report on HOLIVAR activities and plans for
> the future shortly, and there should be plenty to discuss at our next
> Steering Committee meeting on October 3rd (please check your diaries -
> Innsbruck October 3rd).
>
> The main reason for writing, however, is to alert you to the probability of
> a call for proposals on climate change by the EU in FP6 for 2004, and the
> need for us to begin thinking again about an integrated project based on
> HOLIVAR. If you remember Keith Briffa submitted on behalf of the HOLIVAR
> community an Expression of Interest called HOLCLIM that found much favour
> at the time with the EU. Although I have not spoken at length with Keith
> about this I'm sure he is keen to see a project based on HOLCLIM taken
> forwards.
>

> Whilst we can not be sure of the detailed wording of the call I think it is
> nevertheless not too soon to begin designing the project It would be very
> useful to have your thoughts on how to proceed so that we can prepare a
> document for discussion on October 3rd. One issue is the potential overlap
> with DOCC. Eystein, what is your view on this? I'm sure there will be
> only one "palaeo" project funded and therefore if we simply followed the
> original intentions, HOLCLIM and DOCC would be in competition. And putting
> the two together would be difficult, HOLCLIM is an IP, and DOCC a NoE and
> the research community potentially involved would be huge, especially in
> relation to the budget which may be no more than 10 million euros.

>

> Please let me have your views, and then I will get together with Keith and
> come up with some kind of proposed way forwards for the meeting in October.

>

> Best wishes to all,

>

> Rick

> Professor R.W. Battarbee

> Environmental Change Research Centre

> University College London

> 26 Bedford Way, London WC1H 0AP, UK.

> Tel. +44 (0)20 7679 7582, Fax +44 (0)20 7679 7565

> <http://www.geog.ucl.ac.uk/ecrc/>

>

--

Keith Alverson

Executive Director

PAGES International Project Office

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Fax: +41 31 312 31 68

From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>, rls@email.unc.edu
Subject: More on Climate Research.....
Date: Fri, 11 Jul 2003 12:40:57 -0700
Cc: Tom Wigley <wigley@ucar.edu>, "Michael E. Mann" <mann@virginia.edu>, Mike Hulme <m.hulme@uea.ac.uk>

Dear Phil,

In June 2003, Climate Research published a paper by David Douglass et al. The "et al." includes John Christy and Pat Michaels. Douglass et al. attempt to debunk the paper that Tom and I published in JGR in 2001 ("Accounting for the effects of volcanoes and ENSO in comparisons of modeled and observed temperature trends"; JGR 106, 28033-28059). The Douglass et al. paper claims (and purports to show) that collinearity between ENSO, volcanic, and solar predictor variables is not a serious problem in studies attempting to estimate the effects of these factors on MSU tropospheric temperatures. Their work has serious scientific flaws - it confuses forcing and response, and ignores strong temporal autocorrelation in the individual predictor variables, incorrectly assuming independence of individual monthly means in the MSU 2LT data. In the Douglass et al. view of the world, uncertainties in predictor variables, observations, etc. are non-existent. The error bars on their estimated ENSO, volcano, and solar regression coefficients are miniscule.

Over a year ago, Tom and I reviewed (for JGR) a paper by Douglass et al. that was virtually identical to the version that has now appeared in Climate Research. We rejected it. Prior to this, both Tom and I had engaged in a long and frustrating dialogue with Douglass, in which we attempted to explain to him that there are large uncertainties in the deconvolution of ENSO, volcano, and solar signals in short MSU records. Douglass chose to ignore all of the comments we made in this exchange, as he later ignored all of the comments we made in our reviews of his rejected JGR paper.

Although the Douglass et al. Climate Research paper is largely a criticism of our previously-published JGR paper, neither Tom nor I were asked to review the paper for Climate Research. Nor were any other coauthors of the Santer et al. JGR paper asked to review the Douglass et al. manuscript. I'm assuming that Douglass specifically requested that neither Tom nor I should be allowed to act as reviewers of his Climate Research paper. It would be interesting to see his cover letter to the journal.

In the editorial that you forwarded, Dr. Kinne writes the following:

"If someone wishes to criticise a published paper s/he must present facts and arguments and give criticised parties a chance to defend their position." The irony here is that in our own experience, the "criticised parties" (i.e., Tom and I) were NOT allowed to defend their positions.

Based on Kinne's editorial, I see little hope for more enlightened editorial

decision making at Climate Research. Tom, Richard Smith and I will eventually publish a rebuttal to the Douglass et al. paper. We'll publish this rebuttal in JGR - not in Climate Research.

With best regards,

Ben

=====

Phil Jones wrote:

>
> Dear All,
> Finally back in the UK after Asheville and IUGG. Attached is an
> editorial from the
> latest issue of climate research. I can only seem to save it this way.
> Seems like we are
> now the bad guys.

>
> Cheers
> Phil

> At 07:51 04/07/03 -0600, Tom Wigley wrote:

>>Mike (Mann),
>>I agree that Kinne seems like he could be a deFreitas clone. However, what
>>would be our legal position if we were to openly and extensively tell
>>people to avoid the journal?
>>Tom.

>> _____
>>

>>Michael E. Mann wrote:

>>>Thanks Mike
>>>It seems to me that this "Kinne" character's words are disingenuous, and
>>>he probably supports what De Freitas is trying to do. It seems clear we
>>>have to go above him.
>>>I think that the community should, as Mike H has previously suggested in
>>>this eventuality, terminate its involvement with this journal at all
>>>levels--reviewing, editing, and submitting, and leave it to wither way
>>>into oblivion and disrepute,
>>>Thanks,

>>>mike

>>>At 01:00 PM 7/3/2003 +0100, Mike Hulme wrote:

>>>>Phil, Tom, Mike,
>>>>
>>>>So, this would seem to be the end of the matter as far as Climate
>>>>Research is concerned.

>>>>

>>>>Mike

>>>>

> >>>>To
> >>>>CLIMATE RESEARCH
> >>>>Editors and Review Editors
> >>>>
> >>>>Dear colleagues,
> >>>>
> >>>>In my 20.06. email to you I stated, among other things, that I would
> >>>>ask CR editor Chris de Freitas to present to me copies of the
> >>>>reviewers' evaluations for the 2 Soon et al. papers.
> >>>>
> >>>>I have received and studied the material requested.
> >>>>
> >>>>Conclusions:
> >>>>
> >>>>1) The reviewers consulted (4 for each ms) by the editor presented
> >>>>detailed, critical and helpful evaluations
> >>>>
> >>>>2) The editor properly analyzed the evaluations and requested
> >>>>appropriate revisions.
> >>>>
> >>>>3) The authors revised their manuscripts accordingly.
> >>>>
> >>>>Summary:
> >>>>
> >>>>Chris de Freitas has done a good and correct job as editor.
> >>>>
> >>>>Best wishes,
> >>>>Otto Kinne
> >>>>Director, Inter-Research
> >>>>--
> >>>>-----
> >>>>Inter-Research, Science Publisher
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> >>>>-----

> >>>

> >>

> >> Professor Michael E. Mann
> >> Department of Environmental Sciences, Clark Hall
> >> University of Virginia
> >> Charlottesville, VA 22903

> >>

> >> e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

> >> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

> >

>

> Prof. Phil Jones
> Climatic Research Unit Telephone +44 (0) 1603 592090
> School of Environmental Sciences Fax +44 (0) 1603 507784
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> UK

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> CR.txt Type: Plain Text (text/plain)
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PCMDI HAS MOVED TO A NEW BUILDING. NOTE CHANGE OF MAIL CODE!

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-7638
FAX: (925) 422-7675
email: santer1@llnl.gov

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Tom Crowley <tcrowley@duke.edu>
Subject: Re: Fwd: Re: Fwd: Re: Climate Research
Date: Fri Jul 11 13:33:49 2003

Hi Tom,

I'm not sure what format to try if ASCII doesn't work for you. I've attached the same ones again, in case it was just some random reason that corrupted the files. If this doesn't work, then please suggest a format I should try.

The name I have is Yamal not Yarnal. Yamal is coastwards (northward) of the "Polar Urals" and is at a lower elevation than the Polar Urals record. The latitude/longitude I have for it is:

67.5 N, 70 E

Hope that helps

Tim

At 21:40 07/07/2003, you wrote:

Hi Tim, thanks for sending the data - unfortunately I cannot open it, can you send it in some other format? tom

ps what is the location of the Yarnal site?

Hi Tom

Sorry for not replying sooner - its been a hectic week (or two)!

The new Mann and Jones 2000-year series I don't actually have. It appears in Figure 1 of our EOS piece, of course, but Scott Rutherford generated that figure. I generated Figure 2 for EOS and that has the Yamal, Tornetrask, western US and western Greenland O18 stack in it. So I have these data and they are attached in the following files.

western US and western Greenland are in file "mann12prox.dat". I didn't have time to extract just these two series from the full file, so the file contains 11 others series too. Please do **not** use the others because I'm not sure whether I am free to distribute them or not - I just haven't time to extract the 2 you want. I'm sure I can trust you not to use anything that I shouldn't have sent! The top of the file lists the 13 series and the start/end years. These are in the same order as the 13 columns of data that then follow (the first column is simply year AD). So you should be able to find "westgrpfisher.dat" and "wustrees.dat".

The other files are "tornad.rcs" and "yamal.rcs" which are RCS-standardised tree-ring width series. I would really strongly suggest that you contact Keith Briffa about exactly what these series are and what the primary reference to them should be. The reason is that there are multiple version of Tornetrask and Yamal series and the differences are certainly not insignificant!

I'm not sure what the "units" of any of these series are, so I would suggest you normalise them in some way or do your own calibration.

Hope that helps

Cheers

Tim

At 16:28 30/06/2003, you wrote:

Tim, would it be possible to obtain the time series listed below, plus the west Greenland composite? (see below).

tom

X-Sieve: CMU Sieve 2.2

X-Sender: f028@pop.uea.ac.uk

Date: Fri, 20 Jun 2003 08:10:57 +0100

To: Tom Crowley <tcrowley@duke.edu>

From: Phil Jones <p.jones@uea.ac.uk>

Subject: Re: Fwd: Re: Climate Research

Cc: t.osborn@uea.ac.uk

X-Virus-Scanned: by amavid-milter, Duke University ([1]<http://amavis.org/>)

Tom,

I'm off tomorrow to NCDC and then onto IUGG, so away 3 weeks in all. I've asked Tim, who's cc'd on this reply to send you what he can.

You also said sometime ago, you would send your new long series and your latest NH average. Can you do this sometime? Mike and I are making progress on RoG. When we get back we will be working on the figures. I realise you may want to add something once

Tim sends you the series, so if I (and Mike) can get something by July 10 that would be great.

We will be sending whole or part drafts of the RoG piece around - we have most of the text,

but we need the figures for people to look at as well. So you might get a draft in September.

Have a good few weeks.

Cheers

Phil

At 12:33 19/06/03 -0400, you wrote:

Phil,

would it be possible to obtain the Yamal, Tornetrask, and w. U.S. series you illustrate in the eos article? I too am putting together a slightly different long composite and would like to include these records.

would it also be possible to obtain the 2000 year northern hemisphere series? is that 30-90N summer? whatever, we have extended our forcing time series back to before 1 AD and would like to compare with some longer data.

thanks and regards, Tom

Dear All,

Keith and I have discussed the email below. I don't want to start a discussion of it and I

don't want you sending it around to anyone else, but it serves as a warning as to where the debate might go should the EOS piece come out.

I think it might help Tom (W) if you are still going to write a direct response to CR. Some of

de Freitas' views are interesting/novel/off the wall to say the least. I am glad that he doesn't

consider himself a paleoclimatologist - the statement about the LIA having the lowest temperatures since the LGM. The paleo people he's talked to didn't seem to mention the YD,

8.2K or the 4.2/3K events - only the Holocene Optimum. There are also some snipes at CRU and our funding, but we're ignoring these here. Also Mike comes in for some stick, so stay

cool Mike - you're a married man now !

So let's keep this amongst ourselves .

I have learned one thing. This is that the reviewer who said they were too busy was Ray.

I have been saying this to loads of papers recently (something Tom(w) can vouch for). It is

clear from the differences between CR and the ERE piece that the other 4 reviewers did not say much, so a negative review was likely to be partly ignored, and the article would still

have come out. I say this as this might come out if things get nasty.

De Freitas will not say to Hans von Storch or to Clare Goodess who the 4 reviewers were. I

believe his paleoclimatologist is likely to be Anthony Fowler, who does dendro at Auckland.

Cheers

Phil

X-Sender: f037@pop.uea.ac.uk

X-Mailer: QUALCOMM Windows Eudora Version 5.1

Date: Wed, 18 Jun 2003 09:29:22 +0100

To: c.goodess@uea,phil Jones <p.jones@uea.ac.uk>

From: Mike Hulme <m.hulme@uea.ac.uk>

Subject: Fwd: Re: Climate Research

Clare, Phil,

Since Clare and CRU are named in it, you may be interested in Chris de Freitas' reply to the publisher re. my letter to Otto Kinne. I am not responding to this, but await a reply from Kinne himself.

Mike

From: "Chris de Freitas" <c.defreitas@auckland.ac.nz>

To: Inter-Research Science Publisher <ir@int-res.com>

Date: Wed, 18 Jun 2003 13:45:56 +1200

Subject: Re: Climate Research

Reply-to: c.defreitas@auckland.ac.nz

CC: m.hulme@uea.ac.uk

Priority: normal

X-mailer: Pegasus Mail for Win32 (v3.12c)

Otto (and copied to Mike Hulme)

I have spent a considerable amount of my time on this matter and had my integrity attacked in the process. I want to emphasize that the people leading this attack are hardly impartial observers. Mike himself refers to "politics" and political incitement involved. Both Hulme and Goodess are from the Climate Research Unit of UEA that is not particularly well known for impartial views on the climate change debate. The CRU has a large stake in climate change research funding as I understand it pays the salaries of most of its staff. I understand too the journalist David Appell was leaked information to fuel a public attack. I do not know the source

Mike Hulme refers to the number of papers I have processed for CR that "have been authored by scientists who are well known for their opposition to the notion that humans are significantly altering global climate." How many can he say he has processed? I suspect the answer is nil. Does this mean he is biased towards scientists "who are well known for their support for the notion that humans are significantly altering global climate?"

Mike Hulme quite clearly has an axe or two to grind, and, it seems, a political agenda. But attacks on me of this sort challenge my professional integrity, not only as a CR editor, but also as an academic and scientist. Mike Hulme should know that I have never accepted any research money for climate change research, none from any "side" or lobby or interest group or government or industry. So I have no pipers to pay.

This matter has gone too far. The critics show a lack of moral imagination. And the Cramer affair is dragged up over and over again. People quickly forget that Cramer (like Hulme and Goodess now) was attacking Larry Kalkstein and me for approving manuscripts, in Hulme's words, "authored by scientists who are well known for their opposition to the notion that humans are significantly altering global climate."

I would like to remind those who continually drag up the Cramer

affair that Cramer himself was not unequivocal in his condemnation of Balling et al's manuscript (the one Cramer refereed and now says I should have not had published - and what started all this off). In fact, he did not even recommend that it be rejected. He stated in his review: "My review of the manuscript is mainly with the conclusions of the work. For technical assessment, I do not myself have sufficient experience with time series analysis of the kind presented by the authors." He goes on to recommend: "revise and resubmit for additional review". This is exactly what I did; but I did not send it back to him after resubmission for the very reason that he himself confessed to ignorance about the analytical method used.

Am I to trundle all this out over and over again because of criticism from a lobbyist scientists who are, paraphrasing Hulme, "well known for their support for the notion that humans are significantly altering global climate".

The criticisms of Soon and Baliunas (2003) CR article raised by Mike Hume in his 16 June 2003 email to you was not raised by the any of the four referees I used (but is curiously similar to points raised by David Appell!). Keep in mind that referees used were selected in consultation with a paleoclimatologist. Five referees were selected based on the guidance I received. All are reputable paleoclimatologists, respected for their expertise in reconstruction of past climates. None (none at all) were from what Hans and Clare have referred to as "the other side" or what Hulme refers to as people well known for their opposition to the notion that humans are significantly altering global climate." One of the five referees turned down the request to review explaining he was busy and would not have the time. The remaining four referees sent their detailed comments to me. None suggested the manuscript should be rejected. S&B were asked to respond to referees comments and make extensive alterations accordingly. This was done.

I am no paleoclimatologist, far from it, but have collected opinions from other paleoclimatologists on the S&B paper. I summarise them here. What I take from the S&B paper is an attempt to assess climate data lost from sight in the Mann proxies. For example, the raising on lowering of glacier equilibrium lines was the origin of the Little Ice Age as a concept and still seems to be a highly important proxy, even if a little difficult to precisely quantify.

Using a much larger number of "proxy" indicators than Mann did, S&B inquired whether there was a globally detectable 50-year period of unusual cold in the LIA and a similarly warm era in the MWP. Further, they asked if these indicators, in general, would indicate that any similar period in the 20th century was warmer than any other era.

S&B did not purport to do independent interpretation of climate time series, either through 50-year filters or otherwise. They merely adopt the conclusions of the cited authors and make a scorecard. It seems pretty evident to me that temperatures in the LIA were the lowest since the LGM. There are lots of peer-reviewed paleo-articles which assert the existence of LIA.

Frankly, I have difficulty understanding this particular quibble. Some sort of averaging is necessary to establish the 'slower' trends, and that sort of averaging is used by every single study - they average to bring out the item of their interest. A million year average would do little to enlighten, as would detailed daily readings. The period must be chosen to eliminate as much of the 'noise' as possible without degrading the longer-term signals significantly.

As I read the S&B paper, it was a relatively arbitrary choice - and why shouldn't it be? It was only chosen to suppress spurious signals and expose the slower drift that is inherent in nature. Anyone that has seen curves of the last 2 million years must recognize that an averaging of some sort has taken place. It is not often, however, that the quibble is about the choice of numbers of years, or the exact methodology - those are chosen simply to expose 'supposedly' useful data which is otherwise hidden from view.

Let me ask Mike this question. Can he give an example of any dataset where the S&B characterization of the source author is incorrect? (I am not vouching for them, merely asking.)

S&B say that they rely on the original characterizations, not that they are making their own; I don't see a problem a priori on relying on characterizations of others or, in the present circumstances, of presenting a literature review. While S&B is a literature review, so is this section of IPCC TAR, except that the S&B review is more thorough.

The Mann et al multi-proxy reconstruction of past temperatures has many problems and these have been well documented by S&B and others. My reading of the IPCC TAR leads me to the conclusion that Mann et al has been used as the basis for a number of assertions: 1. Over the past millennium (at least for the NH) the temperature has not varied significantly (except for the European/North Atlantic sector) and hence the climate system has little internal variability. This statement is supported by an analysis of model behaviour, which also shows little internal variability in climate models. 2. Recent global warming, as inferred from instrument records, is large and unusual in the context of the Mann et al temperature reconstruction from multi-proxies. 3. Because of the previous limited variability and the

recent warming that cannot be explained by known natural forcing (volcanic activity and solar insolation changes) human activity is the likely cause of the recent global change.

In this context, IPCC mounts a powerful case. But the case rests on two main foundations; the past climate has shown little variability and the climate models reflect the internal variability of the climate system. If either or both are shown to be weak or fallacious then the IPCC case is weakened or fails.

S&B have examined the premise that the globally integrated temperature has hardly varied over the past millennium prior to the instrumental record. I agree it is not rocket science that they have performed. They have looked at the evidence provided by researchers to see if the trend of the temperature record of the European/North Atlantic sector (which is not disputed by IPCC) is reflected in individual records from other parts of the globe (Their three questions). How objective is their assessment? From a purely statistical viewpoint the work can be criticised. But if you took a purely statistical approach you probably would not have sufficient data to reach an unambiguous conclusion, or you could try statistical fiddles to combine the data and end up with erroneous results under the guise of statistical significance. S&B have looked at the data and reached the conclusion that probably the temperature record from other parts of the globe follows the same pattern as that of the European/North Atlantic sector. Of the individual proxy records that I have seen I would agree that this is the case. I certainly have not found significant regions of the NH that were cold during the medieval period and warm during the Little Ice Age period that are necessary offsets of the European/North Atlantic sector necessary to reach a hemispherically flat pattern as derived by Mann et al.

S&B have put forward sufficient evidence to challenge the Mann et al analysis outcome and seriously weaken the IPCC assertions based on Mann et al. Paleo reconstruction of temperatures and the global pattern over the past millennium and longer remains a fertile field for research. It suggests that the climate system is such that a major temporal variation as is universally recognised for the European/North Atlantic region would be reflected globally and S&B have given support to this view.

It is my belief that the S&B work is a sincere endeavour to find out whether MWP and LIA were worldwide phenomena. The historical evidence beyond tree ring widths is convincing in my opinion. The concept of "Little Ice Age" is certainly used practically by all Holocene paleo-climatologists, who work on oblivious to Mann's "disproof" of its existence.

Paleoclimatologists tell me that, for debating purposes, they are more inclined to draw attention to the Holocene Optimum (about 6000 BP) as an undisputed example of climate about 1-2 deg C warmer than at present, and to ponder the entry and exit from the Younger Dryas as an example of abrupt climate change, than to get too excited about the Medieval Warm Period, which seems a very attenuated version. However, the Little Ice Age seems valid enough as a paleoclimatic concept. North American geologists repeatedly assert that the 19th century was the coldest century in North America since the LGM. To that extent, showing temperature increase since then is not unlike a mutual fund salesman showing expected rate of return from a market bottom - not precisely false, but rather in the realm of sleight-of-hand.

Regards
Chris

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Content-Type: application/octet-stream; name="mann12prox.dat"
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Content-Disposition: attachment; filename="tornad.rcs"
Attachment converted: Macintosh HD:tornad.rcs (????/----) (0001B5B7)

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sunclock: [3]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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References

1. <http://amavis.org/>
2. <http://www.cru.uea.ac.uk/~timo/>
3. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

From: Edward Cook <drdendro@ldeo.columbia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Fwd: revised NH comparison manuscript
Date: Tue, 15 Jul 2003 09:32:57 -0400

<x-flowed>

Hi Keith,

Thanks for the paper and help in toning down Mike's efforts to put a stake in the Esper heart. I quickly read the paragraph you mention. Undoubtedly part of what is said is true, but it doesn't explain it all of the differences between the original MBH reconstruction and any of the other NH recons. Now that Mike has moved on to a totally new NH recon, I suppose all of this is a mute point. However, your Blowing Hot and Cold piece clearly showed that the MBH estimates were undoubtedly deficient in low-frequency variability compared to ANY other recon. Enough said. I need to enjoy myself.

Cheers,

Ed

>Ed

>Thought you should see this (in confidence) . Have succeeded in
>getting reasonable citation to your work and much toning down of
>criticism of Esper et al in first draft (see last paragraph before
>Section C) . Cheers

>Keith

>

>P.S. Do not ask me why Ray, Malcolm and Phil are on this cause I
>don't know - work cam out of stuff Tim did with Scott when visiting
>there last year.

>

>>Date: Tue, 3 Jun 2003 14:51:09 -0400

>>Subject: revised NH comparison manuscript

>>Cc: Mike Mann <mann@virginia.edu>

>>To: Malcolm Hughes <mhughes@ltrr.arizona.edu> ,

>> Raymond Bradley <rbradley@geo.umass.edu>, Tim Osborn <t.osborn@uea.ac.uk> ,

>> Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>

>>From: Scott Rutherford <srutherford@gso.uri.edu>

>>X-Mailer: Apple Mail (2.552)

>>

>>

>>

>>Attached to this e-mail is a revision of the northern hemisphere
>>comparison manuscript. First some general comments. I tried as best
>>as possible to incorporate everyone's suggestions. Typically this
>>meant adding/deleting or clarifying text. There were cases where we
>>disagreed with the suggested changes and tried to clarify in the
>>text why.

>>

>>In this next round of changes I encourage everyone to make specific
>>suggestions in terms of wording and references (e.g. Rutherford et
>>al. GRL 1967 instead of "see my GRL paper"). I also encourage
>>everyone to make suggestions directly in the file in coloured text
>>or by using Microsoft Word's "Track Changes" function (this will
>>save me deciphering cryptic penmanship; although I confess, my
>>writing is worse than anyone's). If you would prefer to use the
>>editing functions in Adobe Acrobat let me know and I will send a
>>PDF file. If you still feel strongly that I have not adequately
>>addressed an issue please say so.

>>I will incorporate the suggestions from this upcoming round into a
>>manuscript to be submitted. After review, everyone will get a crack
>>at it again.

>>

>>I will not detail every change made (if anyone wants the file with
>>the changes tracked I can send it). Here are the major changes:

>>

- >>1) removal of mixed-hybrid approach and revised discussions/figures
- >>2) removal of CE scores from the verification tables
- >>3) downscaling of the Esper comparison to a single figure panel and
>>one paragraph.
- >>4) revised discussion of spatial maps and revised figure (figure 8).
- >>5) seasonal comparisons have been revised

>>

>>Several suggestions have been made for where to submit. These are
>>listed on page 1 of the manuscript. Please indicate your preference
>>ASAP and I will tally the votes.

>>

>>I would like to submit by late July, so if you could please get me
>>comments by say July 15 that would be great. I will send out a
>>reminder in early July. If I don't hear from you by July 15 I will
>>assume that you are comfortable with the manuscript.

>>

>>Please let me know if you have difficulty with the file or would

>>prefer a different format.

>>

>>Regards,

>>

>>Scott

>>

>>

>>

>>

>>

>>

Scott Rutherford

>>

>>Marine Research Scientist

>>Graduate School of Oceanography

>>University of Rhode Island

>>e-mail: srutherford@gso.uri.edu

>>phone: (401) 874-6599

>>fax: (401) 874-6811

>>snail mail:

>>South Ferry Road

>>Narragansett, RI 02882

>

>--

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><http://www.cru.uea.ac.uk/cru/people/briffa/>

>

>Attachment converted: Macintosh HD:nhcomparison_v7_1.doc (WDBN/MSWD)

>(0008AC53)

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Dr. Edward R. Cook
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Director, Tree-Ring Laboratory

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Email: drdendro@ldeo.columbia.edu
Phone: 845-365-8618
Fax: 845-365-8152

=====
</x-flowed>

From: "Michael E. Mann" <mann@virginia.edu>
To: Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley
<rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley
<tcrowley@duke.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil
Jones <p.jones@uea.ac.uk>, mann@virginia.edu, jto@u.arizona.edu,
omichael@princeton.edu, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth
<trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>
Subject: letter to Senate
Date: Tue, 22 Jul 2003 14:32:45 -0400

Dear fellow Eos co-authors,
Given the continued assault on the science of climate change by some
on Capitol Hill,
Michael and I thought it would be worthwhile to send this letter to
various members of the
U.S. Senate, accompanied by a copy of our Eos article.
Can we ask you to consider signing on with Michael and me (providing
your preferred title
and affiliation). We would like to get this out ASAP.
Thanks in advance,
Michael M and Michael O

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-
2137

[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\eudora\attach\EOS.senate letter-final.doc"

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: letter to Senate
Date: Tue, 22 Jul 2003 16:49:31 -0700
Cc: Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley
<rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley
<tcrowley@duke.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil
Jones <p.jones@uea.ac.uk>, mann@virginia.edu, jto@u.arizona.edu,
omichael@princeton.edu, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth
<trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>

Hi all - I'm not too comfortable with this, and would rather not sign
- at least not
without some real time to think it through and debate the issue. It is
unprecedented and
political, and that worries me.

My vote would be that we don't do this without a careful discussion
first.

I think it would be more appropriate for the AGU or some other
scientific org to do this -
e.g., in reaffirmation of the AGU statement (or whatever it's called)
on global climate
change.

Think about the next step - someone sends another letter to the
Senators, then we respond,
then...

I'm not sure we want to go down this path. It would be much better for
the AGU etc to do
it.

What are the precedents and outcomes of similar actions? I can imagine
a special-interest
org or group doing this like all sorts of other political actions, but
is it something for
scientists to do as individuals?

Just seems strange, and for that reason I'd advise against doing
anything with out real
thought, and certainly a strong majority of co-authors in support.

Cheers, Peck

Dear fellow Eos co-authors,
Given the continued assault on the science of climate change by some
on Capitol Hill,
Michael and I thought it would be worthwhile to send this letter to
various members of
the U.S. Senate, accompanied by a copy of our Eos article.

Can we ask you to consider signing on with Michael and me (providing your preferred title and affiliation). We would like to get this out ASAP.
Thanks in advance,
Michael M and Michael O

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
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Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment converted: Macintosh HD:EOS.senate letter-final.doc
(WDBN/MSWD) (00055FCF)

--

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http://www.geo.arizona.edu/Faculty_Pages/Overpeck.J.html
<http://www.ispe.arizona.edu/>

From: Tom Wigley <wigley@ucar.edu>
To: Michael Oppenheimer <omichael@Princeton.EDU>
Subject: Re: letter to Senate
Date: Wed, 23 Jul 2003 20:13:12 -0600
Cc: Jonathan Overpeck <jto@u.arizona.edu>, "Michael E. Mann" <mann@virginia.edu>, Caspar M Ammann <ammann@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Ben Santer <santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>

<x-flowed>

Folks,

Here are some thoughts about the Soon issue, partly arising from talking to Ben.

What is worrying is the way this BS paper has been hyped by various groups. The publicity has meant that the work has entered the consciousness of people in Congress, and is given prominence in some publications emanating from that sector. The work appears to have the imprimatur of Harvard, which gives it added credibility.

So, what can we as a community do about this? My concerns are two-fold, and I think these echo all of our concerns. The first is the fact that the papers are simply bad science and the conclusions are incorrect. The second is that the work is being used quite openly for political purposes.

As scientists, even though we are aware of the second issue, we need to concentrate on exposing the scientific flaws. We also need to do this in as authoritative a way as possible. I do not think it is enough to speak as individuals or even as a group of recognized experts. Even as a group, we will not be seen as having the 'power' of the Harvard stamp of approval.

What I think is necessary is to have the expressed support of both AGU and AMS. It would also be useful to have Harvard disassociate themselves from the work. Most importantly, however, we need the NAS to come into the picture. With these 4 institutions, together with us (and others) as experts, pointing out clearly that the work is scientific rubbish, we can certainly win this battle.

I suggest that we try to get NAS to set up a committee to (best option) assess the science in the two BS papers, or (less good, but still potentially very useful) assess the general issue of the paleo record for global- or hemispheric-scale temperature changes over the past 1000 years. The second option seems more likely to be acceptable to NAS. This is arguably an issue of similar importance to the issue of climate sensitivity uncertainties which NAS reviewed earlier this year (report still in preparation).

I am not sure how to fold AGU and AMS into this -- ideas are welcome.

Similarly, perhaps some of you know some influential Harvard types better than I do and can make some suggestions here.

The only way to counter this crap is to use the biggest guns we can muster. The Administration and Congress still seem to respect the NAS (even above IPCC) as a final authority, so I think we should actively pursue this path.

Best wishes,
Tom.

Michael Oppenheimer wrote:

> Dear All:

>

> Since several of you are uncomfortable, it makes good sense to step back and

> think about a more considered approach. My view is that scientists are fully

> justified in taking the initiative to explain their own work and its relevance in

> the policy arena. If they don't, others with less scruples will be heard

> instead. But each of us needs to decide his or her own comfort zone.

>

> In this case, the AGU press release provides suitable context, so it may be that

> neither a separate letter nor another AGU statement would add much at this time.

> But this episode is unlikely to be the last case where clarity from individuals

> or groups of scientists will be important.

>

> Michael

>

>

>

> Tom Wigley wrote:

>

>

>>Folks,

>>

>>I am inclined to agree with Peck. Perhaps a little more thought and time

>>could lead to something with much more impact?

>>

>>Tom.

>>_____

>>

>>Jonathan Overpeck wrote:

>>
>>>Hi all - I'm not too comfortable with this, and would rather not sign
-
>>>at least not without some real time to think it through and debate the
>>>issue. It is unprecedented and political, and that worries me.
>>>
>>>My vote would be that we don't do this without a careful discussion
first.
>>>
>>>I think it would be more appropriate for the AGU or some other
>>>scientific org to do this - e.g., in reaffirmation of the AGU
statement
>>>(or whatever it's called) on global climate change.
>>>
>>>Think about the next step - someone sends another letter to the
>>>Senators, then we respond, then...
>>>
>>>I'm not sure we want to go down this path. It would be much better for
>>>the AGU etc to do it.
>>>
>>>What are the precedents and outcomes of similar actions? I can imagine
a
>>>special-interest org or group doing this like all sorts of other
>>>political actions, but is it something for scientists to do as
individuals?
>>>
>>>Just seems strange, and for that reason I'd advise against doing
>>>anything with out real thought, and certainly a strong majority of
>>>co-authors in support.
>>>
>>>Cheers, Peck
>>>
>>>
>>>
>>>
>>>>Dear fellow Eos co-authors,
>>>>
>>>>Given the continued assault on the science of climate change by some
>>>>on Capitol Hill, Michael and I thought it would be worthwhile to send
>>>>this letter to various members of the U.S. Senate, accompanied by a
>>>>copy of our Eos article.
>>>>
>>>>Can we ask you to consider signing on with Michael and me (providing
>>>>your preferred title and affiliation). We would like to get this out
ASAP.
>>>>
>>>>Thanks in advance,
>>>>
>>>>Michael M and Michael O
>>>>
>>>>_____

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia

>>>>
>>>>

Charlottesville, VA 22903

>>>>e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

>>>> <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
>>>>

>>>>Attachment converted: Macintosh HD:EOS.senate letter-final.doc
>>>>(WDBN/MSWD) (00055FCF)

>>>>

>>>>

>>>>

>>>>--

>>>>

>>>>Jonathan T. Overpeck

>>>>Director, Institute for the Study of Planet Earth

>>>>Professor, Department of Geosciences

>>>>

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>>>>http://www.geo.arizona.edu/Faculty_Pages/Overpeck.J.html

>>>><http://www.ispe.arizona.edu/>

>>

</x-flowed>

From: "Michael E. Mann" <mann@virginia.edu>

To: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: reconstruction errors

Date: Thu, 31 Jul 2003 11:18:24 -0400

Tim,

Attached are the calibration residual series for experiments based on available networks back to:

AD 1000

AD 1400

AD 1600

I can't find the one for the network back to 1820! But basically, you'll see that the residuals are pretty red for the first 2 cases, and then not significantly red for the 3rd case--its even a bit better for the AD 1700 and 1820 cases, but I can't seem to dig them up. In any case, the incremental changes are modest after 1600--its pretty clear that key predictors drop out before AD 1600, hence the redness of the residuals, and the notably larger uncertainties farther back...

You only want to look at the first column (year) and second column (residual) of the files. I can't even remember what the other columns are!

Let me know if that helps. Thanks,
mike

p.s. I know I probably don't need to mention this, but just to insure absolutely clarify on this, I'm providing these for your own personal use, since you're a trusted colleague. So please don't pass this along to others without checking w/ me first. This is the sort of "dirty laundry" one doesn't want to fall into the hands of those who might potentially try to distort things...

At 02:58 PM 7/31/2003 +0100, you wrote:

Thanks for the explanation, Mike. Now I see it, it looks familiar - so perhaps you've explained it to me previously (if you have, then sorry for asking twice!).

I now understand how you compute them in theory. I have two further questions though (sorry):

(1) how do you compute them in practise? Do you actually integrate the spectrum of the residuals?

(2) how would I estimate an uncertainty for a particular band of time scales (e.g. decadal to secular, $f=0.0$ to 0.1)? If integrating the spectrum of the residuals, I wonder whether integrating from $f=0$ to $f=0.02$ and then $f=0.02$ to (e.g.) $f=0.1$ (note this last limit has changed) would give me the right error for time scales of 10 years and longer (i.e. for a 10-yr low pass filter)? The way I had planned to do this was to assume the residuals could be modelled as a first order autoregressive process, with lag-1 autocorrelation $r_1=0.0$ after 1600 (essentially white) and $r_1=???$ before 1600. Do you know what the lag-1 autocorrelation of the residuals is for the network that goes

back to 1000 AD?

The stuff back 2000 years will be interesting, though the GCM runs we're starting to look at go back only 500 (Hadley Centre) or 1000 (German groups), so MBH99 seems fine for now.

Cheers

Tim

At 14:28 31/07/2003, you wrote:

Tim,

The one-sigma *total* uncertainty is determined from adding the low f and high f components of uncertainty in quadrature. The low f and high f uncertainties aren't uncertainties for a particular (e.g. 30 year or 40-year) running mean, they are band integrated estimates of uncertainties (high-frequency band from $f=0$ to $f=0.02$, low-frequency band from $f=0.02$ to $f=0.5$ cycle/year) taking into account the spectrum of the residual variance (the broadband or "white noise" mean of which is the nominal variance of the calibration residuals)

Alternatively, one could calculate uncertainties for a particular timescale average using the standard deviation of the calibration residuals, and applying a square-root-N' argument (where N' is the effective degrees of freedom in the calibration residuals). I believed I did this at one point, and got similar results.

Let me know if this needs further clarification. Thanks,
mike

p.s. you might want to try to using Mann and Jones N. Hem if you're going back further than AD 1000? Crowley has some EBM results now back to 0 AD, and is in the process of comparing w/ that. Sould be interesting...

At 02:04 PM 7/31/2003 +0100, you wrote:

Hi Mike,

we've recently been making plans with Simon Tett at the Hadley Centre for comparing model simulations with various climate reconstructions, including the MBH98 and MBH99 Northern Hemisphere temperatures. I was stressing the importance of including uncertainty estimates in the comparison and that the error estimates should depend on the timescale (e.g. smoothing filter or running mean) that had been applied.

I then looked at the file that I have been using for the uncertainties associated with MBH99 (see attachment), which I must have got from you some time ago. Column 1 is year, 2 is the "raw" standard error, 3 is $2*SE$.

But what are columns 4 and 5? I've been plotting column 4, labelled "1 sig (lowf)" when plotted your smoothed reconstruction, assuming that this is the error appropriate to low-pass filtered data. I'd also assumed that the last column "1 sig (highf)" was appropriate to high-pass filtered data. I also noticed that the sum of the squared high and low errors equalled the square of the raw error, which is nice.

But I've realised that I don't understand how you estimate these errors, nor what time scale the lowf and highf cutoff uses (maybe 40-year smoothed as in the IPCC plots?).

From MBH99 it sounds like post-1600 you assume uncorrelated gaussian calibration residuals. In which case you would expect the errors for a 40-year mean to be reduced by $\sqrt{40}$. This doesn't seem to match the values in the attached file. Pre-1600 you take into account that the residuals are autocorrelated (red noise rather than white), so presumably the reduction is less than $\sqrt{40}$, but some factor (how do you compute this?).

The reason for my questions is that I would like to (1) check whether I've been doing the right thing in using column 4 of the attached file with your smoothed reconstruction, and (2) I'd like to estimate the errors for a range of time scales, so I can compare decadal means, 30-year means, 50-year means etc.

Thanks in advance for any help you can give me here.

Tim

Dr Timothy J Osborn

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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[6]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eutora\attach\nh-ad1000-resid.dat" Attachment Converted: "c:\documents and settings\tim osborn\my documents\eutora\attach\nh-ad1400-resid.dat" Attachment Converted: "c:\documents and settings\tim osborn\my documents\eutora\attach\nh-ad1600-resid.dat"

References

1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
4. <http://www.cru.uea.ac.uk/~timo/>
5. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
6. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: reconstruction errors
Date: Thu Jul 31 14:04:23 2003

Hi Mike,

we've recently been making plans with Simon Tett at the Hadley Centre for comparing model simulations with various climate reconstructions, including the MBH98 and MBH99 Northern Hemisphere temperatures. I was stressing the importance of including uncertainty estimates in the comparison and that the error estimates should depend on the timescale (e.g. smoothing filter or running mean) that had been applied.

I then looked at the file that I have been using for the uncertainties associated with MBH99 (see attachment), which I must have got from you some time ago. Column 1 is year, 2 is the "raw" standard error, 3 is $2*SE$.

But what are columns 4 and 5? I've been plotting column 4, labelled "1 sig (lowf)" when plotted your smoothed reconstruction, assuming that this is the error appropriate to low-pass filtered data. I'd also assumed that the last column "1 sig (highf)" was appropriate to high-pass filtered data. I also noticed that the sum of the squared high and low errors equalled the square of the raw error, which is nice.

But I've realised that I don't understand how you estimate these errors, nor what time scale the lowf and highf cutoff uses (maybe 40-year smoothed as in the IPCC plots?). From MBH99 it sounds like post-1600 you assume uncorrelated gaussian calibration residuals. In which case you would expect the errors for a 40-year mean to be reduced by $\sqrt{40}$. This doesn't seem to match the values in the attached file. Pre-1600 you take into account that the residuals are autocorrelated (red noise rather than white), so presumably the reduction is less than $\sqrt{40}$, but some factor (how do you compute this?).

The reason for my questions is that I would like to (1) check whether I've been doing the right thing in using column 4 of the attached file with your smoothed reconstruction, and (2) I'd like to estimate the errors for a range of time scales, so I can compare decadal means, 30-year means, 50-year means etc.

Thanks in advance for any help you can give me here.

Tim

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: reconstruction errors
Date: Fri Aug 1 14:24:35 2003

Thanks very much for helping me out with this Mike. Rest assured that the data won't be passed on to anyone else. I'll let you know if I use them to compute uncertainties at different time scales.

Cheers

Tim

At 16:18 31/07/2003, you wrote:

Tim,

Attached are the calibration residual series for experiments based on available networks back to:

AD 1000

AD 1400

AD 1600

I can't find the one for the network back to 1820! But basically, you'll see that the residuals are pretty red for the first 2 cases, and then not significantly red for the 3rd case--its even a bit better for the AD 1700 and 1820 cases, but I can't seem to dig them up. In any case, the incremental changes are modest after 1600--its pretty clear that key predictors drop out before AD 1600, hence the redness of the residuals, and the notably larger uncertainties farther back...

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Let me know if that helps. Thanks,
mike

p.s. I know I probably don't need to mention this, but just to insure absolutely clarify on this, I'm providing these for your own personal use, since you're a trusted colleague. So please don't pass this along to others without checking w/ me first. This is the sort of "dirty laundry" one doesn't want to fall into the hands of those who might potentially try to distort things...

From: "Michael E. Mann" <mann@virginia.edu>
To: "Jim Salinger" <j.salinger@niwa.co.nz>, Phil Jones
<p.jones@uea.ac.uk>, Barrie.Pittock@csiro.au, m.hulme@uea.ac.uk, "Neville
Nicholls" <n.nicholls@bom.gov.au>
Subject: RE: Recent climate sceptic research and the journal Climate
Research
Date: Mon, 04 Aug 2003 09:05:47 -0400
Cc: n.nicholls@bom.gov.au, Peter.Whetton@csiro.au,
Roger.Francey@csiro.au, David.Etheridge@csiro.au, Ian.Smith@csiro.au,
Simon.Torok@csiro.au, Willem.Bouma@csiro.au, pachauri@teri.res.in,
Greg.Ayers@csiro.au, Rick.Bailey@csiro.au, Graeme.Pearman@csiro.au,
mmaccrac@comcast.net, tcrowley@duke.edu, rbradley@geo.umass.edu,

Dear Jim,

Thanks for your continued interest and help w/ all this. It's nice to
know that our friends
down under are doing their best to fight the misinformation. It is
true that the skeptics
twist the truth clockwise rather than counterclockwise in the Southern
Hemisphere?

There was indeed a lot of activity last week. Hans Von Storch's
resignation as chief editor

of CR, which I think took a lot of guts, couldn't have come at a
better time. It was on the

night before before the notorious "James Inhofe", Chair of the Senate
"Environment and

Public Works Committee" attempted to provide a public stage for Willie
Soon and David

Legates to peddle their garbage (the Soon & Baliunas junk of course,
but also the usual

myths about the satellite record, 1940s-1970s cooling, "co2 is good
for us" and "but water
vapor is the primary greenhouse gas!").

Fortunately, these two are clowns, neither remotely as sharp as
Lindzen or as slick as

Michaels, and it wasn't too difficult to deal with them. Suffice it to
say, the event did

not go the way Inhofe and the republicans had hoped. The democrats,
conveniently, had

received word of Hans' resignation, but the republicans and
Soon/Legates had not. So when,

quite fittingly, Jim Jeffords (you may remember--he's the U.S. senator
who was in the news

a couple years ago for tilting the balance of power back to the
democrats when he left the

republican party in protest) hit them with this news at the hearing,
they were caught

completely off guard. The "Wall Street Journal" article you cited was
icing on the cake.

Inhofe, who rails against the liberal media, will have a difficult
time doing so against

the WSJ!

Also of interest to you (attached) might be the op-ed that Ray
Bradley, Phil, and I have

written and submitted to the "Seattle News Tribune" in response to an op-ed by Baliunas

(also attached) that some industry group has been sending around to various papers over the

last week. Only two (Providence Journal and Seattle NT) have thusfar bitten...

There is a rumour that Harvard may have had enough w/ their name being dragged through the

mud by the activities of Baliunas and Soon, and that "something is up". Baliunas and Soon,

as alluded to in the WSJ article, are now no longer talking to the media. Will keep you

posted on that...

mike

At 03:58 PM 8/4/2003 +1200, Jim Salinger wrote:

Dear Mike et al

I also share Neville's thanks to you all for the reasoned and evaluated responses over

the last few months. They have been good, and separated out 'academic standards'

from 'academic freedom', which we have to be careful not to abuse.

I also note the following, come through over the weekend from the Wall Street Journal

(below) and would also compliment those of you who, with Hans Von Storch resigned

your editorships when information that should be published was clearly suppressed.

If you have further information that you feel free to share on last week's events then

we

in New Zealand would appreciate hearing it, as we have been extremely concerned

about academic standards in the reviewing of articles from New Zealand sources.

Again thanks to all on your stands.

Best regards

Jim

>>>> July 31, 2003

>>>> DEBATING GLOBAL WARMING

>>>>

>>>> Global Warming Skeptics

>>>> Are Facing Storm Clouds

>>>>

>>>> By ANTONIO REGALADO

>>>> Staff Reporter of THE WALL STREET JOURNAL

>>>>

>>>> A big flap at a little scientific journal is raising questions about

>>>> a study that has been embraced by conservative politicians for its

>>>> rejection of widely held global-warming theories.

>>>>

>>>> The study, by two astronomers at the Harvard-Smithsonian
Center for
>>>> Astrophysics, says the 20th century wasn't unusually warm
compared
>>>> with earlier periods and contradicts evidence indicating man-
made
>>>> "greenhouse" gases are causing temperatures to rise.
>>>>
>>>> Since being published last January in Climate Research, the
paper has
>>>> been widely promoted by Washington think tanks and cited by
the White
>>>> House in revisions made to a recent Environmental Protection
Agency
>>>> report. At the same time, it has drawn stinging rebukes from
other
>>>> climate scientists.
>>>>
>>>> This week, three editors of Climate Research resigned in
protest over
>>>> the journal's handling of the review process that approved the
study;
>>>> among them is Hans von Storch, the journal's recently
appointed
>>>> editor in chief. "It was flawed and it shouldn't have been
>>>> published," he said.
>>>>
>>>> Dr. von Storch's resignation was publicly disclosed Tuesday by
Sen.
>>>> James Jeffords (I., Vt.), a critic of the administration's
>>>> environmental policies, during a hearing of the Senate
Environment
>>>> and Public Works Committee called by its chairman, Sen. James
Inhofe
>>>> (R., Okla.).
>>>>
>>>> The debate over global warming centers on the extent to which
gases
>>>> released from the burning of fossil fuels -- mainly carbon
dioxide --
>>>> are trapping the sun's heat in the Earth's atmosphere,
creating a
>>>> greenhouse effect. The political fight has intensified as the
Senate
>>>> votes on a major energy bill. Sens. John McCain (R., Ariz.)
and
>>>> Joseph Lieberman (D., Conn.) planned to introduce an amendment
this
>>>> week that would cap carbon-dioxide emissions at 2000 levels
starting
>>>> in 2010 for select industries. The Bush administration is
opposed to
>>>> imposing caps, and the measure isn't expected to become law.
>>>>

>>>> The Harvard study has become part of skeptics' arguments. Mr. Inhofe, who is leading the opposition to the emissions measures, cited the research in a speech on the Senate floor Monday in which he said, "the claim that global warming is caused by man-made emissions is simply untrue and not based on sound science."

>>>> The paper was authored by astronomers Willie Soon and Sallie Baliunas, and looked at studies of tree rings and other indicators of past climate. Their basic conclusion: The 20th century wasn't the warmest century of the past 1,000 years. They concluded temperatures may have been higher during the "Medieval Warm Period," the time during which the Norse settled Greenland.

>>>> Dr. Soon couldn't be reached and Dr. Baliunas declined comment. In his testimony before Mr. Inhofe's committee, Dr. Soon reiterated the findings of his study, which was partly funded by the American Petroleum Institute.

>>>> Dr. Soon's findings contradict widely cited research by another scientist, Michael E. Mann of the University of Virginia. Dr. Mann's reconstruction of global temperatures shows a distinct pattern shaped like a hockey stick: Temperatures stayed level for centuries, with a sudden upturn during recent decades.

>>>> A reference to Dr. Soon's paper previously found its way into revisions suggested by the White House to an EPA report on environmental quality. According to an internal EPA memorandum disclosed in June, agency scientists were concerned the version containing the White House edits "no longer accurately represents scientific consensus on climate change." Dr. Mann's data showing the hockey-stick temperature curve was deleted. In its place, administration officials added a reference to Dr. Soon's paper, which the EPA memo called "a limited analysis that supports the administration's favored message."

>>>> The EPA says the memo appears to be an internal e-mail between

>>>> staffers but isn't an "official" document. A spokesman at the
White
>>>> House's Council on Environmental Quality says the addition of
the
>>>> citation to Dr. Soon's paper to the draft report was suggested
during
>>>> an interagency review process overseen by the White House.
>>>>
>>>> Dr. Mann and 13 colleagues published a critique of Dr. Soon's
paper
>>>> in Eos, a publication of the American Geophysical Union, this
month.
>>>> They said the Harvard team's methods were flawed and their
results
>>>> "inconsistent with the preponderance of scientific evidence."
>>>>
>>>> Then, last week Dr. von Storch was contacted by Sen.
Jeffords's
>>>> staff, which was looking into the paper in preparation for
Tuesday's
>>>> hearing, where Dr. Soon and Dr. Mann were scheduled to appear.
After
>>>> hearing from Sen. Jeffords, Dr. von Storch says he decided to
speed
>>>> an editorial into print criticizing publication of the paper.
>>>>
>>>> But publisher Otto Kinne blocked the move, saying that while
he
>>>> favored publication of the editorial, Dr. von Storch's
proposals were
>>>> still opposed by some of the other editors. "I asked Hans not
to rush
>>>> the editorial," Mr. Kinne said in an e-mail.
>>>>
>>>> That is when Dr. von Storch resigned, followed by two other
editors.
>>>>
>>>> --John J. Fialka contributed to this article.
On 30 Jul 2003 at 8:26, Neville Nicholls wrote:
> Dear Mike et al:
>
> Despite my reluctance to get involved in preparing a public
response
> to the SB03 papers, and my feeling that we would be better off
> ignoring it, I have to record my appreciation of the job you have
done
> in preparing the EOS 8 July commentary. I thought it was an
excellent,
> scientific, calm evaluation of SB03. Fortuitously, it arrived the
same
> day I had to prepare a brief about SB03 for my political masters.
It
> was very helpful to have your commentary to include in this brief.
>

> Many thanks.
>
> Neville Nicholls
> Bureau of Meteorology Research Centre
> PO Box 1289K, Melbourne, AUSTRALIA, 3001
> Street address: 13th floor, 150 Lonsdale Street, Melbourne,
AUSTRALIA,
> 3000 Phone: +61 3 9669 4407; Fax: +61 3 9669 4660
>

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\eudora\attach\SeattleNewsTribune-oped-final.doc" Attachment
Converted: "c:\eudora\attach\BaliunasProvidenceJournal25Jul03.pdf"

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Simon Tett <simon.tett@metoffice.com>, Keith Briffa <k.briffa@uea.ac.uk>, Philip Brohan <philip.brohan@metoffice.com>
Subject: Re: Uncertainty in model-paleo uncertainty
Date: Mon, 04 Aug 2003 14:30:35 +0100

<x-flowed>

Simon & Philip,

here's some thoughts on uncertainty...

At 10:42 04/08/2003, Simon Tett wrote:

- >1) Calibration uncertainty -- there is some uncertainty in the
>relationship between proxy and temperature.
- >2) Residual noise -- the proxyies do not capture large-scale temperature
>variability perfectly.
- >3) Internal-climate variability in "real" life -- there is some chaotic
>variability in the real climate system
- >4) Internal-climate variability in the model -- ditto!
- >
- >3) & 4) I suggest we estimate from HadCM3 -- model var agrees well with
>paleo var so can't be too far wrong!

Yes, I'm happy that we use (3) and (4) from the model. If you use a short baseline to take the anomalies from, then the internal variability comes in twice in each case, both in comparing the baseline mean and the anomaly. We can minimise this by using a long baseline.

- >1) & 2) are, to some extent related, as calibration is estimate by
>regression -- thus minimising residual var (2). Nicest thing to do would
>be to estimate residual from indep. data but I don't think there is enough.....

The uncertainties that we've published with our regional and quasi-hemispheric reconstructions attempt to take both (1) and (2) in account already. Thus I use the standard errors on the two regression coefficients (for the linear regression of the sub-continental regions) and the standard errors on all multiple regression coefficients (for the quasi-Northern Hemisphere series). And then I incorporate the variance of the calibration residuals too (i.e., item (2)), modelled as first-order autoregressive terms. The appendix of the Briffa part 1 paper (page 755-757 is the appendix) in the Holocene special issue paper gives an explanation of this. Others quite often ignore (1) and just use the residuals to quantify reconstruction error, but (1) can be important especially for big anomalies (because the regression slope error is multiplied by the predicted anomaly). (1) can be difficult to quantify, of course, using some multi-variate techniques like Mann and Luterbacher use.

The regression standard errors (1) are of course computed from the calibration period. Our published errors also use the residual variance (2) computed from this calibration period. It is possible to compute (2) from independent data, but as you say we are limited by data. AND I think that the residual variance from independent data would also incorporate some or all of error (1) (because that would contribute to differences

between reconstruction and observation). I think it is better to keep the two terms separate and explicitly compute both, especially as their relative magnitudes can depend upon time scale (i.e., time averaging the data).

Am I right in thinking that the error in the *observed* record would, if taken into account, result in *reduced* reconstruction errors, because the residual variance (2) would not all be assumed to be reconstruction error - some would be observation error? But I suppose that the regression coefficient errors (1) would get larger to compensate? Anyway, we don't currently consider observed errors.

Cheers

Tim

Dr Timothy J Osborn
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web: <http://www.cru.uea.ac.uk/~timo/>

suncllock: <http://www.cru.uea.ac.uk/~timo/suncllock.htm>

</x-flowed>

From: "Stephan Singer" <SSinger@wwfepo.org>
To: <grassl@dkrz.de>, <klaus.hasselman@dkrz.de>, <per.carstedt@ecosystem.se>, <mueller@ermine.ox.ac.uk>, <michael.grubb@ic.ac.uk>, <joyeeta.gupta@ivm.vu.nl>, <Carlo.Jaeger@pik-potsdam.de>, <Martin.Welp@pik-potsdam.de>, <Bert.Metz@rivm.nl>, <m.hulme@uea.ac.uk>, <a-michaelowa@wwfepo.org>, <Berk@wwfepo.org>, <hedger@wwfepo.org>
Subject: economic costs of european heat wave
Date: Wed, 06 Aug 2003 15:06:03 +0200
Cc: <Patrick.Hofstetter@wwf.ch>, <morgan@wwf.de>, "Sible Schone" <SSchone@wwf.nl>, "Catarina Cardoso" <CCardoso@wwf.org.uk>, <jleemorgan@wwfepo.org>, "Oliver Rapf" <ORapf@wwfepo.org>, <liam@wwfthai.org>, "Katherine Silverthorne" <Katherine.Silverthorne@WWFUS.ORG>, "Lara Hansen" <Lara.Hansen@WWFUS.ORG>

dear all,
i think we all have seen [if not commented on] the devastating heat wave presently in europe - gives us a feeling on truly global warming. WWF has assured some money - a few thousand EUROS what is not much to be honest but at least a start - to ask an economist with climate policy understanding to assess in a short but fleshy paper [max 10 pages] the economic costs of these weather extremes in europe. This can be put in context with the mitigation costs of ambitious climate policies which are often quoted as a barrier to clean technologies unfortunately. I think, we as an NGO working on climate policy need such a document pretty soon for the public and for informed decision makers in order to get a) a debate started and b) in order to get into the media the context between climate extremes/desasters/costs and finally the link between weather extremes and energy - just the solutions parts what still is not communicated at all.
In short, can you advise us on a competent author who is readily available [can be one of you, of course], to bring together the conventionally accessible costs of reduced transport loads on rivers, in railway networks, forest fires, disruption of water supply and irrigation, closure of hydro power and even nuclear in some locations, health costs, agricultural failures [if accessible] etc etcetc...resulting from the heat wave?
Of course, i could not sent this e-mail to all competent sceintists, so fell free to share please and come back to me - at best ASAP

many regards
stephan singer

Stephan Singer
Head of European Climate and Energy Policy Unit
WWF, the conservation organization
E-mail: ssinger@wwfepo.org

www.panda.org/epo - Stay up-to-date with WWF's policy work in the capital of Europe
www.passport.panda.org - take action on global conservation issues - have you got your Passport yet?

WWF European Policy Office

36 avenue de Tervuren Box 12
1040 Brussels, Belgium
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Fax: +32-2-743-8819

From: Tom Wigley <wigley@ucar.edu>
To: Andr  Berger <berger@astr.ucl.ac.be>
Subject: Re: FW: Shaviv & Veizer in GSA Today
Date: Tue, 19 Aug 2003 09:00:33 -0600
Cc: Mike MacCracken <mmaccrac@comcast.net>, Martin Hoffert
<marty.hoffert@nyu.edu>, Karl Taylor <taylor13@llnl.gov>, Ken Caldiera
<kenc@llnl.gov>, Curt Covey <covey1@llnl.gov>, Stefan Rahmstorf
<rahmstorf@pik-potsdam.de>, "Michael E. Mann" <mann@virginia.edu>,
Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes
<mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin
Trenberth <trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Scott
Rutherford <srutherford@gso.uri.edu>, Caspar Ammann <ammann@ucar.edu>,
Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>,
Michael Oppenheimer <omichael@princeton.edu>, Steve Schneider
<shs@stanford.edu>, Gabi Hegerl <hegerl@duke.edu>, Ellen Mosley-Thompson
<thompson.4@osu.edu>, Eric Steig <steig@ess.washington.edu>,
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stocker@climate.unibe.ch, Urs Neu <urs.neu@sanw.unibe.ch>, J rg Beer
<beer@hermes.emp-eaw.ch>

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Andre,

I have been closely involved in the CR fiasco. I have had papers that I refereed (and soundly rejected), under De Freitas's editorship, appear later in the journal -- without me seeing any response from the authors. As I have said before to others, his strategy is first to use mainly referees that are in the anti-greenhouse community, and second, if a paper is rejected, to ignore that review and seek another more 'sympathic' reviewer. In the second case he can then (with enough reviews) claim that the honest review was an outlier.

I agree that an ethics committee is needed and I would be happy to serve on such a committee. It would have to have endorsement by international societies, like Roy. Soc., US Nat. Acad., Acad. Europ., plus RMS, AMS, AGU, etc.

Jim Titus mentioned to me that in the legal profession here people are disbarred for behavior like that of De Freitas (and even John Christy -- although this is a more subtle case). We cannot do that of course, but we can alert the community of honest scientists to such behavior and formally discredit these people.

The Danish Acad. did something like this recently, but were not entirely successful.

In the meantime, I urge people to dissociate themselves from Climate Research. The residual 'editorial' (a word I use almost tongue in cheek) board is looking like a rogues' gallery of skeptics. Those remaining who are credible scientists should resign.

Tom.

+++++

Andr  Berger wrote:

> Dear Stefan,

> Dear Mike,

> Dear Colleagues,

>

> I admire the courage of Stefan and of all other colleagues who are
> willing to answer these highly controverted papers (garbage as Marty
> said). I am personally tired of analysing these papers, having quit
> doing this for the Ministry and European Commission some 5 years ago.

>

> Nevertheless, I am also sad when I see these papers, mostly because
they

> succeeded to be published. So not only we have to teach their authors
> the Science of climate but also the reviewers and/or the
> editors/publishers who have accepted them. This is a huge effort. I,
> personally, would like to see an International Committee of Ethics (or
> something like this) in Geo-Sciences be created as it is the case for
> Medical Sciences and Biotechnology.

>

> I have been told that AMS has such a Committee who is a kind of super
> peer-review telling what is wrong in some declarations, papers, books
> Is anybody willing to participate in an attempt to create such a
> Committee within AGU-EGU-IUGG ... ?

>

> In the meantime, I am please to send you here attached an email by R.L.
> Park on Soon, Baliunas, Seitz and others.

>

> Best Wishes and Regards,

>

> Andr  BERGER

>

> -----

>

> WHAT'S NEW Robert L. Park Friday, 8 Aug 03 Washington, DC
> 2. POLITICAL CLIMATE: WHAT'S RIGHT FOR THE AMERICAN PEOPLE?
> One of the purported abuses cited in the minority staff report
> involved the insertion into an EPA report of a reference to a
> paper by Soon and Baliunas that denies globl warming (WN 1 Aug
> 03). To appreciate its significance, we need to go back to March
> of 1998. We all got a petition card in the mail urging the
> government to reject the Kyoto accord(WN 13 Mar 98). The cover
> letter was signed by "Frederick Seitz, Past President, National
> Academy of Sciences." Enclosed was what seemed to be a reprint
> of a journal article, in the style and font of Proceedings of the
> NAS. But it had not been published in PNAS, or anywhere else. The
> reprint was a fake. Two of the four authors of this non-article
> were Soon and Baliunas. The other authors, both named Robinson,
> were from the tiny Oregon Institute of Science and Medicine in
> Cave Junction, OR. The article claimed that the environmental
> effects of increased CO2 are all beneficial. There was also a
> copy of Wall Street Journal op-ed by the Robinsons (father and
> son) that described increased levels of CO2 in the atmosphere as
> "a wonderful and unexpected gift of the industrial revolution."
> There was no indication of who had paid for the mailing. It was

> a dark episode in the annals of scientific discourse.
>
>
>
>
>
>
>
>
> At 10:59 4/08/2003 -0400, Mike MacCracken wrote:
>
>> You all might want to get in on response to this paper.
>>
>> Mike
>>
>> -----
>> From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
>> Date: Mon, 04 Aug 2003 16:02:36 +0200
>> To: "Michael E. Mann" <mann@virginia.edu>
>> Cc: Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes
>> <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Kevin
>> Trenberth
>> <trenbert@ucar.edu>, Tom Crowley <tcrowley@duke.edu>, Tom Wigley
>> <wigley@ucar.edu>, Scott Rutherford <srutherford@gso.uri.edu>, Caspar
>> Ammann
>> <ammann@ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn
>> <t.osborn@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Steve
>> Schneider <shs@stanford.edu>, Gabi Hegerl <hegerl@duke.edu>, Mike
>> MacCracken
>> <mmaccrac@comcast.net>, Ellen Mosley-Thompson <thompson.4@osu.edu>, Eric
>> Steig <steig@ess.washington.edu>, jmahlman@ucar.edu,
>> wuebbles@atmos.uiuc.edu, jto@u.arizona.edu, stocker@climate.unibe.ch,
>> Urs
>> Neu <urs.neu@sanw.unibe.ch>, Jⁿrg Beer <beer@hermes.emp-eaw.ch>
>> Subject: Shaviv & Veizer in GSA Today
>>
>> Dear colleagues,
>>
>> the Soon&Baliunas paper has given political lobbyists a field day in
>> their attempts to confuse the public and decision-makers about the
>> state
>> of global warming science. It is quite interesting how a lobby
>> organisation like the Marshall Institute manages to get a paper like
>> that into the peer-reviewed literature with the help of a sympathetic
>> editor, against reviewer concerns, and then capitalise on that right
>> away in Senate hearings and the media. There clearly is a wider and
>> well-funded strategy behind such activities, which has something to do
>> with why the US has backed out of the Kyoto protocol. These same US
>> organisations are also active here in Europe trying to influence
>> policy,
>> albeit so far with less success.
>>
>> In the face of such sophisticated lobbying we scientists should not be

>> too naive. Although simply doing good science remains our main job, I
>> think at some points we need to intervene in the public debate and try
>> to clarify what is science and what is just political lobbying. In
>> particular, I feel that it is important to not let bad, politically
>> motivated science stand unchallenged in the peer-reviewed literature -
>> it is too easy to just shrug and ignore an obviously bad paper. Hence I
I
>> greatly appreciate that Mike and his co-authors responded in Eos to
the
>> errors in the Soon&Baliunas paper.

>>
>> I feel another recent paper may require a similar scientific response,
>> the one by Shaviv&Veizer (attached). It derives a supposed upper limit
>> for the CO2-effect on climate (i.e., 0.5 C warming for CO2 doubling),
>> based on paleoclimatic data on the multi-million-year time scale. This
>> paper got big media coverage here in Germany and I guess it is set to
>> become a climate skeptics classic: the spin is that GCMs show a large
>> CO2 sensitivity, but climate history proves it is really very small.
>> Talking to various colleagues, everyone seems to agree that most of
this
>> paper is wrong, starting from the data themselves down to the
>> methodology of extracting the CO2 effect.

>>
>> I think it would be a good idea to get a group of people together to
>> respond to this paper (in GSA today). My expertise is good for part of
>> this and I'd be willing to contribute. My questions to you are:
>> 1. Does anyone know of any other plans to respond to this paper?
>> 2. Would anyone like to be part of writing a response?
>> 3. Do you know people who may have the right expertise? Then please
>> forward them this mail.

>>
>> Best regards, Stefan
>>
>> --
>> Prof. Stefan Rahmstorf
>> Potsdam Institute for Climate Impact Research (PIK)
>> For contact details, reprints, movies & general infos see:
>> <http://www.pik-potsdam.de/~stefan>
>>
>>
>

> Prof. A. BERGER
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>

>

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>, Tom Wigley <wigley@ucar.edu>, Tom Crowley <tcrowley@duke.edu>
Subject: Re: POLL ON SOON-BALIUNAS
Date: Tue, 19 Aug 2003 09:48:05 +0100
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Michael Oppenheimer <omichael@princeton.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Kevin Trenberth <trenbert@ucar.edu>, Ben Santer <santer1@llnl.gov>, Steve Schneider <shs@stanford.edu>, Caspar Ammann <ammann@ucar.edu>, hegerl@duke.edu, mann@virginia.edu

Tom,

I once met Soon at a meeting organised by the ESA in Tenerife. I think he gave a talk

-

but only think, so it wasn't memorable in any way. As you say they don't come to the regular meetings like EGU/S, AGU, AMS etc. I only went to Tenerife as the organisers paid for me to go.

Citation ratings vary (there are several different scales/indicators as well) a lot

from year to year for most journals. I've never figured out how the counting is done wrt

the highly cited lists that Tom. W., Kevin and I are on. Do only first authorships count for

example? Even with a common name like mine people still get it wrong and mistakes persist.

Surprisingly Jim Hansen doesn't make the above list ([1]<http://www.highlycited.com>), but then

he normally drops his E.

There are few more journals (QSR, Climate Change, IJC, AAR to give a few) where

paleo papers also appear.

Cheers

Phil

At 10:43 13/08/2003 -0400, Michael E. Mann wrote:

I checked this out prior to my senate hearing. Their science citations in the climate

literature are poor, as one would hope and expect.

Interestingly, they both drop their second initials when publishing in the climate

literature so that their names don't turn in up in ISI if you do a search on their

publications in the astronomy literature (which use the full initials)--apparently,

they don't want their astronomy colleagues to be aware that they're moonlighting as

supposed climatologists...

Their numbers are better in the astronomy literature, though Soon's numbers even here are mediocre.

Baliunas had some well-cited publications more than a decade ago. This is her work on the use of sun-like stars as a model for solar variability, etc., which is well

referenced in the astrophysics community. However, most of these appear to be her Ph.D.

work, and appear to have been published w/ her Ph.D adviser.

Not much evidence however that she has made any useful, independent contribution since

then. There are some additional papers she's published on time series analysis of solar

signals--looks like the kind of stuff you might expect to see from a graduate student

first-year research project....

In my opinion, its would be a mistake to evaluate these on their citations numbers in

astronomy. We should focus on their numbers in the climate literature, which are the

only ones relevant when discussing the issue of how their work on climate is received by

their fellow scientists,

mike

At 08:15 AM 8/13/2003 -0600, Tom Wigley wrote:

Might be interesting to see how frequently Soon and Baliunas, individually, are cited

(as astronomers). Are they any good in their own fields?

Perhaps we could start referring to them as astrologers (excusable as ... 'oops, just a typo')

Tom.

+++++

Tom Crowley wrote:

Hi there,

we need some data on Soon and Baliunas. one of my concerns is that they only publish in

low impact journals and completely bypass the normal give and take of presentations at

open scientific meetings (for example, I think I have probably heard 100 presentations

overall from the people on this mailing list).

it is therefore very important to inquire for the sake or our exchanges with

reporters/legislators etc as to how often any of you may have heard Soon or Baliunas

give a talk in an open meeting, where they could defend their analyses.

please respond to me as to whether you have heard either of them present something on

their paleo-analyses (I think I heard Baliunas speak once on her solar-type star work, but that doesn't count).

I will let you know the results of the poll so that we may all be on the same grounds

with respect to the data and reporting such information to press inquiries/legislators etc.

further fyi I list below the journal impact for six geophysical/climate/paleoclimate journals:

Paleoceanography 3.821
J. Climate 3.250
J. Geophysical Res. (Climate) 2.245
Geophysical Research Letters 2.150
The Holocene 1.852
Climate Research 1.016

Science and Nature are much higher (26-30) but their citation numbers are I believe

inflated with respect to our field because their citation ranking also includes many

very widely cited biology publications.

hope to hear from you soon, Tom

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
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Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Prof. Phil Jones
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UK

References

1. <http://www.highlycited.com/>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: [Fwd: VS: [Climate Sceptics] Mann & Jones on 1800 yrs proxies]
Date: Sat, 23 Aug 2003 04:04:54 -0400
Cc: Phil Jones <p.jones@uea.ac.uk>, Gavin Schmidt <gavin@isis.giss.nasa.gov>, Michael Oppenheimer <omichael@princeton.edu>, Mike MacCracken <mmaccrac@comcast.net>, Tom Crowley <tcrowley@duke.edu>, cfk@lanl.gov, jhansen@giss.nasa.gov, Ellen Mosley-Thompson <thompson.4@osu.edu>, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, Keith Briffa <k.briffa@uea.ac.uk>, Kevin Trenberth <trenbert@ucar.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Gabi Hegerl <hegerl@duke.edu>, Stefan Rahmstorf <rahmstorf@pik-potsdam.de>, jto@u.arizona.edu, Eric Steig <steig@ess.washington.edu>, mann@virginia.edu

Thanks Tom,

I agree--the issue is not completely settled, and thanks for the reference (any possibility you can send me a reprint?). The point here of course is that we are talking a potential effect, w/ as you say, at best a weak signal--hardly the dominating overprint that is argued by the Idso brothers! (by the way, weren't they a circus act at one point??),
mike

At 12:48 PM 8/22/2003 -0600, Tom Wigley wrote:

Mike,

Thanks for your clarifications.

With regard to the CO2 fertilization effect on tree ring width, I wrote a paper a number

of years ago pointing out that there were signal-to-noise problems in identifying and quantifying such factors.

Wigley, T.M.L., Jones, P.D. and Briffa, K.R., 1987: Detecting the effects of acidic

deposition and CO2-fertilization on tree growth. (In) Methods of Dendrochronology.

Vol. 1, Proceedings of the Task Force Meeting on Methodology of Dendrochronology:

Krak \leq w, Poland, 26 June 1986, (eds. L. Kairiukstis, Z. Bednarz and E. Feliksik),

International Institute for Applied Systems Analysis, Agricultural Academy of Krak \leq w,

Polish Academy of Science, WOSI Wsp \leq lna Sprawa 38/37 no. 20, 239253. 1988.

While I am confident that you are correct, and that this is not a crucial factor, I

think one should be careful about denying its existence. There are, furthermore,

additional obfuscating factors that make the effects of CO2 fertilization on ring widths

hard to identify.

Perhaps more important is the fact that many tree ring based reconstructions use density data, and the jury is still out on whether more CO2 increases or decreases density.

Tom.

+++++

Michael E. Mann wrote:

Dear Colleagues,

Several you have inquired about the below claims by the notorious "Idso brothers" which relates to the paper by Mann and Jones that appeared in GRL a couple weeks ago.

Of course, its the usual disinformation we've come to expect from these folks, but a few details on why:

1) The supposed "Co2 fertilization" argument is a ruse. The only evidence that such an effect might actually play some role in tree-growth trends has been found in high elevation sites in western North America (consult Malcolm Hughes for more details). As in Mann et al '99 (GRL), any such effect, to the extent it might exist, has been removed from the relevant series used in the latest (Mann and Jones) paper through the removal of anomalous differences between low-elevation and high-elevation western North American temperature trends during the post 1800 period, prior to use of the data in climate reconstruction.

2) We haven't in the past extended the proxy reconstruction beyond 1980 because many of the proxy data drop out. However, the repeated claim by the contrarians that post-1980 proxy data don't show the warming evident in the instrumental record has finally prompted me to go ahead and perform an additional analysis in which the proxy-reconstruction is extended forward as recently as at all possible (to 1995, for

which 3 out of 8 of the NH records are available, and 1 of the 5 SH records are available). The SH and GLB reconstructions are thus obviously tenuous at best, but they do address, to the extent at all possible, the issue as to whether or not the proxy reconstructions show the post-1980 warming--and they do.

See the attached plot which compares the NH (blue), SH (green), and GLB (red) series through 1995. The late 20th century is the nominal maximum for all 3 series *without any consideration of the information in the instrumental mean series*. This thus refutes

the 2nd criticism cited by the Idso brothers.
One note about the 40 year smoothing. As in the trends in the instrumental series shown by Mann and Jones, a boundary constraint on the 40-year smooth has been used that minimizes the 2nd derivative at the boundary--this trends to preserve the trend near the end of the series and has been argued as the optimal constraint in the present of nonstationary behavior near the end of a time series (Park, 1992; Ghil et al, 2002). I favor the use of this constraint in the smoothing of records that exhibit a significant trend as one approaches the end of the available data. This might be worth talking about in the next IPCC when the subject of adopting uniform standards for smoothing data, etc. are discussed...
In retrospect, Phil and I should have included this analysis in the GRL article, but its always hard to know what specifics the contrarians are going to target in their attacks.
This analysis however, will be included in a review paper by Jones and Mann on "climate in past millennia" that is presently being finalized for "Reviews of Geophysics".
I hope that helps clarify any questions any of you might have had. Please feel free to pass this information along to anyone who might benefit from it.
Now, back to fighting the "Shaviv and Veizer" propaganda along w/ Ben Santer and David Parker out in Italy...
mike

----- Original Message -----
Subject: VS: [Climate Sceptics] Mann & Jones on 1800 yrs proxies
Date: Wed, 20 Aug 2003 13:52:40 +0300
From: Timo HEmeranta <timo.hameranta@pp.inet.fi>
To: <climatesceptics@yahoogroups.com>
CC: "Charles F. \"Chick\" Keller" <cfk@lanl.gov>, "Kirill Ya. Kondratyev" <kondratyev@KK10221.spb.edu>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "S. Fred Singer" <singer@sepp.org>, "Sallie Baliunas" <baliunas@cfa.harvard.edu>, "Carl Wunsch" <cwunsch@mit.edu>,
"David R. Legates" <legates@udel.edu>, "George Kukla" <kukla@ldeo.columbia.edu>, "James E. Hansen" <jhansen@giss.nasa.gov>,
"Tom Wigley" <wigley@meeker.ucar.edu>, "Willie Soon" <wsoon@cfa.harvard.edu>
Dear all,
GRL finally published the study
Mann, Michael E. and Phil D. Jones, 2003. Global surface temperatures

over the past two millennia, Geophysical Research Letters Vol. 30,
No.

15, 1820, 10.1029/2003GL017814, August 14, 2003

Abstract

[1] We present reconstructions of Northern and Southern Hemisphere mean surface temperature over the past two millennia based on high-resolution ?proxy? temperature data which retain millennial-scale

variability. These reconstructions indicate that late 20th century warmth is unprecedented for at least roughly the past two millennia for

the Northern Hemisphere. Conclusions for the Southern Hemisphere and global mean temperature are limited by the sparseness of available

proxy data in the Southern Hemisphere at present.

We already noticed the study in

Mann, Michael, Caspar Ammann, Kevin Trenberth, Raymond Bradley, Keith

Briffa, Philip Jones, Tim Osborn, Tom Crowley, Malcolm Hughes, Michael

Oppenheimer, Jonathan Overpeck, Scott Rutherford, and Tom Wigley, 2003.

On Past Temperatures and Anomalous Late-20th Century Warmth. Eos, Vol.

84, No. 27, page 256, July 8, 2003

There we found that " an extension back through the past 2000 years based on eight long reconstructions [Mann and Jones,2003]."

CO2 Science Magazine today presents the study as follows:

Was Late 20th Century Warming Really Unprecedented Over the Past Two Millennia?

Mann, M.E. and Jones, P.D. 2003. Global surface temperatures over the

past two millennia. Geophysical Research Letters 30:
10.1029/2003GL017814.

What was done

Using 23 individual proxy records from 8 distinct regions in the Northern Hemisphere and 5 proxy records from the Southern Hemisphere,

the authors constructed Northern and Southern Hemispheric and global mean temperature histories over the period AD 200 to as close as

they could get to the present employing a 40-year lowpass filter of the data.

What was learned

Mann and Jones say their temperature reconstructions indicate that "late

20th century warmth is unprecedented for at least roughly the past two

millennia for the Northern Hemisphere." They also say their data

and analysis "suggest a similar, but less definitive conclusion, for the global mean."

Although we and many others have many bones to pick with many aspects of

Mann and Jones' analysis, we will here focus on just a couple of points and temporarily grant them the benefit of the doubt in those other areas. First of all, granting them almost everything they have done, it can readily be seen from their own graph of their own results that the point of their reconstructed global mean temperature history is not the warmest period of the prior 1800 years. In fact, their treatment of the data depicts three earlier warmer periods: one just prior to AD 700, one just after AD 700 and one just prior to AD 1000 (see figure below). Reconstructed global temperature anomaly (based on 1961-1990 instrumental reference period) adapted from Mann and Jones (2003). The globe only becomes warmer in the 20th century when its measured temperatures are substituted for its reconstructed temperatures.

This approach is clearly unacceptable; it is like comparing apples and oranges. If one has only reconstructed temperatures from the distant past, one can only validly compare them with reconstructed temperatures from the recent past.

Another important point that is ignored by Mann and Jones is that the last century witnessed a dramatic increase in atmospheric CO2 concentration, which everyone knows is an effective aerial fertilizer.

It also witnessed a dramatic increase in atmospheric nitrogen deposition, which further enhances plant growth. Consequently, as tree-ring data comprise the bulk of the proxy temperature information employed by Mann and Jones, their reconstructed global mean temperature history must possess a non-temperature-induced pseudo-warming signal driven by CO2- and nitrogen-induced increases in growth that make 20th century warming appear significantly greater than it really is.

Hence, there could well be still other periods of the past 1800 years (in addition to the three we have already noted) when the global mean temperature was also warmer than it was at the end of their reconstructed record in the 20th century.

What it means Mann and Jones have clearly failed to demonstrate the key point they desired to make in their paper. Their data, however, speak for themselves in clearly demonstrating that late 20th century warmth was not unprecedented over the past two millennia.

????
We have already discussed about this study in July under title ?Empire

Strikes back on Soon et al.? |
All the best
Timo HΣmeranta
Moderator, Climatesceptics

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References

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: reconstruction uncertainties
Date: Fri Aug 29 16:33:55 2003
Cc: k.briffa@uea.ac.uk
Attachments: Mann uncertainty.doc

Hi Mike,

after a few bits of holiday here and there, I've now had time to complete my (initial) approach to estimating reconstruction errors on your NH temperature reconstruction. This is all based on the calibration residuals that you kindly sent me a few weeks ago.

My rationale for doing this was that I wanted uncertainty/error estimates that were dependent on the time scale being considered (e.g. a decadal mean, an annual mean, a 30-year mean, etc.). I didn't think you had published timescale-dependent errors, hence my attempt.

A second reason is that I wanted to be able to model (i.e., stochastically generate) time series of the errors, with appropriate timescale characteristics. Again, I didn't think that I could get this from your published results.

The attached document summarises the progress I've made. There are a few questions I have, and I'm concerned that the reduction in uncertainty with increasing time scale is too great. Perhaps one should be ultra conservative and have no reduction with time scale? Yet surely there ought to be some cancelling of partly uncorrelated errors? The document is not meant to form part of any paper on this (I hope to use the errors in a paper, but the point of the paper is on trend detection, not estimating errors), it just seemed appropriate to write it up like this to inform you of what I've done so far.

Any comments or criticisms will be very useful.

Cheers

Tim

From: "Michael E. Mann" <mann@virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: reconstruction uncertainties
Date: Tue, 02 Sep 2003 14:30:48 -0400
Cc: Scott Rutherford <srutherford@gso.uri.edu>, mann@virginia.edu

Hi Tim

Thanks for sending this. Unfortunately, I don't really have the time to look into any of this in detail, but let me offer the following additional explanation which will hopefully clarify the nature of any differences between our results. I fear that I may not have been clear enough in my previous explanation.

The reason that our uncertainty estimates reduce little with increasing timescale for the earlier networks is that the effective degrees of freedom are diminished sharply by the redness of the calibration residuals for networks prior to AD 1600 and earlier. But unlike you, we do not model the residuals as an AR process--this may be the source of some of the differences.

Back to AD 1600 (and later networks), the calibration residuals pass for "white noise", and the estimates follow simply from the residual uncalibrated variance, and the reduction of variance upon averaging follows standard \sqrt{N} statistics.

Prior to that, the networks failed the test. So we decomposed the calibration residuals into a "low-frequency" band (all timescales longer than 40 years which are not distinguishable from secular timescales, since I had a roughly 80 year series and was evaluating the spectrum using a multiple-taper estimate with a spectral bandwidth of ± 2 Rayleigh frequencies). We then estimated the enhancement of unresolved variance in the low-frequency band relative to the nominal white noise level. The enhancement was about a factor of 5-6 or so for the earlier networks, as I recall. To get the component of uncertainty for the low-frequency band alone (timescales longer than 40 years), I simply took that enhancement factor \times the nominal unresolved calibration variance \times the bandwidth of the "low-frequency" band (0.025 cycle/year). This yields a reduction in variance that is far less than the nominal " \sqrt{N} " reduction applied to the individual annual uncertainties. Of course, one could calculate the equivalent N' (effective temporal degrees of freedom) that this implies in a model of the residuals as AR(1) red noise, but we didn't take this approach. We modeled it as a simple step-increase spectrum (with the boundary at $f=0.025$ cycle/yr). Modeling the residuals as red noise would, my guess is, generally yield the same result, but it might have the effect of dampening the estimated enhancement of unresolved variance at the longest timescales. In any case, it should yield similar, but it would be very surprising if identical(!), results, consistent with your observations.

My guess for the difference in the AD 1600 network is that, based on the spectrum test, we did not reject the white noise null hypothesis for the residuals. So there was no variance enhancement factor for that, or subsequent, networks. It would appear that your method argues for significant serial correlation in that case. Not sure why we come to different

conclusions in this case (perhaps using different criteria for testing for the significance of redness in the spectrum/serial correlation), but that's probably the reason...

I hope that clarifies this. Please keep me in the loop on this. I've copied to Scott, who may have some additional insights here, since we've been dealing w/ these issues now in the RegEM estimates (Scott:did we ever reject the white noise null hypothesis in the residuals for any of our proxy-based NH reconstructions in the paper submitted to J. Climate? I don't recall).

Thanks,
mike

At 04:33 PM 8/29/2003 +0100, you wrote:

Hi Mike,

after a few bits of holiday here and there, I've now had time to complete my (initial) approach to estimating reconstruction errors on your NH temperature reconstruction. This is all based on the calibration residuals that you kindly sent me a few weeks ago. My rationale for doing this was that I wanted uncertainty/error estimates that were dependent on the time scale being considered (e.g. a decadal mean, an annual mean, a 30-year mean, etc.). I didn't think you had published timescale-dependent errors, hence my attempt.

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Any comments or criticisms will be very useful.

Cheers

Tim

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\Mann uncertainty.doc"

References

1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Edward Cook <drdendro@ldeo.columbia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: An idea to pass by you
Date: Wed, 3 Sep 2003 08:32:11 -0400

<x-flowed>

Hi Keith,

After the meeting in Norway, where I presented the Esper stuff as described in the extended abstract I sent you, and hearing Bradley's follow-up talk on how everybody but him has fucked up in reconstructing past NH temperatures over the past 1000 years (this is a bit of an overstatement on my part I must admit, but his air of papal infallibility is really quite nauseating at times), I have come up with an idea that I want you to be involved in. Consider the tentative title:

"Northern Hemisphere Temperatures Over The Past Millennium: Where Are The Greatest Uncertainties?"

Authors: Cook, Briffa, Esper, Osborn, D'Arrigo, Bradley(?), Jones(??), Mann (infinite?) - I am afraid the Mike and Phil are too personally invested in things now (i.e. the 2003 GRL paper that is probably the worst paper Phil has ever been involved in - Bradley hates it as well), but I am willing to offer to include them if they can contribute without just defending their past work - this is the key to having anyone involved. Be honest. Lay it all out on the table and don't start by assuming that ANY reconstruction is better than any other.

Here are my ideas for the paper in a nutshell (please bear with me):

- 1) Describe the past work (Mann, Briffa, Jones, Crowley, Esper, yada, yada, yada) and their data over-laps.
- 2) Use the Briffa&Osborn "Blowing Hot And Cold" annually-resolved recons (plus Crowley?) (boreholes not included) for comparison because they are all scaled identically to the same NH extra-tropics temperatures and the Mann version only includes that part of the NH (we could include Mann's full NH recon as well, but he would probably go ballistic, and also the new Mann&Jones mess?)

- 3) Characterize the similarities between series using unrotated (maybe rotated as well) EOF analysis (correlation for pure similarity, covariance for differences in amplitude as well) and filtering on the reconstructions - unfiltered, 20yr high-pass, 100-20 bandpass, 100 lowpass - to find out where the reconstructions are most similar and different - use 1st-EOF loadings as a guide, the comparisons of the power spectra could also be done I suppose
- 4) Do these EOF analyses on different time periods to see where they differ most, e.g., running 100-year EOF windows on the unfiltered data, running 300-year for 20-yr data (something like that anyway), and plot the 1st-EOF loadings as a function of time
- 5) Discuss where the biggest differences lie between reconstructions (this will almost certainly occur most in the 100 lowpass data), taking into account data overlaps
- 6) Point out implications concerning the next IPCC assessment and EBM forcing experiments that are basically designed to fit the lower frequencies - if the greatest uncertainties are in the >100 year band, then that is where the greatest uncertainties will be in the forcing experiments
- 7) Publish, retire, and don't leave a forwarding address

Without trying to prejudice this work, but also because of what I almost think I know to be the case, the results of this study will show that we can probably say a fair bit about <100 year extra-tropical NH temperature variability (at least as far as we believe the proxy estimates), but honestly know fuck-all about what the >100 year variability was like with any certainty (i.e. we know with certainty that we know fuck-all).

Of course, none of what I have proposed has addressed the issue of seasonality of response. So what I am suggesting is strictly an empirical comparison of published 1000 year NH reconstructions because many of the same tree-ring proxies get used in both seasonal and annual recons anyway. So all I care about is how the recons differ and where they differ most in frequency and time without any direct consideration of their TRUE association with observed temperatures.

I think this is exactly the kind of study that needs to be done

before the next IPCC assessment. But to give it credibility, it has to have a reasonably broad spectrum of authors to avoid looking like a biased attack paper, i.e. like Soon and Balliunas.

If you don't want to do it, just say so and I will drop the whole idea like a hot potato. I honestly don't want to do it without your participation. If you want to be the lead on it, I am fine with that too.

Cheers,

Ed

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=====

</x-flowed>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Keith Briffa <k.briffa@uea.ac.uk>, Edward Cook <drdendro@ldeo.columbia.edu>
Subject: Fwd: Re: Fwd: Soon & Baliunas
Date: Wed Sep 3 15:54:41 2003

Hi Ed,
first all, yes I agree that we need a paper that takes a more objective look at where we are now and how we can take things forward in terms of NH temperature reconstructions (and possibly global, SH, spatial etc.).

As Keith said, we (mainly I so far) have been planning our version of this (hopefully) "objective assessment", and by chance I was sketching out a vague outline of its possible content. We've been keeping this fairly close to our chests for now, so please keep our plans/ideas to yourself for the moment. There is partial overlap between our ideas and yours, so it might be good to do this jointly. Anyway, my current ideas are a number of forum articles, the first comparing existing reconstructions but without going into more depth, and the other three looking at the way forward (i.e. what should we attempt to do to improve them):

Forum piece (1): Comparison of existing reconstructions

This has most overlaps with your ideas, though I hadn't thought of it being so comprehensive. I was thinking more of:

- (a) comparing original series.
- (b) comparing them after our recalibration to common target data, including discussion of why some things don't change much (e.g. relative positioning of reconstructions), though amplitudes can change - and of course the comparison of Mann et al. with and without oceans/tropics.
- (c) maybe a bit on comparison with boreholes, though maybe not.
- (d) uncertainty estimates and how these may decrease with time scale and hence not all reconstructions lie in the Mann et al. uncertainty ranges.

Forum piece (2): Selection of predictand and predictor data

- (a) What to try to reconstruct and why it matters - e.g. will we get the wrong spectral shape if we reconstruct ocean SST from land-based proxies. Plus some on seasonality, though Jones, Osborn and Briffa cover part of that issue (are you aware of that paper, in press with JGR?).
- (b) What proxies should be used - e.g. does throwing in "poor" proxies cause a problem with simple averaging, weighted averaging and multivariate regression approaches. Plus does using precipitation proxies to reconstruct temperature result in the wrong spectral shape?

Forum piece (3): Reconstruction methods

Something here on different methods (simple averaging, multivariate regression type approaches) and different implementation choices (e.g. calibration against trends/filtered data). Not entirely sure about this, but it would not be new work, just would critically appraise the methods used to date and what their theoretical/potential problems/advantages might be.

Forum piece (4): Estimating uncertainty

Again, not entirely sure yet, but this must emphasise the absolute requirement to estimate AND USE uncertainty when comparing reconstructions against observations or simulations etc. Then something about how to do it, contrasting using calibration residuals, verification residuals, parameter uncertainty, with the type of approach that you've taken (bootstrap uncertainty, or measures of the EPS) to look at the common signal, with additional uncertainty of how the common signal differs from the predictand.

So that's it!! Perhaps rather ambitious, so maybe a reduction to certain key points might be required. I was deliberately avoiding any review of tree-ring contributions and low-frequency per se, thinking that you and Keith would be taking the lead on that kind of review.

One final think to mention, is that the emails copied below and the attached file might be of interest to you as an example of something that *might* go in a comparison paper of existing reconstructions. It's shows how the recalibrated average of existing reconstructions differs from the average of existing calibrated reconstructions. You'll see from Mike Mann's initial request below that he was thinking of it as a contribution to the EOS rebuttal of Soon and Baliunas, but I've not heard much from him since. Also Tom Crowley was very interests in this composite of the reconstructions, and I started to converse with him about it but never finished estimating the uncertainty range on the composite series and kind of stopped emailing him. But I guess either of them might reproduce this idea sometime, if it suits them.

A visit to talk face to face about all these things would be good. Keith and I have been talking about how to fit a visit in.

Cheers

Tim

Date: Wed, 12 Mar 2003 16:16:16 +0000

To: "Michael E. Mann" <mann@virginia.edu>, Tom Crowley <tcrowley@duke.edu>, Phil Jones <p.jones@uea.ac.uk>

From: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: Fwd: Soon & Baliunas

Cc: Malcolm Hughes <mhughes@ltrr.arizona.edu>, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, srutherford@gso.uri.edu, k.briffa@uea.ac.uk, mann@virginia.edu

This is an excellent idea, Mike, IN PRINCIPLE at least. In practise, however, it raises some interesting results (as I have found when attempting this myself) that may be difficult to avoid getting bogged down with discussing.

The attached .pdf figure shows an example of what I have produced (NB. please don't circulate this further, as it is from work that is currently being finished off - however, I'm happy to use it here to illustrate my point).

I took 7 reconstructions and re-calibrated them over a common period and against an observed target series (in this case, land-only, Apr-Sep, >20N - BUT I GET SIMILAR RESULTS WITH OTHER CHOICES, and this re-calibration stage is not critical). You will have seen figures similar to this in stuff Keith and I have published. See the coloured

lines in the attached figure.

In this example I then simply took an unweighted average of the calibrated series, but the weighted average obtained via an EOF approach can give similar results. The average is shown by the thin black line (I've ignored the potential problems of series covering different periods). This was all done with raw, unsmoothed data, even though 30-yr smoothed curves are plotted in the figure.

The thick black line is what I get when I re-calibrate the average record against my target observed series. **THIS IS THE IMPORTANT BIT.** The **re-calibrated** mean of the reconstructions is nowhere near the mean of the reconstructions. It has enhanced variability, because averaging the reconstructions results in a redder time series (there is less common variance between the reconstructions at the higher frequencies compared with the lower frequencies, so the former averages out to leave a smoother curve) and the re-calibration is then more of a case of fitting a trend (over my calibration period 1881-1960) to the observed trend. This results in enhanced variability, but also enhanced uncertainty (not shown here) due to fewer effective degrees of freedom during calibration.

Obviously there are questions about observed target series, which series to include/exclude etc., but the same issue will arise regardless: the analysis will not likely lie near to the middle of the cloud of published series and explaining the reasons behind this etc. will obscure the message of a short EOS piece.

It is, of course, interesting - not least for the comparison with borehole-based estimates - but that is for a separate paper, I think.

My suggestion would be to stick with one of these options:

- (i) a single example reconstruction;
- (ii) a plot of a cloud of reconstructions;
- (iii) a plot of the "envelope" containing the cloud of reconstructions (perhaps also the envelope would encompass their uncertainty estimates), but without showing the individual reconstruction best guesses.

How many votes for each?

Cheers

Tim

At 15:32 12/03/03, Michael E. Mann wrote:

p.s. The idea of both a representative time-slice spatial plot emphasizing the spatial variability of e.g. the MWP or LIA, and an EOF analysis of all the records is a great idea. I'd like to suggest a small modification of the latter:

I would suggest we show 2 curves, representing the 1st PC of two different groups, one of empirical reconstructions, the other of model simulations, rather than just one in the time plot.

Group #1 could include:

- 1) Crowley & Lowery
- 2) Mann et al 1999
- 3) Bradley and Jones 1995

4) Jones et al, 1998

5) Briffa et al 200X? [Keith/Tim to provide their preferred MXD reconstruction]

6) Esper et al [yes, no?--one series that differs from the others won't make much of a difference]

I would suggest we scale the resulting PC to the CRU 1856-1960 annual Northern Hemisphere mean instrumental record, which should overlap w/ all of the series, and which pre-dates the MXD decline issue...

Group #2 would include various model simulations using different forcings, and with slightly different sensitivities. This could include 6 or so simulation results:

1) 3 series from Crowley (2000) [based on different solar/volcanic reconstructions],

2) 2 series from Gerber et al (Bern modeling group result) [based on different assumed sensitivities]

1) Bauer et al series (Claussen group EMIC result) [includes 19th/20th century land use changes as a forcing].

I would suggest that the model's 20th century mean is aligned with the 20th century instrumental N.Hem mean for comparison (since this is when we know the forcings best).

I'd like to nominate Scott R. as the collector of the time series and the performer of the EOF analyses, scaling, and plotting, since Scott already has many of the series and many of the appropriate analysis and plotting tools set up to do this.

We could each send our preferred versions of our respective time series to Scott as an ascii attachment, etc.

thoughts, comments?

thanks,

mike

At 10:08 AM 3/12/2003 -0500, Michael E. Mann wrote:

Thanks Tom,

Either would be good, but Eos is an especially good idea. Both Ellen M-T and Keith Alverson are on the editorial board there, so I think there would be some receptiveness to such a submission.

I see this as complementary to other pieces that we have written or are currently writing (e.g. a review that Ray, Malcolm, and Henry Diaz are doing for Science on the MWP) and this should proceed entirely independently of that.

If there is group interest in taking this tack, I'd be happy to contact Ellen/Keith about the potential interest in Eos, or I'd be happy to let Tom or Phil to take the lead too...

Comments?

mike

At 09:15 AM 3/12/2003 -0500, Tom Crowley wrote:

Phil et al,

I suggest either BAMS or Eos - the latter would probably be better because it is shorter, quicker, has a wide distribution, and all the points that need to be made have been made before.

rather than dwelling on Soon and Baliunas I think the message should be pointedly made against all of the standard claptrap being dredged up.

I suggest two figures- one on time series and another showing the spatial array of temperatures at one point in the Middle Ages. I produced a few of those for the Ambio paper but already have one ready for the Greenland settlement period 965-995 showing the regional nature of the warmth in that figure. we could add a few new sites to it, but if people think otherwise we could of course go in some other direction.

rather than getting into the delicate question of which paleo reconstruction to use I suggest that we show a time series that is an eof of the different reconstructions - one that emphasizes the commonality of the message.

Tom

Dear All,

I agree with all the points being made and the multi-authored article would be a good idea, but how do we go about not letting it get buried somewhere. Can we not address the misconceptions by finally coming up with definitive dates for the LIA and MWP and redefining what we think the terms really mean? With all of us and more on the paper, it should carry a lot of weight. In a way we will be setting the agenda for what should be being done over the next few years.

We do want a reputable journal but is The Holocene the right vehicle. It is probably the best of its class of journals out there. Mike and I were asked to write an article for the EGS journal of Surveys of Geophysics. You've not heard of this - few have, so we declined. However, it got me thinking that we could try for Reviews of Geophysics. Need to contact the editorial board to see if this might be possible. Just a thought, but it certainly has a high profile.

What we want to write is NOT the scholarly review a la Jean Grove (bless her soul) that

just reviews but doesn't come to anything firm. We want a critical review that enables agendas to be set. Ray's recent multi-authored piece goes a lot of the way so we need to build on this.

Cheers

Phil

At 12:55 11/03/03 -0500, Michael E. Mann wrote:

Hi Malcolm,

Thanks for the feedback--I largely concur. I do, though, think there is a particular problem with "Climate Research". This is where my colleague Pat Michaels now publishes exclusively, and his two closest colleagues are on the editorial board and review editor board. So I promise you, we'll see more of this there, and I personally think there **is** a bigger problem with the "messenger" in this case...

But the Soon and Baliunas paper is its own, separate issue too. I too like Tom's latter idea, of a more hefty multi-authored piece in an appropriate journal (Paleoceanography? Holocene?) that seeks to correct a number of misconceptions out there, perhaps using Baliunas and Soon as a case study ('poster child?'), but taking on a slightly greater territory too.

Question is, who would take the lead role. I **know** we're all very busy,
mike

At 10:28 AM 3/11/03 -0700, Malcolm Hughes wrote:

I'm with Tom on this. In a way it comes back to a rant of mine to which some of you have already been victim. The general point is that there are two arms of climatology:

neoclimatology - what you do based on instrumental records and direct, systematic observations in networks - all set in a very Late Holocene/Anthropocene time with hourly to decadal interests.

paleoclimatology - stuff from rocks, etc., where major changes in the Earth system, including its climate, associated with major changes in boundary conditions, may be detected by examination of one or a handful of paleo records.

Between these two is what we do - "mesoclimatology" - dealing with many of the same phenomena as neoclimatology, using documentary and natural archives to look at phenomena on interannual to millennial time scales. Given relatively small changes in boundary conditions (until the last couple of centuries), mesoclimatology has to work in a way that is very similar to neoclimatology. Most notably, it depends on heavily replicated networks of precisely dated records capable of being either calibrated, or whose relationship to climate may be modeled accurately and precisely.

Because this distinction is not recognized by many (e.g. Sonnechkin, Broecker, Karlen) we see an accumulation of misguided attempts at describing the climate of recent millennia. It would be better to head this off in general, rather than draw attention to a bad paper. After all, as Tom rightly says, we could all nominate really bad papers that have been published in journals of outstanding reputation (although there could well be differences between our lists).

End of rant, Cheers, Malcolm

> Hi guys,

>

> junk gets published in lots of places. I think that what could be
> done is a short reply to the authors in Climate Research OR a SLIGHTLY
> longer note in a reputable journal entitled something like "Continuing
> Misconceptions About interpretation of past climate change." I kind
> of like the more pointed character of the latter and submitting it as
> a short note with a group authorship carries a heft that a reply to a
> paper, in no matter what journal, does not.

>

> Tom

>

>

>

>> Dear All,

>> Apologies for sending this again. I was expecting a stack of
>> emails this morning in

>> response, but I inadvertently left Mike off (mistake in pasting)

>> and picked up Tom's old

>> address. Tom is busy though with another offspring !

>> I looked briefly at the paper last night and it is appalling -

>> worst word I can think of today

>> without the mood pepper appearing on the email ! I'll have time to

>> read more at the weekend

>> as I'm coming to the US for the DoE CCPP meeting at Charleston.

>> Added Ed, Peck and Keith A.

>> onto this list as well. I would like to have time to rise to the

>> bait, but I have so much else on at

>> the moment. As a few of us will be at the EGS/AGU meet in Nice, we

>> should consider what

>> to do there.

>> The phrasing of the questions at the start of the paper

>> determine the answer they get. They

>> have no idea what multiproxy averaging does. By their logic, I

> > could argue 1998 wasn't the
> > warmest year globally, because it wasn't the warmest everywhere.
> > With their LIA being 1300-
> > 1900 and their MWP 800-1300, there appears (at my quick first
> > reading) no discussion of
> > synchronicity of the cool/warm periods. Even with the instrumental
> > record, the early and late
> > 20th century warming periods are only significant locally at
> > between 10-20% of grid boxes.
> > Writing this I am becoming more convinced we should do
> > something - even if this is just
> > to state once and for all what we mean by the LIA and MWP. I think
> > the skeptics will use
> > this paper to their own ends and it will set paleo back a number of
> >
> > years if it goes
> > unchallenged.
> >
> > I will be emailing the journal to tell them I'm having
> > nothing more to do with it until they
> > rid themselves of this troublesome editor. A CRU person is on the
> > editorial board, but papers
> > get dealt with by the editor assigned by Hans von Storch.
> >
> > Cheers
> > Phil
> >
> > Dear all,
> > Tim Osborn has just come across this. Best to ignore
> > probably, so don't let it spoil your
> > day. I've not looked at it yet. It results from this journal
> > having a number of editors. The
> > responsible one for this is a well-known skeptic in NZ. He has let
> >
> > a few papers through by
> > Michaels and Gray in the past. I've had words with Hans von Storch
> >
> > about this, but got nowhere.
> > Another thing to discuss in Nice !
> >
> > Cheers
> > Phil
> >

>>>X-Sender: f055@pop.uea.ac.uk
>>>X-Mailer: QUALCOMM Windows Eudora Version 5.1
>>>Date: Mon, 10 Mar 2003 14:32:14 +0000
>>>To: p.jones@uea
>>>From: Tim Osborn <t.osborn@uea.ac.uk>
>>>Subject: Soon & Baliunas
>>>
>>>
>>>
>>>Dr Timothy J Osborn | phone: +44 1603 592089
>>>Senior Research Associate | fax: +44 1603 507784
>>>Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
>>>School of Environmental Sciences | web-site: University of East
>>>Anglia _____ | [1]<http://www.cru.uea.ac.uk/~timo/> Norwich NR4
>>>7TJ | sunclock: UK |
>>>[2]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>
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>>University of East Anglia
>>Norwich Email p.jones@uea.ac.uk
>>NR4 7TJ
>>UK
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>>
>>
>>Attachment converted: Macintosh HD:Soon & Baliunas 2003.pdf (PDF
>>/CARO) (00016021)
>>
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>>--
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> Box 90227
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1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
5. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Something for the weekend !
Date: Fri, 05 Sep 2003 13:34:53 -0400
Cc: Keith Briffa <k.briffa@uea.ac.uk>, mann@virginia.edu

sorry phil, one more relevant item. I've cc'd in Keith on this, since you had mentioned that you had discussed the issue w/ him.

This is from Dave Meko's (quite nice!) statistics lecture notes:

[1]http://www.ltrr.arizona.edu/~dmeko/notes_8.pdf

See page 2, section 8.1.

He provides two (in reality, as I mentioned before, there are really 3!) basic boundary constraints on a smooth (ie, in "filtering"). The first method he refers to is what I called the "minimum norm" constraint (assuming the long-term mean beyond the boundary). The second, which he calls "reflecting the data across the endpoints", is the constraint I have been employing which, again, is mathematically equivalent to insuring a point of inflection at the boundary. This is the preferable constraint for non-stationary mean processes, and we are, I assert, on very solid ground (preferable ground in fact) in employing this boundary constraint for series with trends...

mike

At 05:20 PM 9/5/2003 +0100, Phil Jones wrote:

Mike,

Attached some more plots.

1. Figure 7 - Forcing. Guess this is it. Could cut the y scale to -6 and say in caption that

1258 or 1259 is the only event to go beyond this, then give value in caption. Scale will then widen out. OK to do ? Caspar's solar now there.

2. Fig 2a - first go at coverage. This is % coverage over 1856-2002 from HadCRUT2v.

3. Fig 4 again. Moved legends and reduced scale. Talked to Keith and we both think that

the linear trend padding will get criticised. Did you use this in GRL and or Fig 5 for RoG

with Scott. If so we need to explain it.

On this plot all the series are in different units, so normalised over 1751-1950 (or equiv for

decades) then smoothed. Again here I can reduce scale further and Law Dome can go out of the plot. Thoughts ? Think all should be same scale.

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Scott.

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For the SOI I and Tim reckon that it won't work showing this at interannual timescale with

3 plots. It will then not be like the NAO plot.

Thoughts on colours as well.

Have a good weekend. Logging off once this has gone.

Cheers

Phil

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2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Something for the weekend !
Date: Fri, 05 Sep 2003 13:51:08 -0400
Cc: Keith Briffa <k.briffa@uea.ac.uk>

sorry, meant "is just the minimum slope" constraint, in first sentence...
apologies for the multiple emails,
mike

At 01:47 PM 9/5/2003 -0400, Michael E. Mann wrote:

Actually,

I think Dave's suggestion "reflecting the data across the endpoints" is really just the "minimum norm" constraint, which insures zero slope near the boundary. In other words, he's probably only talking about reflecting about the time axis. I assert that a preferable alternative, when there is a trend in the series extending through the boundary is to reflect both about the time axis and the amplitude axis (where the reflection is with respect to the y value of the final data point). This insures a point of inflection to the smooth at the boundary, and is essentially what the method I'm employing does (I simply reflect the trend but not the variability about the trend--they are almost the same)...

mike

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References

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3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Phil Jones <p.jones@uea.ac.uk>

To: t.osborn@uea.ac.uk,k.briffa@uea.ac.uk,simon.tett@metoffice.com, peter.thorne@metoffice.com,chris.folland@metoffice.com, david.parker@metoffice.com

Subject: Fwd: rural/urban paper

Date: Mon, 15 Sep 2003 16:19:49 +0100

<x-flowed>

Dear All,

Link below is to a paper just out in the US. Could be some press coverage - as it says

there is no difference between urban and rural stations for temperature over the US !

Interesting to see if the skeptics pick up on this. They are probably still going through the

Vinnikov/Grody paper in Science showing MSU2 warming more than the surface, so

they have a lot to look at.

I reviewed Peterson's one with Chris and couldn't see anything wrong with the main message.

Cheers

Phil

>Date: Mon, 15 Sep 2003 10:23:46 -0400

>From: "Thomas C Peterson" <Thomas.C.Peterson@noaa.gov>

>Organization: NOAA/NESDIS/NCDC

>X-Mailer: Mozilla 4.79 [en] (Windows NT 5.0; U)

>X-Accept-Language: en

>To: Phil Jones <p.jones@uea.ac.uk>

>Subject: rural/urban paper

>

>Hi, Phil.

>

>I was going to send you a copy of my rural/urban paper, but I didn't get

>a .pdf before it was published. As it is 6 megs, I'll just give you the

>link instead:

>

><http://ams.allenpress.com/pdfserv/i1520-0442-016-18-2941.pdf>

>

>Regards,

>

> Tom

Prof. Phil Jones

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School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich Email p.jones@uea.ac.uk

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UK

</x-flowed>

From: Irina Fast <f14@zedat.fu-berlin.de>
To: Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>
Subject: COLD season T reconstruction
Date: Tue, 30 Sep 2003 14:24:57 +0200
Reply-to: f14@zedat.fu-berlin.de

Hi Tim, hi Keith,

attached you can find my reconstruction of the cold season temperature anomalies. I have retained the 3rd, 4th, 5th and 6th EOFs for the whole time span (1500-1976). It seems to be a rather strange choice, but if I retain the 1st and/or 2nd EOFs the reconstructed T anomalies for Northern Europe are too large in comparison to observed anomalies. You will see that calibration/verification skills are miserable. But it puts my mind to rest, if you say, that this is an expected result.

Last week you wrote :

>Please let us (me and Keith) know if you are happy with your implementation
>of the Mann et al. method. I remember that you had some strange results
>when you applied it to the model simulations - did you solve those
>problems? We might be able to help or provide advice if you still have
>problems with the method.

The problems I mentioned at the meeting in France arose if I applied my implementation of the method to the INSTRUMENTAL data and I tried to explain this effect through the gaps in the data. In the meantime I was able to eliminate to some degree this problem through the use of other fortran compiler and numeric library. I will prepare an slide with assesment of the performance of the current method implementation for "perfect proxy data" (i.e. instrumental data as proxy data).

And now some words to agenda

- 1) Antje Weisheimer will say initial greeting words and make all organisational announcements.
- 2) As you know, Ulrich take part in the analysis of the simulations performed with ECHO-G by GKSS group. I am not sure, but maybe he will also present his ideas for further (in framework of SO&P reasonable) simulations, that can be conducted by FUB.

For the presentations both OHP and data projector are available.

Best redards

Irina

--

Irina Fast
Freie Universität Berlin
Institut für Meteorologie
Carl-Heinrich-Becker-Weg 6-10
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Germany

phone: +49 (0)30 838 712 21 fax: +49 (0)30 838 711 60

e-mail: f14@zedat.fu-berlin.de

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Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\rectemp_regave_October-March1.dat"

From: "Michael E. Mann" <mann@virginia.edu>

To: "Robert Matthews" <r.matthews@physics.org>

Subject: Re:

Date: Thu, 02 Oct 2003 16:11:02 -0400

Cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, ckkolland@meto.gov.uk, peter.stott@metoffice.com, d.viner@uea.ac.uk, m.hulme@uea.ac.uk

Dear Mr. Matthews,

Unfortunately Phil Jones is travelling and will probably be unable to offer a separate reply. Since your comments involve work that is his as well, I have therefore taken the liberty of copying your inquiry and this reply to several of his British colleagues.

The comparisons made in our paper are well explained therein, and your statements belie the clearly-stated qualifications in our conclusions with regard to separate analyses of the Northern Hemisphere, Southern Hemisphere, and globe.

An objective reading of our manuscript would readily reveal that the comments you refer to are scurrilous. These comments have not been made by scientists in the peer-reviewed literature, but rather, on a website that, according to published accounts, is run by individuals sponsored by ExxonMobile corporation, hardly an objective source of information.

Owing to pressures on my time, I will not be able to respond to any further inquiries from you. Given your extremely poor past record of reporting on climate change issues, however, I will leave you with some final words. Professional journalists I am used to dealing with do not rely upon un-peer-reviewed claims off internet sites for their sources of information. They rely instead on peer-reviewed scientific research, and mainstream, rather than fringe, scientific opinion.

Sincerely,

Michael E. Mann

At 08:30 PM 10/2/2003 +0100, Robert Matthews wrote:

Dear Professor Mann

I'm putting together a piece on global warming, and I'll be making reference to your paper in Geophysical Research Letters with Prof Jones on "Global surface temperatures over the past two millennia".

When the paper came out, some critics argued that the paper actually showed that there have been three periods in the last 2000 years which were warmer than today (one just prior to AD 700, one just after, and one just prior to AD 1000). They also claimed that the paper could only conclude that current temperatures were warmer if one compared the proxy data with other data sets. (For an example of these arguments, see:

[1]<http://www.co2science.org/journal/2003/v6n34c4.htm>)

I'd be very interested to include your rebuttals to these arguments in the piece I'm doing. I must admit to being confused by why proxy data should be compared to instrumental data for the last part of the data-set. Shouldn't the comparison be a consistent one throughout ?

With many thanks for your patience with this

Robert Matthews

Robert Matthews

Science Correspondent, The Sunday Telegraph

C/o: 47 Victoria Road, Oxford, OX2 7QF

Email: [2]r.matthews@physics.org

Homepage: [3]www.ncrg.aston.ac.uk/People/

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References

1. <http://www.co2science.org/journal/2003/v6n34c4.htm>
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From: "Michael E. Mann" <mann@virginia.edu>

To: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, ckfolland@meto.gov.uk, peter.stott@metoffice.com, d.viner@uea.ac.uk, m.hulme@uea.ac.uk

Subject: Re:

Date: Thu, 02 Oct 2003 17:03:15 -0400

For those of you who haven't seen it, this is Robert Matthews last article on the topic.

Hence the fairly brusque tone taken...

mike

Middle Ages were warmer than today, say scientists

By Robert Matthews, Science Correspondent

(Filed: 06/04/2003)

Claims that man-made pollution is causing "unprecedented"

global warming

have been seriously undermined by new research which shows that the

Earth

was warmer during the Middle Ages.

From the outset of the global warming debate in the late 1980s,

environmentalists have said that temperatures are rising higher and

faster

than ever before, leading some scientists to conclude that greenhouse

gases

from cars and power stations are causing these

"record-breaking" global

temperatures.

Last year, scientists working for the UK Climate Impacts Programme said

that

global temperatures were "the hottest since records began"

and added: "We

are pretty sure that climate change due to human activity is here and

it's

accelerating."

This announcement followed research published in 1998, when scientists

at

the Climatic Research Unit at the University of East Anglia declared

that

the 1990s had been hotter than any other period for 1,000 years.

Such claims have now been sharply contradicted by the most

comprehensive

study yet of global temperature over the past 1,000 years. A review of

more

than 240 scientific studies has shown that today's temperatures

are neither

the warmest over the past millennium, nor are they producing the most

extreme weather - in stark contrast to the claims of the

environmentalists.

The review, carried out by a team from Harvard University, examined the findings of studies of so-called "temperature proxies" such as tree rings, ice cores and historical accounts which allow scientists to estimate temperatures prevailing at sites around the world.

The findings prove that the world experienced a Medieval Warm Period between the ninth and 14th centuries with global temperatures significantly higher even than today.

They also confirm claims that a Little Ice Age set in around 1300, during which the world cooled dramatically. Since 1900, the world has begun to warm up again - but has still to reach the balmy temperatures of the Middle Ages.

The timing of the end of the Little Ice Age is especially significant, as it implies that the records used by climate scientists date from a time when the Earth was relatively cold, thereby exaggerating the significance of today's temperature rise.

According to the researchers, the evidence confirms suspicions that today's "unprecedented" temperatures are simply the result of examining temperature change over too short a period of time.

The study, about to be published in the journal Energy and Environment, has been welcomed by sceptics of global warming, who say it puts the claims of environmentalists in proper context. Until now, suggestions that the Middle Ages were as warm as the 21st century had been largely anecdotal and were often challenged by believers in man-made global warming.

Dr Philip Stott, the professor emeritus of bio-geography at the University of London, told The Telegraph: "What has been forgotten in all the discussion about global warming is a proper sense of history."

According to Prof Stott, the evidence also undermines doom-laden predictions about the effect of higher global temperatures. "During the Medieval

Warm

Period, the world was warmer even than today, and history shows that it was a wonderful period of plenty for everyone."

In contrast, said Prof Stott, severe famines and economic collapse followed the onset of the Little Ice Age around 1300. He said: "When the temperature started to drop, harvests failed and England's vine industry died. It makes one wonder why there is so much fear of warmth."

The United Nation's Intergovernmental Panel on Climate Change (IPCC), the official voice of global warming research, has conceded the possibility that today's "record-breaking" temperatures may be at least partly caused by the Earth recovering from a relatively cold period in recent history. While the evidence for entirely natural changes in the Earth's temperature continues to grow, its causes still remain mysterious.

Dr Simon Brown, the climate extremes research manager at the Meteorological Office at Bracknell, said that the present consensus among scientists on the IPCC was that the Medieval Warm Period could not be used to judge the significance of existing warming.

Dr Brown said: "The conclusion that 20th century warming is not unusual relies on the assertion that the Medieval Warm Period was a global phenomenon. This is not the conclusion of IPCC."

He added that there were also doubts about the reliability of temperature proxies such as tree rings: "They are not able to capture the recent warming of the last 50 years," he said.

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Commercial information. [Privacy Policy](#).

At 04:11 PM 10/2/2003 -0400, Michael E. Mann wrote:

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Email: [2]r.matthews@physics.org
Homepage: [3]www.ncrg.aston.ac.uk/People/
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References

1. <http://www.co2science.org/journal/2003/v6n34c4.htm>
2. <mailto:r.matthews@physics.org>
3. <http://www.ncrg.aston.ac.uk/People/>
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
5. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Tim Osborn <t.osborn@uea.ac.uk>

To: "Michael E. Mann" <mann@virginia.edu>, "Robert Matthews" <r.matthews@physics.org>

Subject: Re: Mann and Jones, climate of the last two millennia

Date: Fri Oct 3 09:56:06 2003

Cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, ckfolland@meto.gov.uk, peter.stott@metoffice.com, d.viner@uea.ac.uk, m.hulme@uea.ac.uk

Dear Mr. Matthews,

I have not read the criticism on the website you refer to, but will add to Mike Mann's response in a small, but hopefully helpful, way.

Comparison of the Mann and Jones proxy-based reconstruction with instrumental temperature data *is* a valid comparison to make, provided that the reconstruction is *calibrated* to represent the instrumental record and provided that the *uncertainties* in the calibration are taken into account when making the comparison.

That is, after all, the purpose of calibration - to allow two different data sets to be compared!

As is clear from their article, Mann and Jones do undertake a careful calibration and only make comparisons after the calibration, and their comparison figure includes their estimated uncertainty range. Thus the conclusions they draw (regarding whether recent warming is unprecedented) are valid and are supported by their analysis.

This does not mean that future work, perhaps using new proxy records or different methods for calibration or for estimating calibration uncertainties, will not change those conclusions. But it remains true that their conclusions are supported by their analysis.

As an example of a poor comparison, see the piece by Fred Pearce on page 5 of 12 July 2003 issue of New Scientist. This is a short news article about the Mann and Jones paper, and it unfortunately shows a comparison figure without the associated calibration uncertainties. That is not a good comparison. I mention this in case you were thinking of including a diagram in your article, perhaps showing the Mann and Jones results. If you do, then it will only be valid for comparing the recent instrumental temperatures with the proxy-based reconstruction of earlier temperatures if the reconstruction uncertainties are included. Try to avoid the mistake that Fred Pearce made.

Regards

Tim

At 21:11 02/10/2003, Michael E. Mann wrote:

Dear Mr. Matthews,

Unfortunately Phil Jones is travelling and will probably be unable to offer a separate reply. Since your comments involve work that is his as well, I have therefore taken the liberty of copying your inquiry and this reply to several of his British colleagues.

The comparisons made in our paper are well explained therein, and your statements belie the clearly-stated qualifications in our conclusions with regard to separate analyses of the Northern Hemisphere, Southern Hemisphere, and globe.

An objective reading of our manuscript would readily reveal that the comments you refer

to are scurrilous. These comments have not been made by scientists in the peer-reviewed literature, but rather, on a website that, according to published accounts, is run by individuals sponsored by ExxonMobile corporation, hardly an objective source of information.

Owing to pressures on my time, I will not be able to respond to any further inquiries from you. Given your extremely poor past record of reporting on climate change issues, however, I will leave you with some final words. Professional journalists I am used to dealing with do not rely upon un-peer-reviewed claims off internet sites for their sources of information. They rely instead on peer-reviewed scientific research, and mainstream, rather than fringe, scientific opinion.

Sincerely,

Michael E. Mann

At 08:30 PM 10/2/2003 +0100, Robert Matthews wrote:

Dear Professor Mann

I'm putting together a piece on global warming, and I'll be making reference to your paper in Geophysical Research Letters with Prof Jones on "Global surface temperatures over the past two millennia".

When the paper came out, some critics argued that the paper actually showed that there have been three periods in the last 2000 years which were warmer than today (one just prior to AD 700, one just after, and one just prior to AD 1000). They also claimed that the paper could only conclude that current temperatures were warmer if one compared the proxy data with other data sets. (For an example of these arguments, see:

[1]<http://www.co2science.org/journal/2003/v6n34c4.htm>)

I'd be very interested to include your rebuttals to these arguments in the piece I'm doing. I must admit to being confused by why proxy data should be compared to instrumental data for the last part of the data-set. Shouldn't the comparison be a consistent one throughout ?

With many thanks for your patience with this
Robert Matthews

References

1. <http://www.co2science.org/journal/2003/v6n34c4.htm>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Mann and Jones, climate of the last two millennia
Date: Fri Oct 3 14:43:44 2003

Hi Mike,

I agree completely with your analysis. I don't get so many requests as you, but even so get enough to mean that I ignore most - I just pick a few at random to respond to. As Phil is away, I picked this. He's already come back with a second request, which I answered, but that's all he'll get from me. I'll

At 13:56 03/10/2003, you wrote:

Tim,

Many kind thanks for going out of your way to respond to this. Colleagues have increasingly been warning me against "taking the bait" too often (which this seems another attempt at), and so I resisted giving the detailed response that you have nicely provided (as well as I could have myself, I might add). They tried to bog Ben Santer down with distractions, they've been trying to do the same to me, and its supposed to be a warning to the rest of us. So the trick is to find the middle ground between responding to most egregious and potentially damaging accusations, and not swinging at every ball they throw your way. Its thus very helpful if friends and colleagues can take up a bit of the slack now and then, as you have so graciously done...

This guy has written such trash before on the subject, that I assume he's out to do a hatchet job and there is little that we can do to change that. But your response was very helpful. It will be interesting to see what comes of this, thanks once again,

mike

p.s. I never saw the graph in Fred Pearce's piece, since the online version didn't show it. But it does sound problematic from what you describe.

At 9:56 AM 10/3/2003 +0100, Tim Osborn wrote:

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Michael E. Mann

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<<http://www.co2science.org/journal/2003/v6n34c4.htm>><http://www.co2science.org/journal/2003/v6n34c4.htm>)

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With many thanks for your patience with this

Robert Matthews

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/suncllock.htm>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: Fwd: EOS: Soon et al reply
Date: Wed, 08 Oct 2003 14:15:37 -0400
Cc: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, Keith Briffa <k.briffa@uea.ac.uk>, tcrowley@duke.edu, mhughes@ltrr.arizona.edu, omichael@princeton.edu, t.osborn@uea.ac.uk, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>, mann@virginia.edu, p.jones@uea.ac.uk

Thanks Tom,

In fact, I'm almost done with a brief (<750 word) response that addresses all of these issues, and I'll be looking forward to comments on this. Hope to send it out later today,
mike

At 12:05 PM 10/8/2003 -0600, Tom Wigley wrote:

Folks,
I agree with Kevin that any response should be brief.
On the second page of their comment, SBL quote some of the caveat statements in their earlier papers. The irony is that they do not heed their own caveats. If taken literally, all these proxy data problems would mean that one can draw no conclusions about the existence or otherwise of the MWE or LIA as global phenomena. This is what we say (I hope -- at least I have said this in the paper cited below) - - but our over-bold skeptics say that these anomalous intervals *did* exist. You can't have it both ways -- and basically what BS are doing is a confidence trick. What is still needed here is an analysis of the BS method to show that it could be used to prove anything they wanted. I am still concerned about 'our' dependence on treerings. Are our results really dependent on one region pre 1400 as SNL state? Is the problem of nonclimate obfuscating factors in the 20th century enough to screw up calibrations on moderate to long timescales? If not, we need to state and document this clearly. Does this problem apply to both widths and densities? Are the borehole data largely garbage? I recall a paper of Mike's on this issue that I refereed last year -- and there was something in GRL (I think) very recently pointing out some serious potential problems. Finally, did we really say what SBL claim we did in their p. 1 point (2)? Surely the primary motive for all of this paleo work is that it DOES have a bearing on

human-induced climate effects?

Tom.

+++++

Michael E. Mann wrote:

Thanks Kevin,

I agree w/ your take on this. We need to come up with a short, but powerful rebuttal.

According to Judy Jacobs, we're only allowed 750 words, so we will need to be even more

sparing and precise in our words that in the original Eos piece. By the way, we have 3

weeks to submit (i.e., our response is due October 27).

We need to focus on the key new claims, while simply dismissing, by reference to earlier

writings, the recycled ones. The Kalnay et al paper seems to be the new darling of the

contrarians, and your precise wording on this will be very helpful. Phil, Tim and

others should be able to put to rest, in one or two sentences, the myths about urban

heat bias on the CRU record. A few words from Malcolm and Keith on the biological tree

growth effects would help too. The comments on the various paleo figures are confusing

and inconsistent, but from what I can tell, just plain wrong. I'll draft some words on

that.

I'll just continue to assimilate info and suggestions from everyone over the next week

or so, and then try to put this in the form a rough draft rebuttal to send out.

Thanks for your quick reply. Looking forward to hearing back from others,

mike

At 09:16 AM 10/6/2003 -0600, Kevin Trenberth wrote:

Hi Mike et al

Firstly, you should know that comments by myself and the group at NCDC (Vose et al) on

the Kalnay and Cai Nature paper were accepted (after a rebuttal and review process), and

then fine tuned. But it is a slow process and Kalnay and Cai have yet to finalize their

rebuttal. I am attaching FYI the "final" version of my comment. NCDC deals with the

problems with the records.

My reaction to the reply is as follows:

The first page deals with comments on proxy records and their problems. I think we

should agree that there are issues with proxy records, they are not the same as

instrumental records (which have their own problems), but they are all we have.

However, some are better than others (e.g. borehole) and annual or better resolution is

highly desirable in particular to make sure that anomalies are synchronous. The records

are not really the issue here, it is their use (and abuse).

There are several charges about only US or Northern Europe that can be quickly dealt

with. However the main points are on p 2.

We know from the observational record that global or hemispheric means are typically

small residuals of large anomalies of opposite signs so that large warm spots occur

simultaneously with large cold regions (witness last winter).

This fact means that we need high temporal resolution (annual or better) AND an ability

to compute hemispheric averages based on a network. The Soon and Baliunas approach

fails dismally on both of these critical points.

BS point out that Fig 2 of Mann and Jones show some temperatures as high as those in the

20th C. (They are wrong, do they mean Fig 2 of

M03?) You can counter that by looking at China where this is far from true.

I would be inclined to respond with a fairly short minimalist but powerful rebuttal,

focussing mostly on the shortcomings of BS and not defending the M03 and other records.

It should point out (again) that their methodology is fundamentally flawed and their

conclusions are demonstrably wrong. For this, the shorter the better.

Regards

Kevin

Michael E. Mann wrote:

Dear Colleagues,

Sorry to have to bother you all with this-- I know how busy our schedules are, and this

comes at an unfortunately busy time for many of us I would guess. But I think we *do*

have to respond, and I'm hoping that the response can be, again, something we all sign

our names to.

I've asked Ellen for further guidance on the length limits of our response, and the due

date for our response. The criticisms are remarkably weak, and easy to reply to in my

view. S&B have thus unwittingly, in my view, provided us with a further opportunity to

expose the most egregious of the myths perpetuated by the contrarians (S&B have managed

to cram them all in there) in the format of a response to their comment.

Their comment includes a statement about how the article is all based on Mann et al [1999] which is pretty silly given what is stated in the article, and what is shown in Figure 1. It would be appropriate to begin our response by pointing out this obvious straw man.

Then there is some nonsense about the satellite record and urban heat islands that Phil, Kevin, and Tom W might in particular want to speak to. And Malcolm and Keith might like to speak to the comments on the supposed problems due to non-biological tree growth effects (which even if they were correctly described, which they aren't, have little relevance to several of the reconstructions shown, and all of the model simulation results shown). There is one paragraph about Mann and Jones [2003] which is right from the Idsos' "Co2 science" website, and Phil and I and Tim Osborn and others have already spoken too. I will draft a short comment on that.

I'd like to solicit individual comments, sentences or paragraphs, etc. from each of you on the various points raised, and begin to assimilate this into a "response". I'll let you know as soon as I learn from Ellen how much space we have to work with.

Sorry for the annoyance. I look forward to any contributions you can each provide towards a collective response.

Thanks,
mike

Date: Sun, 05 Oct 2003 08:23:03 -0400
To: Caspar Ammann <ammann@ucar.edu> <[1]mailto:ammann@ucar.edu>, rbradley@geo.umass.edu <[2]mailto:rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk> <[3]mailto:k.briffa@uea.ac.uk>, Tom Crowley, "Malcolm Hughes" <mhughes@ltrr.arizona.edu> <[4]mailto:mhughes@ltrr.arizona.edu>, omichael@princeton.edu <[5]mailto:omichael@princeton.edu>, Tim Osborn <t.osborn@uea.ac.uk> <[6]mailto:t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu> <[7]mailto:jto@u.arizona.edu>, Scott Rutherford <srutherford@rwu.edu> <[8]mailto:srutherford@rwu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu> <[9]mailto:trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu> <[10]mailto:wigley@ucar.edu>
From: "Michael E. Mann" <mann@virginia.edu> <[11]mailto:mann@virginia.edu>
Subject: Fwd: EOS: Soon et al reply

Comments?

Mike

Delivered-To: mem6u@virginia.edu <[12]mailto:mem6u@virginia.edu>
Date: Sat, 04 Oct 2003 12:33:04 -0400
From: Ellen Mosley-Thompson <thompson.4@osu.edu>
<[13]mailto:thompson.4@osu.edu>
Subject: EOS: Soon et al reply
X-Sender: ethompso@pop.service.ohio-state.edu
<[14]mailto:ethompso@pop.service.ohio-state.edu>
To: "Michael E. Mann" <mann@virginia.edu>
<[15]mailto:mann@virginia.edu>
Cc: lzirkel@agu.edu <[16]mailto:lzirkel@agu.edu>, jjacobs@agu.org
<[17]mailto:jjacobs@agu.org>
X-Mailer: QUALCOMM Windows Eudora Version 6.0.0.22

Dear Dr. Mann (and co-authors of the Forum piece that appeared in EOS),

Dr. Willie Soon and his co-authors have submitted a reply to your Forum piece that I

have accepted. Let me outline below the official AGU procedure for replies so that you

know the options available. I have sent these same instructions to Dr. Soon.

As you wrote the original piece you now have the opportunity to see their comment

(attached) on your Forum piece. You may decide whether or not to send a reply. If you

choose not to reply - their reply will be published alone.

Should you decide to reply then your response will be published along with their comment

on your paper. One little twist is that if you submit a reply, they are allowed to see

the reply, but they can't comment on it. They have two options: they can let both

their and your comments go forward and be published together or (after viewing your

reply) they also have the option of withdrawing their comment. In the latter case, then

neither their comment or your reply to the comment will be published. Yes this is a

little contorted, but these are the instructions that I received from Judy Jacobs at

AGU.

I have attached the pdf of their comment. Please let me know within the next week

whether you and your colleagues plan to prepare a reply. If so, then you would have

several weeks to do this.

I have copied Lee Zirkel and Judy Jacobs of AGU as this paper is out of the ordinary and

I want to be sure that I am handling all this correctly.

I look forward to hearing from you regarding your decision on a reply.

Best regards,

Ellen Mosley-Thompson
EOS, Editor
cc: Judy Jacobs and Lee Zirkel
attachment

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4. <mailto:mhughes@ltrr.arizona.edu>
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25. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tom Crowley <tcrowley@duke.edu>
Subject: Re: draft
Date: Thu, 09 Oct 2003 14:16:31 -0400
Cc: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, Keith Briffa <k.briffa@uea.ac.uk>, tcrowley@duke.edu, mhughes@ltrr.arizona.edu, omichael@princeton.edu, t.osborn@uea.ac.uk, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Tom Wigley <wigley@ucar.edu>, mann@virginia.edu

HI Tom,

My understanding of the papers from the borehole community ever since the 1997 GRL article by Huang et al is that they no longer believe that the data has proper sensitivity to variations prior to about AD 1500--in fact, I don't believe anyone in that community now feels they can meaningfully go farther back than that. Huang contributed the section on boreholes in chapter 2 for IPCC (2001), and wrote the very words to that effect... Now, the possible influences on boreholes might lead to inferred trends in GST that are different from those in SAT is a different one. A number of independent recently published papers by (Beltrami et al; Stiglitz et al; Mann and Schmidt) and others have demonstrated that there should be expectations for significant differences between past SAT (what we care about) and GST variations (what boreholes in the best case scenario see) due to snowcover influences, etc. We don't have time to discuss that in this very short piece, so I tried, as briefly as possible, to cover our bases on this issue, in a way that doesn't really stir up the pot w/ the borehole folks... I'm interested in any further thoughts on the above, mike
At 12:38 PM 10/9/03 -0400, Tom Crowley wrote:

Hi, I don't understand why we cannot cite the borehole data for the MWP - that in a sense is the only legitimate data set that shows a ~1 C cooling from the MWP to the LIA - forget the deforestation problem for the moment, that is later in time - if the borehole data for the MWP are legitimate then there is still a case for concluding that the MWP was significantly warmer than the LIA
tom

Thanks Phil,
a few brief responses and inquiries below...
cheers,

mike

At 04:17 PM 10/9/03 +0100, Phil Jones wrote:

Mike,

Away Oct 11-16, so here are a few comments. A few times the tone could be a little less

antagonistic. We don't want to inflame things any further. So remove the word laundry.

fair enough. You *should* have seen the first draft I wrote. This is quite toned down now...

1. With the boreholes do we want to get one of the borehole group to sign up, eg Henry Pollack?

Would add a lot of weight to the last 500 year argument.

this has merit. unfortunately though I think it might open up a hornets nest of the author list is not identical to the original list of authors on the Eos article. Other thoughts on this...

2. On the UHI, there was a paper in a very recent issue of J. Climate by Tom Peterson, arguing

for the USA that this is non-existent. Issue with UHI is one of large versus local scale. One

station doesn't influence large-scale averages. All studies which look at the UHI

comprehensively find very little effect (an order of magnitude smaller than the warming). Also the warming

in the 20th century is very similar between the NH and SH and between the land and

ocean components.

let me see if I can fit one or two sentences in on this and keep the article under the length.

Also, if we can't estimate temperature histories accurately, then SB can't say it

was

warmer in their MWP period. They believe the 20th century instrumental data when they

want to.

yes, one of a large number of amazing contradictions in their reasoning...

3. Keith is away till next week. I doubt we will have the space to do the 'tree issues' justice.

Best just to say that there are an (equal) number of non tree-based proxy series??

I do think we need to address their spurious description of the putative biological effects. Any way that you can get in touch w/ Keith for a response, perhaps just to this one point? Also, Malcolm might want to comment on the current wording?

4. Ray, Malcolm and Henry Diaz have a Science Perspectives piece coming out in the next couple of weeks on the MWP/E. This is also relevant.

good!

5. Don't think we will get away with the last paragraph. Whether we want it is an issue ??

Shouldn't we be sticking to the science.

ok, I wasn't sure myself--yet it is a powerful rebuke, and reminds people that the objection to the validity of their work goes beyond just our article--and that's important. Does someone want to try to rephrase this paragraph, maybe reducing it to a couple sentences?

Cheers
Phil

At 21:37 08/10/2003 -0400, Michael E. Mann wrote:

Dear co-authors,
Attached is a draft response, incorporating suggestions Kevin, Tom W, and Michael. I've aimed to be as brief as possible, but hard to go much lower than 750 words and still address all the key issues. 750 words, by the way, is our allotted limit.

Looking forward to any comments. Feel free to send an edited version if you prefer, and

I'll try to assimilate all of the suggested edits and suggestions into a single revised draft. If you can get comments to me within the next couple days, that would be very

helpful as we're working on a late October deadline for the final version.

Thanks for your continued help,
mike

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

--

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[3][http://www.evsc.virginia.edu/faculty/people/mann.\[4\].shtml](http://www.evsc.virginia.edu/faculty/people/mann.[4].shtml)

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Edward Cook <drdendro@ldeo.columbia.edu>
To: Jan Esper <esper@wsl.ch>
Subject: Re: data again
Date: Fri, 10 Oct 2003 07:28:43 -0400
Cc: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Jan,

Did you finally get the raw ring-width data from Malcolm? Does Keith know about this? He asked Malcolm for the data as well, but did not receive a reply as far as I know.

Ed

>Dear Malcom

>

>thank you for the series of mails and attachments! I just came back
>into office (and I am already close to leave for another fieldtrip
>next week), and had no time yet to look in all the files you sent
>me. As soon as I get an overview of what you sent, I will keep you
>informed.

>

>About the Central Asian data, I am just putting another draft
>together also describing some of the new data Kerstin Treydte (who
>is now in our team) sampled. Kerstin herself started working on a
>bigger analysis including her new ring width and stable isotope data
>(she processed 1000-yr. records of carbon and oxygen stable
>isotopes). This will be the major paper of her PhD, and once this
>paper is accepted, we are intending to release data to the ITRDB.
>Will keep you posted.

>

>Thank you again and take care

>Jan

>

>

>

>

>

>>Dear Jan - did you get the e-mail I sent on September 22? It may have caused
>>problems, because there were 10 attachemnts. In fact, I include
>>some that were

>>missed with this message. In addition, you should be able to get
>>the *.rwl files
>>for the 27 western chronologies used in Mann, Bradley, Hughes 1998 at the
>>following web location:
>>http://www.ltrr.arizona.edu/~fenbiao/For_Jan_27rwl/
>>Please let me know if you experience any problems with this.
>>I also omitted some of the attachments from the earlier message. They should
>>be attached to this one. Good luck! Malcolm

>>
>>----- Forwarded message follows -----

>>From: Malcolm Hughes <mhughes@ltrr.arizona.edu>
>>To: esper@wsl.ch
>>Subject: data
>>Copies to: fenbiao@ltrr.arizona.edu
>>Date sent: Mon, 22 Sep 2003 17:30:24 -0700

>>
>>Dear Jan - I have recently started to clear up all outstanding
>>business related to the next analysis by Mike Mann, Ray Bradley, et
>>al., and found, to my horror, that I had not replied to your e-mail of
>>last April 8 (copy at end of this message). In response to our
>>request for access to the data on which your 2000 and 2002 papers were
>>based, you indicated that you would need to check with a colleague at
>>WSL. Have you been able to do this, and if so, what is the result?
>>Obviously we are keen to include all important data already in the
>>peer reviewed literature, such as yours, in our analyses. You also
>>requested "the raw measurements of (y)our sequoia data and the western
>>conifer data used in the Mann et al 1998, 1999 papers". 1) data used
>>in Mann et al 1998 - these are all listed in the Nature on-line
>>supplementary materials (attached), and were all from the ITRDB, so
>>they may be downloaded from there. The same list is also attached. We
>>think we can find the raw data (the *.rwl files) and send them to you
>>if you would like - please let me know. 2) The western conifer data
>>used in MBH 99 are a subset of these, as indicated in another set of
>>attached MS-Excel files. These are a little bit repetitive, but
>>contain the following particularly useful information for these 27
>>longer chronologies: vchron11000 contains, inter alia, the ITRDB ID,
>>species code, first year, last year, collector's name

>>
>>vchron41000 contains the ITRDB ID, then the first and last
>>years with 5, 10, etc samples

>>
>>vchron81000 contains the ID, etc and then in the following
>>cols: V mn sensitivity W chronology autocorrelation, AE

>>number of series, AG mean correlation of series with
>>chronology AH mean series autocorrelation, AI series mean
>>length, series median segment length.
>>Please remember that this set ranges from lower forest
>>border to upper forest border, so that various mixtures from
>>all precip to precip plus temp locally apply.
>>
>>As I recently told Keith Briffa, you should be aware that it
>>would be completely unjustified to assume that the first
>>measured ring was anywhere near the pith in many of these
>>sites, especially as you go back in time, where the
>>chronologies are based on remnants that have weathered on
>>the inside and the outside. For this, and related, reasons, it
>>would also be completely unjustified to assume any
>>constant, or small, distance in years of the first measured
>>rings from pith. That is, I can see no way of making a
>>remotely reliable estimate of cambial age in the vast
>>majority of these samples. I am sitting on the
>>bones of a manuscript in which I had someone spend
>>several months checking many hundreds of bristlecone and
>>similar cross-sections and cores in our store. They found
>>only a few dozen - less than 10%, where either pith was
>>present, or the innermost ring could reasonably be described
>>as 'near pith'. If you have seen these stripbark montane 5-
>>needle pines, and ever tried to core them, you will
>>understand why. A further problem arises from the
>>observation that radial increment may increase rather
>>dramatically in the period after most of the bark dies back,
>>but of course we don't know when that was. Andy Bunn at
>>Montana State University has, I think, a manuscript in
>>preparation of review on this. I have a manuscript in
>>preparation where we restandardized many of these series
>>in the following way -
>>identify the long, flat part of the sample ringwidth curve
>>(i.e. remove the 'grand period of growth', if present) and
>>then fit a straight line of no or negative slope.
>>3) I attach *rwl and chronology files from three sequoia sites (those
>>referred to by Hughes and Brown, 1992 Drought frequency in central
>>California since 101 B.C. recorded in giant sequoia tree rings.
>>Climate Dynamics, 6, 161-167) Please note the reasons given for the
>>rather strong standardization used (explained in text) and for the
>>splitting of the Mountain Home samples at AD 1297 (this explains my
>>sending you 4 of each kind of file, even though there were only three

>>sites in this case). We do not have pith dates for these samples, but
>>it is important to note the following caution - most of the radials
>>and cross- sections were from stumps, where we found that very slow
>>growth near the pith was often an indicator of great age. This of
>>course tells us that trees destined to be very old were often
>>suppressed for many years in their early life (but not all of them).
>>The tricky part comes from the observation that, although we could see
>>slow growth on the top of the stump near the pith, the wood was often
>>in too poor a state of preservation there to date and measure.
>>Therefore, do not assume that the first ring measured was anywhere
>>near pith - it could easily be off by centuries. There is a *.crn and
>>*.rwl for each of the four chronologies. Gfo is Giant Forest, CSX is
>>Camp Six, and MH is Mountain Home, split into MH1 and MH 2 as
>>indicated above. I'd be interested to know how you get on with this.
>>Cheers, Malcolm . .

>> ----- Forwarded message from Jan Esper <esper@wsl.ch> -----

>>> Date: Tue, 8 Apr 2003 16:15:35 +0200

>>> From: Jan Esper <esper@wsl.ch>

>>> Reply-To: Jan Esper <esper@wsl.ch>

>>> Subject: Re: from Malcolm Hughes

>>> To: fenbiao@ltr.arizona.edu

>>>

>>> Dear Fenbiao and Malcom

>>>

>>> Since I got funding from the Swiss Science Foundation to do some
>>> similar research, I really like the idea to share our tree ring
>>> data. However, I have to discuss this again with Kerstin Treydte who
>>> now started to work at the WSL and is running a re-analysis
>>> (including new samplings) for western central Asia.

>>>

>>> In principle, would it be possible to receive the raw measurements
>>> of your Sequoia data and the western conifer data used in the Mann
>>> et al. 1998, 1999 papers?

>>>

>>> What do you think?

>>>

>>> Take care

>>> Jan

>>>

>>> CC

>>> K Treydte

>>> D Frank

>>>

>>> >Dear Jan,
>>> >You may be familiar with our earlier attempts at very large scale
>>> multi-proxy
>>> >reconstruction of certain aspects of climate, (for example, Mann,
>>> >Bradley
>>> and
>>> >Hughes, 1998, Nature, 392, 779-787). This work was possible because
>>> >many colleagues made their data available. We are now assembling an
>>> >updated and extended dataset for new work along similar lines. We
>>> >hope to take advantage of data that were not available five years
>>> >ago, and to use improved methods in our analyses.
>>> >
>>> >Would you be willing to permit us to use the
>>> >(chronologies/reconstruction?) reported in your paper (s) listed
>> >>below?
>>> >
>>> >Esper J. (2000). Long-term tree-ring variations in Juniperus at the
>>> >upper timber-line in karakorum (Pakistan). Holocene 10 (2),
>>> >253-260.
>>> >
>>> >Esper J., Schweingruber F.H., Winiger M. (2002). 1300 years of
>>> >climatic history for western central Asia inferred from tree-rings.
>>> >Holocene 12 (3),
>>> 267-277.
>>> >
>>> >We are particularly interested in (1) the ring-width series of
>>> >Juniperus excelsa M. Bieb and Juniperus turkestanica Kom. From 6
>>> >different sites in
>>> the
>>> >Hunza-karakorum;
>>> >(2) 20 individual sites ranging from the lower to upper local
>>> >timber-lines
>>> in
>>> >the Northwest karakorum of Pakistan and the Southern Tien Shan of
>>> Kirghizia.
>>> >
>>> >If at all possible, we would prefer to receive tree-ring data as
>>> >both raw
>>> data
>>> >(individual unmodified measurement series for all samples used) and
>>> >your
>>> final
>>> >chronologies used in the publication.

>>> >
>>> >If you are willing to share your data for the purposes of our
>>> >analyses, but
>>> do
>>> >not
>>> >wish them to be passed on to anyone else by us, please tell us, and
>>> >we will mark the data accordingly in our database. If data have
>>> >been marked as not being publicly available, we will pass on any
>>> >requests for them to you.
>>> >
>>> >Please reply to Dr. Fenbiao Ni's email address (this one). Many
>>> >thanks.
>>> >
>>> >Sincerely,
>>> >Malcolm K. Hughes
>>> >(team: Michael E. Mann, Ray Bradley, Malcolm Hughes, Scott
>>> >Rutherford,
>>> Fenbiao
>>> >Ni)
>>> >
>>> >Malcolm Hughes
>>> >Professor of Dendrochronology
>>> >Laboratory of Tree-Ring Research
>>> >University of Arizona
>>> >Tucson, AZ 85721
>>> >520-621-6470
>>> >fax 520-621-8229
>>>
>>>
>>> --
>>> Dr. Jan Esper
>>> Swiss Federal Research Institute WSL
>>> Zuercherstrasse 111, 8903 Birmensdorf
>>> Switzerland
>>> Phone: +41-1-739 2510
>>> Fax: +41-1-739 2215
>>> Email: esper@wsl.ch
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>>> ----- End forwarded message -----
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>>----- End forwarded message -----

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>>
>>Attachments:
>> D:\Projects\Bradley and Mann\Newest June 9 1997\westernforjan.xls
>> D:\Projects\Bradley and Mann\Nature figures\naturesupmat.doc
>> D:\Projects\SEQUOIA\for esper\csx.rwl D:\Projects\SEQUOIA\for
>> esper\csxars.crn D:\Projects\SEQUOIA\for esper\gfo.rwl
>> D:\Projects\SEQUOIA\for esper\gfoars.crn D:\Projects\SEQUOIA\for
>> esper\mhf1.rwl D:\Projects\SEQUOIA\for esper\mhf2.rwl
>> D:\Projects\SEQUOIA\for esper\MHF2ARS.CRN D:\Projects\SEQUOIA\for
>> esper\MHF1ARS.CRN

>>----- End of forwarded message -----Malcolm

>>Hughes
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>
>
>--
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--
=====

Dr. Edward R. Cook
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Director, Tree-Ring Laboratory

Lamont-Doherty Earth Observatory
Palisades, New York 10964 USA
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Phone: 845-365-8618
Fax: 845-365-8152

=====

</x-flowed>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: draft
Date: Mon Oct 13 15:23:20 2003
Cc: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu, Keith Briffa <k.briffa@uea.ac.uk>, tcrowley@duke.edu, mhughes@ltrr.arizona.edu, omichael@princeton.edu, jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Tom Wigley <wigley@ucar.edu>, p.jones@uea.ac.uk, Kevin Trenberth <trenbert@cgd.ucar.edu>

At 20:02 09/10/2003, Michael E. Mann wrote:

Dear All,
I like all of Kevin's changes. Please work with his version as a template for any additional suggested changes. I'll incorporate the additional comments received from Phil and Tom W and others afterwards...
thanks,
mike

Dear Mike and co-authors,
I've now had a chance to go through the drafts and comments etc. Working from Kevin's version, here are some suggestions to consider:
(1) Are you sure that what we saw is the final version of S03, after any EOS editing, etc.? Wouldn't want any of the S03 quotes used here to get changed if they had to edit to reduce the length of their piece!
(2) Suggested re-ordering of the end of point (1): 'it holds in some cases for tree-ring density measurements at higher latitudes, but rarely for annual ring widths.'
(3) Suggested re-wording near start of point (2): '"clearly shows temperatures in the MWP that are as high as those in the 20th century" is misleading because it is true for only the early 20th century. The hemispheric warmth of the late 20th century is anomalous in a long-term context.' (with underlining of either 'late' or 'is' for emphasis). Of course, this suggestion needs to be checked carefully (e.g., is it only the 'early' 20th century that is exceeded by some earlier temperatures?). But it is an important change because it is not actually 'false' or 'untrue' if some part of the 20th century was exceeded earlier - they don't specify which part, so their statement is (probably deliberately) vague rather than wrong. The above suggestion simply points this out.
(4) Related to this comment, is the question of whether the actual reconstruction (not

instrumental observations) in the late 20th century exceeds all reconstructed values (central estimates) prior to the 20th century. My copy of Mann and Jones (2003) has poor quality figures, so this is hard for me to tell. It appears that it might be true, but only right at the end - i.e. the 1980 value of the filtered series. If it is really only at the end, and a 40-year smoothing filter is used, then I would be concerned about this statement appearing in the response if it depends upon applying the filter right up to the end of the record. Doing so requires some assumption about values past the end of the series. This in itself is problematic, but especially so if the assumption were that the trend was extrapolated to produce values for input to the filter. Of course, if the straight 40-year mean from 1941-1980 of the reconstruction exceeds all other 40-year means of the reconstruction, then I'd be happy with the statement.

(5) I don't like point (3) on the boreholes. It relies on the "optimal" borehole series of Mann et al. (2003), a result that I have some concerns about and which is being used here to imply less uncertainty than really exists over this issue. In the EOS paper we included this and the "non-optimal" gridded borehole series, so we were leaving open some uncertainty. I'm not saying that I prefer/believe the Huang et al. series either, since I agree that extracting the temperature signal from the borehole data is very difficult. I just don't like to imply it has been solved when it hasn't.

(6) Can we provide a supporting reference for the statement in point (4) about land use changes leading to an overall cooling?

(7) I like the final paragraph as it is, possibly dropping the last "We feel it is time to move on" line.

Cheers
Tim

From: Keith Briffa <k.briffa@uea.ac.uk>
To: t.osborn@uea.ac.uk
Subject: Fwd: minor explosion
Date: Mon Oct 13 15:57:13 2003

X-Sender: esper@mail.wsl.ch
Date: Mon, 13 Oct 2003 15:21:03 +0200
To: Keith Briffa <k.briffa@uea.ac.uk>
From: Jan Esper <esper@wsl.ch>
Subject: minor explosion
Cc: Wilson Rob <rjwilson_dendro@blueyonder.co.uk>

Hi Keith

thank you for the message and the comments to the Siberia draft. We are intending to finalize a draft when Rob is coming over and we go on a sampling trip to the Bavarian Forest and E-Germany. We will then also discuss of data-overlap issue again and might include some extra figure with our record re-calculated (without Tornetraesk and Polar Ural).

However, I (Jan) am not sure that we should have another figure with only the Mann and the (reduced) Esper series. Second, it seems that Mann used the density records from these two sites only (not ring width). Lets see.

We would really like to send you the final draft, and ask you to become the fourth author? We ask this not only because of the "minor explosion" that might happen, but also because some of the arguments in the draft were made earlier by you anyway. What do you think?

Take care

Jan and Dave

CC

R Wilson

Jan

with respect to the overlap problem we could agree to differ for now -I think the problem is much more in the earlier period anyway but I suggest you go ahead and submit it anyway. There are some minor wording points but nothing that affects the meaning. You know that in my opinion the recent similarity in the records is driven by instrumental data inclusion (or calibration against instrumental data) and that Mann's earlier data are strongly biased towards summer and northern land signals. I think you will start a minor explosion - but that is what science needs .

I looked at your tree-line data and thought them very interesting. In my opinion the way you directed the interpretation was what drew your criticisms . For a climate journal you should have been pointing out the complicated regional responses (to the temperature record) rather than trying to state a simple overall response. The data are clearly important and you should have no trouble publishing them if you rethink the approach to

the description (no work needed). I think Boreas or Arctic and Alpine Res. are better targets though. I enjoyed the discussions also and it is frustrating not to be able to get up to speed with your other projects. I will get back to you when I have looked more at the idea of the big review paper.

the very best to you and all

Keith

At 09:55 AM 10/8/03 +0200, Jan Esper wrote:

Hi Keith

with respect to our EOS draft, I am still thinking about the data overlap argument you made.

1. I still believe that the overlap is not that significant, and that the significance is changing dramatically with time (less in more recent centuries).
2. With respect to the aim of the paper, we do NOT intend to explain the similarity between the records. We rather address that the recons differ in the lower frequency domains AND are much more similar in the higher frequency domains. I believe that this is crucial. (One could also say that we only address the dissimilarity, and the arguments related to that.)

I appreciated the discussions we had very, very much (especially the one in the night before the official meeting).

Take care

Jan

CC

D Frank

R Wilson

--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

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[2]<http://www.cru.uea.ac.uk/cru/people/briffa>[3]/

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>, "Michael E. Mann"
<mann@virginia.edu>
Subject: Re: draft
Date: Mon Oct 13 16:36:52 2003
Cc: Caspar Ammann <ammann@ucar.edu>, rbradley@geo.umass.edu,
tcrowley@duke.edu, mhughes@ltrr.arizona.edu, omichael@princeton.edu,
t.osborn@uea.ac.uk, jto@u.arizona.edu, Scott Rutherford
<srutherford@rwu.edu>, Tom Wigley <wigley@ucar.edu>, p.jones@uea.ac.uk

Mike and all

Hi , just back from a trip and only now catching up with important emails. Given

the restricted time and space available to furnish a response to SB comments ,

I offer the following mix of comment and specific wording changes:

I agree that the S+B response is designed to deflect criticism by confusing the issues

rather than answering our points.

In fact they fail to address any of the 3 specific

issues we raised Namely , 1. the need for critical evaluation of proxy inputs , 2. the

need for a consistent assimilation of widespread (dated and well resolved) records,

3. the essential requirement for objective/quantitative calibration (scaling) of the input

records to allow for assessment of the uncertainties when making comparisons of different reconstructions and when comparing early with recent

temperatures.

Their own , ill-conceived and largely subjective approach did not take

account of the uncertainties and problems in the use of palaeodata that they chose to

highlight in their opening remarks.

I would be in favour of stating something to this effect at the outset of our response.

Also , as regards the tree-ring bit , I fully concur with the sense of your text as

regards Section 1, but suggest the following wording (to replace ",rarely for annual

ring widths, and almost entirely at higher latitudes.")

"but in certain high-latitude regions only. Where this is the case , these relatively

recent

(ie post 1950) data are not used in calibrating temperature reconstructions. In many other

(even high-latitude) areas density or ring-width records display no bias."

In the spirit of healthy debate - I agree with Tim's remarks , warning against presenting a

too

sanguine impression that the borehole debate is closed (though I do think it is closing!).

I also believe , as you already know, that the use of a recent padding algorithm to extend smoothed data to the present time, is inappropriate if it assumes the continuation of a recent trend. This is likely to confuse , rather than inform, the wider public about the current climate state .
Finally , I repeat my earlier remarks (made before EOS piece published) that we are missing an opportunity to say that a warm Medieval period per se is not a refutation of anthropogenic warming , {as its absence is no proof}, if we do not understand the role of specific forcings (natural and anthropogenic) that influenced medieval and current climates.
Cheers

Keith

At 12:48 PM 10/9/03 -0600, Kevin Trenberth wrote:

Hi all

Here are my suggested changes: toned down in several places.
Tracking turned on

Kevin

Michael E. Mann wrote:

Dear co-authors,

Attached is a draft response, incorporating suggestions Kevin, Tom W, and Michael. I've

aimed to be as brief as possible, but hard to go much lower than 750 words and still

address all the key issues. 750 words, by the way, is our allotted limit.

Looking forward to any comments. Feel free to send an edited version if you prefer, and

I'll try to assimilate all of the suggested edits and suggestions into a single revised

draft. If you can get comments to me within the next couple days, that would be very

helpful as we're working on a late October deadline for the final version.

Thanks for your continued help,

mike

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References

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6. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Michael E. Mann" <mann@virginia.edu>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Re: Fwd: Re: draft
Date: Tue, 14 Oct 2003 12:35:34 -0400
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Malcolm Hughes
<mhughes@ltrr.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>,
rbradley@geo.umass.edu, tcrowley@duke.edu, omichael@princeton.edu,
jto@u.arizona.edu, Scott Rutherford <srutherford@rwu.edu>, Tom Wigley
<wigley@ucar.edu>, p.jones@uea.ac.uk, Kevin Trenberth
<trenbert@cgd.ucar.edu>

thanks Caspar,
I agree--its important to emphasize this point, and I'm glad you
recognized that we were
underplaying it...
mike
At 10:25 AM 10/14/2003 -0600, Caspar Ammann wrote:

Mike,
looks good to me. It is one of these points where they can persuade
journalists that
they are 'correct' and it actually got into newspapers and finally
to the senate floor
this way. The more we are able to explain why the first half of the
20th century warmed
up naturally, the more confidence we get on the detection of the
anthropogenic signal
afterwards.
Caspar
Michael E. Mann wrote:

Dear All,
In response to Caspar's suggestion, which I agree with, I propose
rephrasing item "2"
as follows:
2) The statement by S03 that the Mann and Jones [2003]
reconstruction "clearly shows
temperatures in the MWP that are as high as those in the 20th
century" is misleading if
not false. M03 emphasize that it is the late, and not the early or
mid 20th century
warmth, that is outside the range of past variability. Mann and
Jones emphasize
conclusions for the Northern Hemisphere, noting that those for the
Southern Hemisphere
(and globe) are indeterminate due to a paucity of southern
hemisphere data. Consistent
with M03, they conclude that, late 20th century Northern Hemisphere
mean temperatures
are anomalous in a long-term (nearly two millennium) context.
Any comments?
Thanks,
mike

Delivered-To: [1]mem6u@virginia.edu
Date: Tue, 14 Oct 2003 09:18:37 -0600
From: Caspar Ammann [2]<ammann@ucar.edu>
Organization: NCAR
User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.0; en-US; rv:1.4)
Gecko/20030624

Netscape/7.1 (ax)
X-Accept-Language: en-us, en
To: "Michael E. Mann" [3]<mann@virginia.edu>
Subject: Re: draft
Hi Mike,

it now looks good to me indeed including the new last paragraph following Tom's wording.

The only point I would highlight a little more is in point 2): Maybe it could be stated

that the early part of the 20th century is within the natural range whereas the late

20th century, the main point of the AGU position statement and also in M03, is clearly

outside. Please also add a second 'n' in my name...

Cheers, and thanks for your momentum on this,

Caspar

Michael E. Mann wrote:

Dear All,

I agree with each of Tom W's suggestions. Adopting them, by the way, brings us down to

738 words.

So pending any revised language from Keith/Malcolm in response to Michael O's comment on

paragraph 2, I'm putting out a last call for comments, sign-ons, etc...

Thanks,

mike

At 08:00 AM 10/14/2003 -0600, Tom Wigley wrote:

Some minor points

para. 2 -- should it be 'an' ensuing rather than 'the' ensuing?

para. 2 -- I still think 'each' (line 3) is unnecessary

para. 4 -- no comma after '(and globe)'

re boreholes, does the point about comparing late 20th century with a 'much longer

period' 1000 years ago help us? Given that the 1000 years ago data is highly lowpass

filtered, if one *did* have a series with a temporal resolution that allowed a

legitimate comparison, then the likelihood of a warmer interval 1000 years ago must be

higher.

In any event, the time scale issue will not be meaningful to most readers. The key point

is the data reliability/uncertainty. I would just say something like

...

".... taken into account. For times more than 500 years ago, uncertainties in the borehole reconstructions preclude any useful quantitative comparison."

Finally, I would like the last para. retained, but I suggest shorter wording as ...

".... as indicating that SB03 misinterpreted and misrepresented the paleoclimatological literature. The controversy".

My problem here is twofold. First, they really say nothing directly about 'mainstream

scientific opinion' (except that they clearly disagree with it). At issue is not the

mainstream opinion, but their interpretation of the literature and their illogical

conclusions. Second, they may have misrepresented the results of their work, but we do

not address this issue so it comes here as a non sequitur. In fact, just what such

'misrepresentation' consists of, and why it might be judged as 'misrepresentation' is a

subtle issue. Hence my revision -- which retains the word 'misrepresentation', but in a

different context.

Tom.

+++++

Michael E. Mann wrote:

Thanks Tim and Malcolm,

The latest round of suggestions were extremely helpful. I've accepted them w/ a few

minor tweaks (attached). We're at 765 words--I think AGU will let us get away w/ that...

So, comments from others?

Thanks,

mike

At 02:11 PM 10/14/2003 +0100, Tim Osborn wrote:

S03 argue that borehole data provide a conflicting view of past temperature histories.

To the contrary, the borehole estimates for recent centuries shown in M03 may be

consistent with other estimates, provided consideration is given to statistical

uncertainties, spatial sampling and possible influences on the ground surface [e.g.,

snow cover changes--Beltrami and Kellman, 2003]. It is not meaningful to compare the

late 20th century with a much longer period 1000 years ago [Bradley et al., 2003],

especially given the acknowledged limitations [Pollack et al., 1998] of borehole data.

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References

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From: "Michael E. Mann" <mann@virginia.edu>
To: Tom Wigley <wigley@ucar.edu>, Kevin Trenberth
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<p.jones@uea.ac.uk>, ckfolland@meto.gov.uk, tkarl@ncdc.noaa.gov,
jto@u.arizona.edu, mann@virginia.edu
Subject: Fwd: Re: smoothing
Date: Tue, 14 Oct 2003 17:27:24 -0400

Sorry--one more error. The MSE values for "minimum norm" and "minimum roughness" are switched in the figure legend. Obviously the former is a better fit...
mike

Date: Tue, 14 Oct 2003 17:08:49 -0400
To: Tom Wigley <wigley@ucar.edu>, Kevin Trenberth
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ckfolland@meto.gov.uk,
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From: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: smoothing
Bcc: Scott Rutherford <srutherford@rwu.edu>
correction '1)' should read:
'1) minimum norm: sets padded values equal to mean of available data beyond the available data (often the default constraint in smoothing routines)'
sorry for the confusion,
mike
At 05:05 PM 10/14/2003 -0400, Michael E. Mann wrote:

Dear All,
To those I thought might be interested, I've provided an example for discussion of smoothing conventions. Its based on a simple matlab script which I've written (and attached) that uses any one of 3 possible boundary constraints [minimum norm, minimum slope, and minimum roughness] on the 'late' end of a time series (it uses the default 'minimum norm' constraint on the 'early' end of the series).
Warning: you needs some matlab toolboxes for this to run...
The routines uses a simple butterworth lowpass filter, and applies the 3 lowest order constraints in the following way:
1) minimum norm: sets mean equal to zero beyond the available data (often the default constraint in smoothing routines)
2) minimum slope: reflects the data in x (but not y) after the last available data point. This tends to impose a local minimum or maximum at the edge of the data.
3) minimum roughness: reflects the data in both x and y (the latter w.r.t. to the y

value of the last available data point) after the last available data point. This tends to impose a point of inflection at the edge of the data---this is most likely to preserve a trend late in the series and is mathematically similar, though not identical, to the more ad hoc approach of padding the series with a continuation of the trend over the past 1/2 filter width. The routine returns the mean square error of the smooth with respect to the raw data. It is reasonable to argue that the minimum mse solution is the preferable one. In the particular example I have chosen (attached), a 40 year lowpass filtering of the CRU NH annual mean series 1856-2003, the preference is indicated for the "minimum roughness" solution as indicated in the plot (though the minimum slope solution is a close 2nd)... By the way, you may notice that the smooth is effected beyond a single filter width of the boundary. That's because of spectral leakage, which is unavoidable (though minimized by e.g. multiple-taper methods). I'm hoping this provides some food for thought/discussion, esp. for purposes of IPCC...
mike

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References

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2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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To: Malcolm Hughes <mhughes@ltrr.arizona.edu>, Tim Osborn
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p.jones@uea.ac.uk, mann@virginia.edu, Tom Wigley <wigley@ucar.edu>
Subject: Fwd: Correspondence on Harvard Crimson coverage of Soon /
Baliunas views on climate
Date: Thu, 16 Oct 2003 16:43:41 -0400

Dear All,

Thought you would be interested in this exchange, which John Holdren
of Harvard has been
kind enough to pass along...
mike

Delivered-To: mem6u@virginia.edu
X-Sender: jholdren@camail2.harvard.edu
X-Mailer: QUALCOMM Windows Eudora Version 5.0.2
Date: Thu, 16 Oct 2003 13:53:08 -0400
To: "Michael Mann" <mem6u@virginia.edu>, "Tom Wigley"
<wigley@ucar.edu>

From: "John P. Holdren" <john_holdren@harvard.edu>
Subject: Correspondence on Harvard Crimson coverage of Soon /
Baliunas

views on climate
Michael and Tom --
I'm forwarding for your entertainment an exchange that followed from
my being quoted in
the Harvard Crimson to the effect that you and your colleagues are
right and my
"Harvard" colleagues Soon and Baliunas are wrong about what the
evidence shows
concerning surface temperatures over the past millennium. The
cover note to faculty
and postdocs in a regular Wednesday breakfast discussion group on
environmental science
and public policy in Harvard's Department of Earth and Planetary
Sciences is more or
less self-explanatory.
Best regards,
John

Date: Thu, 16 Oct 2003 11:02:24 -0400
To: schrag@eps.harvard.edu, oconnell@eps.harvard.edu,
holland@eps.harvard.edu,
pearson@eps.harvard.edu, eli@eps.harvard.edu,
ingalls@eps.harvard.edu,
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poussart@fas.harvard.edu, jshaman@fas.harvard.edu,
sivan@fas.harvard.edu,
bec@io.harvard.edu, saleska@fas.harvard.edu
From: "John P. Holdren" <john_holdren@harvard.edu>

Subject: For the EPS Wednesday breakfast group: Correspondence on Harvard Crimson

coverage of Soon / Baliunas views on climate

Cc: jeremy_bloxham@harvard.edu, william_clark@harvard.edu, patricia_mclaughlin@harvard.edu,

Bcc:

Colleagues--

I append here an e-mail correspondence I have engaged in over the past few days trying

to educate a Soon/Baliunas supporter who originally wrote to me asking how I could think

that Soon and Baliunas are wrong and Mann et al. are right (a view attributed to me,

correctly, in the Harvard Crimson). This individual apparently runs a web site on which

he had been touting the Soon/Baliunas position.

While it is sometimes a mistake to get into these exchanges (because one's interlocutor

turns out to be ineducable and/or just looking for a quote to reproduce out of context

in an attempt to embarrass you), there was something about this guy's formulations that

made me think, at each round, that it might be worth responding.

In the end, a couple

of colleagues with whom I have shared this exchange already have suggested that its

content would be of interest to others, and so I am sending it to our "environmental

science and policy breakfast" list for your entertainment and, possibly, future

breakfast discussion.

The items in the correspondence are arranged below in chronological order, so that it

can be read straight through, top to bottom.

Best,

John

At 09:43 PM 9/12/2003 -0400, you wrote:

Dr. Holdren:

In a recent Crimson story on the work of Soon and Baliunas, who have written for my

website [1]www.techcentralstation.com, you are quoted as saying:

My impression is that the critics are right. It s unfortunate that so much attention is

paid to a flawed analysis, but that s what happens when something happens to support the

political climate in Washington.

Do you feel the same way about the work of Mann et. al.? If not why not?

Best,

Nick

Nick Schulz

Editor

TCS

1-800-619-5258

From: John P. Holdren [[2]mailto:john_holdren@harvard.edu]
Sent: Monday, October 13, 2003 11:06 AM
To: Nick Schulz
Subject: Harvard Crimson coverage of Soon / Baliunas controversy
Dear Nick Schultz --

I am sorry for the long delay in this response to your note of September 12. I have been swamped with other commitments.

As you no doubt have anticipated, I do not put Mann et al. in the same category with Soon and Baliunas.

If you seriously want to know "Why not?", here are three ways one might arrive at what I regard as the right conclusion:

(1) For those with the background and patience to penetrate the scientific arguments, the conclusion that Mann et al. are right and Soon and Baliunas are wrong follows from reading carefully the relevant Soon / Baliunas paper and the Mann et al. response to it:

W. Soon and S. Baliunas, "Proxy climatic and environmental changes of the past 1000 years", *Climate Research*, vol. 23, pp 89ff, 2003.

M. Mann, C. Amman, R. Bradley, K. Briffa, P. Jones, T. Osborn, T. Crowley, M. Hughes, M.

Oppenheimer, J. Overpeck, S. Rutherford, K. Trenberth, and T. Wigley, "On past temperatures and anomalous late-20th century warmth", *EOS*, vol 84, no. 27, pp 256ff, 8 July 2003.

This is the approach I took. Soon and Baliunas are demolished in this comparison.

(2) Those lacking the background and/or patience to penetrate the two papers, and seriously wanting to know who is more likely to be right, have the option of asking somebody who does possess these characteristics -- preferably somebody outside the handful of ideologically committed and/or oil-industry-linked professional climate-change skeptics -- to evaluate the controversy for them.

Better yet, one could poll a number of such people. They can easily be found by checking the web pages of earth sciences, atmospheric sciences, and environmental sciences departments at any number of major universities.

(3) The least satisfactory approach, for those not qualified for (1) and lacking the time or initiative for (2), would be to learn what one can about the qualifications

(including publications records) and reputations, in the field in question, of the authors on the two sides. Doing this would reveal that Soon and Baliunas are, essentially, amateurs in the interpretation of historical and paleoclimatological records of climate change, while the Mann et al. authors include several of the most published and most distinguished people in the world in this field. Such an investigation would also reveal that Dr. Baliunas' reputation in this field suffered considerable damage a few years back, when she put her name on an incompetent critique of mainstream climate science that was never published anywhere respectable but was circulated by the tens of thousands, in a format mimicking that of a reprint from the Proceedings of the National Academy of Sciences, in pursuit of signatures on a petition claiming that the mainstream findings were wrong. Of course, the third approach is the least satisfactory because it can be dangerous to assume that the more distinguished people are always right. Occasionally, it turns out that the opposite is true. That is one of several good reasons that it pays to try to penetrate the arguments, if one can, or to poll others who have tried to do so. But in cases where one is not able or willing to do either of these things -- and where one is able to discover that the imbalance of experience and reputation on the two sides of the issue is as lopsided as here -- one ought at least to recognize that the odds strongly favor the proposition that the more experienced and reputable people are right. If one were a policy maker, to bet the public welfare on the long odds of the opposite being true would be foolhardy.

Sincerely,
John Holdren

PS: I have provided this response to your query as a personal communication, not as fodder for selective excerpting on your web site or elsewhere. If you do decide that you would like to propagate my views on this matter more widely, I ask that you convey my response in its entirety.

At 11:16 AM 10/13/2003 -0400, you wrote:

I have the patience but, by your definition certainly, not the background, so I suppose

it is not surprising I came to a different conclusion. I guess my problem concerns what lawyers call the burden of proof. The burden weighs heavily much more heavily, given the claims on Mann et.al. than it does on Soon/Baliunas. Would you agree?

Falsifiability for the claims of Mann et. al. requires but a few examples, does it not? Soon/Baliunas make claims that have no such burden. Isn't that correct?

Best,
Nick

From: John P. Holdren [[3]mailto:john_holdren@harvard.edu]
Sent: Tuesday, October 14, 2003 5:54 PM
To: Nick Schulz
Subject: RE: Harvard Crimson coverage of Soon / Baliunas controversy
Nick--

Yes, I can see how it might seem that, in principle, those who are arguing for a strong

and sweeping proposition (such as that "the current period is the warmest in the last

1000 years") must meet a heavy burden of proof, and that, because even one convincing

counter-example shoots the proposition down, the burden that must be borne by the

critics is somehow lighter. But, in practice, burden of proof is an evolving thing --

it evolves as the amount of evidence relevant to a particular proposition grows.

To choose an extreme example, consider the first and second laws of thermodynamics.

Both of these are "empirical" laws. Our confidence in them is based entirely on

observation; neither one can be "proven" from more fundamental laws. Both are very

sweeping. The first law says that energy is conserved in all physical processes. The

second law says that entropy increases in all physical processes. So, is the burden of

proof heavier on somebody who asserts that these laws are correct, or on somebody who

claims to have found an exception to one or both of them? Clearly, in this case, the

burden is heavier on somebody who asserts an exception. This is in part because the

two laws have survived every such challenge in the past. No exception to either has

ever been documented. Every alleged exception has turned out to be traceable to a

mistake of some kind. This burden on those claiming to have found an exception is so

strong that the US Patent Office takes the position, which has been upheld in court,

that any patent application for an invention that violates either law can be rejected

summarily, without any further analysis of the details.

Of course, I am not asserting that the claim we are now in the warmest period in a

millennium is in the same league with the laws of thermodynamics. I used the latter

only to illustrate the key point that where the burden is heaviest depends on the state

of prior evidence and analysis on the point in question -- not simply on whether a

proposition is sweeping or narrow.

In the case actually at hand, Mann et al. are careful in the nature of their claim.

They write along the lines of "A number of reconstructions of large-scale temperature

changes support the conclusion" that the current period is the warmest in the last

millennium. And they write that the claims of Baliunas et al. are "inconsistent with

the preponderance of scientific evidence". They are not saying that no shred of

evidence to the contrary has ever been produced, but rather that analysis of the

available evidence as a whole tends to support their conclusion.

This is often the case in science. That is, there are often "outlier" data points or

apparent contradictions that are not yet adequately explained, but still are not given

much weight by most of the scientists working on a particular issue if a strong

preponderance of evidence points the other way. This is because the scientists judge it

to be more probable that the outlier data point or apparent contradiction will

ultimately turn out to be explainable as a mistake, or otherwise explainable in a way

that is consistent with the preponderance of evidence, than that it will turn out that

the preponderance of evidence is wrong or is being misinterpreted. Indeed, apparent

contradictions with a preponderance of evidence are FAR more often due to measurement

error or analysis error than to real contradiction with what the preponderance

indicates.

A key point, then, is that somebody with a PhD claiming to have identified a

counterexample does not establish that those offering a general proposition have failed

in their burden of proof. The counterexample itself must pass muster as both valid in

itself and sufficient, in the generality of its implications, to invalidate the

proposition.

In the case at hand, it is not even a matter of an "outlier" point or other seeming

contradiction that has not yet been explained. Mann et al. have explained in detail why

the supposed contrary evidence offered by Baliunas et al. does NOT constitute a

counterexample. To those with some knowledge and experience in studies of this kind,

the refutation by Mann et al is completely convincing.

Sincerely,

John Holdren

At 08:08 AM 10/15/2003 -0400, you wrote:

Dr. Holdren:

Thank you for your thoughtful reply. I genuinely appreciate you taking the time.

You are quite right about the laws of thermodynamics. And you are quite right that Mann

et al is not in the same league as those laws and that s not to take anything from their

basic research.

You write to those with knowledge and experience in studies of this kind, the refutation

by Mann et all is completely convincing. Since I do not have what you would consider

the requisite knowledge or experience, I can t speak to that. I ve read the Mann papers

and the Baliunas Soon paper and the Mann rebuttal and find Mann s claims based on his

research extravagant and beyond what he can legitimately claim to know. That said, I m

willing to believe it is because I don t have the tools necessary to understand.

But if you will indulge a lay person with some knowledge of the matter, perhaps you

could clear up a thing or two.

Part of the confusion over Mann et al it seems to me has to do not with the research

itself but with the extravagance of the claims they make based on their research.

And yet you write: Mann et al. are careful in the nature of their claim. They write

along the lines of A number of reconstructions of large-scale temperature changes

support the conclusion that the current period is the warmest in the last millennium.

And they write that the claims of Baliunas et al. are inconsistent with the

preponderance of scientific evidence .

That makes it seem as if Mann s not claiming anything particularly extraordinary based

on his research.

But Mann claimed in the NYTimes in 1998 that in their Nature study from that year Our conclusion was that the warming of the past few decades appears to be closely tied to emission of greenhouse gases by humans and not any of the natural factors." Does that seem to be careful in the nature of a claim? Respected scientists like Tom Quigley responded at the time by saying "I think there's a limit to how far you can ever go." As for using proxy data to detect a man-made greenhouse effect, he said, "I don't think we're ever going to get to the point where we're going to be totally convincing." These are two scientists who would agree on the preponderance of evidence and yet they make different claims about what that preponderance means. There are lots of respected climatologists who would say Mann has insufficient scientific basis to make that claim. Would you agree? The Soon Baliunas research is relevant to that element of the debate what the preponderance of evidence enables us to claim within reason. To that end, I don't think claims of Soon Baliunas are inconsistent with the preponderance of scientific evidence. I'll close by saying I'm willing to admit that, as someone lacking a PhD, I could be punching above my weight. But I will ask you a different but related question How much hope is there for reaching reasonable public policy decisions that affect the lives of millions if the science upon which those decisions must be made is said to be by definition beyond the reach of those people? All best,
Nick

Date: Thu, 16 Oct 2003 08:46:23 -0400

To: "Nick Schulz" <nschulz@techcentralstation.com>

From: "John P. Holdren" <john_holdren@harvard.edu>

Subject: RE: Harvard Crimson coverage of Soon / Baliunas controversy
Nick--

You ask good questions. I believe the thoughtfulness of your questions and the progress

I believe we are making in this interchange contain the seeds of the answer to your

final question, which, if I may paraphrase just a bit, is whether there's any hope of

reaching reasonable public-policy decisions when the details of the science germane to

those decisions are impenetrable to most citizens.

This is a hard problem. Certainly the difficulty is not restricted to climate science and policy, but applies also to nuclear-weapon science and policy, nuclear-energy science and policy, genetic science and policy, and much more. But I don't think the difficulties are insurmountable. That's why I'm in the business I'm in, which is teaching about and working on the intersection of science and technology with policy.

Most citizens cannot penetrate the details of what is known about the how the climate works (and, of course, what is known even by the most knowledgeable climate scientists about this is not everything one would like to know, and is subject to modification by new data, new insights, new forms of analysis). Neither would most citizens be able to understand how a hydrogen bomb works (even if the details were not secret), or what factors will determine the leak rates of radioactive nuclides from radioactive-waste repositories, or what stem-cell research does and promises to be able to do.

But, as Amory Lovins once said in addressing the question of whether the public deserved and could play a meaningful role in debates about nuclear-weapon policy, even though most citizens would never understand the details of how nuclear weapons work or are made, "You don't have to be a chicken to know what to do with an egg." In other words, for many (but not all) policy purposes, the details that are impenetrable do not matter.

There CAN be aspects of the details that do matter for public policy, of course. In those cases, it is the function and the responsibility of scientists who work across the science-and-policy boundary to communicate the policy implications of these details in ways that citizens and policy makers can understand. And I believe it is the function and responsibility of citizens and policy makers to develop, with the help of scientists and technologists, a sufficient appreciation of how to reach judgments about plausibility and credibility of communications about the science and technology relevant to policy choices so that the citizens and policy makers are NOT disenfranchised in policy decisions where science and technology are germane.

How this is best to be done is a more complicated subject than I am prepared to try to

explicate fully here. (Alas, I have already spent more time on this interchange than I could really afford from other current commitments.) Suffice it to say, for now, that improving the situation involves increasing at least somewhat, over time, the scientific literacy of our citizens, including especially in relation to how science works, how to distinguish an extravagant from a reasonable claim, how to think about probabilities of who is wrong and who is right in a given scientific dispute (including the question of burden of proof as you and I have been discussing it here), how consulting and polling experts can illuminate issues even for those who don't understand everything that the experts say, and why bodies like the National Academy of Sciences and the Intergovernmental Panel on Climate Change deserve more credibility on the question of where mainstream scientific opinion lies than the National Petroleum Council, the Sierra Club, or the editorial page of the Wall Street Journal. Regarding extravagant claims, you continue to argue that Mann et al. have been guilty of this, but the formulation of theirs that you offer as evidence is not evidence of this at all. You quote them from the NYT in 1998, referring to a study Mann and co-authors published in that year, as saying

"Our conclusion was that the warming of the past few decades appears to be closely tied to emission of greenhouse gases by humans and not any of the natural factors."

and you ask "Does that seem to be careful in the nature of a claim?" My answer is:

Yes, absolutely, their formulation is careful and appropriate.

Please note that they

did NOT say "Global warming is closely tied to emission of greenhouse gases by humans

and not any of the natural factors." They said that THEIR

CONCLUSION (from a

particular, specified study, published in NATURE) was that the warming of THE PAST FEW

DECADES (that is, a particular, specified part of the historical record) APPEARS (from

the evidence adduced in the specified study) to be closely tied...

This is a carefully

specified, multiply bounded statement, which accurately reflects what they looked at and

what they found. And it is appropriately contingent --"APPEARS to be closely tied" --

allowing for the possibility that further analysis or new data could later lead to a

different perspective on what appears to be true.

With respect, it does not require a PhD in science to notice the appropriate boundedness

and contingency in the Mann et al. formulation. It only requires an open mind, a

careful reading, and a degree of understanding of the character of scientific claims and

the wording appropriate to convey them that is accessible to any thoughtful citizen.

That is why I'm an optimist.

You go on to quote the respected scientist "Tom Quigley" as holding a contrary view to

that expressed by Mann. But please note that: (1) I don't know of any Tom Quigley

working in this field, so I suspect you mean to refer to the prominent climatologist Tom

Wigley; (2) the statements you attribute to "Quigley" do not directly contradict the

careful statement of Mann (that is, it is entirely consistent for Mann to say that his

study found that recent warming appears to be tied to human emissions and for Wigley to

say that that there are limits to how far one can go with this sort of analysis, without

either one being wrong); and (3) Tom Wigley is one of the CO-AUTHORS of the resounding

Mann et al. refutation of Soon and Baliunas (see attached PDF file).

I hope you have found my responses to be of some value. I now must get on with other

things.

Best,

John Holdren

JOHN P. HOLDREN

Teresa and John Heinz Professor of Environmental Policy
& Director, Program in Science, Technology, & Public Policy,
Belfer Center for Science and International Affairs,
John F. Kennedy School of Government

Professor of Environmental Science and Public Policy,
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[4]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.techcentralstation.com/>
2. mailto:john_holdren@harvard.edu
3. mailto:john_holdren@harvard.edu
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: evelyn.smith@noaa.gov, "Christopher D Miller" <Christopher.D.Miller@noaa.gov>
Subject: Fwd: confidential assessment of GC04-203
Date: Fri Oct 24 10:20:33 2003

Dear Evelyn and Chris,
re. proposal review GC04-203, Meko et al. "A synthesis of 19th century climate data for the United States from paleo, archival and instrumental sources".

I have read the "Reviewer conflict of interest and confidentiality..." document and can state that I have no conflict of interest and will abide by the confidentiality provisions etc.

I reviewed a very similar proposal by this group 1 year ago, and enclose my review of that proposal below. The new proposal has taken into account my two main concerns from last time, which were:

- (i) that creation only of a blended data set that contained a time varying mixture of proxy and instrumental data would limit the usefulness because its quality would be time varying, perhaps in an unquantified way, and independent study of errors between proxy and observed data would be prevented; and
- (ii) that the proposed work was not very innovative in terms of the applications for which the new information would be used.

Both of these points have been addressed adequately and so I now rate it "Excellent (5)" for scientific/technical merit, and "High (5)" for importance/relevance and applicability. One issue that I would like to raise, however, is that the need for quantifying uncertainty/error in the reconstructions/database is not given much coverage in the proposal. It is mentioned, but not focused on. For many applications (testing models, comparison with other reconstructions, detection of unusual climate trends/events), explicitly quantified error estimates are essential. These often change magnitude through time, and thus should be estimated in such a way as to allow this. They may also change with time scale (often being lower for, e.g., a decadal mean than for a single year's value), and again the error estimation method should capture this. I do not think that this issue detracts from the quality of the proposal. Instead I am mentioning it in the hope that this comment can be passed on to the proposers, in the event that the project is funded, so that they can be prompted into placing the appropriate emphasis on quantifying uncertainty.

Apologies for being late yet again, and best regards,
Tim

Date: Thu, 24 Oct 2002 17:14:31 +0000
Subject: confidential assessment of GC03-512
From: Tim Osborn <t.osborn@uea.ac.uk>
To: <irma.dupree@noaa.gov>
CC: <t.osborn@uea.ac.uk>,

<christopher.d.miller@noaa.gov>

Dear Irma and Chris,

Re. proposal review GC03-512, PI: David Meko "A 19th century data catalog"

First of all, I confirm that there is no conflict of interest etc.

Now to my review...

(1) Scientific Merit

Rating: Good

Comments:

I completely agree with the rationale behind improving data sets of 19th century climate (see my comments below on "Relevance to climate change programme"), and the proposers have identified the most relevant data sources available for the US. The objectives and workplan are generally reasonable, but I have rated it "good" rather than "very good" or "excellent" because it does not seem as scientifically innovative or challenging as it might. Some particular concerns are highlighted below. I am very wary about the proposed approach of integrating the data sources together to produce a single climate product. Obviously the data sources have to be used in combination, for calibration of proxy data or for assessment of possibly dubious early instrumental data, *but* combining them all into a single product only will be very restrictive for future use, assessment, improvements. Much better would be to produce instrumental-only series for whatever length is available, and tree-ring only series for the full length (i.e., into the late 19th and 20th centuries, despite the availability of instrumental data for these periods). Blending them into a single analysis is of some, but limited, use and comparisons of different periods and with (e.g.) model simulations can only ever be done by taking into account error bars that vary dramatically in time and are only estimates of the "true" errors - and the error estimates may be underestimates if based only on residuals or covariances during the 20th century.

No mention is made of using the 19th century data to consider key issues such as difference between tree-ring and ground borehole temperatures (they differ more in the 19th century, in terms of trend, than in other centuries), possibly taking into account land-use change. No mention is made of using the 19th century data to assess multi-century temperature reconstructions and why they differ. These are issues of great importance. No mention is investigating seasonal dependence of temperature changes, which are greater in existing temperature products during the 19th century than in the 20th century and which has important implications for the calibration of proxy (including tree-ring) data against summer or annual data and the need to more clearly define the true seasonal response of proxy data.

Despite these concerns, the proposed work is certainly worthy of funding and

the extra items of interest that I mention above could be achieved using the data generated here, in some future project.

(2) Relevance to climate change programme

Rating: High

Comments:

The 19th century is certainly of particular importance, not just for the reasons outlined in the proposal but also because this century shows some of the biggest disagreements in warming trend between various quasi-hemispheric temperature reconstructions and between proxy and instrumental data and between different seasons of instrumental data. Additional data sources are definitely required, and additional digitisation, homogenisation and intercomparison of data sets is necessary. For these reasons, work such as that proposed here is essential for helping to refine answers to questions such as how unusual is late twentieth century climate and detection of climate change signals against the noise of natural climate variability.

Best regards

Tim

From: "Michael E. Mann" <mann@virginia.edu>
To: Ray Bradley <rbradley@geo.umass.edu>, "Malcolm Hughes"
<mhughes@ltrr.arizona.edu>, Mike MacCracken <mmaccrac@comcast.net>, Steve
Schneider <shs@stanford.edu>, tom crowley <tom@ocean.tamu.edu>, Tom
Wigley <wigley@meeke.UCAR.EDU>, Jonathan Overpeck <jto@u.arizona.edu>,
asocci@cox.net, Michael Oppenheimer <omichael@Princeton.EDU>, Keith
Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Tim Osborn
<t.osborn@uea.ac.uk>, Tim_Profeta@lieberman.senate.gov, Ben Santer
<santer1@llnl.gov>, Gabi Hegerl <hegerl@duke.edu>, Ellen Mosley-Thompson
<thompson.4@osu.edu>, "Lonnie G. Thompson" <thompson.3@osu.edu>, Kevin
Trenberth <trenbert@cgd.ucar.edu>
Subject: CONFIDENTIAL Fwd:
Date: Sun, 26 Oct 2003 13:47:44 -0500
Cc: mann@virginia.edu

Dear All,

This has been passed along to me by someone whose identity will remain
in confidence.

Who knows what trickery has been pulled or selective use of data
made. Its clear that

"Energy and Environment" is being run by the baddies--only a skill
for industry would have

republished the original Soon and Baliunas paper as submitted to
"Climate Research" without

even editing it. Now apparently they're at it again...

My suggested response is:

1) to dismiss this as stunt, appearing in a so-called "journal" which
is already known to

have defied standard practices of peer-review. It is clear, for
example, that nobody we

know has been asked to "review" this so-called paper

2) to point out the claim is nonsense since the same basic result has
been obtained by

numerous other researchers, using different data, elementary
compositing techniques, etc.

Who knows what sleight of hand the authors of this thing have pulled.
Of course, the usual

suspects are going to try to peddle this crap. The important thing is
to deny that this has

any intellectual credibility whatsoever and, if contacted by any
media, to dismiss this for

the stunt that it is..

Thanks for your help,

mike

two people have a forthcoming 'Energy & Environment' paper that's
being unveiled tomoro

(monday) that -- in the words of one Cato / Marshall/ CEI type --
"will claim that Mann

arbitrarily ignored paleo data within his own record and substituted
other data for

missing values that dramatically affected his results.

When his exact analysis is rerun with all the data and with
no data

substitutions, two very large warming spikes will appear that are greater than the 20th century.

Personally, I'd offer that this was known by most people who understand Mann's

methodology: it can be quite sensitive to the input data in the early centuries.

Anyway, there's going to be a lot of noise on this one, and knowing Mann's very thin

skin I am afraid he will react strongly, unless he has learned (as I hope he has) from

the past...."

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: stocker@climate.unibe.ch, joos@climate.unibe.ch,
knutti@climate.unibe.ch
Subject: some info you'll want to have...
Date: Wed, 29 Oct 2003 13:05:07 -0500
Cc: Gabi Hegerl <hegerl@duke.edu>, tom crowley <tom@ocean.tamu.edu>,
mhughes@ltrr.arizona.edu, "raymond s.bradley" <rbradley@geo.umass.edu>,
Keith Briffa <k.briffa@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>,
Stefan Rahmstorf <rahmstorf@pik-potsdam.de>, Steve Schneider
<shs@stanford.edu>, peter.stott@metoffice.com, Gavin Schmidt
<gavin@isis.giss.nasa.gov>, mann@multiproxy.evsc.virginia.edu

Dear Thomas, Fortunat, Reto:

You might have wanted to check w/ us first, but thanks anyway for responding to this. We've uncovered the error in what they did. They didn't use the proxy data available on our public ftp site, which I had pointed them too--instead they used a spreadsheet file that my associate Scott Rutherford had prepared. In this file, most of the early series were overprinted at later years. This resulted in the reconstruction becoming increasingly spurious as one goes further back in time--the estimates prior to 1700 or so were rendered meaningless. There were also some other methodological errors that will be detailed shortly, but this was the big one. So they will probably have to retract the paper. You can find out more about this here, on journalist David Appell's "blog": [1]<http://www.davidappell.com/> We also have an op-ed piece going out this afternoon, further detailing the problems. Will send that as soon as its available. I've attached a few other relevant documents, and I'm forwarding another email I sent out to colleagues yesterday, just after I had discovered the main problem in what they've done...
mike

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\eudora\attach\Journalists.re.EandEfin-revised.doc"

References

1. <http://www.davidappell.com/>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: "raymond s.bradley" <rbradley@geo.umass.edu>, mhughes@ltrr.arizona.edu, "Phil Jones" <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, mann@multiproxy.evsc.virginia.edu, Scott Rutherford <srutherford@rwu.edu>
Subject: Can you believe it???
Date: Thu, 30 Oct 2003 09:02:53 -0500

Guys, can you take a look at this.
I think that everything I say here is true! But we've got to be sure.
There are more technical things they did wrong that I want to add, but this is the critical bit--what do you think. Comments? Thanks...
mike

The recent paper by McIntyre and McKittrick (Energy and Environment, 14, 751-771) claims to be an "audit" of the analysis of Mann, Bradley and Hughes (1998) or "MBH98". An audit involves a careful examination, using the same data and following the exact procedures used in the report or study being audited. McIntyre and McKittrick ("MM") have done no such thing, having used neither the data nor the procedures of MBH98. Their analysis is notable only in how deeply they have misrepresented the data, methods, and results of MBH98. Journals that receive critical comments on a previously published papers always provide the authors who are being criticized an opportunity to review the study prior to publication, and offer them the chance to respond. This is standard operating procedure in any legitimate peer-reviewed scientific journal. Mann and colleagues were never given this opportunity, nor were any other leading paleoclimate scientists that we're familiar with. It is unfortunate that the profound errors, and false and misleading statements, and entirely spurious results provided in the McIntyre and McKittrick article were ever allowed to see the light of day by those would have been able to detect them. We suspect the extremely checkered history of "Energy and Environment" has some role to play in this. The authors should retract their article immediately, and issue a public apology to the climate research community for the injustice they have done in publishing and promoting this deeply deceptive and flawed analysis.

Not only were critical errors made in their analysis that render it thoroughly invalid, but

there appear to have been several strikingly subjective decisions made to remove key indicators of the original MBH98 network prior to AD 1600, with a dramatic impact on the resulting reconstruction. It is precisely the over which the numerous indicators were removed (pre 1600 period) during which MM reconstruct anomalous warmth that is in sharp opposition to the cold conditions observed in MBH98 and nearly all other independent published estimates that we know of.

While the authors dutifully cite the small inconsistency between the number of proxy indicators reported by, and found in the public data archive, of Mann et al back in time (there indeed appear to have been some minor typos in the MBH98 paper), it is odd that they do not cite the number of indicators in their putative version of the Mann et al network based on the independent collection of data, back time. The reader is literally left to do a huge amount of detective work, based on the tables in their pages 20-23, to determine just what data have been eliminated from the original Mann et al network. It seems odd, indeed, that their "substitutions" of other versions (or in some case, only apparent, and not actual, versions) of proxy data series for those in the original Mann et al (1998) network has the selective effect of deleting key proxy indicators that contribute dramatic cooling during the 16th century, when the MM reconstruction shows an anomalous warming departure from the Mann et al (1998) and all other published Northern Hemisphere temperature reconstructions.

Here are some blatant examples:

1) The authors (see their Figure 4) substitute a younger version of one of the Jacoby et al Northern Treeline series for the older version used by MBH98. This substitution has effect of removing a predictor of 15th century cooling [Incidentally, MM make much of the tendency for some tree ring series, such as this one, to show an apparent cooling over the past couple decades. Scientists with expertise in dendroclimatology know that this behavior represents a decrease in the sensitivity to temperature in recent decades that likely is related to conditions other than temperature which are limiting tree growth]

2) The authors eliminate, without any justification, the entire dataset of 70 Western North American (WNA) tree-ring series available between 1400 and 1600 (this dataset is represented, by MBH98, in terms of a smaller number of representative Principal Component time series). The leading pattern of variance in this data set exhibits conditions from 1400-1800 that are dramatically colder than the mid and late 20th century, and a very prominent cooling in the 15th century in particular. The authors eliminated this entire dataset because they claimed that the underlying data was not available in the public domain.

In point of fact, not only were the individual WNA data all available on the public ftp site provided by Mann and colleagues: [1]ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB/NOAMER/, but they were also available, despite the claims to the contrary by MM, on NOAA's website as well:

[2]ftp://ftp.ngdc.noaa.gov/paleo/treering/chronologies/northamerica/usa

The deletion of this critical (see Mann et al, 1999) dataset appears to one of the more important censorings performed by MM that allows them to achieve their spurious result of apparent 15th-16th century warmth.

We have not, as yet, finished determining just how many important indicators were subtly censored from the MBH98 dataset by the various subjective substitutions described on pages 20-23. However, given the relatively small number of indicators available between 1400-1500 in the MBH98 network (22-24) and their elimination of some of the more critical ones, it would appear that this subjective censoring of data, alone, explains the spurious, misleading, and deceptive result achieved by the authors.

Incidentally, MBH98 go to great depths to perform careful cross-validation experiments as a function of increasing sparseness of the candidate predictors back in time, to demonstrate statistically significant reconstructive skill even for their earlier (1400-1450) reconstruction interval. MM describe no cross-validation experiments. We wonder what the

verification resolved variance is for their reconstruction based on their 1400-1450 available network, during the independent latter 19th century period?

There are numerous other serious problems that would render the MM analysis completely invalid, even in the absence of the serious issue raised above, and these are detailed below

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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1. <ftp://holocene.evsc.virginia.edu/pub/MBH98/TREE/ITRDB/NOAMER/>
2. <ftp://ftp.ngdc.noaa.gov/paleo/treering/chronologies/northamerica/usa>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "raymond s. bradley" <rbradley@geo.umass.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, p.jones@uea.ac.uk, k.briffa@uea.ac.uk
Subject: One way out....
Date: Thu, 30 Oct 2003 11:55:18 -0500
Cc: mann@multiproxy.evsc.virginia.edu, mhughes@ltrr.arizona.edu

<x-flowed>

Tim, Phil, Keef:

I suggest a way out of this mess. Because of the complexity of the arguments involved, to an unformed observer it all might be viewed as just scientific nit-picking by "for" and "against" global warming proponents. However, if an "independent group" such as you guys at CRU could make a statement as to whether the M&M effort is truly an "audit", and if they did it right, I think that would go a long way to defusing the issue.

It's clear from the figure that Reno Knuti sent yesterday that something pretty whacky happened in their analysis prior to ~AD1600, and this led Mike to figure out the problem. See:
file:///c:/eudora/attach/nh_temp_rec.jpg

If you are willing, a quick and forceful statement from The Distinguished CRU Boys would help quash further arguments, although here, at least, it is already quite out of control.....yesterday in the US Senate the debate opened on the McCain-Lieberman bill to control CO2 emissions from power plants. Sen Inhofe stood up & showed the M & M figure and stated that Mann et al--& the IPCC assessment --was now disproven and so there was no reason to control CO2 emissions....I wonder how many times a "scientific" paper gets reported on in the Senate 3 days after it is published....

Ray

</x-flowed>

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, "raymond s. bradley" <rbradley@geo.umass.edu>, Tim Osborn <t.osborn@uea.ac.uk>, p.jones@uea.ac.uk
Subject: Re: One way out....
Date: Thu, 30 Oct 2003 14:26:55 -0500
Cc: mhughes@ltrr.arizona.edu

Hi Keith,
sorry--yes, I think the Nature idea would be great. Definitely give it a try!
thanks,
mike

At 06:53 PM 10/30/2003 +0000, Keith Briffa wrote:

Things obviously moving over there - this result looks good. Just thought I'd send this first bit (up to dotted line) of edited version, to illustrate possible toning down?
Have to go now and feed daughter. Will wait til see your joint version first thing tomorrow - rest assured, that am entirely with you on this and still appalled by the MM stuff - but keeping your distance and calm stance is still urged.
all the best to all
any objections if I talk to Nature tomorrow?

Keith

At 01:31 PM 10/30/03 -0500, Michael E. Mann wrote:

Guys,

So the verification RE for the "censored" NH mean reconstruction? -6.64
The verification RE for the original MBH98 NH mean reconstruction: 0.42
I think the case is really strong now!

What if were to eliminate the discussion of all the other technical details (and just say they exist), and state more nicely that these series were effectively censored by their substitutions, and that by removing those series which they censored, I get a similar result, with a dismal RE.

And most people would keep the RE of 0.42 over the RE of -6, right? So this would make that point. I think we also need to say something about the process, etc. (the intro was based on something that Malcolm/Ray had originally crafted).

Thoughts, comments? Thanks,

mike

I'm thinking of a note saying basically this, and attaching this figure.

Could everybody sign on to something like this?

Thanks for all your help,

mike

At 05:11 PM 10/30/2003 +0000, Keith Briffa wrote:

Ray et al

I agree with this idea in principle. Whatever scientific differences and fascination with the nuances of techniques we may /may not share, this whole process represents the most despicable example of slander and down right deliberate perversion of the scientific process, and bias (unverified) work being used to influence public perception and due political process. It is, however, essential that you (we) do not get caught up in the frenzy that these people are trying to generate, and that will more than likely lead to error on our part or some premature remarks that we might regret. I do think the statement re Mike's results needs making, but only after it can be based on repeated work and in full collaboration of us all. I am happy to push Tim to take the lead and collaborate in this - and I feel we could get sanction very quickly from the DEFRA if needed. BUT this must be done calmly, and in the meantime a restrained statement but out saying we have full confidence in Mike's objectivity and independence - which we can not say of the sceptics. In fact I am moved tomorrow to contact Nature and urge them to do an editorial on this. The political machinations in Washington should NOT dictate the agenda or scheduling of the work - but some cool statement can be made saying we believe the "prats have really fucked up someway" - and that the premature publication of their paper is reprehensible. Much of the detail in Mikes

response though is not sensible (sorry Mike) and is rising to their bate.

Keith

At 11:55 AM 10/30/03 -0500, raymond s. bradley wrote:

Tim, Phil, Keef:

I suggest a way out of this mess. Because of the complexity of the arguments involved, to an unformed observer it all might be viewed as just scientific nit-picking by "for" and "against" global warming proponents. However, if an "independent group" such as you guys at CRU could make a statement as to whether the M&M effort is truly an "audit", and if they did it right, I think that would go a long way to defusing the issue.

It's clear from the figure that Reno Knuti sent yesterday that something pretty whacky happened in their analysis prior to ~AD1600, and this led Mike to figure out the problem. See:

[1]file:///c:/eudora/attach/nh_temp_rec.jpg

If you are willing, a quick and forceful statement from The Distinguished CRU Boys would help quash further arguments, although here, at least, it is already quite out of control.....yesterday in the US Senate the debate opened on the McCain-Lieberman bill to control CO2 emissions from power plants. Sen Inhofe stood up & showed the M & M figure and stated that Mann et al--& the IPCC assessment --was now disproven and so there was no reason to control CO2 emissions.....I wonder how many times a "scientific" paper gets reported on in the Senate 3 days after it is published....

Ray

--

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[2]<http://www.cru.uea.ac.uk/cru/people/briffa/>

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

--

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[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. file://c:\eudora\attach\nh_temp_rec.jpg/
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
4. <http://www.cru.uea.ac.uk/cru/people/briffa/>
5. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: f055 <T.Osborn@uea.ac.uk>, "p.jones" <p.jones@uea.ac.uk>, "raymond s. bradley" <rbradley@geo.umass.edu>, f055 <T.Osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>
Subject: RE: CLIMLIST
Date: Fri, 31 Oct 2003 05:37:03 -0500
Cc: mhughes <mhughes@ltr.arizona.edu>

Thanks very much Tim,
I was hoping that the revisions would ally concerns people had.
I'll look forward to your comments on this latest draft. I agree w/ Malcolm on the need to be careful w/ the wording in the first paragraph. The first paragraph is a bit of relic of a much earlier draft, and maybe we need to rethink it a bit. Takinig the high road is probably very important here. If *others* want to say that their actions represent scientific fraud, intellectual dishonesty, etc. (as I think we all suspect they do), lets let *them* make these charges for us!
Lets let our supporters in higher places use our scientific response to push the broader case against MM. So I look forward to peoples attempts to revise the first par. particular. I took the liberty of forwarding the previous draft to a handfull of our closet colleagues, just so they would have a sense of approximately what we'll be releasing later today--i.e., a heads up as to how MM achieved their result...
look forward to us finalizing something a bit later--I still think we need to get this out ASAP...
mike
SAAt 03:01 AM 10/31/2003 +0000, f055 wrote:

Dear all,
I've just finished preparing a detailed response offline, only to log on to send it to you all and find new versions from Mike plus more comments and information. Well, I don't have time to change my message now, so will paste it below this message. But bear in mind that the new draft may well have allayed many of my concerns - in particular, a quick glance shows the figure to be much more convincing than the one Mike circulated earlier, indeed it seems to be utterly convincing! I'll reply again on Friday morning once I've had time to read the new draft. In the meantime, here is my message as promised.

Dear MBH (cc to CRU),
The number of emails has been rather overwhelming on this issue and I'm struggling to catch up with them! But I will attempt to catch up with a few things here...
(1) The single worst thing about the whole M&M saga is not that they did their study, not that they did things wrong (deliberately or by accident), but that neither they nor the journal took the necessary step of investigating whether the difference between their results and yours could be explained simply by some error or set of errors in their use of the data or in their implementation of your method. If it turns out, as looks likely from Mike's investigation of this, that their results are erroneous, then they and the journal will have wasted countless person-hours of time and caused much damage in the climate policy arena.
(2) Given that this is the single worst thing about the saga, we must not go and do exactly the same in rushing out a response to their paper. If some claims in the response turned out to be wrong, based on assumptions about what M&M did or assumptions about how M&M's assumptions affect the result, then it would end up with a number of iterations of claim

and counter claim. Ultimately the issue might be settled, but by then the waters could be so muddied that it didn't matter.

(3) Not only do I advise against an overly rushed response, but I'm also wondering whether it really ought to be only from MBH, for three reasons.

(i) It is your paper/results that are being attacked.

(ii) It is difficult to endorse everything that Mike has put in the draft response because I don't know 100% of the details of MBH and the MBH data. Sure, I can endorse some things, but others I wouldn't know. Sure, I accept Mike's explanation because he's looked at this stuff for 4 days and I believe he'll have got it right - but that's different to an independent check. That must come from Ray or Malcolm if possible.

(iii) If it does come to any independent assessment of who's right and who's wrong, then it would be difficult for us to be involved if we had already signed up to what some might claim to be a knee-jerk reaction to the M&M paper. If that happened, then you would want us to be free to get involved to make sure the process was fair and informed.

This sounds like a cop out, but - like I say - I'm not sure about point (3) so feel free to try to convince me otherwise if you wish. Anyway Keith or Phil may be happy to sign up to a (quick or slow) response, despite my reservations above.

I really advise a very careful reading of M&M and their supplementary website to ensure that everything in the response is clearly correct - precisely to avoid point (2). I've only just started to do this, but already have some questions about the response that Mike has drafted.

(a) Mike, you say that many of the trees were eliminated in the data they used. Have you concluded this because they entered "NA" for "Not available" in their appendix table? If so, then are you sure that "NA" means they did not use any data, rather than simply that they didn't replace your data with an alternative (and hence in fact continued to use what Scott had supplied to them)? Or perhaps "NA" means they couldn't find the PC time series published (of course!), but in fact could find the raw tree-ring chronologies and did their own PCA of those? How would they know which raw chronologies to use? Or did you come to your conclusion by downloading their "corrected and updated" data matrix and comparing it with yours - I've not had time to do that, but even if I had and I

found some differences, I wouldn't know which was right seeing as I've not done any PCA of western US trees myself? My guess would be that they downloaded raw tree-ring chronologies (possibly the same ones you used) but then applied PCA only to the period when they all had full data - hence the lack of PCs in the early period (which you got round by doing PCA on the subset that had earlier data). But this is only a guess, and this is the type of thing that should be checked with them - surely they would respond if asked? - to avoid my point (2) above. And if my guess were right, then your wording of "eliminated this entire data set" would come in for criticism, even though in practise it might as well have been.

(b) The mention of ftp sites and excel files is contradicted by their email record on their website, which shows no mention of excel files (they say an ASCII file was sent) and also no record that they knew the ftp address. This doesn't matter really, since the reason for them using a corrupted data file is not relevant - the relevant thing is that it was corrupt and had you been involved in reviewing the paper then it could have been found prior to publication. But they will use the email record if the ftp sites and excel files are mentioned.

(c) Not sure if you talk about peer-review in the latest version, but note that

they acknowledge input from reviewers and Fred Singer's email says he refereed it - so any statement implying it wasn't reviewed will be met with an easy response from them.

(d) Your quick-look reconstruction excluding many of the tree-ring data, and the verification RE you obtain, is interesting - but again, don't rush into

using these in any response. The time series of PC1 you sent is certainly different from your standard one - but on the other hand I'd hardly say you "get a similar result" to them, the time series look very different (see their fig 6d). So the dismal RE applies only to your calculation, not to their reconstruction. It may turn out that their verification RE is also very negative, but again we cannot assume this in case we're wrong and they easily counter the criticism.

(e) Claims of their motives for selective censoring or changing of data, or for the study as a whole, may well be true but are hard to prove. They would claim that their's is an honest attempt at reproducing a key scientific result. If they made errors in what they did, then maybe they're just completely out of their depth on this, rather than making deliberate errors for the purposes of achieving preferred results.

(f) The recent tree-ring decline they refer to seems related to tree-ring-width not density. Regardless of width of density, this issue cannot simply be dismissed as a solved problem. Since they don't make much of an issue out of it, best just to ignore it.

(g) [I'm rambling now into an un-ordered list of things, so I'll stop soon!] The various other problems relating to temperature data sets, detrended standard deviations, PCs of tree-ring subsets etc. sound likely errors - though I've got no way of providing the independent check that you asked for. But it is again a bit of a leap of faith to say that these *explain* the different results that they get. Certainly they throw doubt on the validity of

their results, but without actually doing the same as them it's not possible to say if they would have replicated your results if they hadn't made these errors. After all, could the infilling of missing values have made much difference to the results obtained, something that they made a good deal of fuss about?

(h) To say they "used neither the data nor the procedures of MBH98" will also be an easy target for them, since they did use the data that was sent to them and seemed to have used approximately the method too (with some errors that you've identified). This reproduced your results to some extent (certainly not perfectly, but see Fig 6b and 6c). Then they went further to redo it with the "corrected and updated" data - but only after first

doing approximately what they claimed they did (i.e. the audit).

These comments relate to random versions of the draft response, so apologies if they don't all seem relevant to the current draft. I don't have these in front of me, here at home, so I'm doing this from memory of what I've read over the past few days. But nevertheless, the point is that a quick response would ultimately require making a number of assumptions about what they did and assumptions about whether this explains the differences or not - assumptions that might be later shot down (in part only, at most, but still sufficient to muddy the debate for most outsiders).

A quick response ought to be limited to something like:

The recent paper by McIntyre and McKittrick (2003; hereafter MM03) claims to be an "audit" of the analysis of Mann, Bradley and Hughes (1998; hereafter MBH98). MM03 are unable to reproduce the Northern

Hemisphere temperature reconstruction of MBH98 when attempting to use the same proxy data and methods as MBH98, though they obtain something similar with clearly anomalous recent warming (their Figure 6c). They then make many modifications to the proxy data set and repeat their analysis, and obtain a rather different result to MBH98.

Unfortunately neither M&M nor the journal in which it was published took the necessary step of investigating whether the difference between their results and MBH98 could be explained simply by some error or set of errors in their use of the data or in their implementation of the MBH98 method. This should have been an essential step to take in a case such as this where the difference in results is so large and important. Simple errors must first be ruled out prior to publication. Even if the authors had not undertaken this by presenting their results to the authors of MBH98, the journal should certainly have included them as referees of the manuscript.

A preliminary investigation into the proxy data and implementation of the method has already identified a number of likely errors, which may turn out to be the cause of the different results. Rather than repeating M&M's failure to follow good scientific practise, we are withholding further comments until we can - by collaboration with M&M if possible - be certain of exactly what changes to data and method were made by M&M, whether these changes can really explain the differences in the results, and eventually which (if any) of these changes can be justified as equally valid (given the various uncertainties that exist) and which are simply errors that invalidate their results.

Hope you find this all helpful, and despite my seemingly critical approach, take them in the spirit with which they are aimed - which is to obtain a strong and hard hitting rebuttal of bad science, but a rebuttal that cannot be buried by any minor inaccuracies or difficult-to-prove claims.

Best regards

Tim

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>, "Keith Briffa" <k.briffa@uea.ac.uk>
Subject: Fwd: Re: McIntyre-McKitrick and Mann-Bradley-Hughes
Date: Fri, 07 Nov 2003 16:12:53 +0000

<x-flowed>

>From: "Sonja.B-C" <Sonja.B-C@hull.ac.uk>
>Date: Fri, 7 Nov 2003 15:58:06 +0000
>To: Steve McIntyre <smcintyre@cgxenergy.com>
>Subject: Re: McIntyre-McKitrick and Mann-Bradley-Hughes
>Cc: L.A.Love@hull.ac.uk, Tim Osborn <t.osborn@uea.ac.uk>,
> Ross McKitrick <rmckitri@uoguelph.ca>
>Priority: NORMAL
>X-Mailer: Execmail for Win32 5.1.1 Build (10)

>Dear Steve

>Please send your material for comment direct to Tim, Osborne. I
>would like to publish the whole debate early next year, but
>'respectful' comments in the meantime can only help and the CRU people
>seem genuinely interested and have integrity. I have never heard of
>such bad behaviour here as appears to have been the case between
>Sallie and Soon and the rest..the US adversarial system and too many
>egos??

>As you know ,the contact is Tim Osborn <t.osborn@uea.ac.uk> and I take
>the liberty to forward this to him now. You seem to suggest that this
>is welcome and are making make direct comments on his remarks to me
>concerning your paper.

>We shall get the printed proof, as a single electronic file today, and
>shall look through it early next week. I am sure you do not want to see
>your paper again? I think that adding anymore now (the exchanges
>between you and Mann/Bradley and perhaps now Tim as well) is premature
>and we shall wait until the next issue. Mann is said to be writing
>something, but he has not yet contacted me, though I just hang up on
>that journalist Appell who keeps on ringing. I told him that I will
>deal only directly with Mann. What cheek, after threatening me with
>litigation...Just keep me in the loop. Thanks.

>Sonja

>PS .By the way The Economist has taken up a previous paper from E&E
>(Castles and Henderson, the social science critique of teh emission

>scenarios), and teh Australian and UK Treasuries have become involved.
>I have not seen it yet. As you know, I have always argued that the real
>'driver' of teh IPCC deception, if that is the right word, has been on
>teh social /technology forcing side, with focus of WG III.
>
>In London I heard two days ago that the WTO might make ratification of
>Kyoto conditional for something Russia wants. The source was speaker
>from the Deutsche Bank, a Justin Mundy, former advisor to the EU
>Commission on EU-Russia coordination and once senior advisor to the
>European Centre for Nature Conservation, he also worked for the World
>Bank.)
>Sonja
>
>On Fri, 7 Nov 2003 09:50:33 -0500
>Steve McIntyre <smcintyre@cgxenergy.com> wrote:
>
>> Dear Sonja,
>>
>>> The interesting thing about their preliminary response, however, is
> that it
>>>> indicates that the difference in results might be fully explained by a
>>>> simple error in not using many of the early tree-ring data. If
> this is
>>>> confirmed by their fuller response, then, even though there may be
> some
>>>> problems with the proxy data used by Mann et al., it implies that
> these
>>>> problems do not actually make a lot of difference to the results -
> the main
>>>> difference comes from omitting the early tree-ring data. A paper that
>>>> identifies some problems with the proxy data used by Mann et al. would
>>>> still be interesting, but if these problems made very little
> difference to
>>>> the results obtained, then it would be of rather minor importance.
>>>
>>> (1) IMHO the data issues rise above "some problems". When you're
> doing a prospectus, audit or engineering-level feasibility study, there
> is a concerted effort to eliminate every error. I have never seen such
> sloppy data as MBH98. Perhaps from my business experience, I am used to
> a more demanding approach to data integrity than the above comment
> suggests about academic studies. Even the MBH response criticizes us for
> failing to use obsolete data. How silly is that. Bradley has also said
> that an "audit" should use original data and should not verify against

> source data and says that I should know better. I think that my
> experience with audits and engineering studies is more substantial than
> Bradley's and this is an extraordinarily silly thing for him to
> say. After the fact, one of the key mis-steps in the Bre-X fraud was
> the engineering report in which ore reserves were calculated using false
> data supplied to the consulting engineers by Bre-X, without any
> verification being carried out by the engineers.

>>> (2) There was not a "simple error" of simply not using many of the
> early tree-ring data. The early tree-ring data in question are principal
> components of North American tree ring sites and of Stahle/SWM (also
> North American) tree ring sites . MBH98 states that they used
> conventional principal components methods for temperature. They do not
> explicitly say that they used conventional principal components methods
> for tree ring regions, but, in the absence of disclosure otherwise, this
> is certainly the most reasonable interpretation of the public disclosure
> (leaving aside Mann's refusal to provide clarification in response to our
> inquiries on methods.) A "conventional" principal component calculation
> requires that there be no missing data. Accordingly this indicator became
> unavailable in the earlier years using conventional principal component
> calculations - it was not "left out". MBH now disclose for the very
> first time that they used a "stepwise principal components approach",
> although this is nowhere disclosed in MBH98 or in the SI thereto. They
> have still not disclosed the rosters of principal components involved. If
> this method is material to their results, as they now state, then it was
> a material omission in their prior disclosure. It seems like a very
> strange rebuttal for MBH to say: you're at fault because we made a
> material non-disclosure on methodology in our papers. If I were in MBH's
> shoes, I would be embarrassed at this non-disclosure and mitigating the
> situation by making full disclosure now. . When you do a prospectus, you
> have to sign an affidavit that there are no material omissions. I have
> approached disclosure questions on the basis that prospectus-level
> disclosure is the minimum level of public disclosure in this matter,
> assuming that this level of disclosure would be exceeded.

>>

>> (3) I've redone calculations with a re-calculated US PC1 in and get
> results similar to those in E&E, rather than the MBH response. This is
> not a guarantee that I have fully replicated still undisclosed MBH
> methodology. However, MBH disclosure of their methodology is very
> inadequate and without full disclosure by MBH of their methods, it is
> possible to be somewhat at cross-purposes. This defective disclosure is
> entirely their responsibility. It should be remedied immediately through
> FTP disclosure of their computer programs and full description of their
> methodology.

>>
>> [snip]
>>
>>>
>>>>>It is quite obvious that if the opinion of these three people
> from the
>>>>>UK University of East Anglia concerning publication of teh M&M paper
>>>>>had been sought and taken, there would not have been no publication.
>>>>
>>>> Then I suggest you read our commentary again, which does not state
> this at all.
>>
>>
>> Part 2 has been drafted and I would be delighted to obtain comments on
> it from UEA/CRU. Indeed, I think that it would be very constructive,
> since Part 2 is significantly more hard-edged than Part 1. Because we
> have stated that we would post up a reply to the MBH response, we would
> have to disclose something on our websites, but I'd be prepared to deal
> with this. Intuitively, full, true and plain disclosure would be to state
> that we have prepared a reply and submitted it to UEA/CRU for
> comments. I think that the many data errors will be self-evident to
> UEA/CRU; we have organized our materials to show this, as will be the
> material non-disclosures on methodology by MBH. However, if they are
> prepared to comment, this would have to be agreed on very quickly as we
> are very close to finalizing our reply.
>>
>> Regards,
>> Steve
>
>-----
>Dr.Sonja Boehmer-Christiansen
>Reader,Department of Geography,
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</x-flowed>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Keith Briffa" <k.briffa@uea.ac.uk>,"Phil Jones" <p.jones@uea.ac.uk>
Subject: Fwd: MBH98
Date: Wed, 12 Nov 2003 11:01:22 +0000

<x-flowed>

Keith and Phil,

you will have seen Stephen McIntyre's request to us. We need to talk about it, though my initial feeling is that we should turn it down (with carefully worded/explained reason) as another interrim stage and prefer to make our input at the peer-review stage.

In the meantime, here is an email (copied below) to Mike Mann from McIntyre, requesting data and programs (and making other criticisms). I do wish Mike had not rushed around sending out preliminary and incorrect early responses - the waters are really muddied now. He would have done better to have taken things slowly and worked out a final response before publicising this stuff. Excel files, other files being created early or now deleted is really confusing things!

Anyway, because McIntyre has now asked Mann directly for his data and programs, his request that *we* send McIntyre's request to Mann has been dropped (I would have said "no" anyway).

So it's just the second bit, that we review part 2 of this response, that needs to be answered.

Cheers

Tim

>From: "Steve McIntyre" <smcintyre@cgxenergy.com>
>To: "Michael E. Mann" <mann@virginia.edu>
>Cc: "Tim Osborn" <t.osborn@uea.ac.uk>,
> "Ross McKittrick" <rmckitri@uoguelph.ca>
>Subject: MBH98
>Date: Tue, 11 Nov 2003 23:39:46 -0500
>
>November 11, 2003
>

>
>
>Professor Michael E. Mann
>
>School of Earth Sciences
>
>University of Virginia

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>
>Dear Professor Mann,

>
>
>We apologize for not sending you a copy of our recent paper ("MM") in
>Energy and Environment for comment, as we understood from your email of
>September 25, 2003 that time constraints prevented you from considering
>our material. We notice that you seem to have subsequently changed your
>mind and hope that you will both be able to clarify some points for us and
>to rectify the public record on other points.

>
>
>1) You have claimed that we used the wrong data and the wrong
>computational methodology. We would like to reconcile our results to
>actual data and methodology used in MBH98. We would therefore appreciate
>copies of the computer programs you actually used to read in data (the 159
>data series referred to in your recent comments) and construct the
>temperature index shown in Nature (1998) ("MBH98"), either through email
>or, preferably through public FTP or web posting.

>
>
>2) In some recent comments, you are reported as stating that we requested
>an Excel file and that you instead directed us to an FTP site for the
>MBH98 data. You are also reported as saying that despite having pointed us
>to the FTP site, you and your colleague took trouble to prepare an Excel
>spreadsheet, but inadvertently introduced some collation errors at that
>time. In fact, as you no doubt recall, we did not request an Excel

>spreadsheet, but specifically asked for an FTP location, which you were
>unable or unwilling to provide. Nor was an Excel spreadsheet ever supplied
>to us; instead we were given a text file, pcproxy.txt. Nor was this file
>created in April 2003. After we learned on October 29, 2003 that the
>pertinent data was reported to be located on your FTP site
><ftp://holocene.evsc.virginia.edu/pub>ftp://holocene.evsc.virginia.edu/pub
>(and that we were being faulted for not getting it from there), we
>examined this site and found it contains the exact same file (pcproxy.txt)
>as the one we received, bearing a date of creation of August 8, 2002. On
>October 29, 2003, your FTP site also contained the file pcproxy.mat, a
>Matlab file, the header to which read: "MATLAB 5.0 MAT-file, Platform:
>SOL2, Created on: Thu Aug 8 10:18:19 2002." Both files contain identical
>data to the file pcproxy.txt emailed to one of us (McIntyre) in April
>2003, including all collation errors, fills and other problems identified
>in MM. It is therefore clear that the file pcproxy.txt as sent to us was
>not prepared in April 2003 in response to our requests, nor was it
>prepared as an Excel spreadsheet, but in fact it was prepared many months
>earlier with Matlab. It is also clear that, had we gone to your FTP site
>earlier, we would simply have found the same data collation as we received
>from Scott Rutherford. Would you please forthwith issue a statement
>withdrawing and correcting your earlier comments.

>

>

>

>3) In reported comments, you also claimed that we overlooked the collation
>errors in pcproxy.txt and "slid" the incorrect data into our calculations,
>a statement which is untrue and made without a reasonable basis. In MM, we
>described numerous errors including, but not limited to, the collation
>errors, indicating quite obviously that we noticed the data problems. We
>then describe how we "firewalled" our data from the errors contained in
>the data you provided us, by re-collating tree ring proxy data from
>original sources and carrying out fresh principal component calculations.
>We request that you forthwith withdraw the claim that we deliberately used
>data we knew to be in error.

>

>

>

>4) On November 8, 2003, when we re-visited your FTP site, we noticed the
>following changes since October 29, 2003: (1) the file pcproxy.mat had
>been deleted from your FTP site; (2) the file pcproxy.txt no longer was
>displayed under the /sdr directory, where it had previously been located,
>although it could still be retrieved through an exact call if one
>previously knew the exact file name; (3) without any notice, a new file

>named "mbhfilled.mat" prepared on November 4, 2003 had been inserted into
>the directory. Obviously, the files pcproxy.mat and pcproxy.txt are
>pertinent to the comments referred to above and we view the deletion of
>pcproxy.mat from the archival record under the current circumstances as
>unjustifiable. Would you please restore these files to your FTP site,
>together with an annotated text file documenting the dates of their
>deletion and restoration.

>
>
>

>5) We note that the new file mbhfilled.mat is an array of dimension
>381x2016. Could you state whether this file has any connection to MBH98,
>and, if so, please explain the purpose of this file, why it has been
>posted now and why it was not previously available at the FTP site.

>
>
>

>6) Can you advise us whether the directory MBH98 has been a subdirectory
>within the folder "pub" since July 30, 2002 or whether it was transferred
>from another (possibly private) directory at a date after July 30, 2002?
>If the latter, could you advise on the date of such transfer.

>
>
>
>
>

>We have prepared a 3-part response to your reply to MM. The first, which
>we have released publicly, goes over some of the matters raised in points
>#2-#5 above. The second is undergoing review. It deals with additional
>issues of data quality and disclosure, resulting from inspection of your
>FTP site since October 29, 2003. The third part will consider the points
>made in your response, both in terms of data and methodology, and will
>attempt a careful reconciliation of our calculation methods, hence the
>necessity of our request in point #1. Thank you for your attention.

>
>
>
>
>

>Yours truly,

>
>
>

>Stephen McIntyre Ross McKitrick

>

>

>

>

>cc: Timothy Osborn

Dr Timothy J Osborn
Climatic Research Unit
School of Environmental Sciences, University of East Anglia
Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk
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web: <http://www.cru.uea.ac.uk/~timo/>
sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

</x-flowed>

From: RichardSCourtney@aol.com
To: t.osborn@uea.ac.uk, m.allen1@physics.ox.ac.uk, Russell.Vose@noaa.gov
Subject: Re: Workshop: Reconciling Vertical Temperature Trends
Date: Sun, 23 Nov 2003 18:42:59 EST
Cc: trenbert@cgd.ucar.edu, timo.hameranta@pp.inet.fi,
Thomas.R.Karl@noaa.gov, ceforest@mit.edu, sokolov@mit.edu,
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kenc@llnl.gov, klaus-p-heiss@msn.com, kump@geosc.psu.edu,
thompson.3@osu.edu, jacobson@stanford.edu, claussen@pik-potsdam.de,
m.manning@niwa.cri.nz, marty.hoffert@nyu.edu, mike.bergin@ce.gatech.edu,
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rodolfo@dge.inpe.br, olavi@aai.ee, ocanz@ciudad.com.ar, air@mpch-
mainz.mpg.de, pdoran@uic.edu, p.jones@uea.ac.uk,
tpatters@ccs.carleton.ca, rmyneni@crsa.bu.edu, rasmus.benestad@met.no,
rbradley@geo.umass.edu, anthes@ucar.edu, robert.sausen@dlr.de,
shs@leland.stanford.edu, wofsy@fas.harvard.edu, smenon@giss.nasa.gov,
ssolomon@al.noaa.gov, tbarnett@ucsd.edu, ulrich.berner@bgr.de,
cubasch@zedat.fu-berlin.de, Uli.Neff@iup.uni-heidelberg.de,
vramanathan@ucsd.edu, vr@gfdl.noaa.gov, broecker@ldeo.columbia.edu

Dear All:

The excuses seem to be becoming desperate. Unjustified assertion that I fail to understand

"Myles' comments and/or work on trying the detect/attribute climate change" does not stop

the attribution study being an error. The problem is that I do understand what is being

done, and I am willing to say why it is GIGO.

Tim Allen said;
In a message dated 19/11/03 08:47:16 GMT Standard Time,
m.allen1@physics.ox.ac.uk writes:

I would just like
to add that those of us working on climate change detection and
attribution
are careful to mask model simulations in the same way that the
observations
have been sampled, so these well-known dependencies of nominal
trends on the
trend-estimation technique have no bearing on formal detection and
attribution results as quoted, for example, in the IPCC TAR.

I rejected this saying:
At 09:31 21/11/2003, RichardSCourtney@aol.com wrote:
>It cannot be known that the 'masking' does not generate additional
>spurious trends. Anyway, why assume the errors in the data sets are
>geographical and not?. The masking is a 'fix' applied to the model
>simulations to adjust them to fit the surface data known to contain
>spurious trends. This is simple GIGO.
Now, Tim Osborn says of my comment;
In a message dated 21/11/03 10:04:56 GMT Standard Time,
t.osborn@uea.ac.uk writes:

Richard's statement makes it clear, to me at least, that he
misunderstands
Myles' comments and/or work on trying the detect/attribute climate
change.
As far as I understand it, the masking is applied to the model to
remove
those locations/times when there are no observations. This is quite
different to removing those locations which do not match, in some
way, with
the observations - that would clearly be the wrong thing to do. To
mask
those that have no observations, however, is clearly the right thing
to do
- what is the point of attempting to detect a simulated signal of
climate
change over some part of (e.g.) the Southern Ocean if there are no
observations there in which to detect the expected signal? That
would
clearly be pointless.

Yes it would. And I fully understand Myles' comments. Indeed, my
comments clearly and
unarguably relate to Myles comments. But, as my response states,
Myles' comments do not
alter the fact that the masked data and the unmasked data contain
demonstrated false
trends. And the masking may introduce other spurious trends. So, the
conducted

attribution study is pointless because it is GIGO. Ad hominem insults don't change that.

And nor does the use of peer review to block my publication of the facts of these matters.

Richard

From: "Janice Darch" <J.Darch@uea.ac.uk>
To: <env.faculty@uea>, <env.researchstaff@uea>
Subject: Towards a Sustainable Energy Economy deadline
Date: Wed, 7 Jan 2004 10:35:14 -0000

Dear All,
Is any one involved in proposals for this initiative?

Please let me know.
Janice

First call for research proposals
A call for expressions of interest for participation in Consortia, Research
Groups, Networks, Collaborative Proposals and Capacity Building
Closing date: 5pm, Monday 19 January 2004

Intending applicants should note that all those receiving funding from this
programme will be expected to collaborate with the UK Energy Research Centre
following its establishment on 1st April 2004.

Introduction

The Towards a Sustainable Energy Economy programme (TSEC) is aimed at
enabling the UK to access a secure, safe, diverse and reliable energy supply
at competitive prices, while meeting the challenge of global warming. The
Engineering and Physical Science Research Council (EPSRC), Economic and
Social Research Council (ESRC) and Natural Environment Research Council
(NERC) jointly have funding of £28 million for the programme, which is
co-ordinated by NERC on behalf of the three Research Councils, with
participation from the Biotechnology and Biological Sciences Research
Council (BBSRC) and Council for the Central Laboratory of the Research
Councils (CCLRC). The Councils are advised on the use of the programme's
funds by the TSEC Scientific Advisory Committee.

TSEC is an interdisciplinary research programme that will adopt whole
systems integrated approaches. The Research Councils' working definition of
'a whole systems approach' is: "A whole systems integrated methodology
demanding a truly interdisciplinary approach that facilitates the joint
working of engineering, technological, natural, environmental, social and
economic scientists to tackle fundamental issues (such as sustainable
energy)." A whole systems approach should ensure that new work carried out
complements current and planned activities of the individual Research
Councils in the area concerned and will take into account known

understanding for the issues addressed.

The TSEC programme will provide a focus for, but will not be the only source of, energy research in the UK. As such, the TSEC programme will aim to make an impact on UK energy research by promoting this whole systems approach. Proposers wishing to carry out research under TSEC should familiarise themselves with the role of TSEC in the energy research landscape, as described in Annex 1.

What research will TSEC support?

Up to £12 million of the programme's funding will be used to establish the UK Energy Research Centre (UKERC) by 1st April 2004, for which the Councils have already invited full proposals. The Centre's two major activities will be its own research programme and the co-ordination of a National Energy Research Network.

The remainder of the TSEC programme's funds (at least £16 million) will be used to support research that will operate independently of, but complementary to, the research done by UKERC. Calls for proposals will be broadly under the following themes:

- .
carbon management

- .
nuclear power

- .
renewable energy

- .
managing new uncertainties.

In keeping with the whole systems approach of the programme, applications are invited from all disciplines that have a research interest in any of the themes (eg the environmental, social, economic and technological aspects of nuclear power).

What areas are covered in this call?

This first call covers all aspects of the TSEC programme but the Research

Councils wish to focus initially on two of the themes: nuclear power and managing new uncertainties. It is anticipated that a further call focused in particular on the other two themes - carbon management and renewable energy - will be issued in mid-2004.

The present call invites expressions of interest for participation in:

- Consortia under the theme Nuclear Power - Keeping the nuclear option open

- Research Groups under the theme Managing new uncertainties - The socio-economic challenges and implications of moving towards a sustainable energy economy

- Expressions of interest for Networks and Collaborative proposals will also be considered, under either of the themes Carbon management and Renewable energy.

- Expressions of Interest for preparation for projects (Capacity Building) will also be considered under any of the areas except Nuclear power.

The key features of Consortia, Research Groups, Networks, Collaborative Proposals and Capacity Building are described in the Application Process.

Consortium bids: Nuclear power - Keeping the Nuclear Option Open
The research challenges in fission R&D span areas as diverse as maintaining and extending the life of existing generation plant; management of the current and future fission waste legacy; technology for future fission power generation; and research that can contribute to an open and informed debate on the current and future role for nuclear power in the UK's energy supply industry. The scope of this theme has been broken down into three main topics:

- maintaining current generation capacity

fission within a sustainable energy economy

·
future fission power.

The sponsors intend to commission one or more large, integrated, multidisciplinary projects that can address the research challenges, with the scope of projects potentially cutting across the three topics.

Further details on the scope of the theme and consortia requirements can be found in Annex 2.

Research Group bids: Managing new uncertainties - The Socio-Economic Challenges and Implications of Moving Towards a Sustainable Energy Economy

The aim of this theme is to facilitate research on the cross-cutting socio-economic challenges and implications of moving towards a sustainable energy economy and their interactions with broader technological, engineering, and environmental issues. It offers opportunities for productive, interdisciplinary research within and beyond the socio-economic field, with the potential to contribute to the development of whole-systems approaches to energy issues. Many of the potential research issues have resonance in a number of other areas of public policy and are not specific to energy. In line with the aims of the programme, this theme is not constrained by traditional disciplinary or Research Council boundaries, whilst focusing on the socio-economic research agenda. Although a number of the proposed topics and questions focus on UK and European issues, many are generic and could be applied to both OECD and developing country contexts.

Possible topics identified under this theme include:

·
Processes of long-run change in socio-technical systems

·
Vulnerability, resilience and adaptiveness

·
Services, systems of provision and consumption practices

·
Policies in natural monopolies and liberalised markets

Public attitudes and processes of governance

Energy in the global context

Integrated appraisal of energy systems.

This framework should be regarded as illustrative, not definitive. Researchers are encouraged to define and justify alternative topics and questions which would contribute towards the TSEC programme's overall objectives.

More detail on this theme can be found in Annex 3.

Expressions of Interest under the themes Carbon management and Renewable energy will be considered in this call. However, the following brief indication of the scope of these two themes is given for initial guidance only; a detailed scope will be provided in the next call, expected to be mid 2004.

Carbon management

Conventional energy research is often vertically divided, so that research looks at the use of individual fuels, or energy use in particular industrial, commercial or domestic sectors. There needs to be more "cross-boundary" and "whole systems" research, looking at how different technologies and social/environmental factors might be optimised to deliver the overall objectives. The following are two examples of the type of issues which should be addressed.

Fuel switching and renewables

Displacing coal and petroleum with natural gas and/or biogas, or biofuels, or renewables are alternative ways of reducing carbon dioxide (CO₂) emissions. These options require a full whole lifecycle approach to carbon management, integrating environmental, engineering, resource, economic and social dimensions. Issues such as length and type of supply chains, emissions associated with agriculture, fuel processing, infrastructure and construction need to be fully understood to limit the risk that emissions are increased or displaced to another part of the energy/resource chain.

Carbon dioxide capture and storage

The continued use of fossil fuels will demand effective carbon management, particularly through reduction of the associated CO₂ emissions. The greatest long-term potential for reduced CO₂ emissions to the atmosphere from fossil fuels is likely to be through capturing CO₂ from large industrial point sources before it enters the atmosphere, and then sequestering it back into the bio/geosphere by geological means. The research challenges include: the mechanisms of large-scale carbon capture at source, CO₂ storage, transport and distribution, and geological sequestration, monitoring and verification technologies as well as modelling the long term fate of CO₂ injected into a variety of geological scenarios. Understanding is also needed of the potential risk posed by CO₂ leakage into terrestrial and marine settings, and of the economic risks, costs and benefits, public acceptability and regulatory issues associated with moving towards large-scale CO₂ capture.

Renewable energy

The objectives for TSEC in this area will centre on work that supports the development of renewable and sustainable energy systems of relevance to the UK economy. Specifically, it will: encourage the introduction of renewable and sustainable energy systems into the UK economy; encourage consideration of renewable energy in the context of social/economic/environmental issues and carbon management; and provide data for the development of policy. TSEC will fund research that is complementary to that supported through other Research Council activities, such as the ongoing Sustainable Power Generation and Supply Programme (SUPERGEN). Again, the following is purely an example of the type of research which could be funded.

Carbon cycle audits

Audits of full lifecycle carbon (or carbon equivalents of other greenhouse gases emitted in the lifecycle) need to be undertaken, and the energy balances of different renewable energy generating technologies need to be considered and understood, if true impacts on carbon reduction are to be achieved. For example, if energy crops are to be encouraged, then consequences on land use change, aquifer recharge, and rainfall run off need to be fully understood. It would also be important to ensure that the crops are 'low-input' in terms of energy usage and that the energy balance is therefore positive. Environmental impacts of growing energy crops would have to be compared with the alternative land use (food crops, set-aside, etc)), and consideration given to their potential economic and social impacts.

Risks, barriers and incentives in renewables innovation

Innovation will be essential in the renewables industry if the sector is to

play a central role in future energy supply. Research is required to understand and quantify the risks inherent in the development of new technology and the barriers preventing its exploitation to inform both the priorities of future renewable energy R&D and the development of future market instruments and incentives that can encourage the effective management of risk and enable the exploitation of the outputs of R&D. In the longer term, new disruptive technology may significantly affect the operation of the energy market, and research is required to investigate how incentives and market instruments can adapt to changing market conditions while still providing a long term framework within which companies can make capital investments requiring a return on capital over long (20-30 year) timescales. (In addition to research on such issues relating specifically to renewables there are opportunities for broader cross-cutting research on these issues under the Managing New Uncertainties Theme).

The Application Process

The schemes and theme areas under which EoIs will be accepted in this call are highlighted in colour in the table below.

Nuclear power
Managing new uncertainties
Carbon management
Renewable energy
Consortia
Research groups
Networks
Collaborative proposals
Capacity building

Characteristics of the schemes

Consortium

A Consortium will comprise a number of academic groups, normally from different disciplines and institutions, working in partnership with appropriate stakeholders and users to design and deliver a collaborative programme of world-class research. It is expected that the consortium will deliver higher quality research outputs than groups working in isolation. This call for expressions of interest is open to all potential partners of a research consortium, irrespective of their existing links to academic research in the field. Consortia may be funded at a value of up to £5m. Expressions of interest can be submitted by individuals, existing groups, and existing or new collaborations. However, where expressions of interest are made by a group or collaboration, the Research Councils reserve the right to take forward those expressions in total or in part during the Consortium-building process, potentially excluding elements of proposed

collaborations.

Research Groups

A Research Group will be a national focal point for research where researchers can collaborate on long-term inter-disciplinary projects. It will facilitate the building of strong relationships with research users, international collaboration and the development of the careers of new and outstanding researchers.

Funded initially for five years, Research Groups will be expected to provide the training for postgraduate students and other new researchers where appropriate, and to improve opportunities for securing co-funding or sponsorship from sources outside the Science Vote. Applications for Research Group funding will normally be expected to be in the range of £200k - £600k per annum although applications outside this range can be considered.

Networks

A major task of UKERC will be to co-ordinate a National Energy Research Network that will draw in all significant research activities. However, once the components of this network are known, the TSEC programme will wish to support new research 'nodes' that complement them. Such complementary activities would normally be UK-based networks that link research groups and industrial organisations, across disciplines, to develop new or enhanced collaborations.

Collaborative Proposals

These will be intended to support focussed, co-ordinated, collaborative research into specific issues and will be expected to enhance opportunities for inter-disciplinary collaboration. A minimum of three eligible institutions are required for a proposal under this scheme, each of which will be separately awarded funds. The consortium will retain ownership and management of the science programme, and a lead institution will be expected to act as co-ordinator.

Collaboration awards will provide funding for up to five years with costs ranging, as required by the research, from modest sums up to approximately £2M. Proposals may include tied research studentships.

Proposers are free to submit expressions of interest for one or more themes.

Capacity building

For projects that require considerable preparation, applicants may submit an Expression of Interest for capacity building, to a maximum of £50k, for:

support for a researcher to work in a different science department for a period of up to 12 months (eg for a natural scientist to work in a social science department);

support for an overseas researcher to work in a UK institution, or for a UK researcher to work in an overseas institution, for up to 12 months focusing on interdisciplinary research issues;

support for a series of four or more interdisciplinary events (involving social and natural scientists) over a 12 month period;

scoping studies, focusing on any of the TSEC themes. Applicants must demonstrate the interdisciplinary nature of the proposed research. Awards may be up to 12 months in duration

Eligibility

Standard Research Council eligibility criteria will apply to this call; those normally eligible to participate in any Research Council programme can apply. Research Council funding can only be awarded to UK universities, Research Council institutes, Government Research Establishments and not-for-profit research organisations. Organisations and industry which are themselves ineligible for receipt of Research Council funding may participate, using their own cash or in-kind support.

Applications from members of the public or individuals outside academia will not be accepted.

Academic expressions of interest may be submitted by leaders of individual research groups within one or more universities. While existing groups of researchers are able to apply as a team, it should be recognised that the Research Councils may recommend the building of new partnerships involving only a minority of members from existing collaborations. Where there is scope to do so, it is recommended that individuals submit their own expression of interest on behalf of their group.

The Selection Process

An initial sift of EoIs will be conducted by expert panels established by the Programme Scientific Advisory Committee or by the SAC. Applications will be judged on their quality, innovation, originality and compliance with the objectives of the programme.

Quality - The proposal should indicate clear potential to support innovative and high quality research of international standing and include information on the capacity and track record of the applicants in delivering such high quality research. This should not rely on publication lists, but present evidence of recognised first-class research, innovation and collaboration.

Innovation - The proposal should present novel approaches to current research challenges and persuasive approaches to roadmap solutions. This should be in the context of the research theme defined in the technical appendix.

Originality - The proposal should demonstrate innovative approaches to problem solving with evidence of ability, creativity and vision and added value to current research in the field. The application should be focused toward addressing research challenges of the theme.

Objectives - The applicant should communicate an enthusiasm for collaboration and ability to contribute to a programme of research that delivers the objectives of the TSEC programme. They should demonstrate awareness of the drivers affecting the research agenda and the potential to contribute to the development of whole-systems approaches to energy issues.

Applicants for consortia will be informed of the outcome of their bids in January 2004 and if successful will be invited to a workshop in March 2004 to facilitate the formation of consortia partnerships. Attendance at the workshops will be mandatory for consortium members, including users and industrial collaborators. Following the workshops, consortium partners will be invited to submit EPSRC grant applications, which will be subject to rigorous peer review.

Applicants for Research Groups will be informed of the outcome of their bids by mid-March 2004 and if successful invited to submit full proposals by

mid-June. Assessment of full proposals will entail applicants being interviewed by the assessment panel in September/October 2004.

All other applicants will be informed of the outcome of their bids in February 2004 and successful applicants invited to submit full proposals as appropriate.

How to Apply

Expressions of Interest

Expressions of Interest must be submitted using the Research Councils' joint application form (available in Word or PDF versions) and (with the exception of proposals for Research Groups on Managing the New Uncertainties - see below) be accompanied by no more than four sides of A4 text (minimum font 12 pt), including diagrams, figures and charts etc. in support of the application. This should include any relevant information that will assist assessment of the project that is not covered in the sections of the application form. It should include

- Details of the track record of the applicant or business and the particular qualities they would bring to the proposal.

- Identification of the broad challenge which the applicant would seek to address or to which they would be able to contribute

- Definition of the perceived key research challenges within the theme.

- Indication of potential deliverables.

- Information on the collaborating organisation in terms of cash or in-kind support and proposed benefits from collaboration.

Expressions of interest for Research Groups under the 'Managing the New Uncertainties' theme must be submitted using the Research Councils joint application form. However instead of the four sides outlined above the form should be accompanied by the following information:

A research proposal of no more than 3,000 words outlining the main proposed elements of the proposed Group's research programme and how this would contribute towards the achievement of the objectives of the Towards a Sustainable Energy Economy Programme

Plus the following appendices:

- no more than 1 side of A4 (minimum font 12 pt) providing details of references cited in the research proposal

- no more than 1 side of A4 (minimum font 12 pt) giving details of the proposed strategies for involving non-academic users at all stages and outlining the potential for collaboration and/or co-funding

- no more than two sides of A4 (minimum font 12 pt) outlining the proposed management structure of the Research Group, including time commitments of the proposed Director(s) and abbreviated cvs for all named applicants.

- no more than one side of A4 (minimum font 12 pt) outlining the Group's strategy for contributing to the development of inter-disciplinary research capacity in the field.

In section E of the form, under Scheme applicants should state Consortium, Centre Group, Network, Collaborative proposal, or Capacity building, as appropriate; and under Call should insert 'TSEC call 1': followed by the appropriate theme name: Nuclear; Managing new uncertainties; Carbon Management, or Renewable energy.

As the majority of institutions have not yet registered with the Research Councils for electronic submission, in this call electronic submissions cannot be accepted. An original plus ONE copy are required in hard copy. Faxed copies are not acceptable.

All applications should be submitted to reach the NERC at the address below no later than 5pm on 19th January 2004. Personal callers may deliver applications during normal office hours only (9am - 5pm Monday - Friday). The Research Councils will reject late or incomplete submissions and those that do not comply with the application criteria set out above.

Receipt of applications will be acknowledged after the closing date. It will assist administration of the call if applicants do not telephone to enquire if their proposal has been received.

Applications and administrative queries should be addressed in the first instance to:

Dr Chris Baker (e-mail preferred)
Programme Co-ordinator
Science and Innovation Programmes
NERC, Polaris House, North Star Avenue
SWINDON, Wiltshire SN2 1EU.
Telephone 01793 411758.

Queries regarding the technical aspects of the Nuclear Power theme should be addressed to: Dr Peter Hedges, EPSRC, telephone 01793 444176. Queries regarding the application criteria or eligibility for the Nuclear Power theme should be addressed to the Associate Programme Manager Mr Robert Heathman, Room GFN, EPSRC, telephone 01793 444131.

Queries regarding the application criteria or eligibility for the Managing New Uncertainties theme should be addressed to Mr Paul Rouse, Senior Science and Development Manager, Research Training and Development Directorate (RTD), ESRC, at the above address, telephone 01793 413030, or Mr Oliver Moss, Science and Development Manager, RTD, ESRC, telephone 01793 413064.

All other queries should

Dr. J.P. Darch
Research Administrator
School of Environmental Sciences
University of East Anglia
Norwich
NR4 7TJ
U.K.

Tel : 44 (0)1603 592994

Fax : 44 (0)1603 593035

Attachment Converted: up151.gif: 00000001,00000001,00000000,00000000

From: Jan Esper <esper@wsl.ch>
To: Briffa Keith <k.briffa@uea.ac.uk>, Cook Ed <drdendro@ldeo.columbia.edu>
Subject: EOS revision
Date: Mon, 12 Jan 2004 10:26:27 +0100

<x-flowed>

Hi Ed and Keith

for your information, I attached the revision of the EOS article. In this version we added some lines about the data-overlap between the MBH and ECS records.

I also attached a figure showing a comparison between MBH and EsperFULL (using all data) and EsperSUB (without Tornetraesk and the Polar Urals).

Take care

Jan

--

Dr. Jan Esper
Swiss Federal Research Institute WSL
Zuercherstrasse 111, 8903 Birmensdorf
Switzerland
Phone: +41-1-739 2510
Fax: +41-1-739 2215
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</x-flowed>

Attachment Converted: "c:\eudora\attach\!Low_and_High_rev.pdf"

Attachment Converted: "c:\eudora\attach\Figure1.eps.pdf"

Attachment Converted: "c:\eudora\attach\Response_Figure.eps.pdf"

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: CLIMATIC CHANGE needs your advice - YOUR EYES ONLY !!!!!
Date: Fri Jan 16 13:25:59 2004

Mike,

This is for YOURS EYES ONLY. Delete after reading - please ! I'm trying to redress the balance. One reply from Pfister said you should make all available !! Pot calling the kettle

black - Christian doesn't make his methods available. I replied to the wrong Christian message

so you don't get to see what he said. Probably best. Told Steve separately and to get more

advice from a few others as well as Kluwer and legal.

PLEASE DELETE - just for you, not even Ray and Malcolm

Cheers

Phil

Date: Fri, 16 Jan 2004 12:37:29 +0000

To: Christian Azar <christian.azar@fy.chalmers.se>, christian.pfister@hist.unibe.ch

From: Phil Jones <p.jones@uea.ac.uk>

Subject: Re: AW: CLIMATIC CHANGE needs your advice

Cc: "'David G. VICTOR'" <dgvictor@stanford.edu>, 'Katarina Kivel' <kivel@stanford.edu>, N.W.Arnell@soton.ac.uk, firtca@fy.chalmers.se, d.camuffo@isac.cnr.it, scohen@sdri.ubc.ca, pmfearn@inpa.gov.br, jfoley@facstaff.wisc.edu, pgleick@pipeline.com, harvey@geog.utoronto.ca, ahs@ansto.gov.au, Thomas.R.Karl@noaa.gov, rwk@ucar.edu, rik.leemans@rivm.nl, diana.liverman@eci.ox.ac.uk, mccarl@tamu.edu, lindam@atd.ucar.edu, rmoss@usgcrp.gov, ogilvie@spot.colorado.edu, barrie.pittock@dar.csiro.au, pollard@essc.psu.edu, nj.rosenberg@pnl.gov, crosenzweig@giss.nasa.gov, j.salinger@niwa.co.nz, santer1@lnl.gov, h.j.schellnhuber@uea.ac.uk, F.I.Woodward@sheffield.ac.uk, gyohe@wesleyan.edu, leonid@atmosph.physics.utoronto.ca, shs@stanford.edu

Dear Steve et al,

I've been away this week until today. Although the responses so far all make valid points, I

will add my thoughts. I should say I have been more involved in all the exchanges between

Mike and MM so I'm probably biased in Mike's favour. I will try and be impartial, though, but

I did write a paper with Mike (which came out in GRL in Aug 2003) and we currently have a long paper tentatively accepted by Reviews of Geophysics. With the latter all 4

reviewers

think the paper is fine, but the sections referring to MM and papers by Soon and Baliunas

are not and our language is strong. We need to work on this.

Back to the question in hand:

1. The papers that MM refer came out in Nature in 1998 and to a lesser extent in GRL in

1999. These reviewers did not request the data (all the proxy series) and the code. So, acceding to the request for this to do the review is setting a VERY dangerous precedent.

Mike has made all the data series and this is all anyone should need. Making model code available is something else.

2. The code is basically irrelevant in this whole issue. In the GRL paper (in 2003 Mann and Jones), we simply average all the series we use together. The result is pretty much the same as MBH in 1998, Nature and MBH in 1999 in GRL.

3. As many of you know I calculate gridded and global/hemispheric temperature time series

each month. Groups at NCDC and NASA/GISS do this as well. We don't exchange codes - we do occasionally though for the data. The code here is trivial as it is in the paleo work.

MBH get spatial patterns but the bottom line (the 1000 year series of global temps) is almost the same if you simply average. The patterns give more, though, when it comes to trying to understand what has caused the changes - eg by comparison with models. MM are only interested in the NH/Global 1000-year time series - in fact only in the MBH work

from 1400.

4. What has always intrigued me in this whole debate, is why the skeptics (for want of a better term) always pick on Mike. There are several other series that I've produced, Keith Briffa has and Tom Crowley. Jan Esper's work has produced a slightly different series

but we don't get bombarded by MM. Mike's paper wasn't the first. It was in Nature and is well-used by IPCC. I suspect the skeptics wish to concentrate their effort onto one person as they did with Ben Santer after the second IPCC report.

5. Mike may respond too strongly to MM, but don't we all decide not to work with or co-operate with people we do not get on with or do not like their views. Mike will say that MM are disingenuous, but I'm not sure how many of you realise how vicious the attack on him has been. I will give you an example.

When MM came out, we had several press calls (I don't normally get press calls about my papers unless I really work at it - I very rarely do). This was about a paper in E&E, which when we eventually got it several days later was appalling. I found out later that the authors were in contact with the reviewers up to a week before the article

appeared. So there is peer review and peer review !! Here the peer review was done by

like-minded colleagues. Anyway, I'm straying from the point. Tim Osborn, Keith Briffa and I felt we should put something on our web site about the paper and directs people to Mike's site and also to E&E and the MM's site. MM have hounded us about this for the last four months. In the MM article, they have a diagram which says 'corrected version' when comparing with MBH. We have seen people refer to this paper (MM) as an alternative reconstruction - yet when we said this is our paragraph MM claim they are not putting forward a new reconstruction but criticizing MBH 1998 !! We have decided to remove the sentence on our web page just to stop these emails. But if a corrected version isn't a new or alternative reconstruction I don't know what is.

So, in conclusion, I would side with Mike in this regard. In trying to be scrupulously

fair, Steve, you've opened up a whole can of worms. If you do decide to put the Mann response into CC then I suspect you will need an editorial. MM will want to respond also.

I know you've had open and frank exchanges in CC before, but your email clearly shows that you think this is in a different league. MM and E&E didn't give Mann the chance to

respond when they put their paper in, but this is a too simplistic. It needs to be pointed

out in an editorial though - I'm not offering by the way.

I could go on and on

Cheers

Phil

At 10:36 15/01/2004 +0100, Christian Azar wrote:

Dear all,

I agree with most of what has been said so far. Reproducibility is the key word. If the Mann et al material (to be) posted on the website is sufficient to ensure reproducibility, then there is no compelling need to force them to hand it out. If not, then the source code is warranted. Also, even if there is no compelling need to make the source code public, doing it anyway would clearly be beneficial for the entire debate.

Yours,

Christian

Christian Azar

Professor

Department of physical resource theory

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Göteborg University

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ph: ++46 31 772 31 32

[1]www.frt.fy.chalmers.se

[2]www.miljo.chalmers.se/cei

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References

1. <http://www.frt.fy.chalmers.se/>
2. <http://www.miljo.chalmers.se/cei>

From: Edward Cook <drdendro@ldeo.columbia.edu>
To: "Art Johnson" <ahj@sas.upenn.edu>
Subject: RE: Seminar
Date: Sat, 17 Jan 2004 07:55:24 -0500
Cc: druid@ldeo.columbia.edu, druidrd@ldeo.columbia.edu, k.briffa@uea.ac.uk

<x-flowed>

Hi Art,

Sorry for the lack of response to your emails. Been over the top as usual on things. I go off to Tasmania and New Zealand on Jan 20 and return on Feb 15. Bhutan was a bit strange this time. I was sick most of the time, but we did get some useful stuff done nonetheless.

>Hi Ed,

>

>I hope your trip to Bhutan went well. We did OK in Chile but encountered
>some glitches. I am emailing about a three things to see if you are
>interested:

>

>1) What does Gordon know about the big white spruce in the Mackenzie R.
>basin of the northern NWT? I am going to be in Alberta this summer and it is
>one plane ride and a few hundred \$ from those big spruce. If I can get the
>cores, are you interested in collaborating on their measurement and
>analysis? If I can track down the person that told us that some of the trees
>were 600 y old, we might be able to find some of them. There are many spruce
>pilings in town that were probably cut in the 50's-70's and some of those
>might have been pretty old trees given their size. What is the availability
>of climate data? Inuvik probably has records back into the 50's when they
>rebuilt the town. Dick Jagels is interested in those trees too, as we are
>led to believe that they need 24 hr photoperiods when they are seedlings.
>Could this be a race of trees that respond to differences in growing-season
>sunlight?

I am cc'ing this email to Gordon and Rosanne. I think that they would be interested in what you describe. They also know what climate data are available. I recall that Aklavik has a older record that was discontinued a few years back. It may be possible to merge Aklavik with Inuvik temperature records to cover most of the 20th century.

>

>2) The Forest Service has an RFP out for projects in the "northern forest"

>I think this is defined as mostly Vermont and New Hampshire since it is a
>Senate-funded program sponsored by senators from those states. The "threat"
>(their term) of global warming to forest health is one of the themes that
>Chris Eagar is in charge of. We have been working with Vermont northern
>hardwood data collected by Post and Curtis in the 1950's and redone by us in
>the early 90's. There is a very nice multiple regression model that shows
>clearly that temperature (altitude/latitude) and soil moisture are very good
>predictors of site index (height at 75 yrs. e.g. productivity potential).
>Nutrients do not explain any additional variance. This model would suggest
>that warming would improve productivity, not decrease it. I am wondering if
>a dendroclimatological analysis of maple, beech and ash and yellow birch
>>would show a response of growth to summer temperatures? I think we have all
>the cores from our 1990 study, and it would be an easy matter to get more. I
>still owe the Forest Service a couple of papers from the 90-91 work which
>they funded, but I am actually working on them now, and could have them done
>by the March 30 deadline for the full proposal, if not for the Feb. 13
>preproposal deadline. I'm sure I could talk to Chris to see if our ideas are
>viable, and if we would be penalized for not publishing the Vermont stuff in
>a timely manner.

This sounds interesting. Are you measuring up all of the tree cores?
I wouldn't have the resources to do that without some technician
support, but I could participate in some dendroclimatic analyses of
the data with you.

>
>3) We are running cellulose O reasonably well at this time, and are still
>interested in seeing if cellulose O is useful in determining whether the
>temperature signal in mideval wood is similar to that of the past century,
>and if there is an isotopic signature in the Little Ice Age wood that
>indicates it was cold. What do you think about the availability of wood
>samples from dated rings from those periods? Is any of the Esper wood
>available? When we talked after your seminar, it seemed to me that the
>Scandanavian wood collection might be useful.

I did ask Keith Briffa about this stuff. He is tied in closely with
much of the work that has been done in Fennoscandia and even over to
the Polar Urals. He also said that there has been some isotopic work
done on wood, but he wasn't sure about results. I suggest that you
contact Keith directly (k.briffa@uea.ac.uk) and maybe he can direct
you to sources of wood for your proposed study. It is interesting, if
a bit chancy in my estimation.

Cheers,

Ed

>
>
>What do you think?

>
>Art

>
>
>
>
>-----Original Message-----

>From: Edward Cook [mailto:drdendro@ldeo.columbia.edu]
>Sent: Saturday, October 11, 2003 2:28 PM
>To: Art Johnson
>Subject: RE: Seminar

>
>
>Hi Art,

>
>I will be driving down to your digs on Friday, Oct 17 to give the
>seminar I promised. When is it scheduled so I know how early I
>definitely have to leave. I need directions to get there as well, as
>I have never been to Penn before. Also, it would be useful to have a
>place to stay Friday night, I suppose. My wife is off to CT to
>celebrate a 50th birthday with a friend that weekend, so there is no
>point in zipping back in any case.

>
>Cheers,

>
>Ed

>--
>=====

>Dr. Edward R. Cook
>Doherty Senior Scholar and
>Director, Tree-Ring Laboratory
>Lamont-Doherty Earth Observatory
>Palisades, New York 10964 USA
>Email: drdendro@ldeo.columbia.edu
>Phone: 845-365-8618
>Fax: 845-365-8152

>=====

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=====

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=====

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From: Keith Briffa <k.briffa@uea.ac.uk>

To: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>,"Michael E. Mann" <mann@virginia.edu>

Subject: Re: J. Climate paper - in confidence

Date: Tue, 20 Jan 2004 09:45:44 +0000

Cc: Scott Rutherford <srutherford@rwu.edu>

<x-flowed>

Malcolm seems to have done a good job sorting out these constituent sets , and I don't have anything to add other than agreeing that as a general principal , where possible, original chronologies should be used in preference to reconstructed temperature series (the latter having been already optimized using simple or multiple regression to fit the target temperature series). This applies not only to our western US reconstructions (which it should be stressed are based on very flexible curve fitting in the standardisation - and inevitably can show little variance on time scales longer than a decade or so) but also to the Tornetrask and Polar Urals reconstructions (each of which was based on ring width and density data , but standardised to try to preserve centennial variability - though the density series had by far the largest regression coefficients). There is though a question regarding the PCs of the Siberian network (presumably provided by Eugene?) . The correlation between density and ring width can get high in central and eastern parts of the network , so even though these are different variables , it might not be strictly true to think of them as truly independent (statistically) of the density chronologies we use from the Schweingruber network (there may also be a standardisation issue here , as the density chronologies were standardised with Hugershoff functions for our initial network work (as reported in the Holocene Special Issue) whereas your PC amplitudes may be based on "Corridor Standardisation" - which likely preserves less low frequency?) . These remarks are simply for clarification and discussion , and I too will wait on your response draft , though I would throw in the pot the fact that omitting the time dependent stuff would simplify the message at his stage. cheers

Keith

At 01:42 PM 1/19/04 -0700, Malcolm Hughes wrote:

>Mike - there are the following density data in that set:

>1) 20 Schweingruber/Frttss series from the ITRDB (those that

>met the criteria described in the Mann et al 2000 EI paper)

>2) Northern Fennoscandia reconstruction (from Keith)

>3) Northern Urals reconstruction (from Keith)

>4) 1 density series for China (Hughes data) and one from India

>(also Hughes data) - neither included in Keith's data set, I think.

>5) To my great surprise I find that you used the Briffa gridded

>temperature reconstruction from W. N. America (mis-attributed

>to Fritts and Shao) - of course I should have picked up on this 6

>years ago when reading the proofs of the Nature sup mat. It was
>my understanding that we had decided not to use these
>reconstructions, as the data on which they were based were in the
>ITRDB, and had been subject to that screening process. So
>depending on whether you used the long or the shorter versions
>of these, there will have been a considerable number of density
>series included, some of them twice. It means that there is
>considerably more overlap between the two data sets, in North
>America, than I have been telling people. I stand corrected.
>Cheers, Malcolm

>.

>.Malcolm Hughes
>Professor of Dendrochronology
>Laboratory of Tree-Ring Research
>University of Arizona
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>fax 520-621-8229

--

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<http://www.cru.uea.ac.uk/cru/people/briffa/>

</x-flowed>

From: "Malcolm Hughes" <mhughes@ltr.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>, "Malcolm Hughes" <mhughes@ltr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>

Subject: Re: J. Climate paper - in confidence

Date: Tue, 20 Jan 2004 10:27:09 -0700

Cc: Scott Rutherford <srutherford@rwu.edu>, mann@virginia.edu

Mike - you are right that we should probably leave the network unchanged for this mss. In fact, however, as Keith indicated, the Vaganov data probably retained a fair amount of low frequency because of the use of the corridor method (i.e. were not "heavily standardized"). Cheers, Malcolm

On 20 Jan 2004 at 7:58, Michael E. Mann wrote:

> Thanks Keith,

>
> I agree w/ this--I think the Vaganov chronologies were pretty heavily
> standardized, and the other issues you raise are important. In the
> future, we would (and will) be a bit more circumspect about the use of
> some of these data.

>
> In the present case, however, I think we are forced to use the exact
> same network.

>
> Re, the omission of some results. I think we can probably keep them.
> Simply by cleaning up the text, removing redundancy, etc. I've
> shortened and tightened the manuscript considerably, and I think I've
> improved the logical flow a bit in the process. So my feeling is that
> we will not have to split this up, but I'll leave this to all of you
> to decide after you see the revised draft from Scott and me...

>
> Thanks,

>
> mike

>
> At 09:45 AM 1/20/2004 +0000, Keith Briffa wrote:

> Malcolm seems to have done a good job sorting out these
> constituent sets , and I don't have anything to add other than
> agreeing that as a general principal , where possible, original
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> (which it should be stressed are based on very flexible curve
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> Cheers, Malcolm . .Malcolm Hughes Professor of Dendrochronology

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fax 520-621-8229

From: Keith Briffa <k.briffa@uea.ac.uk>
To: p.jones@uea.ac.uk
Subject: Fwd: EOS revision
Date: Wed Jan 28 08:51:12 2004

X-Sender: esper@mail.wsl.ch
Date: Mon, 12 Jan 2004 10:26:27 +0100
To: Briffa Keith <k.briffa@uea.ac.uk>,
Cook Ed <drdendro@ldeo.columbia.edu>

From: Jan Esper <esper@wsl.ch>
Subject: EOS revision

Hi Ed and Keith

for your information, I attached the revision of the EOS article. In this version we added some lines about the data-overlap between the MBH and ECS records.

I also attached a figure showing a comparison between MBH and EsperFULL (using all data) and EsperSUB (without Tornetraesk and the Polar Urals).

Take care

Jan

--

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References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Iain Brown <Iain.Brown@uea.ac.uk>
To: a.watkinson@uea.ac.uk
Subject: Inter-reg proposal update
Date: Thu, 29 Jan 2004 11:25:44 +0000
Cc: m.hulme@uea.ac.uk, s.jude@uea.ac.uk

Andrew,

Here is an update on the Inter-reg proposal, based upon the recent Oxford workshop.

Organisations involved:

EA, EN, Oxford ECI, Oxford Brooks (Planning), Alterra (Netherlands), Hampshire CC, Kent CC, Conservatoire de Littoral, Clare CC, Maynooth U., Tyndall

Funding:

Aiming for a 3 year project of 3-4 million Euros. Inter-reg 3B most closely fits project objectives but still unknown whether sufficient funds remain for this. Inter-reg 3C represents an alternative, but requires more high-level policy. Inter-reg deadline is April 29th. Other alternatives are LIFE and Framework VI.

Key issue:

Are Tyndall to be included as a Partner or a Contractor? Partners have more influence on project development but would require 50% matched funding (however this can be met through including other contributing R&D projects). Contractors do not need matched funding but may have to officially tender for sub-contract.

Proposed Work Packages:

1 Policy Review of spatial planning mechanisms for biodiversity (European, national, regional, local). How will this cope with climate change? Oxford Brooks & Oxford ECI to lead on developing this WP.

2 Broad-scale Review of impacts of climate change on biodiversity in NW Europe. To identify main drivers, issues and vulnerabilities on a network basis. Lead: Alterra, Oxford ECI, Tyndall

3 Coastal case studies - Hamble (England), Shannon (Ireland), Baie de Vaie (France). Objectives to evaluate local management issues with regard to simulation of future coastal evolution. Lead: EA, Hampshire CC

4 Terrestrial case studies - 2 regions: SE England, Limburg. Lead Alterra, ECI

5 Policy Development & Guidance - based on review of research outputs. Lead EN

6 Dissemination

Cross-cutting issues - stakeholder engagement, assessment/management of key habitats

Next steps - develop WPs, workplans and costing of proposal by 27th Feb.

Next meeting 4th/5th March, Oxford.

regards,

Iain

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Fwd: John L. Daly dead
Date: Thu Jan 29 14:17:01 2004

From: Timo Hämeranta <timo.hameranta@pp.inet.fi>
To: <timo.hameranta@pp.inet.fi>
Subject: John L. Daly dead
Date: Thu, 29 Jan 2004 12:04:28 +0200
X-Mailer: Microsoft Outlook, Build 10.0.4510
Importance: Normal

Mike,

In an odd way this is cheering news ! One other thing about the CC paper - just found another email - is that McKittrick says it is standard practice in Econometrics journals to give all the data and codes !! According to legal advice IPR overrides this.

Cheers
Phil

"It is with deep sadness that the Daly Family have to announce the sudden death of John Daly. Condolences may be sent to John's email account (daly@john-daly.com)
"

Reported with great sadness

Timo Hämeranta
XX

Timo Hämeranta, LL.M.
Moderator, Climatesceptics
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Finland, Member State of the European Union

Moderator: timohame@yahoo.co.uk
Private: timo.hameranta@pp.inet.fi

Home page: [1]<http://personal.inet.fi/koti/hameranta/climate.htm>

Moderator of the discussion group "Sceptical Climate Science"
[2]<http://groups.yahoo.com/group/climatesceptics>

"To dwell only on horror scenarios of the future shows only a lack of imagination". (Kari Enqvist)

"If the facts change, I'll change my opinion. What do you do, Sir" (John Maynard Keynes)

XX

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. <http://personal.inet.fi/koti/hameranta/climate.htm>
2. <http://groups.yahoo.com/group/climatesceptics>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Rashit Hantemirov <rashit@ecology.uran.ru>
Subject: Re[2]: Stephen McIntyre
Date: Mon Feb 2 14:37:36 2004

Rashit

that sounds great - at least I am happy you are working on the sub fossil material still. I have done some work comparing the Swedish and Finnish long series after standard RCS detrending and there is good similarity at the century timescale for some considerable periods - but significant differences over some others , even allowing for uncertainty in the series These are only 300 km separated so this is an interesting indication of changes in continentality perhaps. I am also interested in extending the high-frequency density series before 1400 AD , to show earlier volcanoes , even though the spatial coverage is poor. It would be interesting to see your extreme year series - do you have a preprint of your paper? I would really like to get support to continue a wider collaboration , including other northern long series to produce wide scale integrated series . What is the latest state of your tree-line reconstruction , for periods earlier than you showed in the Holocene paper? I am still hoping such support may come again from Europe.

very best wishes

Keith

At 07:28 PM 2/2/04 +0500, you wrote:

Dear Keith,

it is very nice to hear from you.

We live and work in the old way. Stepan has been updated his woody vegetation descriptions in the Polar Urals to reconstruct dynamics of forest structure near upper timberline for the last century.

Because of some reasons (sometimes without any reasons) the work on constructing Yamal chronology is going not very well. Duration of chronology is now 7315 years (7314 BC - AD 2000). The last valuable field work has been realized in 2000, when we have collected 370 subfossil samples. Half of them have been dated. Now I successfully collect money for field work (for helicopter rent). I hope this field season will be fruitful. Meantime we have analyzed frost- and light-ring frequency in Yamal tree rings for the last 2100 years to reconstruct extreme events. The later half of this reconstruction, I hope, will be published this year in Palaeo3. Now I contracted (together with Stepan) to write by June something like textbook on tree-ring dating for archeologists (in Russian). Then I'm going to return to work on Yamal chronology. It would be pleasure to keep on our joint work.

Best regards

Rashit Hantemirov
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144
Russia

Tel: +7(3432)51-40-92

Fax: +7(3432)51-41-61

E-mail: rashit@ecology.uran.ru

Monday, February 2, 2004, 1:57:37 PM, you wrote:

KB> Dear Rashit

KB> thanks for this - these people ask many questions as they try constantly to
KB> attack the global warming proponents . I answer sometimes , but it usually
KB> means they come back with many more questions. All part of science I suppose.

KB> How are you , and Stepan? I have a student working on trying to refine the

KB> RCS approach , to allow less trees and reduce bias that comes from using

KB> only recent data . Hope to get him to test new methods on your and

KB> Vaganov's data if that is OK with you . I wish to work towards a new

KB> EuroSiberian series for several millennia at least. Are you still adding

KB> new data? How are you all?

KB> Keith

--

Professor Keith Briffa,
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University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784

[1][http://www.cru.uea.ac.uk/cru/people/briffa\[2\]/](http://www.cru.uea.ac.uk/cru/people/briffa[2]/)

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Rashit Hantemirov <rashit@ecology.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re[2]: Stephen McIntyre
Date: Mon, 2 Feb 2004 19:28:31 +0500
Reply-to: Rashit Hantemirov <rashit@ecology.uran.ru>

Dear Keith,
it is very nice to hear from you.

We live and work in the old way. Stepan has been updated his woody vegetation descriptions in the Polar Urals to reconstruct dynamics of forest structure near upper timberline for the last century.

Because of some reasons (sometimes without any reasons) the work on constructing Yamal chronology is going not very well. Duration of chronology is now 7315 years (7314 BC - AD 2000). The last valuable field work has been realized in 2000, when we have collected 370 subfossil samples. Half of them have been dated. Now I successfully collect money for field work (for helicopter rent). I hope this field season will be fruitful. Meantime we have analyzed frost- and light-ring frequency in Yamal tree rings for the last 2100 years to reconstruct extreme events. The later half of this reconstruction, I hope, will be published this year in Palaeo3. Now I contracted (together with Stepan) to write by June something like textbook on tree-ring dating for archeologists (in Russian). Then I'm going to return to work on Yamal chronology. It would be pleasure to keep on our joint work.

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Rashit Hantemirov

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Fax: +7(3432)51-41-61
E-mail: rashit@ecology.uran.ru

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KB> new data? How are you all?
KB> Keith

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Rashit Hantemirov <rashit@ecology.uran.ru>
Subject: Re[3]: Stephen McIntyre
Date: Tue Feb 3 14:30:38 2004

Rashit

thanks for these - I think you are making magnificent progress , and I wish you the very best . I would like to see the information you mention if you do not mind . It would be useful to compare with the long density data.

cheers again

Keith

At 07:20 PM 2/3/04 +0500, you wrote:

Content-Type: text/plain; charset=Windows-1251

X-MIME-Autoconverted: from 8bit to quoted-printable by alanllein.uran.ru id i13EL9co081373

Dear Keith,

attached manuscript concerning frost and light rings has been submitted to Paleo3 special issue (PAGES conference in Moscow in 2002). I'm still waiting for final decision.

Meantime we prepare next version of extremes reconstruction (on the base of Yamal data only) for the last 2100 years using frost, light, missing and very narrow rings. Unfortunately, I could not find time to prepare even draft version of this paper. I can send to you the picture and list of the "extreme" years for this period, if you are interested. Now analysis is going on, little by little. Most probably, we will prepare for publication data for longer reconstruction (up to 4000 years).

As to tree-line reconstruction, we have almost no progress. To get more reliable reconstruction we need more samples from sites northwards of 68°N. In 2002 we have sampled subfossil wood in this area. However, without success (only 30 samples, only 5 of them I was able to date). Now we have in all 30 dated samples from the area to the north of 68°. Attached .pcx files show reconstructions that have been published before in the local publications. Only one correction we can do after 2002 field season, namely that big shift of tree line took place after 2420 BC. Hope I will succeed finally in dating of rest of samples to improve reconstruction.

Best regards

Rashit Hantemirov

Institute of Plant and Animal Ecology

8 Marta St., 202

Ekaterinburg, 620144

Russia

Tel: +7(3432)51-40-92

Fax: +7(3432)51-41-61

E-mail: rashit@ecology.uran.ru

Monday, February 2, 2004, 7:37:36 PM, you wrote:

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KB> material still. I have done some work comparing the Swedish and Finnish
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KB> I am still hoping such support may come again from Europe.
KB> very best wishes
KB> Keith

--

Professor Keith Briffa,
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[1]<http://www.cru.uea.ac.uk/cru/people/briffa>[2]/

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Rashit Hantemirov <rashit@ecology.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re[4]: Stephen McIntyre
Date: Wed, 4 Feb 2004 16:53:49 +0500
Reply-to: Rashit Hantemirov <rashit@ecology.uran.ru>

Dear Keith,
attached file contains results of analysis of anomalous rings in Yamal material for 100BC - 2000 AD.

I forgot to inform you about one more thing. We have organized data bank of Russian tree-ring chronologies. Unfortunately (for you), in Russian.

<http://ipae.uran.ru/dendrochronology/>

(and then click on the icon in the bottom (in center) of page).

This databank is made for archeologists and people that need to date woody constructions and etc. The aim is to give them information about where and what kind of chronologies there are in Russia. For some locations chronology is available or links to other databanks, for others - information only. Site is still filling up. If you are interested to see you can ask Vladimir Shishov to translate. By the way, you can remind him about my request to place chronologies of their lab in this bank.

Best regards

Rashit Hantemirov

Institute of Plant and Animal Ecology
8 Marta St., 202
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Russia
Tel: +7(3432)51-40-92
Fax: +7(3432)51-41-61
E-mail: rashit@ecology.uran.ru

Tuesday, February 3, 2004, 7:30:38 PM, you wrote:

KB> Rashit

KB> thanks for these - I think you are making magnificent progress , and I wish

KB> you the very best . I would like to see the information you mention if you

KB> do not mind . It would be useful to compare with the long density data.

KB> cheers again

KB> Keith

Attachment Converted: "c:\eudora\attach\Extreme2100.pdf"

From: Phil Jones <p.jones@uea.ac.uk>
To: "Peter H. Gleick" <pgleick@pipeline.com>, Mearns Linda O
<lmearns@ictp.trieste.it>
Subject: Re: MBH Submission (fwd)
Date: Fri Feb 6 10:58:17 2004
Cc: Stephen H Schneider <shs@stanford.edu>, N.W.Arnell@soton.ac.uk,
frtca@fy.chalmers.se, d.camuffo@isac.cnr.it, scohen@sdri.ubc.ca,
pmfearn@inpa.gov.br, jfoley@facstaff.wisc.edu, harvey@geog.utoronto.ca,
ahssec@ansto.gov.au, Thomas.R.Karl@noaa.gov, rwk@ucar.edu,
rik.leemans@wur.nl, diana.liverman@eci.ox.ac.uk, mccarl@tamu.edu,
lindam@atd.ucar.edu, rmoss@usgcrp.gov, ogilvie@spot.colorado.edu,
pfister@hist.unibe.ch, barrie.pittock@csiro.au, pollard@essc.psu.edu,
nj.rosenberg@pnl.gov, crosenzweig@giss.nasa.gov, j.salinger@niwa.co.nz,
santer1@llnl.gov, h.j.schellnhuber@uea.ac.uk, dgvector@stanford.edu,
F.I.Woodward@sheffield.ac.uk, gyohe@wesleyan.edu, yurganov@hotmail.com

Dear All,

So now it seems that we're separating 'providing the code' from
'running the code'. I

can't

see the purpose of one without the other. Even if Mike complies I
suspect there will need

to be several sessions of interaction, which neither side will be
very keen on. As I said

before

I know the code will involve lots of combinations (for different
periods with different

proxies).

Also I would expect, knowing the nature of the PC-type regression
approach, that there

will

be library routines. If the code is sent, there needs to be
conditions. We don't want

McIntyre

(MM) to come out and say he can't get it to work after a few days.

So, it is far some simple. I'm still against the code being
given out. Mike has made

the

data available. That is all they should need. The method is detailed
in the original

paper -

in the online (methods) and also in several other papers Mike has
written.

As an aside, Mike is now using a different method from MBH98.
Also, as an aside,

whilst we've been deliberating, MM have submitted another comment on
MBH98 to another

journal. In this they say they have a program that replicates MBH98
(although it isn't

very convincing that they have it exactly right, as they never show a
like for like

comparison) , but

most of the comment goes on about the results being different due to
different

combinations of
proxies. The latter isn't surprising.

It might appear they want the code to check whether their version works properly. If

this
is the case, then there are issues of IPR. So, if they get the code,
how do we stop them
using it for anything other than this review.

Cheers

Phil

At 11:40 04/02/2004 -0800, Peter H. Gleick wrote:

Yes, excellent point. This should be what we do. Further, we can
point out that we've
bent over backward here and provided more than typically necessary
in order to satisfy
persistent but inappropriate demands.

Peter

At 08:46 PM 2/4/04 +0100, Mearns Linda O wrote:

Peter et al.,

Thanks for reminding me about the new email list.

My point about the code is still that 'providing the code' can be
interpreted alot of ways. I have thought about this, and imagined
if in

one of my larger and more complex projects, I was asked to provide
all
code. I could do that just by sending the pieces with a summary
file

explaining what each piece was used for. It still theoretically
allows

someone to see how coding was done. And I do think that is a far
sight

easier than providing stuff that can be run, etc. I am suggesting
that

one could do the minimum. Then the point is, one isn't faced with
garish

headlines about 'refusal to provide code'. I think it is harder to
come

up with a garish headline about 'refusal to provide completely
documented

code with appropriate readme files and handholding for running it'.

Linda

Dr. Peter H. Gleick

Director, 2003 MacArthur Fellow

Pacific Institute for Studies in Development, Environment, and
Security

654 13th Street

Oakland, California 94612

510 251-1600 phone

510 251-2203 fax

[1]www.worldwater.org (World Water site)

[2]www.pacinst.org (Pacific Institute site)

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
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Norwich Email p.jones@uea.ac.uk
NR4 7TJ
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References

1. <http://www.worldwater.org/>
2. <http://www.pacinst.org/>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Tas van Ommen" <tas.van.ommen@utas.edu.au>
Subject: Re: FW: Law Dome O18
Date: Mon Feb 9 09:23:43 2004
Cc: mann@virginia.edu

Dear Tas,

Thanks for the email. Steve McIntyre hasn't contacted me directly about Law Dome (yet), nor about any of the series used in the 1998 Holocene paper or the 2003 GRL one with Mike. I suspect (hope) that he won't. I had some emails with him a few years ago when he wanted to get all the station temperature data we use here in CRU. At that time, I hid behind the fact that some of the data had been received from individuals and not directly from Met Services through the Global Telecommunications Service (GTS) or through GCOS.

I've cc'd Mike on this, just for info. Emails have also been sent to some other paleo people asking for datasets used in 1998 or 2003. Keith Briffa here got one, for example. Here, they have also been in contact with some of Keith's Russian contacts. All seem to relate to trying to get series we've used. In the Russian case, issues relate to the Russian (Rashit Hantemirov) having a paper out with the same series Keith used (for the Yamal Peninsula). Series are different for two reasons. One Keith used the RCS standardization method and secondly Rashit has added some series since Keith got the data a couple of years ago.

I'll just sit tight here and do nothing. Mike will likely do the same, but we'll expect another publication in the nearish future.

As for the series for LD you sent us, we used it in the paper for Reviews of Geophysics. This paper has had 4 good reviews and we've just sent back a revised version. This will likely get reviewed by 1 or 2 of the same reviewers of the editor, but I think it will come out this year some time. When it does, we will put all the series onto a web site. Hope this is OK with you. It will unlikely be before our summer months.

Cheers
Phil

At 17:56 09/02/2004 +1100, you wrote:

Dear Phil,

What you will find below is (in reverse chronological order) an email interchange between Steve McIntyre and myself. He has been asking for LD data for a while (since your GRL paper came out) and to my chagrin, I have put him off once already, for reasons I spell out below. For your information, I am close to submitting the full LD isotope record, which I hope to present at SCAR Bremen, along with some interesting spectral analyses and comparison to EPICA Dome C.

Anyway, I am aware of McIntyre's controversial history and am trying to handle things in a non-inflammatory way. He seems not to be troubling me over my own delay, but has asked for data that was used in your Holocene paper of 1998. For this, I have referred him to you. I expect he wants to replicate your synthesis, and so he should use the identical data set, and I give you permission to pass on whatever it was I gave you for that work - with the caveat that it is representative of where the LD proxy record was in 1997, not 2004. I leave it to you to decide how to deal with this - you may prefer to ignore the issue, and I would understand.

Let me know if there is anything I can do to assist.

Cheers,
Tas

Dr Tas van Ommen, Principal Research Scientist | Postal Address:
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Antarctic Climate & Ecosystems CRC | Private Bag 80
Tel: +61 (03) 6226 2981 Fax: +61 (03) 6226 2902 | Hobart
[1]www.antcrc.utas.edu.au/~tas | Tasmania 7001
[2]tas.van.ommen@utas.edu.au | Australia

-----Original Message-----

From: Tas van Ommen [[3]mailto:tas.van.ommen@utas.edu.au]
Sent: Monday, 9 February 2004 17:46
To: 'Steve McIntyre'
Subject: RE: Law Dome O18
Dear Stephen,

I suggest you ask Phil Jones for a copy of that older data set. Jones et al cite Morgan and van Ommen 1997, although that data set was heavily smoothed (gaussian of rms=13 years from memory), so the one they show is not a direct version of Morgan and van Ommen 1997. I think that I provided them with a high resolution version, and from their notation, it seems that they are using a November-April subset, but you would have to ask Phil - especially if what you seek is to replicate their analyses. Apart from anything else, our set has been continually in a state of development, which is why I have not wanted to widely circulate it until now. Over this period we have had made new measurements (which improved our layer counted dating and filled the gap that you see in Jones et al.), retrieved more cores using better technology and derived a robust gas-tied flow-model that dates the core to 90ky. Now that the new development has ceased, we will soon be releasing the full data set, as I have indicated to you. This is the set I would want to see in wider use, and it is worth noting that it is essentially the same as the portion used by Mann and Jones in their GRL paper in 2003.

All the best,

Tas

Dr Tas van Ommen, Principal Research Scientist | Postal Address:
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Antarctic Climate & Ecosystems CRC | Private Bag 80
Tel: +61 (03) 6226 2981 Fax: +61 (03) 6226 2902 | Hobart
[4]www.antcrc.utas.edu.au/~tas | Tasmania 7001
[5]tas.van.ommen@utas.edu.au | Australia

-----Original Message-----

From: Steve McIntyre [[6]mailto:stephen.mcintyre@utoronto.ca]

Sent: Monday, 9 February 2004 09:46

To: Tas van Ommen

Subject: Re: Law Dome O18

There is a Law Dome O18 data set which was used in Jones et al (Holocene 1998) and printed as a graphic. Is this one available? Regards, Steve McIntyre

----- Original Message -----

From: [7]Tas van Ommen

To: [8]'Steve McIntyre'

Sent: Saturday, February 07, 2004 11:15 PM

Subject: RE: Law Dome O18

Dear Stephen,

The 18O data used in Mann and Jones 2003 was provided as an advance copy in 2003, and you are welcome to have access to it and it will certainly be placed in public archives.

The data in question is part of the full 90 ky isotope record from Law Dome, for which a peer-reviewed dating scale has only recently been published (actually it is in press see van Ommen et al, in press Annals of Glaciology 39 at [9]<http://www.antrc.utas.edu.au/~tas/home/openaccess.html#vanommen04LD1>). Now this job is done, I am finalizing a paper that will allow me to release the isotope record more widely.

It is this next paper that controls the timeframe for release to you and archives. While I should await peer review for a release to the archives, I am happy to pass on a copy of the data set to you on an advance basis as soon as the paper is submitted I expect in a couple of months. You will appreciate that at this time of the year, we in the south are in our vacation season, not to mention dealing with our Antarctic Summer field program, so I thank you for your patience. Do check back with me in a while if you dont hear more.

Regards,

Tas

-----Original Message-----

From: Steve McIntyre [[10]mailto:stephen.mcintyre@utoronto.ca]
Sent: Sunday, 8 February 2004 6:29 AM
To: Tas Van Ommen
Subject: Law Dome O18

Dear Dr van Ommen,

some time ago I inquired as to the availability of the O18 data set which was used in Mann and Jones 2003. Is this the same data as was used in Jones et al 1998 (Holocene) . Do you plan to archive this data? Otherwise, I would appreciate an email copy of the data.

Thanks for your consideration.
Stephen McIntyre

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia

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Email p.jones@uea.ac.uk

References

1. <http://www.antcrc.utas.edu.au/~tas>
2. <mailto:tas.van.ommen@utas.edu.au>
3. <mailto:tas.van.ommen@utas.edu.au>
4. <http://www.antcrc.utas.edu.au/~tas>
5. <mailto:tas.van.ommen@utas.edu.au>
6. <mailto:stephen.mcintyre@utoronto.ca>
7. <mailto:tas.van.ommen@utas.edu.au>
8. <mailto:stephen.mcintyre@utoronto.ca>
9. <http://www.antcrc.utas.edu.au/~tas/home/openaccess.html#vanommen04LD1>
10. <mailto:stephen.mcintyre@utoronto.ca>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fw: Law Dome O18
Date: Mon Feb 9 15:50:09 2004

Mike,

These were two simple ones to provide. Also Tas told him I had one of them. I guess these are the ones that aren't available on web sites.

Anyway, it is done now. If he starts asking for them in dribs and drabs, I'll balk at that.

Ben waded in with very positive comments re the CC issue. Steve's going to find it very hard to ask you to send the code. Those that say on the CC board that you should send the code, have little idea what is involved. Most are on the social science side.

Cheers

Phil

At 10:19 09/02/2004 -0500, you wrote:

HI Phil,

Personally, I wouldn't send him anything. I have no idea what he's up to, but you can be sure it falls into the "no good" category.

There are a few series from our '03 paper that he won't have--these include the latest Jacoby and D'Arrigo, which I digitized from their publication (they haven't made it publicly available) and the extended western North American series, which they wouldn't be able to reproduce without following exactly the procedure described in our '99 GRL paper to remove the estimated non-climatic component.

I would not give them *anything*. I would not respond or even acknowledge receipt of their emails. There is no reason to give them any data, in my opinion, and I think we do so at our own peril!

talk to you later,

mike

At 02:46 PM 2/9/2004 +0000, Phil Jones wrote:

Mike,

FYI. Sent him the two series - the as received versions. Wonder what he's up to? Why these two series ? Used a lot more in the 1998 paper. Didn't want the Alerce series.

Must already have the Tassy series from Ed. I know Ed has a more recent series than we used in 1998. Got this for the 2003 work.

Cheers

Phil

From: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Fw: Law Dome O18
Date: Mon, 9 Feb 2004 08:05:23 -0500
X-Mailer: Microsoft Outlook Express 6.00.2800.1158
X-Authentication-Info: Submitted using SMTP AUTH LOGIN at
fep04-mail.bloor.is.net.cable.rogers.com from [65.49.25.138] using ID
<nmcintyre77@rogers.com> at Mon, 9 Feb 2004 08:02:13 -0500

Dear Phil,

Tas van Ommen has referred me to you for the version of his dataset that you used in Jones et al Holocene 1998 and I would appreciate a copy. I would also appreciate a copy of the Lenca series used in this study. Regards, Steve McIntyre

Prof. Phil Jones
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Prof. Phil Jones
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References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Pete Mayes
Date: Thu Feb 19 09:28:22 2004

Ben,

Every now and then - generally around an England game (probably now as we've just drawn with Portugal) or lamenting the fall of Liverpool, I get emails and sometimes phone calls from Pete Mayes !! Pete wants to get back into climate change and do some comparisons between real world data and some models. It is a pity he wasn't this keen, when he first went to the US !

Anyway I suggested he contact you. He has but he's not got a reply. I guess you're busy and/or don't know how to reply. I'm sure he doesn't know what he really wants. I gave him some references etc to look over and your name/email - so SORRY !!!!

I guess I'll see you just after Easter. Will you be here for the HC meeting as well as IDAG?

It will be good to see Tom in Oxford - he should liven up the IDAG discussions.

Hope all is well with you and Nick !

Cheers

Phil

PS I see Steve has replied to MM re the MBH review. This nearly got out of hand - it still could. Appalling paper in GRL in the Feb04 issue - Mike Mann's written a response. Clearly another case of the GRL editor's having no idea of the science. Who in their right mind would accept that for publication. Nowhere on the CRU site does it say that HadCRUT2v is the IPCC data. According to the HC the IPCC data is the OA version HadCRUT - no v, no 2.

The data is on the HC web site. There is a link to it from the CRU site. When getting data from the CRU site we ask people to refer to some of the papers and to use the dataset names. Soon et al didn't do either. Paper attached as I have it.

Just had a paper accepted by Reviews of Geophysics with Mike Mann on the climate of the last 2k years. Expecting flak for this, but it had 4 very positive reviews.

For some inane reason I put my name forward to do the chapter on atmospheric obs. for AR4. Hope I don't get picked.

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From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Crap Papers
Date: Thu Feb 26 15:59:12 2004

Mike,

Just agreed to review a paper for GRL - it is absolute rubbish. It is having a go at the CRU temperature data - not the latest vesion, but the one you used in MBH98 !! We added lots of data in for the region this person says has Urban Warming ! So easy review to do.

Sent Ben the Soon et al. paper and he wonders who reviews these sorts of things. Says GRL hasn't a clue with editors or reviewers. By chance they seem to have got the right person with the one just received.

Can I ask you something in CONFIDENCE - don't email around, especially not to Keith and Tim here. Have you reviewed any papers recently for Science that say that MBH98 and MJ03 have underestimated variability in the millennial record - from models or from some low-freq proxy data. Just a yes or no will do. Tim is reviewing them - I want

to make sure he takes my comments on board, but he wants to be squeaky clean with discussing them with others. So forget this email when you reply.

Cheers

Phil

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From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: [Fwd: More PCM-ERA40 comparisons]
Date: Tue Mar 2 09:06:41 2004

Ben,

Thanks for the plots and keeping me up to date. The ERA-40/CRU comparisons are quite interesting. I'm hopeful Adrian will write up a summary for publication in addition to an ECMWF report.

This sort of thing is important wrt IPCC and also papers such as Kalnay and Cai.

I'm also working with Russ Vose and others at NCDC to get a comparison of CRU/GHCN and NASA datasets in GRL. NCDC have used their first difference technique with CRU data. Differences are very, very small due to data and the technique doesn't matter much either. All seems to boil down to how the global average is defined. Calculated as one domain as NCDC (and until recently the HC as well) want to do it, it is biased to the NH. If you do it the CRU way ($G=0.5(NH+SH)$) then it looks much more like an OA version of HadCRUT2v that the HC have just produced. Been saying this for years as has Tom, so no surprises. Finally got the HC to realise it, now just need to convince NCDC.

NCDC will also have a new 5 by 5 deg gridded dataset of Tx and Tn soon, right up to the present. Need to compare this with ERA-40.

Cheers
Phil

At 18:46 01/03/2004 -0800, you wrote:

Dear Phil,

Here are the PCM/ERA-40 2m temperature comparisons that I mentioned in my email to Adrian....

Cheers,
Ben

--

PCMDI HAS MOVED TO A NEW BUILDING. NOTE CHANGE OF MAIL CODE!

Benjamin D. Santer
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P.O. Box 808, Mail Stop L-103
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-----Return-Path:

<santer1@llnl.gov>

Received: from smtp-3.llnl.gov ([128.115.41.83] verified)

by popcorn.llnl.gov (CommuniGate Pro SMTP 4.0.6)

with ESMTP id 34392268 for santer1@popgun.llnl.gov; Thu, 26 Feb 2004 18:00:27 -0800

Received: from pierce.llnl.gov (localhost [127.0.0.1])

by smtp-3.llnl.gov (8.12.3p2-20030917/8.12.3/LLNL evision: 1.13 \$) with ESMTP id i1R20OE6003673

for <santer1@popgun.llnl.gov>; Thu, 26 Feb 2004 18:00:24 -0800 (PST)

Received: from smtp-3.llnl.gov (smtp-3.llnl.gov [128.115.41.83])

by pierce.llnl.gov (8.12.3p2-20030917/8.12.3/LLNL evision: 1.5 \$) with ESMTP id i1R20NkO028603

for <santer1@llnl.gov>; Thu, 26 Feb 2004 18:00:23 -0800 (PST)

Received: from popcorn.llnl.gov (localhost [127.0.0.1])

by smtp-3.llnl.gov (8.12.3p2-20030917/8.12.3/LLNL evision: 1.13 \$) with ESMTP id i1R208Af003594;

Thu, 26 Feb 2004 18:00:09 -0800 (PST)

Received: from [128.115.57.176] (account santer1 HELO llnl.gov)

by popcorn.llnl.gov (CommuniGate Pro SMTP 4.0.6)

with ESMTP id 34392176; Thu, 26 Feb 2004 18:00:08 -0800

Sender: bsanter@smtp-3.llnl.gov

Message-ID: <403EA554.20D01DFD@llnl.gov>

Date: Thu, 26 Feb 2004 18:03:00 -0800

From: Ben Santer <santer1@llnl.gov>

Organization: LLNL

X-Mailer: Mozilla 4.79 [en] (X11; U; Linux 2.4.18-14 i686)

X-Accept-Language: en

MIME-Version: 1.0

To: Adrian.Simmons@ecmwf.int, wmw@ucar.edu, meehl@ucar.edu, wigley@ucar.edu, ammann@ucar.edu

Subject: More PCM-ERA40 comparisons

References: <403B1219.4060905@ecmwf.int>

Content-Type: multipart/mixed;

boundary="-----7A520C5A8CA7CE01BA097390"

X-Mozilla-Status2: 00000000

Dear Adrian,

Thanks very much for sending me your comparison of surface air temperature changes in CRU and ERA-40. I've been looking at a related issue - the correspondence between 2m temperature changes in ERA-40 and PCM.

Here's the background to this work. Increasingly, there is some interest in the problem of identifying anthropogenic climate change at regional scales. I have to give a brief talk on this subject tomorrow. In preparing for this talk, I decided that it would be useful to show how signal and noise change as a

function of spatial scale. I looked at the behavior of 2m temperature in the four individual realizations of the PCM "ALL forcings" experiment (the same experiment that we analysed in our joint Nature paper). For each realization, I computed spatial averages over the globe, the Northern Hemisphere, and the western United States (30-50N, 126W-114W). These spatial averages were then expressed as anomalies relative to climatological monthly means over 1979-1999. The orange shading in the three panels of the figure entitled "tas_tseries3.ps" is a measure of the between-realization variability in PCM. The envelope is simply the range (during any given month) between the maximum and minimum values of the four realizations. This range was then low-pass filtered. The solid red is the low-pass filtered ensemble mean.

To facilitate comparison with PCM data, I've defined 2m temperature anomalies in ERA-40 in the same way (i.e., relative to climatological monthly means over 1979-1999), and have used the same low-pass filter. One can then ask whether the 2m temperature changes in ERA-40 are consistent with those in PCM - in other words, are they encompassed by PCM's envelope of possible climate responses to combined anthropogenic and natural forcing?

They are. Surprisingly, this consistency occurs not only at the global-mean level, but also for the NH and western U.S. For the global-mean and the NH, the ERA-40 2m temperature changes are outside PCM's envelope of 2m temperature changes during the first 5-10 years of the reanalysis. After the late 1960s, however, the ERA-40 2m temperature changes are entirely consistent with those in PCM. Over the western U.S., 2m temperature changes in PCM and ERA-40 are consistent throughout the reanalysis period.

Such qualitative consistency, while interesting, is no substitute for formal, pattern-based fingerprint detection studies at global, hemispheric, and regional scales. For example, an overestimate of the regional-scale variability of 2m temperature by PCM could explain why PCM's 2m temperature changes over the western U.S. fully encompass the ERA-40 result (see panel C). On the other hand, there is some real similarity in the low-frequency component of the 2m temperature changes in ERA-40 and PCM (look at the similar responses to Agung, Chichon, and Pinatubo in panel B!)

The bottom line is that PCM's 2m temperature changes are reasonably consistent with those in ERA-40, even at sub-global spatial scales. This suggests that formal regional-scale detection work might be useful. If you are interested, perhaps we could collaborate on such work. A collaboration would also involve the PCM group at NCAR (to whom I'm copying this email).

The second figure that I've appended shows the global-mean changes in synthetic MSU channel 2 temperatures in PCM and ERA-40. The message is pretty much the same as for 2m temperatures: PCM's "envelope" of possible changes in tropospheric temperatures largely encompasses the ERA-40 results, except during a few large El Nino and La Nina events. Once again, there is surprising similarity in the low-frequency component of the model and reanalysis T2

changes.

It would be fun to take these simple comparisons a little further!

With best regards,

Ben

--

PCMDI HAS MOVED TO A NEW BUILDING. NOTE CHANGE OF MAIL CODE!

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From: Kevin Trenberth <trenbert@cgd.ucar.edu>
To: tom crowley <tcrowley@duke.edu>
Subject: Re: REQUEST FOR INFORMATION ON CLIMATE CHANGE AND HUMAN
ATTRIBUTIONS
Date: Fri, 12 Mar 2004 11:22:56 -0700
Cc: Chick Keller <cfk@lanl.gov>, Richard Somerville
<rsomerville@ucsd.edu>, Tom Wigley <wigley@cgd.ucar.edu>, "Howard Hanson,
LDRD" <hph@lanl.gov>, "James E. Hansen" <jhansen@giss.nasa.gov>, Michael
Schlesinger <schlesin@atmos.uiuc.edu>, Phil Jones <p.jones@uea.ac.uk>,
Thomas R Karl <Thomas.R.Karl@noaa.gov>, Mike MacCracken
<mmaccrac@comcast.net>, Ben Santer <santer1@llnl.gov>,
thompson.4@osu.edu, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu,
Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>

<x-flowed>

I agree with Tom: I sent you (without copying others) a whole host of
material..

Kevin

tom crowley wrote:

> For goodness sakes, I don't know where to start - let me just make one
> point with respect to solar - solar projects onto the GHG signal in
> the 20th c. so solar cannot be distinguished during that time. if one
> were to independently correlate solar and GHG with temp. since 1750,
> solar would "explain" about 75% of the variance, GHG about 70% - a
> spectacular 140% of the variance explained!

>

> the only way to evaluate solar is to look at intervals when GHG was
> not changing and solar was - the preanthropogenic interval - perhaps
> the most comprehensive evaluation of the solar effect is in the
> attached paper, where it is quite clear that solar effect is either
> negligible or just barely significant, ie., 5-10% of the decadal
> scaled variance.

>

> with respect to the MWP all you have to do is plot the data up and
> compile them - the numbers don't work out as being warmer than the
> present - at best approaching or slightly exceeding mid-20th c. the
> reason is that it was warm at different times. Soon and Baliunas of
> course never showed this - but if you actually look at the damn data
> and plot up, the same answer as I stated above keeps showing up, over
> and over.

>

> with respect to UAH, there are now two other reconstructions that show
> otherwise.

>

> enough, this is like trying to convert someone with one religion to
> another.

>

> tom

>

> Chick Keller wrote:

>

>> Richard and Friends,

>>
>> thanks for the point of view. I'll put some of this into my
>> presentation.
>>
>> However, it won't wash when facing critics head-on.
>>
>> Their latest arguments are more subtle. Their main point is that
>> their counter information hangs together into a logically coherent
>> picture.
>>
>> Models: no real finger print that distinguishes AGHG forcings from
>> others! Models using AGHG forcings predict warming is function of
>> latitude yet the Arctic is hardly warming (north of $\sim 65^{\circ}\text{N}$), and high
>> latitude Antarctic (excepting for the peninsula) is actually cooling
>> slightly.
>>
>> Models: As you say need AGHG forcings to simulate last 30 years of
>> observed warming. But, they counter, UAH satellite reductions show
>> no such warming so don't need AGHG forcing (or at least don't need
>> effects of positive feedbacks and just increases in AGHGs don't cause
>> so much warming).
>>
>> Solar forcing--not able to generate last 30 years of observed
>> warming. Same counter as last one--"See, they say, no increased
>> solar in last 25 years is consistent with no warming!!
>>
>> Also, since no warming since 1945, MWP most likely to have been as
>> warm as now and thus sun can indeed explain (with proper lags)
>> observed warming thus far.
>>
>> Their model--climate varies depending on solar activity. all
>> observations are consistent with this.
>>
>> Models predict that any surface warming will be seen in the
>> troposphere. Since UAH satellite reduction shows no such warming--1.
>> models are wrong and/or no warming at surface just lousy observations.
>> 2. If no warming at surface in last 30 years AGHG forcing predictions
>> by models is incorrect probably due to poor cloud/water vapor
>> modeling--no positive feedbacks to speak of.
>>
>> Sooooo, you can say all you want that all the prestigious societies
>> and folks say it's AGHGs, but they've been bamboozled by a few of
>> elitist scientists. As long as satellites show no recent warming,
>> the entire AGHG hypothesis collapses, not because multi-atomic
>> molecules don't cause the atmosphere to be more opaque, but because
>> there are no positive feedbacks which the models need to get the
>> "right" answer.
>>
>> So, what I need is strong evidence that the surface record is indeed
>> correct (UHI effect is small, and marine boundary layer approximation
>> is correct).
>>
>> Now, Richard, toss in large effects of land use changes and of black
>> soot forcing changing earth's albedo, and you now have additional

>> forcings which may be causing warming but can't be countered by
>> reducing AGHGs.
>>
>> Soooo, it still ain't all that easy to convince an audience that the
>> Singer's of this world aren't on to at least part of the problem.
>>
>> AND keep in mind that increased CO2 is good for us--more agriculture,
>> etc.
>>
>> Nope it just ain't that easy. So any information--graphics, etc on
>> these issues will be greatly appreciated.
>>
>> Regards to all,
>> chick
>>
>>
>> Hi Chick and friends,
>>
>> Good to hear from you, Chick. I'm busy, like all of us, and
>> responding to Singer is not my cup of tea, so I'm glad you and others
>> are willing. I hate to be in the same room with him, frankly. He's
>> a third-rate scientist and is ethically challenged, to say the least.
>>
>> From others on your email list, I am sure you will receive tons of
>> useful information. However, I think your entire basic strategy for
>> confronting Singer might not be optimal. Sometimes the most pressing
>> issues in the research community, or the most interesting questions
>> scientifically, are not necessarily the best ways to carry on the
>> public conversation. I am thinking in particular of your statement:
>>
>> "Perhaps the most important is that satellites don't show much
>> warming since 1979 and disagree substantially with the surface
>> record, which must then be incorrect. Were we able to resolve this
>> conundrum, I think most of the other objections to human generated
>> climate change would lose their credibility."
>>
>> For what it's worth, here's my take on your approach. I
>> respectfully disagree with you that hammering away on reconciling the
>> MSU data with radiosonde and surface data is the right way to go in
>> dealing with the Fred Singers of the world. Even though much of the
>> differences may now be apparently explained, it's still a terribly
>> messy job. The satellite system wasn't designed to measure
>> tropospheric temperatures, the calibration and orbital decay and
>> retrieval algorithm and all the other technical issues are ugly, and
>> nobody knows how much the lower stratospheric cooling ought to have
>> infected the upper troposphere, among other points one might make.
>>
>> No matter what one does on trying to make the MSU data tell us a
>> clean story, there are remaining serious uncertainties. That's
>> basically what the NAS/NRC study chaired by Mike Wallace concluded,
>> and it's still true, in my view. Plus the data record is so short.
>> In addition, as you say, you are retired, and research on these
>> things is not what you have first-person experience with, so when you
>> try to study up on the latest published results, you're at a

>> disadvantage compared with the Singers of the world, whose full-time
>> job is to cherry-pick the literature for evidence to support their
>> preconceived positions.

>>

>> One of the tactics of the skeptics is to create the impression among
>> nonscientists, especially journalists, that the entire science of
>> climate change rests on the flimsy foundation of one or two lines of
>> evidence, so that casting doubt on that foundation ought to bring
>> down the entire structure. For temperature, that approach is clearly
>> behind the attacks on the "hockey stick" curve over the last 1,000
>> years or the satellite vs. in situ differences over the last 25
>> years. Refuting the errors of the papers by Soon and Baliunas or by
>> McIntyre and Mckitrick doesn't faze these people. They just shift
>> their ground and produce another erroneous attack. Their goal is not
>> to advance the science, but to perpetuate the appearance of
>> controversy and doubt.

>>

>> I don't think the skeptics should be allowed to choose the
>> battlefield, and I certainly don't think the issue of whether
>> anthropogenic influences are a serious concern should be settled by
>> looking at any single data set. I do think the IPCC TAR was right to
>> stress that you simply can't plausibly make GCMs replicate the
>> instrumental record without including GHGs (and aerosols). I also
>> think the recent AGU and AMS public statements, which you will
>> doubtless find on their web sites, are right on target. Many of us
>> were pleasantly surprised that our leading scientific societies have
>> recently adopted such strong statements as to the reality and
>> seriousness of anthropogenic climate change. There really is a
>> scientific consensus, and it cannot be refuted or disproved by
>> attacking any single data set.

>>

>> I also think people need to come to understand that the scientific
>> uncertainties work both ways. We don't understand cloud feedbacks.
>> We don't understand air-sea interactions. We don't understand
>> aerosol indirect effects. The list is long. Singer will say that
>> uncertainties like these mean models lack veracity and can safely be
>> ignored. What seems highly unlikely to me is that each of these
>> uncertainties is going to make the climate system more robust against
>> change. It is just as likely a priori that a poorly understood bit
>> of physics might be a positive as a negative feedback. Meanwhile,
>> the climate system overall is in fact behaving in a manner consistent
>> with the GCM predictions. I have often wondered how our medical
>> colleagues manage to escape the trap of having their entire science
>> dismissed because there are uncured diseases and other remaining
>> uncertainties. Maybe we can learn from the physicians.

>>

>> People on airplanes, when they find out what I do for a living,
>> usually ask me if I "believe in" global warming. It's not religion,
>> of course. What I actually tend to believe in, if they really wanted
>> to try to understand, is quantum mechanics. CO2 and CH4 and all
>> those other interesting trace gases have more than two atoms, and
>> that fact simply has inescapable consequences. You just can't keep
>> adding those GHG molecules indefinitely without making the atmosphere
>> significantly more opaque in the IR. The "debates" in the reputable

>> research community are all quantitative. If skeptics don't worry
>> about doubling, they ought to be pressed to tell us why they are
>> unconcerned about tripling or quadrupling or worse. That's where the
>> planet is headed. The fact that remote sensing and model building
>> are hard work, and that much remains to be done, shouldn't be allowed
>> to obscure the basic obvious facts.

>>
>> Bonne chance et bon courage,

>>
>> Richard

>
>

--

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From: Phil Jones <p.jones@uea.ac.uk>
To: Jorge Sánchez Sesma <jsanchez@tlaloc.imta.mx>
Subject: Re: Global Temperature
Date: Mon Mar 15 16:01:14 2004

Dear Jorge,

Happy for you to use me in an additional attempt to get some Mexican support to come to CRU next year. What exactly do you need? Send me an example of what you want? Life is very busy here at the moment as I'll be away for several meetings over the next 6 weeks and I must prepare some material for most of them.

GKSS is just one model and it is a model, so there is no need for it to be correct. I am also aware that Ed Cook is revising the ECS curve in a paper he's submitting to Quaternary Science Reviews.

Remember that if ECS (and GKSS) are correct then the climate is more sensitive to external forcing (the factors that cause past changes/variability). If the climate is more sensitive then the likely changes in the future will be greater. The curves that we've produced here (and also Mike Mann's) suggest a climate sensitivity of about 2.5 deg C for a CO2 doubling. Getting volcanic forcing right in the past (along with solar)

are crucial in any study.

Cheers

Phil

At 12:22 12/03/2004 -0600, you wrote:

Dear Dr. Jones:

I am very happy because I went to a Workshop in Kona Hawaii (with support of NASA-CRCES after to gain a contest with a review paper about global temperature reconstructions, it was a different version of the paper that you have read). There I met with Dr. Michael Mann. Mann was very kind with me, however when he did know my work he changed his attitude. I met there also Dr. Hans von Storch who presented a global temperature reconstructions with a AOCGCM with natural and anthropogenic forcings. His results agree more or less with ECS, and my results. I am in contact with the GKSS group in order to compare and share information.

However, the key point of my studies, as you have pointed out, is to justify that the background Ice Albedo (without volcanic activity) from polar caps could be considered as a proxy. I have contacted Dr. Hammer and Dr. Crowley to have information and advice.

In order to continue this kind of studies I would like to propose you again (as we have tried last year) to ask support from the AMC (Mexican Academy of Sciences) to support a visit to CRU-UEA next year to continue my work, with your help and advice, about global temperature for the Holocene. I will

need only an official invitation for my visit. It would be in March 2005 for 3 or 4 weeks.

Also, I am asking support to travel to Japan this year (this fall), however I would like to stop in England a week, in order to visit CRU-UEA and to continue our collaboration.

I would like to know your opinion,

cheers,

Jorge

Jorge Sánchez-Sesma

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References

1. <http://nimbus.imta.mx/>

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: [Fwd: More PCM-ERA40 comparisons]
Date: Thu Mar 25 18:24:06 2004

Ben,
Thanks I picked it up last Friday. See you after Easter.

Cheers

Phil

At 09:22 25/03/2004 -0800, you wrote:

Dear Phil,

Our exchange with Roger Pielke finally appeared in Science (copy appended). I'm glad I've gotten this particular albatross off my neck. Timo et al. have already been circulating this stuff to all and sundry.....

See you in a few weeks' time,

Cheers,

Ben

Phil Jones wrote:

>

> Ben,

> Right decision ! She sent me an email to review a paper two weeks ago.

> Said I didn't

> have time until May. I'll continue to say that now.

> See you just after Easter. Have a good short break, as you'll have to

> miss part of it

> to come to London and IDAG.

>

> Cheers

> Phil

>

> At 19:06 22/03/2004 -0800, you wrote:

>>Dear Phil,

>>

>>I just don't have much luck with the Heikes of this world. Heike L.

>>rejected our

>>Nature paper on the analysis of changes in tropopause height and

>>equivalent MSU

>>temperatures in ERA-40. She took six weeks to make this decision, and didn't

>>even send the paper out for review! Very disappointing. I doubt whether

>>I'll be

>>submitting any papers to Nature in the next few years. We're now revising the

>>erstwhile Nature paper for submission to Journal of Climate, and I hope to

>>have

>>it sent off before I leave for the U.K. on April 11th.

>>

>>I look forward to seeing you at the SRG meeting. Hope everything is well with

>>you, Ruth, Hannah, and Matthew.

>>

>>Best regards,

>>

> >Ben

> >-----

>

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PCMDI HAS MOVED TO A NEW BUILDING. NOTE CHANGE OF MAIL CODE!

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From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: have you seen this?
Date: Wed Mar 31 09:09:04 2004

Mike,

Yes, but not had a chance to read it yet. Too much else going on. Ed has a paper reworking Esper et al. as you'll know. If you're going to Tucson, I suggest you talk to Keith about it then - don't email him as he's too busy preparing to go and marking essays.

Jan is in one of our EU projects. Seems that Keith thinks Jan is reinventing a lot of Keith's work, renamed the RCS method and much more. Jan doesn't always take in what is in the literature even though he purports to read it. He's now looking at homogenization techniques for temperature to check the Siberian temperature data. We keep telling him the decline is also in N. Europe, N. America (where we use all the recently homogenized Canadian data). The decline may be slightly larger in Siberia, but it is elsewhere as well.

Also Siberia is one of the worst places to look at homogeneity, as the stations aren't that close together (as they are in Fennoscandia and most of Canada) and also the temperature varies an awful lot from year to year.

Recently rejected two papers (one for JGR and for GRL) from people saying CRU has it wrong over Siberia. Went to town in both reviews, hopefully successfully. If either appears

I will be very surprised, but you never know with GRL.

Cheers

Phil

Cheers

Phil

At 11:20 30/03/2004 -0500, you wrote:

Phil,

Have you seen this piece of crap by Esper?

The JGR paper, which Scott is supposed to be finalizing, demonstrates quite convincingly that the greater amplitude of Esper et al is due to spatial and seasonal sampling,
mike

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Phil Jones <p.jones@uea.ac.uk>
To: Scott Rutherford <srutherford@rwu.edu>
Subject: RoG Data
Date: Fri May 7 16:34:52 2004
Cc: "Michael E. Mann" <mann@virginia.edu>

Scott and Mike,

It's been a long week catching up from 3 weeks away. Getting another email from McIntyre asking me for paleo data series I don't have (I'm not going to reply, by the way even though he calls me Phil and other emails he sends me are to Dr Crowley and Dr. Briffa who've also not replied) reminded me that I agreed with Mike to put together as many of the series from the RoG paper onto a page on the CRU web site.

So, with this in mind, can you send me the data for the various plots. I checked the paper and Fig 1 doesn't need anything, so this leave Figs 3 (on the boreholes), 5 (with the various NH/SH/Global series) and 8 (with all the various model runs).

Figure 3 should be trivial as borehole data are only every 50 years. For the other 2 plots

I'm after the annual values of each series and the smoothed ones that get plotted. Hope this

won't take too long to do. I'm going to send emails to a few people to check we can make the data available (mainly the modellers, but also Tas van Ommen).

Cheers

Phil

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From: Phil Jones <p.jones@uea.ac.uk>

To: "Tas van Ommen" <tas.van.ommen@utas.edu.au>, Caspar Ammann <ammann@ucar.edu>,

Subject: RoG paper

Date: Fri May 7 16:43:21 2004

Dear Tas and Caspar,

Attached is the proof version of the RoG paper with Mike Mann. This is about 99.99% the final one. Mike and I sent back a few small changes to AGU a month or so ago. Keep this to yourself for a while yet - I would expect the paper out sometime in the July/August period.

Many of us in the paleo field get requests from skeptics (mainly a guy called Steve McIntyre in Canada) asking us for series. Mike and I are not sending anything, partly because we don't have some of the series he wants, also partly as we've got the data through contacts like you, but mostly because he'll distort and misuse them.

Despite this, Mike and I would like to make as many of the series we've used in the RoG plots available from the CRU web page. Can we do this with the series we've got from you? You don't have to do anything, except to reply yes or no !

Cheers

Phil

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From: f037 <M.Hulme@uea.ac.uk>
To: Aiguo Dai <adai@cgd.ucar.edu>
Subject: denial or delusion? ... Aiguo's response
Date: Sat, 8 May 2004 07:59:14 +0100
Cc: <jprospero@rsmas.miami.edu>, <m.hulme@uea.ac.uk>, <p.jones@uea.ac.uk>, <plamb@ou.edu>, <trenbert@cgd.ucar.edu>

Dear Aiguo,

You've done a great job in putting this together so quickly and clearly. I have a couple of additional comments to make on it, but can't do so until Tuesday. You (we?) might also like to think of the reply being multi-authored, including Phil, Pete, Kevin, Joe and myself.

I must say that when I first read this paper a couple of weeks ago I wrote it off as so bad (so, so bad) that it didn't even deserve a response. To pretend that the Sahel drought didn't happen (i.e., a pure artifact of wrongful use of rainfall data) is the most astounding assertion, almost on a par with holocaust denial. Try putting that proposition to the millions of inhabitants of the Sahel in the 1970s, 1980s and 1990s, many of whom died as a direct consequence and whose livelihoods were devastated. Adrian Chappell may never have visited the region, but I know Clive Agnew has (many times) - and he should know better. I did my PhD research in the region in the early 1980s and I know exactly what the rainfall conditions were like and how much ordinary people suffered as a consequence. My PhD was on rainfall variability and local water supplies in Sudan and I visited and talked to many villagers in the region.

Anyway, Phil first suggested that a corrective reply was needed and I can see the value of doing so, especially with IPCC AR4 approaching. It just seems to me such a shame that such poor science is being done by some people - in this case I don't think there is a deeper motive on the part of Chappell and Agnew than pure delusion and incompetence - and, worse, that a journal like IJC will publish it.

Thanks again for your efforts,

Mike

>===== Original Message From Aiguo Dai <adai@cgd.ucar.edu> =====

>Dear All,

>

>Soon after I sent out my last email, I quickly realized that there is
>another fundamental error in their rainfall model eq.(1): the regional
>station numbers n_a and n_b should be replaced with regional areas. This
>can be seen clearly in the following example: suppose region a has only
>one station whose long-term mean rainfall happens to be the same as
>region a's mean, and region b has 100 stations. Then their model would
>give the completely wrong estimate of rainfall for region (a+b), while
>the area-weighted version would still work. This is an obvious error, but
>it apparently could be easily overlooked. Their model seems to be
>originated from their incorrect perception that regional rainfall has

>been traditionally derived using the simple arithmetic mean of all station
>data. After reading the leader author's response to Joe's comments, I
>could not believe that they still think previous analyses are simpler than
>theirs!

>
>I also forgot to point out in my earlier draft the fact that even if their
>modelled time series were a reasonable proxy of Sahel rainfall, their
>results would still have had little implications to previous analyses of
>Sahel rainfall. This is because their analysis maximized the effects of
>changing station networks by the design of their model and by choosing
>the boundary of the two sub-Sahel region at 6deg.W, whereas in most previous
>analyses these effects were minimized by area-weighted averaging (Jones and
>Hulme, 1996).

>
>Sorry for the overlook of these issues in my earlier email.

>
>Regards,

>
>--Aiguo Dai

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>
>

>> Dear All,

>>
>> I was asked by Kevin to work out a rebuttal to Chappell and Agnew
>> (2004). After reading
>> it a couple of times, I found the main reason why they came to their
>> results: they devised a
>> Sahel rainfall model (eq. 1) with a necessary condition that the
>> constants a and b
>> represent the mean rainfall for the west and east part of the Sahel.
>> However, later in their
>> paper, they estimated a and b by a non-linear least-squares fitting to
>> observed rainfall
>> data, and their a (=973mm) and b (=142mm) are nowhere near the actual
>> mean rainfall
>> for these sub-Sahel regions (~645.5 mm and 471.2mm). In essence, their
>> rainfall model
>> and thus their modelled rainfall time series are no longer relevant to
>> Sahel rainfall!

>>
>> I have seen many bad papers, but this one is the worst of all, not only
>> because they
>> misled the reader with their model (intentionally or unintentionally),
>> but also because they
>> made all kinds of unfounded pure speculations about the implications of
>> their results.

>>
>> I did some quick analyses using data extracted from the update GHVN2 and
>> wrote a
>> comment paper, which is attached as Word file. Any comments will be

>> appreciated.

>>

>> Regards,

>>

>> Aiguo

>>

>> Phil Jones wrote:

>>

>>>

>>> Dear All,

>>> Several emails today. Kevin's encouraging Aiguo Dai to write a

>>> response as well,

>>> so it might be worth some co-ordination. 2 responses might be better

>>> than one, though, so I'll

>>> leave it up to you.

>>> They have dug themselves into a bigger hole in their response to

>>> Joe. Joe's assessment

>>> of their reasoning is exactly right. Also you can't write a paper

>>> saying an analysis is flawed and

>>> then say we don't dispute the local evidence for drought ! This is

>>> naive in the extreme and

>>> dumb. I've heard this excuse several times in the past with other

>>> contentious papers.

>>> The one problem there might be in a response is getting a quick

>>> turnaround with IJC.

>>> With the response a strongly worded letter should go to the editor

>>> (Glenn McGregor)

>>> requesting a fast-track review. The journal does this. As Kevin says

>>> any response short

>>> be short and to the point.

>>>

>>> Cheers

>>> Phil

>>>

>>>

>>> At 18:17 06/05/2004 -0400, Joseph M. Prospero wrote:

>>>

>>>> From: "A.Chappell" <A.Chappell@salford.ac.uk>

>>>> To: "Joseph M. Prospero" <jprospero@rsmas.miami.edu>

>>>> Cc: "Clive Agnew" <clive.agnew@man.ac.uk>

>>>> Subject: Re: Sahel drought "artifact"

>>>> Date: Tue, 13 Apr 2004 12:13:48 +0100

>>>>

>>>> Dear Professor Prospero,

>>>>

>>>> Thank you for your email. I read your paper with interest. It does

>>>> indeed show a strong correlation with conventional estimates of mean

>>>> annual rainfall. However, the paper implicitly assumes that the

>>>> mean annual rainfall represents the variation in rainfall for the

>>>> entire region. Our paper shows that those statistics are flawed

>>>> because of the changing station networks and that those regional

>>>> statistics do not show a 'drought' in the Sahel. Our paper does not

>>>> dispute the local scale evidence for drought.

From: Tom Wigley <wigley@cgd.ucar.edu>
To: Sarah Raper <sraper@awi-bremerhaven.de>, Sarah Raper <s.raper@uea.ac.uk>
Subject: volc paper
Date: Sat, 15 May 2004 08:56:00 -0600
Cc: Ben Santer <santer1@llnl.gov>, Caspar Ammann <ammann@ucar.edu>
Attachment: volc.doc

Dear Sarah,

Ben and I have had some long discussions about this paper, and I have made quite a few changes as a consequence. Most of these are minor -- but I realized that my statement that the peak cooling depended logarithmically on the sensitivity was potentially confusing. For this to be the case one has to have a relationship like

$$T_{\max} = A + B \ln(S)$$

which implies odd results for very low sensitivity. Instead, I have fitted a relationship of the form

$$T_{\max} = A [S^{**n}]$$

which gives $T_{\max} = 0$ when $S = 0$.

I have fitted a similar relationship to the decay time results, and I have done the same for the LG98 results. All this information has been added to the manuscript. It helps in understanding the differences between us and LG98.

I had hoped to send this off earlier this week, i.e., before I go to Buenos Aires (tomorrow), but I never received the copyright form from you. Then I remembered that you were at that IPCC meeting in Ireland. So I have asked Liz Rothney to send the ms off next week as soon as she gets the copyright form from you. So please fax this back (303 497 1333) as soon as possible.

Best wishes,
Tom.

From: Keith Briffa <k.briffa@uea.ac.uk>
To: v.shishov@uea.ac.uk
Subject: Fwd: Re: Russian daily data
Date: Tue Jun 8 15:20:06 2004

From: Dale Patrick Kaiser <kaiserdp@ornl.gov>
Reply-To: kaiserdp@ornl.gov
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Russian daily data
Date: Mon, 7 Jun 2004 10:31:02 -0400
User-Agent: KMail/1.5.3
Cc: d9k@ornl.gov
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean

Dear Keith,

I wish I could say that updating the Russian data is on the front burner for us right now, but I'm afraid it's not. I'm having to plan some proposals and have been pulled off part of my normal CDIAC work for about 6 months to work on a special project. And in our small group, I'm the only climate guy (and the one that has done the Russian work thus far). Thus, the first suggestion I have is to discuss the data with NCDC; perhaps the best person to start with would be Pasha Groisman. Years ago, when I did the Russian work, the data were actually transferred from Russia to NCDC and then on to us, so I wouldn't be surprised if NCDC was holding updated data or at least could get ahold of data relatively easily. Perhaps you've already corresponded directly w/Slava Razuvaev or one of his colleagues at RIHMI-WDC? I'm afraid it's been quite a while since I've spoken w/Slava.

Wait, maybe there is another way.... I've just remembered about NCDC's Global Daily Climate Network:

[1] <http://www.ncdc.noaa.gov/oa/climate/research/gdcn/gdcn.html>

I have not learned much about these holdings, but if you check it out perhaps they've incorporated more recent data daily into this database for the FSU.

I sure hope so.

I'm sorry that I cannot be of more help at this time. With any luck CDIAC can turn its attention to updates of these data in 2005.

Regards,

Dale

On Friday 04 June 2004 7:18 am, you wrote:

> Dear Dale

> sorry to contact you out of the blue , but Phil Jones suggested I check

> with you about the status of daily temperature (and possibly precipitation)

> data for Russia that I believe you and colleagues might be planning to

> update. I work with tree-ring data in Northern Russia and we are
> particularly interested in looking at growing season and snow lie changes
> in recent years that may be influencing the growth rates of trees and the
> position of the tree line . We are especially interested in data for the
> Yamal Peninsula ,Taimyr and Indigirka (though we would also like to explore
> snow lie changes over the whole of northern Siberia eventually). Is there
> any chance of getting updated data for these initial regions in the near
> term , and perhaps the wider area eventually? We would be really grateful
> for any help in this regard.

> Very best wishes and thanks for your help

> Keith

>

> --

> Professor Keith Briffa,

> Climatic Research Unit

> University of East Anglia

> Norwich, NR4 7TJ, U.K.

>

> Phone: +44-1603-593909

> Fax: +44-1603-507784

>

> [2]<http://www.cru.uea.ac.uk/cru/people/briffa/>

--

Dale P. Kaiser

Carbon Dioxide Information

Analysis Center

Environmental Sci. Division

Oak Ridge National Laboratory

Oak Ridge, TN 37831

(865) 241-4849

(865) 574-2232 (fax)

kaiserdp@ornl.gov

[3]<http://cdiac.ornl.gov>

--

Professor Keith Briffa,

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[4]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.ncdc.noaa.gov/oa/climate/research/gdcn/gdcn.html>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>
3. <http://cdiac.ornl.gov/>
4. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Tom Wigley <wigley@cgd.ucar.edu>
To: Sarah Raper <sraper@awi-bremerhaven.de>, Sarah Raper <s.raper@uea.ac.uk>
Subject: [Fwd: IPCC announcement of opportunity]
Date: Thu, 10 Jun 2004 18:00:14 -0600
Cc: Ben Santer <santer1@llnl.gov>

This is a multi-part message in MIME format. -----060109000609030501070308
Content-Type: multipart/alternative; boundary="-----070901080902050505090308"
-----070901080902050505090308 Content-Type: text/plain; charset=us-ascii;
format=flowed Content-Transfer-Encoding: 7bit Sarah, I realize that you have got a copy of
this. What I am concerned about is the use of MAGICC in AR4. It is likely that the only way
that MAGICC can be legitimately used is for it to be (again!) calibrated against the
various AOGCMs being run for AR4. The AOGCM data that will be available this time will
allow us to do this more comprehensively than your TAR analysis. I think this is something
we should do together this time. I will talk to Jerry Meehl about this tomorrow or next
week, and also discuss how best to do this statistically with Doug Nychka -- with a view to
submitting a joint proposal. I would also like to involve Ben, since he is adept at getting
appropriate data from PCMDI/CMIP data files, and he can add insights that we may otherwise
miss. So the proposal would involve you, me, Doug and Ben. Tom. =====

Original Message ----- Subject: IPCC announcement of opportunity Date: Thu, 10 Jun 2004
16:22:15 -0700 From: Curtis Covey To: George Boer , Ed Schneider , Wei-Chyung Wang , Tim
Barnett , Scott Power , Jouni Raisanen , Yanli Jia , David Webb , Pierre Friedlingstein ,
Sarah Raper , Jonathan Gregory , Marc Pontaud , Greg Flato , Tom Wigley , Phil Duffy , Dave
Ritson , Valentina Pavan , Ken Caldeira , Ietret , Ken Sperber , Brian Soden , Fred Singer
, David Karoly , DUFRESNE Jean-Louis , Andrei Sokolov , Olivier de Viron , kattsov , Ping
Liu , Tom Knutson , Youichi Tanimoto , Kwang-Yul Kim , "Siobhan O'Farrell" , Kristin
Kuntz-Duriseti , Steve Marcus , "Francisco E. Werner" , Mingfang Ting , Cecilia Bitz ,
"Cathrine.Myrmehl" , "Gregory M. Ostermeier" , Dave Stephenson , "Ola.Johannessen" ,
Svetlana Kuzmina , Alpert Pinhas , Hirsch Tali , Evgeny Volodin , Dan Vimont , Ken Kunkel ,
Huei-Ping Huang , Zeng-Zhen Hu , "I.-S. Kang" , "Vikram M. Mehta" , Bob Iacovazzi ,
hengliu@students.uiuc.edu, Daithi Stone , Ray Bradley , Robert Kaufmann ,
d.stainforth1@physics.ox.ac.uk, raghu@ncmrwf.gov.in, Rob Colman , jhurrell@ucar.edu, Chris
Huntingford , Peter Webster , shj@atmos.yonsei.ac.kr, ysun@al.noaa.gov, Irina Gorodetskaya
CC: Ron Stouffer , Mojib Latif , Jerry Meehl , Bryant McAvaney , Peter Gleckler Dear
colleague, Attached (in PDF) is an announcement of opportunity to participate in analyses
of global coupled model output for the Fourth Assessment Report of the Intergovernmental
Panel on Climate Change. This is an open announcement, so please feel free to forward it to
anyone who may be interested. Sincerely, The WGCM Climate Simulation Panel Gerald Meehl,
Chair IPCC_analysis@ucar.edu -----070901080902050505090308 Content-Type:

text/html; charset=us-ascii Content-Transfer-Encoding: 7bit Sarah,
I realize that you have got a copy of this.
What I am concerned about is the use of MAGICC in AR4. It is likely that
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than your TAR analysis. I think this is something we should do together this time.
I will talk to Jerry Meehl about this tomorrow or next week, and also discuss
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a joint proposal. I would also like to involve Ben, since he is adept at getting
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we may otherwise miss. So the proposal would involve you, me, Doug and Ben.

Tom.

=====

----- Original Message -----

Subject: IPCC announcement of opportunity

Date: Thu, 10 Jun 2004 16:22:15 -0700

From: Curtis Covey [1]<covey1@llnl.gov>

To: George Boer [2]<george.boer@ec.gc.ca>, Ed Schneider [3]<schneide@cola.iges.org>, Wei-Chyung Wang [4]<wang@climate.cestm.albany.edu>, Tim Barnett [5]<tbarnett@ucsd.edu>, Scott Power [6]<s.power@bom.gov.au>, Jouni Raisanen [7]<jouni.raisanen@smhi.se>, Yanli Jia [8]<Yanli.Jia@soc.soton.ac.uk>, David Webb [9]<David.J.Webb@soc.soton.ac.uk>, Pierre Friedlingstein [10]<pierre@lsce.saclay.cea.fr>, Sarah Raper [11]<s.raper@uea.ac.uk>, Jonathan Gregory [12]<jonathan.gregory@metoffice.com>, Marc Pontaud [13]<marc.pontaud@meteo.fr>, Greg Flato [14]<gflato@ec.gc.ca>, Tom Wigley [15]<wigley@ucar.edu>, Phil Duffy [16]<pduffy@llnl.gov>, Dave Ritson [17]<critson@slac.stanford.edu>, Valentina Pavan [18]<pavan@cineca.it>, Ken Caldeira [19]<kenc@llnl.gov>, letreut [20]<letreut@lmd.jussieu.fr>, Ken Sperber [21]<sperber1@llnl.gov>, Brian Soden [22]<bjs@gfdl.gov>, Fred Singer [23]<singer@sepp.org>, David Karoly [24]<dkaroly@ou.edu>, DUFRESNE Jean-Louis [25]<dufresne@icess.ucsb.edu>, Andrei Sokolov [26]<sokolov@mit.edu>, Olivier de Viron [27]<o.deviron@oma.be>, kattsov [28]<kattsov@main.mgo.rssi.ru>, Ping Liu [29]<pliu@hawaii.edu>, Tom Knutson [30]<tk@gfdl.noaa.gov>, Youichi Tanimoto [31]<tanimoto@ees.hokudai.ac.jp>, Kwang-Yul Kim [32]<kwang@cyclo.met.fsu.edu>, "Siobhan O'Farrell" [33]<Siobhan.O'Farrell@csiro.au>, Kristin Kuntz-Duriseti [34]<kkd@stanford.edu>, Steve Marcus [35]<slmarcus@mail1.jpl.nasa.gov>, "Francisco E. Werner" [36]<cisco@unc.edu>, Mingfang Ting [37]<ting@atmos.uiuc.edu>, Cecilia Bitz [38]<bitz@apl.washington.edu>, "Cathrine.Myrmehl" [39]<Cathrine.Myrmehl@nersc.no>, "Gregory M. Ostermeier" [40]<greg@atmos.washington.edu>, Dave Stephenson [41]<daves@met.reading.ac.uk>, "Ola.Johannessen" [42]<Ola.Johannessen@nersc.no>, Svetlana Kuzmina [43]<Svetlana.Kuzmina@niersc.spb.ru>, Alpert Pinhas [44]<pinhas@cyclone.tau.ac.il>, Hirsch Tali [45]<tali@vortex.tau.ac.il>, Evgeny Volodin [46]<volodin@inm.ras.ru>, Dan Vimont [47]<dvimont@atmos.washington.edu>, Ken Kunkel [48]<k-kunkel@uiuc.edu>, Huei-Ping Huang [49]<huei@ldeo.columbia.edu>, Zeng-Zhen Hu [50]<hu@cola.iges.org>, "I.-S. Kang" [51]<kang@climate.snu.ac.kr>, "Vikram M. Mehta" [52]<vikram@crces.org>, Bob Iacovazzi [53]<rai jr@crces.org>, [54]hengliu@students.uiuc.edu, Daithi Stone [55]<stoned@atm.ox.ac.uk>, Ray Bradley [56]<rbradley@geo.umass.edu>, Robert Kaufmann [57]<kaufmann@crsa.bu.edu>, [58]d.stainforth1@physics.ox.ac.uk, [59]raghu@ncmrwf.gov.in, Rob Colman [60]<r.colman@bom.gov.au>, [61]jhurrell@ucar.edu, Chris Huntingford [62]<chg@ceh.ac.uk>, Peter Webster [63]<pjw@eas.gatech.edu>, [64]shj@atmos.yonsei.ac.kr, [65]ysun@al.noaa.gov, Irina Gorodetskaya [66]<irina@ldeo.columbia.edu>

CC: Ron Stouffer [67]<Ronald.Stouffer@noaa.gov>, Mojib Latif [68]<mlatif@ifm.uni-kiel.de>, Jerry Meehl [69]<meehl@ucar.edu>, Bryant McAvaney [70]<B.McAvaney@bom.gov.au>, Peter Gleckler [71]<gleckler1@llnl.gov>

Dear colleague,

Attached (in PDF) is an announcement of opportunity to participate in analyses of global coupled model output for the Fourth Assessment Report of the Intergovernmental Panel on Climate Change. This is an open announcement, so please feel free to forward it to anyone who may be interested.

nQyVqdq0CsWJX2LHPQJUwL7xGBRbzBOR9LBTInE/xqo2lObLMYrXWcdwf3I3nE0fVx4tT70s
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Subject: Global change and ecosystems
Date: Thu, 17 Jun 2004 16:39:42 +0100

2. Call for proposals - Thematic call in the area of 'Global change and ecosystems'.

OJ C159 (16.06.2004) p.3
Deadline for submissions: 26.10.2004

Activity: Priority thematic area 'Sustainable Development, Global Change and Ecosystems'; Sub-priority 'Global Change and Ecosystems'.

Call identifier: FP6-2004-Global-3

Total indicative budget: EUR 205 million

Areas called and Instruments:

- Area 6.3.I: Impact and mechanisms of greenhouse gas emissions and atmospheric pollutants on climate, ozone depletion and carbon sinks (IP, STREP, CA)
- Area 6.3.II: Water cycle, including soil related aspects (IP, STREP, CA)
- Area 6.3.III: Biodiversity and ecosystems (IP, STREP, CA, NoE)
- Area 6.3.IV: Mechanisms of desertification and natural disasters (IP, STREP, CA)
- Area 6.3.V: Strategies for sustainable land management, including coastal zones, agricultural land and forests (IP, STREP, CA)
- Area 6.3.VI: Operational forecasting and modelling including global climatic change observation systems (IP)
- Area 6.3.VII: Complementary research (IP, CA)
- Area 6.3.VIII: Cross-cutting issue: Sustainable Development concepts and tools (STREP, CA)
- Area 6.3.IX: Specific Support Actions (SSA)

FURTHER INFORMATION:

European Commission
The FP6 Information Desk
Directorate General RTD
B-1049 Brussels
www.cordis.lu/

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From: Phil Jones <p.jones@uea.ac.uk>
To: David Viner <d.viner@uea.ac.uk>
Subject: Re: Proposal for a new Tyndall-led European research initiative
Date: Fri Jun 18 16:14:57 2004
Cc: Clare Goodess <C.Goodess@uea.ac.uk>

I'll leave it up to you then.

Phil

At 16:04 18/06/2004 +0100, David Viner wrote:

Phil

Err! yes i think this would be good to get involved.

D

On 18 Jun 2004, at 15:40, Phil Jones wrote:

Dave and Clare,

I am presuming we (CRU) don't want to get involved with this.

Cheers

Phil

From: "Alex Haxeltine" <Alex.Haxeltine@uea.ac.uk>
To: "Terry Barker \ (DAE\)" <Terry.Barker@econ.cam.ac.uk>,
<wj.watson@sussex.ac.uk>,
"Andrew Jordan" <a.jordan@uea.ac.uk>,
"Bob Nicholls" <'rjn@soton.ac.uk'>,
"emily boyd" <e.boyd@uea.ac.uk>,
"Emma Tompkins" <e.tompkins@uea.ac.uk>,
"Franziska Matthies" <f.matthies@uea.ac.uk>,
"jonathan Kohler" <J.Kohler@uea.ac.uk>,
"Kate Brown" <k.brown@uea.ac.uk>,
<kevin.anderson@umist.ac.uk>,
<n.w.arnell@soton.ac.uk>,
"Neil Adger" <N.Adger@uea.ac.uk>,
"Nick Brooks" <nick.brooks@uea.ac.uk>,
"Phil Jones" <p.jones@uea.ac.uk>,
"rachel warren" <r.warren@uea.ac.uk>,
"simon shackley" <simon.shackley@umist.ac.uk>,
"Steve Sorrell" <S.R.Sorrell@sussex.ac.uk>,
"suraje Dessai" <s.dessai@uea.ac.uk>

Subject: Proposal for a new Tyndall-led European research initiative

Date: Fri, 18 Jun 2004 15:16:20 +0100

Organization: University of East Anglia

X-Mailer: Microsoft Outlook, Build 10.0.3311

Importance: Normal

Dear Colleague,

The Tyndall Centre is intending to lead a bid for a large EU research project (ca 12-15 million Euros in the initial bid) on climate change adaptation and mitigation strategies in Europe. The call was announced this week with outline bids (ca. 20 pages) due by October (3rd call of the sixth framework programme, FP6).

Please find attached a copy of an invitation that has been sent out to a key set of European partners. This provides a little further information on the proposed scope and content of the project. We will be holding a planning meeting with European partners from the evening of Monday 19th July to end of Tuesday 20th July 2004.

You are receiving this email because we thought that you might have some interest in participating in this project. We would therefore like to hold an internal planning meeting of all interested Tyndall-linked researchers on the 19th July (starting at lunchtime; ca 3-4 hours long). Please let us know by 25th June, if you would like to take part in this internal planning meeting; and also whether you would like to make a short presentation at the meeting, about how your work with the Tyndall Centre might contribute. If you cannot attend on the 19th but are nevertheless interested in contributing to the proposal, please also let us know.

Warm regards,

Mike Hulme

John Schellnhuber

Alex Haxeltine

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich Email p.jones@uea.ac.uk

NR4 7TJ

UK

<ADAM invite to

planning meeting on 19-20 July.rtf>

+++++

Dr David Viner

Climatic Research Unit

University of East Anglia

Norwich NR4 7TJ

Tel: +44 1603 592089

Fax: +44 1603 507784

[1]<http://www.cru.uea.ac.uk/link> (With Information Forum)

[2]<http://www.e-clat.org> Tourism and Climate Change (With Information Forum)

[3]<http://ipcc-ddc.cru.uea.ac.uk>

+++++

</blockquote></x-html>

Prof. Phil Jones

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Norwich Email p.jones@uea.ac.uk

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References

1. <http://www.cru.uea.ac.uk/link>

2. <http://www.e-clat.org/>

3. <http://ipcc-ddc.cru.uea.ac.uk/>

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From: Tom Wigley <wigley@cgd.ucar.edu>
To: Sarah Raper <sraper@awi-bremerhaven.de>, Ben Santer <santer1@llnl.gov>, Doug Nychka <nychka@cgd.ucar.edu>
Subject: [Fwd: AR4 analyses]
Date: Thu, 01 Jul 2004 10:07:36 -0600

This is a multi-part message in MIME format. -----020800020009020904000309
Content-Type: multipart/alternative; boundary="-----020101090700030501080805"
-----020101090700030501080805 Content-Type: text/plain; charset=us-ascii;
format=flowed Content-Transfer-Encoding: 7bit ----- Original Message ----- Subject:
AR4 analyses Date: Thu, 01 Jul 2004 09:23:32 -0600 From: Jerry Meehl To: Curtis Covey ,
wigley Thanks Tom. We have registered you, and will keep you posted. You are correct that
the forcing data you require may not be available from all models. Hopefully there will be
a few who will have what you need. Jerry and Curt ----- Original Message -----
Subject: AR4: missing attachment Date: Wed, 30 Jun 2004 17:51:11 -0600 From: Tom Wigley
Organization: NCAR/CGD To: Jerry Meehl , Sarah Raper , Sarah Raper , Ben Santer , Doug
Nychka -----020101090700030501080805 Content-Type: text/html; charset=us-ascii
Content-Transfer-Encoding: 7bit
----- Original Message -----

Subject: AR4 analyses
Date: Thu, 01 Jul 2004 09:23:32 -0600
From: Jerry Meehl [1]<meehl@ucar.edu>
To: Curtis Covey [2]<covey1@llnl.gov>, wigley [3]<wigley@ucar.edu>

Thanks Tom. We have registered you, and will keep you posted. You are correct that the
forcing data you require may not be available from all models. Hopefully there will be a
few who will have what you need.

Jerry and Curt
----- Original Message -----

Subject: AR4: missing attachment
Date: Wed, 30 Jun 2004 17:51:11 -0600
From: Tom Wigley [4]<wigley@cgd.ucar.edu>
Organization: NCAR/CGD
To: Jerry Meehl [5]<meehl@cgd.ucar.edu>, Sarah Raper [6]<sraper@awi-bremerhaven.de>, Sarah
Raper [7]<s.raper@uea.ac.uk>, Ben Santer [8]<santer1@llnl.gov>, Doug Nychka
[9]<nychka@cgd.ucar.edu>

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From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: HIGHLY CONFIDENTIAL
Date: Thu Jul 8 16:30:16 2004

Mike,

Only have it in the pdf form. FYI ONLY - don't pass on. Relevant paras are the last 2 in section 4 on p13. As I said it is worded carefully due to Adrian knowing Eugenia for years. He knows they're wrong, but he succumbed to her almost pleading with him to tone it down as it might affect her proposals in the future !

I didn't say any of this, so be careful how you use it - if at all. Keep quiet also that you have the pdf.

The attachment is a very good paper - I've been pushing Adrian over the last weeks to get it submitted to JGR or J. Climate. The main results are great for CRU and also for ERA-40. The basic message is clear - you have to put enough surface and sonde obs into a model to produce Reanalyses. The jumps when the data input change stand out so clearly. NCEP does many odd things also around sea ice and over snow and ice.

The other paper by MM is just garbage - as you knew. De Freitas again. Pielke is also losing all credibility as well by replying to the mad Finn as well - frequently as I see it.

I can't see either of these papers being in the next IPCC report. Kevin and I will keep them out somehow - even if we have to redefine what the peer-review literature is !

Cheers

Phil

Mike,

For your interest, there is an ECMWF ERA-40 Report coming out soon, which shows that Kalnay and Cai are wrong. It isn't that strongly worded as the first author is a personal friend of Eugenia. The result is rather hidden in the middle of the report.

It isn't peer review, but a slimmed down version will go to a journal. KC are wrong because

the difference between NCEP and real surface temps (CRU) over eastern N. America doesn't happen with ERA-40. ERA-40 assimilates surface temps (which NCEP didn't) and doing this makes the agreement with CRU better. Also ERA-40's trends in the lower atmosphere are all physically consistent where NCEP's are not - over eastern US.

I can send if you want, but it won't be out as a report for a couple of months.

Cheers

Phil

Prof. Phil Jones

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From: Phil Jones <p.jones@uea.ac.uk>

To: t.m.melvin@uea.ac.uk

Subject: Polar Urals

Date: Wed Jul 21 15:06:31 2004

Tom,

Can you send me via email the two sets of results you showed this morning of the dating for the trw and mxd series from the Polar Urals? Just the two separate ones - forget Yamal.

Cheers

Phil

Prof. Phil Jones

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University of East Anglia

Norwich Email p.jones@uea.ac.uk

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From: Phil Jones <p.jones@uea.ac.uk>
To: dwlarson@uoguelph.ca
Subject: Re:
Date: Fri Jul 23 15:29:11 2004

Doug,

Maybe Steve sent you the two emails I've resent. Ignore my ramblings at the end of one, but I was getting a little fed up. The Legates email is at the end, in case you're interested.

The pdf is worth a read. Odd that he writes a press release, then starts working on a paper.

We've very occasionally written a press release, but only after the paper has come out.

I tried to explain the 'missing' rings. They aren't missing, but due to the samples not being right for density measurements. All Schweingruber's chronologies are constructed this way - traditional ring width measurements aren't made. Some of the Russian groups he's worked with have added extra ring width cores and sometime get longer series, but all the data Keith and I work with is from Fritz, so if density is missing, then RW is also.

Fritz did almost all the coring - 99% of the sites. We only help coring on a couple of occasions.

This comes from alignment tracking as you say, but Fritz also says it is partly due to the need to extract the lignin and to avoid resin. When we cored together, he was always saying we weren't doing it properly getting twisted cores. I'm not a proper dendro person,

as I only got into this because of Keith - it may not be lignin, but something has to be extracted with solvents.

The Polar Urals site was collected by Fritz and Stepan Shiyatov. There are living trees back to the 1500s and then stumps at a slightly higher elevation. Stepan has been back more recently and regeneration is occurring at higher levels, but it is taking time. Tree lines

take a while to respond to the recent warmth in some regions. Once the trees are established

and not killed by frosts/snow in winter they survive even if it gets cooler. I discussed this

in a review paper in RoG attached. The section on the issue is brief.

All the cores were collected over a couple of days. Fritz made a mistake with the labelling

for one core and that explains the 400 years of missing values. Someone at WDCP must have combined the cores with the same ids. Dendro people are always looking for the oldest trees and we kept the earliest series in. Steve seems to have a thing about these and the 10th and 11th centuries, but they are correctly dated. Fritz uses loads of plots and pointer years and doesn't make mistakes normally. There is a very distinct year at

AD 1032. Fritz is also cross dating with LWW and EWW and other features and not just on RW. I say not just, he normally does with density. At the coring stage Fritz had no idea

of the ages of the stumps (well just the number of years). There may have been samples off the front that couldn't be dated at all, for all I know. I suspect though they are roughly

the same calendar age, as the site has distinct dates for the start of trees, which represent

regeneration periods. Maybe you can try and explain the tree-line argument to Steve.

When he had to omit parts of cores, he was always able to know where the two parts sat in the sequence. We need to keep them together to do things like RCS.

Anyway, I have to go home - it's been very wet lately and the grass has grown. The lawn must be mowed when the sun shines.

Keep pushing that he should write up what he does (and Ross) in proper journals. E&E and Climate Research are not read by many now. I only look at them when I get alerted and I remain exasperated.

Cheers

Phil

Legates email

Phil Jones has made a valid point in that some of the articles cited in my critique do not 'directly' address problems with Mann and Jones (MJ) but rather, address problems with earlier works by Mann, Bradley, and Hughes (MBH) and other colleagues. Fair enough - I have changed the critique to reflect that fact. The revised version has been posted since July 19 at:

[1]<http://www.ncpa.org/pub/ba/ba478/ba478.pdf>

However, I still contend that most of my original arguments - namely, the problems with the shaft, blade, and sheath - apply equally to Mann and Jones as well as the other Mann et al. manifestations of the 'hockey stick'.

MJ incorporate data from a number of the same sources as those used by MBH; for example, Mann's unpublished PC1 from the western North American tree-ring data, Cook's Tasmanian tree rings, Thompson's Quelccaya and Dunde ice core oxygen isotope records (the latter embedded in Yang's Chinese composite), and Fisher's stacked Greenland ice core oxygen isotope record. Calibration and verification of MJ includes the flawed MBH curve. Thus, any errors in MBH effectively undermine the calibration-verification results of MJ, leaving this study unsupported and any problems with the underlying common proxies identified in critiques of MBH will also result in identical problems in MJ.

My criticism regarding the blade is that 0.6 deg C warming for the last century is noted by the IPCC whereas MJ (and other M et al representations) have up to 0.95 deg C warming in their observed record.

See MJ's figure 2 where for the global and NH reconstruction, their estimates for 2000 exceed +0.4 and +0.5 (nearly +0.6), respectively. MJ's NH curve is included in the attached graph. Thus, I stand by my criticism of MJ on this point, which is more egregious in MJ than other M et al representations.

>From Jones: "The trend over the 20th century in the Figure and in the instrumental data. IPCC quotes 0.6 deg C over the 1901-2000 period. Fact - but Legates is eyeballing the curve to get 0.95 deg C. A figure isn't given in Mann and Jones (2003). Take it from me the trend is about the same as the instrumental record."

Funny, but there IS a figure in MJ - see their Figure 2. As for me 'eyeballing' an apparently non-existent curve, I attach a figure from Soon et al. (2004) that contains a portion of MJ's Figure 2 to allow others to decide for themselves whether MJ suggest a twentieth century warming of 0.6 deg C or 0.95 deg C. Moreover, maybe someone can explain why every time Mann and his colleagues draft another curve, the temperature in 2000 gets warmer and warmer after the fact...

My criticisms regarding the sheath (largely from a paper on which I am working) stem from the characterization of the uncertainty by MJ that arises solely from the 'fit' statistics to the 1600-1855 period using cross-validation with, not observations, but composites of three previously compiled reconstructions, including that developed by MBH - the focus of known flaws and errors in the shaft. Note that some of the same data are used in both MBH and MJ, which doesn't allow for a truly independent cross-validation. My rather obvious point was not that fit statistics should not be included (as Jones asserts) but that MJ included no errors in either input realization (observations or proxy data) or other obvious sources of error. The claim by MBH and MJ is that only the model lack-of-fit contributes to uncertainty is inherently flawed.

Considerable errors exist in the representation of both fields - annual temperatures from both observations and proxy records - and must be incorporated. Clearly, there is a spatial bias associated with observations that are biased away from the oceans, high latitudes, and high altitudes. The spatial problem is far more pronounced when only a handful of proxies are used to represent the global temperatures at earlier time periods. Both MBH and MJ are equally guilty in this regard.

David R. Legates

Several people have asked me for the full references to the works I have cited. They are:

Chapman, D.S., M.G. Bartlett, and R.N. Harris (2004): Comment on 'Ground vs. surface air temperature trends: Implications for borehole surface temperature reconstructions' by M.E. Mann and G. Schmidt. *Geophysical Research Letters*, 31, L07205, doi:10.1029/2003GL019054.

Esper, J, E.R. Cook, and F.H. Schweingruber (2002): Low-frequency signals in long tree-ring chronologies for reconstructing past temperature variability, *Science*, 295, 2250-2253.

Esper, J, D.C. Frank, and R.J.S. Wilson (2004): Climate reconstructions: Low-frequency ambition and high-frequency ratification. *EOS, Transactions of the American Geophysical Union*, Vol. 85 (12):113,120.

IPCC TAR (Intergovernmental Panel on Climate Change, Third Assessment Report) (2001): *Climate Change 2001: The Scientific Basis*, Houghton, J.T., Ding, Y., Griggs, D.J., Noguer, M., van der Linden, P. J., Dai, X., Maskell, K., Johnson, C.A. (Eds.), Cambridge University Press.

Mann, M.E., R.S. Bradley, and M.K. Hughes (1998): Global-Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries, *Nature*, 392, 779-787. [see also the correction in *Nature* - Mann, Bradley, and Hughes, 2004]

Mann, M.E., R.S. Bradley, and M.K. Hughes (1999): Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations. *Geophysical Research Letters*, 26, 759-762.

Mann, M.E., and P.D. Jones (2003): Global surface temperature over the past two millennia, *Geophysical Research Letters*, 30(15), 1820, doi: 10.1029/2003GL017814.

Mann, M.E., and G. Schmidt (2003): Ground vs. surface air temperature trends: Implications for borehole surface temperature reconstructions. *Geophysical Research Letters*, 30(12), 1607, doi:10.1029/2003GL017170.

McIntyre, S., and R. McKittrick (2003): Corrections to the Mann et al (1998) Proxy Data Based and Northern Hemispheric Average Temperature Series. *Energy and Environment*, 14, 751-771.

Pollack, H.N., and J.E. Smerdon (2004): Borehole climate reconstructions: Spatial structure and hemispheric averages. *Journal of Geophysical Research*, 109, D11106, doi:10.1029/2003JD004163.

Rutherford, S., and M.E. Mann (2004): Correction to 'Optimal surface temperature reconstructions using terrestrial borehole data'. *Journal of Geophysical Research*, 109, D11107, doi:10.1029/2003JD004290.

Soon, W.-H., S.L. Baliunas, C. Idso, S. Idso, and D.R. Legates (2003): Reconstructing Climatic and Environmental Changes of the Past 1000 Years: A Reappraisal. *Energy and Environment*, 14:233-296.

Soon, W.-H., D.R. Legates, and S.L. Baliunas (2004): Estimation and Representation of Long-Term (>40 year) trends of Northern-Hemisphere-gridded Surface Temperature: A Note of Caution. *Geophysical Research Letters*, 31(3).

Prof. Phil Jones

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Email p.jones@uea.ac.uk

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References

1. <http://www.ncpa.org/pub/ba/ba478/ba478.pdf>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Janice Lough" <j.lough@aims.gov.au>
Subject: Re: liked the paper
Date: Fri Aug 6 09:26:49 2004

Janice,

Most of the data series in most of the plots have just appeared on the CRU web site. Go to data then to paleoclimate. Did this to stop getting hassled by the skeptics for the data series. Mike Mann refuses to talk to these people and I can understand why. They are just trying to find if we've done anything wrong. I sent one of them loads of series and he barely said a thankyou. It seems they are now going for Tom Crowley, Lonnie Thompson and Gordon Jacoby as most of their series are not on web sites.

Below is a link to an awful piece by Legates. He told me he is a writing a paper, but wrote the press release first ! The pdf is worth getting for a couple of sentences, when he said that MJ restricted their use of paleo series to those that had correlations with instrumental data ! It is a classic. 'Our uncertainty estimates are based solely on how well the proxy records match the observed data' !

The Legates piece must have been sent to loads of environment correspondents across the world and a number of op-ed pieces appeared. Some were awful. Most have had responses from Ray Bradley, Caspar Amman and others.

Hope all is well with you and all the best to all. Glad you enjoyed the paper.

Cheers

Phil

PS Do you want to get involved in IPCC this time? I'm the CLA of the atmospheric obs. chapter with Kevin Trenberth and we'll be looking for Contributing Authors to help the Lead Authors we have. Paleo is in a different section this time led by Peck and Eystein Janssen. Keith is a lead author as well.

Phil Jones has made a valid point in that some of the articles cited in my critique do not 'directly' address problems with Mann and Jones (MJ) but rather, address problems with earlier works by Mann, Bradley, and Hughes (MBH) and other colleagues. Fair enough - I have changed the critique to reflect that fact. The revised version has been posted since July 19 at:

[1]<http://www.ncpa.org/pub/ba/ba478/ba478.pdf>

However, I still contend that most of my original arguments - namely, the problems with the shaft, blade, and sheath - apply equally to Mann and Jones as well as the other Mann et al. manifestations of the 'hockey stick'.

MJ incorporate data from a number of the same sources as those used by MBH; for example, Mann's unpublished PC1 from the western North

American tree-ring data, Cook's Tasmanian tree rings, Thompson's Quelccaya and Dunde ice core oxygen isotope records (the latter embedded in Yang's Chinese composite), and Fisher's stacked Greenland ice core oxygen isotope record. Calibration and verification of MJ includes the flawed MBH curve. Thus, any errors in MBH effectively undermine the calibration-verification results of MJ, leaving this study unsupported and any problems with the underlying common proxies identified in critiques of MBH will also result in identical problems in MJ.

My criticism regarding the blade is that 0.6 deg C warming for the last century is noted by the IPCC whereas MJ (and other M et al representations) have up to 0.95 deg C warming in their observed record. See MJ's figure 2 where for the global and NH reconstruction, their estimates for 2000 exceed +0.4 and +0.5 (nearly +0.6), respectively. MJ's NH curve is included in the attached graph. Thus, I stand by my criticism of MJ on this point, which is more egregious in MJ than other M et al representations.

>From Jones: "The trend over the 20th century in the Figure and in the instrumental data. IPCC quotes 0.6 deg C over the 1901-2000 period. Fact - but Legates is eyeballing the curve to get 0.95 deg C. A figure isn't given in Mann and Jones (2003). Take it from me the trend is about the same as the instrumental record."

Funny, but there IS a figure in MJ - see their Figure 2. As for me 'eyeballing' an apparently non-existent curve, I attach a figure from Soon et al. (2004) that contains a portion of MJ's Figure 2 to allow others to decide for themselves whether MJ suggest a twentieth century warming of 0.6 deg C or 0.95 deg C. Moreover, maybe someone can explain why every time Mann and his colleagues draft another curve, the temperature in 2000 gets warmer and warmer after the fact...

My criticisms regarding the sheath (largely from a paper on which I am working) stem from the characterization of the uncertainty by MJ that arises solely from the 'fit' statistics to the 1600-1855 period using cross-validation with, not observations, but composites of three previously compiled reconstructions, including that developed by MBH - the focus of known flaws and errors in the shaft. Note that some of the same data are used in both MBH and MJ, which doesn't allow for a truly independent cross-validation. My rather obvious point was not that fit statistics should not be included (as Jones asserts) but that MJ included no errors in either input realization (observations or proxy data) or other obvious sources of error. The claim by MBH and MJ is that only the model lack-of-fit contributes to uncertainty is inherently flawed.

Considerable errors exist in the representation of both fields - annual temperatures from both observations and proxy records - and must be incorporated. Clearly, there is a spatial bias associated with

observations that are biased away from the oceans, high latitudes, and high altitudes. The spatial problem is far more pronounced when only a handful of proxies are used to represent the global temperatures at earlier time periods. Both MBH and MJ are equally guilty in this regard.

David R. Legates

At 15:55 06/08/2004 +1000, you wrote:

Dear Phil

Just finished reading your paper with Mike M in Rev of Geophysics which I very much enjoyed - will let you know when it hits the Mission Beach Chronicle!

Hope all is well

best wishes

Janice

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References

1. <http://www.ncpa.org/pub/ba/ba478/ba478.pdf>

From: Phil Jones <p.jones@uea.ac.uk>
To: Gabi Hegerl <hegerl@duke.edu>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Mann and Jones (2003)
Date: Tue Aug 10 15:47:04 2004
Cc: Tom Crowley <tcrowley@duke.edu>

Gabi,

No second attempt - don't know what the first was? We'll be doing a new instrumental data

set (surprisingly called HadCRUT3), but that's it at the moment.

Attached is a good review of corals - just out.

Cheers

Phil

At 10:36 10/08/2004 -0400, Gabi Hegerl wrote:

Hi Mike and Phil,

Thanks! Yes, factor 1.29 will get me closer to my best guess scaling (factor 1.6 to same-size signals).

The scaling is a tough issue, and I think there are lots of possibilities to do it depending on what one wants

to do. For comparing underlying forced signals, I think t1s is best. To get a conservative size paleo reconstruction

(like what part of instrumental do we reconstruct with paleo), the traditional scaling is best.

I'll write up what Myles and I have been thinking and send it.

Phil, if there is a second attempt at that with the Hadley Centre, let me know, I don't like racing anybody!

Gabi

Michael E. Mann wrote:

Dear Phil and Gabi,

I've attached a cleaned-up and commented version of the matlab code that I wrote for doing the Mann and Jones (2003) composites. I did this knowing that Phil and I are likely to have to respond to more crap criticisms from the idiots in the near future, so best to clean up the code and provide to some of my close colleagues in case they want to test it, etc. Please feel free to use this code for your own internal purposes, but don't pass it along where it may get into the hands of the wrong people.

In the process of trying to clean it up, I realized I had something a bit odd, not necessarily wrong, but it makes a small difference. It seems that I used the 'long' NH instrumental series back to 1753 that we calculated in the following paper:

* Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., [1]Optimal Surface Temperature Reconstructions using Terrestrial Borehole Data, Journal of Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003.

(based on the sparse available long instrumental records) to set the scale for the decadal standard deviation of the proxy composite. Not sure why I used this, rather than using the CRU NH record back to 1856 for this purpose. It looks like I had two similarly named series floating around in the code, and used perhaps the less preferable one for setting the scale.

Turns it, this has the net effect of decreasing the amplitude of the NH reconstruction by a factor of $0.11/0.14 = 1.29$.

This may explain part of what perplexed Gabi when she was comparing w/ the instrumental series. I've attached the version of the reconstruction where the NH is scaled by the CRU NH record instead, as well as the Matlab code which you're welcome to try to use yourself and play around with. Basically, this increases the amplitude of the reconstruction everywhere by the factor 1.29. Perhaps this is more in line w/ what Gabi was estimating (Gabi?)

Anyway, doesn't make a major difference, but you might want to take this into account in any further use of the Mann and Jones series...

Phil: is this worth a followup note to GRL, w/ a link to the Matlab code?

Mike

p.s. Gabi: when do you and Tom plan to publish your NH reconstruction that now goes back about 1500 years or so? It would be nice to have more independent reconstructions published in the near future! Maybe I missed this? Thanks...

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```
% COMPOSITENH"
%
% (c) 2003, M.E. Mann
%
% THIS ROUTINE PERFORMS A RECONSTRUCTION OF NORTHERN HEMISPHERE
% MEAN ANNUAL TEMPERATURE BASED ON A WEIGHTED COMPOSITE OF LONG-TERM TEMPERATURE
% PROXY RECORDS SCALED AGAINST THE INSTRUMENTAL HEMISPHERIC MEAN TEMPERATURE
% SERIES, AS USED IN THE FOLLOWING TWO PUBLICATIONS:
%
%
% Jones, P.D., Mann, M.E., Climate Over Past Millennia, Reviews of Geophysics,
% 42, RG2002, doi:10.1029/2003RG000143, 2004
%
% Mann, M.E., Jones, P.D., Global Surface Temperatures over the Past two Millennia,
% Geophysical Research Letters,
% 30 (15), 1820, doi: 10.1029/2003GL017814, 2003
%
%
% 1. READ IN INSTRUMENTAL RECORD
%
% Read in CRU instrumental NH mean temperature record (1856-2003)
load nh.dat;
yearinstr=nh(:,1);
% calculate both warm-season and annual means
warmseason=(nh(:,5)+nh(:,6)+nh(:,7)+nh(:,8)+nh(:,9)+nh(:,10))/6;
```

```

annualmean=nh(:,14);
% use annual mean record in this analysis
nhmean=annualmean;
%
% 2. READ IN PREVIOUSLY PUBLISHED PROXY-RECONSTRUCTIONS OF NH ANNUAL MEAN
% RECONSTRUCTIONS AND FORM APPROPRIATELY SCALED COMPOSITE
%
% Read in Mann et al (1998), Crowley and Lowery (2000), and Jones et al (1998)
% NH temperature reconstructions
load nhem-millennium.dat;
load crowleylowery.dat;
load joneshemisrecons.dat;
nhmbh=nhem_millennium(1:981,2);
nhjones=joneshemisrecons(1:981,2);
nhcl=crowleylowery(1:981,2);
yearmillen=nhem_millennium(1:981,1);
% since some reconstructions are only decadal resolved, smooth each on
% decadal timescales through use of a lowpass filter with cutoff at
% f=0.1 cycle/year. Based on use of the filtering routine described in:
%
% Mann, M.E., On Smoothing Potentially Non-Stationary Climate Time Series,
% Geophysical Research Letters, 31, L07214, doi: 10.1029/2004GL019569, 2004.
%
% using 'minimum norm' constraint at both boundaries for all time series
nhsmooth=lowpass(nhmean,0.10,0,0);
nhmbhsmooth=lowpass(nhmbh,0.10,0,0);
nhjonessmooth=lowpass(nhjones,0.10,0,0);
nhclsmooth=lowpass(nhcl,0.10,0,0);
% Mann et al (1998) already calibrated in terms of hemispheric annual mean temperature, but
% reference mean has to be adjusted to equal that of the instrumental series
% over the 1856-1980 overlap period (which uses a 1961-1990 reference period)
admbh=mean(nhsmooth(1:125))-mean(nhmbhsmooth(857:981));
newmbh=nhmbhsmooth+admbh;
% need to adjust and scale Jones et al (1998) and Crowley and Lowery (2000)
% reconstructions to match mean and trend of smoothed instrumental series
% over 1856-1980
t1=1856;
t2=1980;
x=(t1:t2)';
nhlong=nhmean(1:125);
smoothlong=lowpass(nhlong,0.10,0,0);
amean0=mean(smoothlong);
y=smoothlong;
[yc,t,trend0,detrend0,xm,ym] = lintrend(x, y);
%
y=nhclsmooth(t1-999:t2-999);
[yc,t,trendcl,detrendcl,xm,ym] = lintrend(x, y);
%
y=nhjonessmooth(t1-999:t2-999);
[yc,t,trendjones,detrendjones,xm,ym] = lintrend(x, y);

```

```

%
multjones=norm(trend0)/norm(trendjones);
adjustedjones=nhjonessmooth*multjones;
offsetjones=amean0-mean(adjustedjones(t1-999:t2-999));
newjones=adjustedjones+offsetjones;
newjones=newjones';
%
multcl=norm(trend0)/norm(trendcl);
adjustedcl=nhclsmooth*multcl;
offsetcl=amean0-mean(adjustedcl(t1-999:t2-999));
newcl=adjustedcl+offsetcl;
newcl=newcl';
%
nhlongcompose=0.3333*(newmbh+newjones'+newcl)';
%
% 3. READ IN AND PROCESS PROXY TEMPERATURE RECORDS
%
M=8;
load 'china-series1.dat'
load 'itrdb-long-fixed.dat'
load 'westgreen-o18.dat'
load 'torny.dat'
load 'chesapeake.dat'
load 'mongolia-darrigo.dat'
load 'dahl-jensen-gripbh1yrinterp.txt'
load 'dahl-jensen-dye3bh1yrinterp.txt'
% read in years
x1=china_series1(:,1);
x2=itrdb_long_fixed(:,1);
x3=westgreen_o18(:,1);
x4=torny(:,1);
x5=chesapeake(:,1);
x6=mongolia_darrigo(:,1);
x7=dahl_jensen_gripbh1yrinterp(:,1);
x8=dahl_jensen_dye3bh1yrinterp(:,1);
% read in proxy values
y1=china_series1(:,2);
y2=itrdb_long_fixed(:,2);
y3=westgreen_o18(:,2);
y4=torny(:,2);
y5=chesapeake(:,2);
y6=mongolia_darrigo(:,2);
y7=dahl_jensen_gripbh1yrinterp(:,2);
y8=dahl_jensen_dye3bh1yrinterp(:,2);
% Store decadal correlation of each proxy record with local available
% overlapping CRU gridpoint surface temperature record (see Mann and Jones, 2003)
corr(1)=0.22;
corr(2)=0.52;
corr(3)=0.75;
corr(4)=0.32;

```

```

corr(5)=0.31;
corr(6)=0.40;
corr(7)=0.53;
corr(8)=0.52;
% Estimate Area represented by each proxy record based on latitude of
% record and estimated number of temperature gridpoints represented by record
pi=3.14159;
factor=pi/180.0;
lat(1)=32.5;
dof(1)=4;
lat(2)=37.5;
dof(2)=2;
lat(3)=77;
dof(3)=0.667;
lat(4)=68;
dof(4)=3.5;
lat(5)=37.0;
dof(5)=1.0;
lat(6)=47;
dof(6)=1;
lat(7)=73;
dof(7)=0.667;
lat(8)=65;
dof(8)=0.667;
for j=1:M
    area(j)=dof(j)*cos(lat(j))*factor);
end
% determine min and max available years over all proxy records
%
minarray=[min(x1) min(x2) min(x3) min(x4) min(x5) min(x6) min(x7) min(x8)];
maxarray=[max(x1) max(x2) max(x3) max(x4) max(x5) max(x6) max(x7) max(x8)];
tbegin=max(minarray);
tend1=min(maxarray);
tend=max(maxarray);
% initialize proxy data matrix
notnumber = -9999;
for j=1:M
for i=1:minarray(j)-1
    time(i)=i;
    mat(i,j)=notnumber;
end
for i=minarray(j):tend
    time(i)=i;
end
for i=minarray(j):maxarray(j)
    if (j==1) mat(i,j)=y1(i-minarray(j)+1);
    end
    if (j==2) mat(i,j)=y2(i-minarray(j)+1);
    end
    if (j==3) mat(i,j)=y3(i-minarray(j)+1);

```

```

end
if (j==4) mat(i,j)=y4(i-minarray(j)+1);
end
if (j==5) mat(i,j)=y5(i-minarray(j)+1);
end
if (j==6) mat(i,j)=y6(i-minarray(j)+1);
end
if (j==7) mat(i,j)=y7(i-minarray(j)+1);
end
if (j==8) mat(i,j)=y8(i-minarray(j)+1);
end
end
% added in Jones and Mann (2004), extend series ending between
% 1980 calibration period end and 2001 boundary by persistence of
% last available value through 2001
for i=maxarray(j)+1:tend
    if (j==1) mat(i,j)=y1(maxarray(j)-minarray(j)+1);
    end
    if (j==2) mat(i,j)=y2(maxarray(j)-minarray(j)+1);
    end
    if (j==3) mat(i,j)=y3(maxarray(j)-minarray(j)+1);
    end
    if (j==4) mat(i,j)=y4(maxarray(j)-minarray(j)+1);
    end
    if (j==5) mat(i,j)=y5(maxarray(j)-minarray(j)+1);
    end
    if (j==6) mat(i,j)=y6(maxarray(j)-minarray(j)+1);
    end
    if (j==7) mat(i,j)=y7(maxarray(j)-minarray(j)+1);
    end
    if (j==8) mat(i,j)=y8(maxarray(j)-minarray(j)+1);
    end
end
end
end
time=time';
data=[time mat];
% decadal lowpass of proxy series at f=0.1 cycle/year as described earlier
for j=1:M
    unfiltered=mat(minarray(j):tend,j);
    filt=lowpass(unfiltered,0.1,0,0);
    for i=1:minarray(j)-1
        filtered(i,j)=mat(i,j);
    end
    for i=minarray(j):tend
        filtered(i,j)=filt(i-minarray(j)+1);
    end
end
end
% standardize data
% first remove mean from each series
for j=1:M

```

```

icount=0;
amean(j)=0;
for i=1:tend
    if (filtered(i,j)>notnumber)
        icount=icount+1;
        amean(j)=amean(j)+filtered(i,j);
    end
end
amean(j)=amean(j)/icount;
end
% now divide through by standard deviation
for j=1:M
    icount=0;
    asum=0;
    for i=1:tend
        if (filtered(i,j)>notnumber)
            asum=asum+(filtered(i,j)-amean(j))^2;
            icount=icount+1;
        end
    end
    sd(j)=sqrt(asum/icount);
    for i=1:tend
        standardized(i,j)=filtered(i,j);
        if (mat(i,j)>notnumber)
            standardized(i,j)=(filtered(i,j)-amean(j))/sd(j);
        end
    end
end
end
%
% 4. Calculate NH mean temperature reconstruction through weighted (and
% unweighted) composites of the decadal-smoothed proxy indicators
%
% impose weighting scheme for NH mean composite
for j=1:M
% weighting method 1: weight each proxy series by approximate area
% weighting method 2: weight each proxy series by correlation between
% predictor and local gridpoint series over available overlap period
% during calibration interval
% weighting method 3: weight each proxy series by correlation between
% predictor and NH mean series over calibration interval:
% weightlong(j)=lincor(nhlong,standardized(1856:1980,j));
% weighting method 4: combine 1 and 3
% weighting method 5: combine 1 and 2 (this is the 'standard' weighting
% scheme chosen by Mann and Jones (2003)
% use standard weighting scheme
    weight(j)=corr(j)*area(j);
end
% perform reconstructions based on:
% (1) the 6 proxy temperature records available over interval AD 200-1980
% (2) all 8 proxy temperature records available over interval AD 553-1980

```

```

istart0=200;
istart1=200;
istart2=553;
nseries1=0;
nseries2=0;
weightsum1=0;
weightsum2=0;
for j=1:M
    if (istart1>=minarray(j))
        nseries1=nseries1+1;
        weightsum1=weightsum1+weight(j);
    end
    if (istart2>=minarray(j))
        nseries2=nseries2+1;
        weightsum2=weightsum2+weight(j);
    end
end
% calculate composites through 1995 (too few series available after that date)
% As discussed above, persistence is used to extend any series ending
% between 1980 and 1995 as described by Jones and Mann (2004).
tend=1995;
for i=istart1:tend
    unweighted1(i)=0;
    unweighted2(i)=0;
    weighted1(i)=0;
    weighted2(i)=0;
    for j=1:M
        if (istart1>=minarray(j))
            unweighted1(i)=unweighted1(i)+standardized(i,j);
            weighted1(i)=weighted1(i)+weight(j)*standardized(i,j);
        end
        if (istart2>=minarray(j))
            unweighted2(i)=unweighted2(i)+standardized(i,j);
            weighted2(i)=weighted2(i)+weight(j)*standardized(i,j);
        end
    end
end
unweighted1=unweighted1/nseries1;
unweighted2=unweighted2/nseries2;
weighted1=weighted1/weightsum1;
weighted2=weighted2/weightsum2;
unweighted1(1:istart1-1)=0;
unweighted2(1:istart2-1)=0;
weighted1(1:istart1-1)=0;
weighted2(1:istart2-1)=0;
% scale composite to have same variance as decadal-smoothed instrumental
% NH series

% Mann and Jones (2003) and Jones and Mann (2004) used for this purpose
% the extended (1753-1980) NH series used in:

```

```

% Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T.,
% Optimal Surface Temperature Reconstructions using Terrestrial Borehole Data,
% Journal of Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003.
% That series has a decadal standard deviation sd=0.1123
% If instead, the 1856-2003 CRU instrumental NH mean record is used, with
% a decadal standard deviation of sd=0.1446, the amplitude of the reconstruction
% increases by a factor 1.29 (this scaling yields slightly lower verification
% scores)
load nhem-long.dat
nhemlong=nhem_long(:,2);
longsmooth=lowpass(nhemlong,0.10,0,0);
sd0=std(longsmooth);
% use weighted (rather than unweighted) composite in this case
series1=weighted1;
% center composites on 1856-1980 calibration period
y=series1(t1:t2)';
amean1=mean(series1(t1:t2));
compseries1=series1(t1:t2)-amean1;
mult1=sd0/std(compseries1);
% scale composite to standard deviation of instrumental series and re-center
% to have same (1961-1990) zero reference period as CRU NH instrumental
% temperature record
adjusted1=series1*mult1;
offset1=amean0-mean(adjusted1(t1:t2));
compose1=adjusted1+offset1;
compose1=compose1';
series2=weighted2;
y=series2(t1:t2)';
amean2=mean(series2(t1:t2));
compseries2=series2(t1:t2)-amean2;
mult2=sd0/std(compseries2);
adjusted2=series2*mult2;
offset2=amean0-mean(adjusted2(t1:t2));
compose2=adjusted2+offset2;
compose2=compose2';
%
% 5. UNCERTAINTY ESTIMATION, AND STATISTICAL VERIFICATION
%
% estimate uncertainty in reconstruction
% nominal (white noise) unresolved calibration period variance
calibvar=lincor(smoothlong,compose1(t1:t2))^2;
uncalib=1-calibvar;
sdunc=sd0*sqrt(uncalib);
% note: this is the *nominal* white noise uncertainty in the reconstruction
% a spectral analysis of the calibration residuals [as discussed briefly in
% Mann and Jones, 2003] indicates that a peak at the multidecadal timescale
% that exceeds the white noise average residual variance by a factor of
% approximately 6. A conservative estimate of the standard error in the
% reconstruction thus inflates the nominal white noise estimate "sdunc" by a
% factor of sqrt(6)

```

```
sdlow = sdunc*sqrt(6)
% calculate long-term verification statistics for reconstruction
% use composite of Mann et al (1998)/Crowley and Lowery (2000)/Jones et al (1998)
% and AD 1600-1855 interval
overlapcomp=nhlongcompose(1:981);
% work with longer reconstruction (back to AD 200)
overlaprecon=compose1(1000:1980)';
%overlaprecon=compose2(1000:1980)';
%calculate verification R^2
series11=overlaprecon(601:856);
series22=overlapcomp(601:856);
verifrsq=lincor(series11,series22)^2
% calculate verification RE
var1=0.0;
var2=0.0;
var3=0.0;
var4=0.0;
var5=0.0;
am0=0.0;
% insure convention of zero mean over calibration interval
for i=857:981
    am0=am0+overlapcomp(i);
end
am0=am0/125;
for i=601:856
    var1=var1+(overlapcomp(i)-am0)^2;
    var2=var2+(overlapcomp(i)-overlaprecon(i))^2;
end
verifRE=1-var2/var1
```

--

~~~~~

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Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090  
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---

## References

1. <ftp://holocene.evsc.virginia.edu/pub/mann/borehole-jgr03.pdf>
2. <mailto:mann@virginia.edu>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
4. <mailto:hegerl@duke.edu>
5. <http://www.env.duke.edu/faculty/bios/hegerl.html>

From: Phil Jones <p.jones@uea.ac.uk>  
To: "Michael E. Mann" <mann@virginia.edu>  
Subject: Re: Fwd: RE: IJOC040512 review  
Date: Fri Aug 13 13:38:32 2004

Mike,

I'd rather you didn't. I think it should be sufficient to forward the para from Andrew Conrie's

email that says the paper has been rejected by all 3 reviewers. You can say that the paper was an extended and updated version of that which appeared in CR.

Obviously, under no circumstances should any of this get back to Pielke.

Cheers

Phil

At 08:11 13/08/2004 -0400, you wrote:

Thanks a bunch Phil,

Along lines as my other email, would it be (?) for me to forward this to the chair of our committee confidentially, and for his internal purposes only, to help bolster the case against MM??

let me know...

thanks,

mike

At 03:43 AM 8/13/2004, Phil Jones wrote:

Mike,

The paper ! Now to find my review. I did suggest to Andrew to find 3 reviewers.

Phil

From: "Andrew Comrie" <comrie@climate.geog.arizona.edu>

To: "'f028'" <P.Jones@uea.ac.uk>

Subject: RE: IJOC040512 review

Date: Mon, 24 May 2004 01:29:44 -0700

X-Mailer: Microsoft Outlook, Build 10.0.4024

Importance: Normal

X-Virus-Scanned: by amavisd-new at email.arizona.edu

X-UEA-MailScanner-Information: Please contact the ISP for more information

X-UEA-MailScanner: Found to be clean

X-UEA-MailScanner-SpamScore: ssss

<<...>>

Dear Phil,

IJOC040512 "A Socioeconomic Fingerprint on the Spatial Distribution of Surface Air Temperature Trends"

Authors: RR McKittrick & PJ Michaels

Target review date: July 5, 2004

Following from our email, many thanks for agreeing to review the paper above that has been submitted to the International Journal of Climatology for consideration. I have attached the manuscript, and the information for reviewers is provided below. Please let me know that you received the file.

In the interests of expediting the review process, I encourage you to email your review as soon as is convenient. I would like to hear from you by the target date above, or as soon after as possible.

Referee's names are kept anonymous. When composing your review, please keep your "Comments to the Author" separate from your confidential comments to the editor. With your comments to me, please be sure to provide one of these summary recommendations:

1. Accept without further revision.
2. Accept subject to minor revisions (changes to the text only, or simple follow-on analyses).
3. Accept subject to major revisions (major text changes, recalculations or new analyses).
4. Reject.

In the case of minor revisions, the revised manuscript will be checked only by the editor. For major revisions, the revised manuscript may be sent to you again for a second review. It will also be useful if you will grade the contribution overall on the following scale:

- A. Very good (a continuing and useful advance in an area of importance).
- B. Good (satisfactory and of sufficient importance to merit publication).
- C. Adequate (of marginal interest).
- D. Poor (not significant enough to merit publication).
- E. Very poor (trivial, or incorrect, or of no interest, or not new, etc.).

For your review, please also comment if any of the following points are not satisfactory or suitable: topic appropriate for the journal, correctness of the title, reduction in paper length, quality and quantity of illustrations, units, use of English, and key words.

Your contribution to the review process is essential and greatly valued.

Sincerely,

Andrew Comrie

Dr. Andrew C. Comrie

Associate Professor and Director of Graduate Studies

Dept. of Geography and Regional Development

University of Arizona

409 Harvill Building

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Tel: (+1) (520) 621 1585

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Regional Editor for the Americas, International Journal of Climatology

[2]<http://www.interscience.wiley.com/ijoc>

-----Original Message-----

From: f028 [[3]mailto:f028@uea.ac.uk] On Behalf Of f028

Sent: Monday, May 24, 2004 1:04 AM

To: Andrew Comrie

Subject: RE: IJOC040512 review

Andrew,

I can do this. I am in France this week but back in the UK all June.

So send and it will be waiting my return.

Phil

>===== Original Message From "Andrew Comrie" <comrie@climate.geog.arizona.edu>

>=====

>Dear Prof. Jones,

>

>IJOC040512 "A Socioeconomic Fingerprint on the Spatial Distribution of

>Surface Air Temperature Trends"

>Authors: RR McKittrick & PJ Michaels

>Target review date: July 5, 2004

>

>I know you are very busy, but do you have the time to review the above

>manuscript for the International Journal of Climatology? If yes, can

>you complete the review within about five to six weeks, say by the

>target review date listed above? I will send the manuscript

>electronically.

>

>If no, can you recommend someone who you think might be a good choice to

>review this paper?

>

>Thanks for considering my request.

>

>Best wishes,

>

>Andrew Comrie

>

>Dr. Andrew C. Comrie

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Professor Michael E. Mann  
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e-mail: [mann@virginia.edu](mailto:mann@virginia.edu) Phone: (434) 924-7770 FAX: (434) 982-2137  
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---

## References

1. <http://geog.arizona.edu/~comrie/>
2. <http://www.interscience.wiley.com/ijoc>
3. <mailto:f028@uea.ac.uk>
4. <http://geog.arizona.edu/~comrie/>
5. <http://www.interscience.wiley.com/ijoc>
6. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Keith Briffa <k.briffa@uea.ac.uk>

To:

John.Birks@bot.uib.no, masson@lsce.saclay.cea.fr, dirk.verschuren@UGent.be, Laurent.Labeyrie@lsce.cnrs-

gif.fr, juerg.beer@eawag.ch, A.Lotter@bio.uu.nl, t.osborn@uea.ac.uk, hufischer@awi-bremerhaven.de, dan.charman@plymouth.ac.uk, karin@natgeo.su.se

Subject: IMPRINT

Date: Fri Aug 13 17:37:10 2004

Cc: wanner@giub.unibe.ch, esper@wsl.ch, Basil.Davis@bgc-

jena.mpg.de, sigfus@gfy.ku.dk, guiot@cerege.fr, Ian.Snowball@geol.lu.se, antti.ojala@gsf.fi, atle.nesje@geol.uib.no, atte.korhola@helsinki.fi, Keith.Barber@soton.ac.uk, Sandy.Tudhope@ed.ac.uk, eavaganov@forest.akadem.ru,

Eystein Jansen <eystein.jansen@geo.uib.no>, Rick Battarbee

<r.battarbee@geog.ucl.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Jan

Esper <esper@wsl.ch>, brazdil@sci.muni.cz, benito@ccma.csis.es

Dear Colleagues,

This note is to solicit your possible collaboration in an application to the European

Commission under Framework 6, possibly as one of the partners in IMPRINT. This is an

integrated palaeoclimate/climate modelling project concerned primarily with the Holocene,

but also incorporating specific studies on other interglacial warm periods. AT THIS STAGE

THIS IS A PROVISIONAL ENQUIRY RATHER THAN A DEFINITE REQUEST FOR YOUR INVOLVEMENT.

The project has been some time (years) in gestation and has evolved from other proposals.

An unfinished draft is appended to this message for your information - but we would ask

that you respect its confidentiality, whether or not you are interested in working with

us. Eystein Jansen has agreed to coordinate IMPRINT. We are now refining the initial

submission. I, and Valerie Masson, are nominally fronting WorkPackage 1: concerned with

assembling, reinterpreting, amalgamating and analysing the climate data; a combination of

instrumental, documentary and other indirect, proxy climate information. This Workpackage

will also organise the aggregation of best possible climate forcing proxy evidence, as

means of exploring links with the empirical climate data, but also as input to the

significant effort in climate modelling to be undertaken in other workpackages.

WorkPackage 1 has been divided into a number of sub themes or Tasks and these, along with

the content of all Workpackages, is described in the attached document. Note that this is

very much work in progress at this stage and your comments and input to all parts will be

welcome. We will refine the wider list of collaborating institutes at a later stage.

At this stage we envisage a total budget application of about 17 million Euro with a nominal share of 5 million for WorkPackage 1. While this is a large sum, I am sure you will appreciate that when distributed among many partners and stretched over five years it imposes a severe limitation on the total number of partners that can be feasibly included. Therefore we have had to conceive of different degrees, or levels, of involvement of the very many colleagues and institutions that are required to make this project a success. Thus, we envisage a distinction between a number of full partners, though again with varying resource allocation depending on specific inputs and requirements (still to be determined), and a larger number of collaborators. Specific funding will be allocated to facilitate the involvement of these many other groups, who we see taking part in workshops, in return for full access to joint data and modelling results. This is the only way that we see of overcoming the envisaged restriction imposed by the EC on total partner numbers.

We have chosen partners who we hope will be able to furnish expertise in specific research areas and, hopefully, facilitate data assembly and exchange between members of the wider communities.

PLEASE NOTE THAT THOSE PEOPLE LISTED IN THE "TO" LINE OF ADDRESSES ARE THOSE TENTATIVELY EARMARKED TO BE TASK LEADERS WITHIN WORKPACKAGE 1. THOSE LISTED UNDER THE "CC" HEADING ARE EARMARKED TO be PARTNERS - ORGANISING WORK AND DATA EXCHANGE WITHIN THEIR COMMUNITY. We

have a suggested list of many others who we would hope to involve - but not at full partner level. Your input to the completion of this list will be asked for later. We would ask that, for now, you do not circulate this provisional proposal. We realise that many other partners could have been fully justifiably included, but the need for pragmatism must eventually limit their formal roles. We hope that this reality will be accepted by those colleagues not included as primary partners and they will still be willing to collaborate to achieve the wider aims of IMPRINT.

The specific partner roles, as suggested to date, are described in the Workpackage 1

section of the appended IMPRINT document. Would you now please indicate whether or not you are willing to join this effort, and please feel free to comment on any aspect: of Workpackage 1 to myself and Valerie; or of the project as a whole to Eystein.

With very best wishes,

Keith

--

Professor Keith Briffa,  
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University of East Anglia  
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909  
Fax: +44-1603-507784

From: Phil Jones <p.jones@uea.ac.uk>

To: "Susan Solomon" <Susan.Solomon@noaa.gov>, <trenbert@cgd.ucar.edu>, IPCC-WG1 <ipcc-wg1@al.noaa.gov>, martin.manning@noaa.gov, Susan.Solomon@noaa.gov

Subject: Re: [Fwd: Re: [Wg1-ar4-clas] WGI AR4 LA1 Programme]

Date: Sun Aug 15 10:56:37 2004

Cc: p.jones@uea.ac.uk

Susan,

Thanks for the comments.

Cheers

Phil

At 15:51 13/08/2004 -0600, Susan Solomon wrote:

Dear Phil, dear Kevin,

Thanks for your message. It's very good to hear that you are getting together and will have time to talk about this. I will make a few points and suggestions below for your consideration.

Safe travels,

Susan

Martin, Susan et al,

Kevin and I will be at a GCOS meeting Mon-Weds next week in Geneva, so will have some

time to discuss our chapter. I've sent Kevin some thoughts about boundaries between

chapters. If you can provide your views on a few issues, then it will help us in our discussions.

1. We have extended outlines, which clarify some issues, but how rigid are they? I say this

wrt the overviews/visions you expect on the Monday pm of the Trieste meeting.

The extended outlines show you what the thought process was at Marrakech and Potsdam that led to the present outlines. It's your report, and you may wish to do things differently. Where that may involve other chapters, such work would need to be coordinated/decided jointly but most things are not like that.

2. In Chapter 3, we have a section 3.9 on synthesis/consistency amongst obs. Does this

involve obs such as glacier retreat and changes in sea ice, snow cover from chapters

4-6? Chapters 4-6 don't have similar sections.

We had some discussions on that in Potsdam in particular if I recall. Dividing up the observations into three chapters solves some problems and raises others, and this is one

of them. My own thinking has been that issues such as the consistency of glacier retreat with observations may be better handled in the ice chapter, which presumably will be going into a bit more depth on processes affecting glaciers from the ice physics point of view, providing a bit deeper basis for the assessment. The consistency of observations between the three observations chapters could then be dealt with in the technical summary, drawing on the findings from all three. But it is probably going to be helpful if we have a discussion on this among the three chapters and come to a common view.

3. Chapter 1 has a section on new data and data rescue. I guess we should be involved in that, but also Ch 9 on attribution as it has to be worthwhile. Also the new data and rescued data could be useful for model validation. I expect Ch 3 to heavily use Reanalysis-based results.

Yes, we expected there would need to be discussion on that. It may involve a subset of people who should be urged to get together as needed.

4. Chapter 3 has SST and all the circulation indices, so here we need to liaise with Ch 5 and 6 and eventually with 9.

Yes, agreed, and Kevin and others tried to work that into the outline in Potsdam.

5. I agree with Kevin though on whether formal meetings of the whole of the chapters are needed. Might this be better done with the CLAs and you?

There will be a lot to do in Trieste and we want to make efficient use of people's time - it is probably true that not all the people need to be involved when the points you've made so far are discussed. The morning 1-hour sessions with all CLAs are also intended to be a forum where some of these kinds of issues (the broader ones) could be handled.

6. Considering all the above, I reckon we need to meet with Ch 4 and 6 (on glacier retreat, snow, sea ice and temperature), Chapters 6 and 9 on what they expect from us and similarly with Chapter 5 (although I feel this is clear in the extended outline). Finally, Chapters 1, 3 and 6 (and maybe 9) need to discuss data rescue and new techniques.

That sounds right to me. I would add your number 7 below into that mix as well.

It's really up to you to decide how you want to handle it. But prompted by your message, the one from Kevin below, and some others, I think it will be helpful for us to compile a list of all such issues raised - so I am asking the TSU to do that, combining with another set that we received in the comments from governments (they actually raised a number of such comments, quite rightly).

7. The Appendices in Chapters 3-5 need some sort of co-ordination.

Bests,  
Susan

At 11:31 11/08/2004 -0600, Kevin Trenberth wrote:

Martin, Susan et al:

In thinking more about Chapter 3, I believe we will have issues on who and what is covered on

- 1) ENSO related stuff Chapter 3 vs Chapter 5
- 2) Consistency of retreat of glaciers, snow and ice vs temperatures Chapter 3 vs chapter 4.

There are probably others, but these may require some negotiation unless it is already settled in your mind? Whether a formal meeting between chapters is needed or whether the CLAs can meet and agree is not yet clear to me.

Kevin  
IPCC-WG1 wrote:

Dear WGI CLAs and Bureau Members,  
Please find attached a draft programme for the upcoming WGI AR4 First Lead Authors Meeting, 26-29 September 2004, Trieste, Italy. Please note the section regarding "cross-chapter breakout sessions". We have suggested four breakouts of this type, but would appreciate any suggestions from you regarding other cross-chapter breakouts that you feel may be needed. We kindly ask that you provide the WGI TSU <[1]mailto:ipcc-wg1@al.noaa.gov><ipcc-wg1@al.noaa.gov> any feedback you may have by Friday, 20 August 2004.

Best regards,  
WGI TSU

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~~~~~  
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--

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~~~~~

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Please note my new email address for your records:

Susan.Solomon@noaa.gov

Prof. Phil Jones

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School of Environmental Sciences Fax +44 (0) 1603 507784

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References

1. <mailto:ipcc-wg1@al.noaa.gov>
2. <mailto:ipcc-wg1@al.noaa.gov>
3. <mailto:Wg1-ar4-clas@joss.ucar.edu>
4. <http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas>
5. <mailto:trenbert@ucar.edu>
6. <http://www.cgd.ucar.edu/cas/>
7. <http://www.cgd.ucar.edu/cas/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: t.m.melvin@uea.ac.uk
Subject: Fwd: Yamal treeline figures
Date: Mon Aug 23 16:48:58 2004

Date: Mon, 9 Oct 2000 18:08:04 +0500
From: Rashit Hantemirov <rashit@ipae.uran.ru>
X-Mailer: The Bat! (v1.00 Build 1311) Registered to Andy Malyshev
Reply-To: Rashit Hantemirov <rashit@ipae.uran.ru>
Organization: IPAE
Priority: Normal
X-Confirm-Reading-To: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Yamal treeline figures

Dear Keith,

Stepan Shiyatov tell me that you need some figures concerning Yamal chronology and tree line dynamics to show somewhere in France.

Attached are archived files contained some figures.

File MAP - the map of region of research. Red dots - subfossil wood sites, green marks - recent northern border of larch along river valleys.

File FIGURES - in Excel format, contains several figures.

Sheet "Values-10" - data on northernmost position of trees and number of trees dated for corresponding year (decadal step)

Sheet "Treeline" - dynamics of treeline in Yamal during last 7000 years reconstructed using about 1000 subfossil wood remains. Recent treeline position is about 67°34.

One year ago we supposed (C-14 data, Hantemirov, Shiyatov 1999) that significant drop of treeline (the transition from "middle" to "late" Holocene) was about 1700-1600 AD. According new data it was earlier (about 2550 BC). May be it is because of lack of data from region northward of 68°N (only 25 datings)?

Sheet "Treeline and Nu" - treeline dynamics and number of dated trees. May be number of trees reflects the long scale climate fluctuations as well.

Sheet "2600-all" - for last 4600 years: treeline dynamics, number of trees, 11 most cold summers for last 7000 years (according our version of reconstruction), most expressed frosts in July (reconstructed using junipers from Polar Urals, see file PATHOL, frost in 1626 BC - based on subfossil larch -

you can put away it), summer temperatures reconstruction smoothed with 20- and 100-year filters (our version of reconstruction).

Sheet "Values-2" - values for preceding figures, in 2-years step.

Sheet "Yam-Ur-fig" - comparing of treeline data for Yamal and Polar Urals upper treeline dynamics (data by S.G.Shiyatov)

Sheet "Yamal-Ural" - values for preceding figure, in 2-years step.

Sheet "Treeline-std" - treeline dynamics and 50-year standard deviations of summer temperatures (our version of reconstruction). This figure shows surprising high negative correlation. However may be both of them just reflect long scale climate fluctuations?

Sheet "Std" - 50-year standard deviations of summer temperatures (our version of reconstruction) .

File PATHOL - in Excel format, contains data and figure on pathological structures in tree rings of Siberian juniper (*Juniperus sibirica* Burgsd.). According our data (Hantemirov et al., 2000) the presence of frost rings provides evidence for frosts that occurred in late June or first days of July (frost rings in earlywood) and in the first half of July (frost rings in late wood). Long term and pronounced temperature drop in the middle of very warm period in the second half of July is the factor responsible for wood density fluctuations (false rings).

Please let me know when you receive this. Some time large messages get lost.

P.S. We (Eugene Vaganov, Stepan Shiyatov, Leonid Agafonov and I) will be in Birmensdorf from 23 till 29 October. Are you going to Switzerland after your meeting? We would be happy to see you there.

Best regards,

Rashit M. Hantemirov

Lab. of Dendrochronology

Institute of Plant and Animal Ecology

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--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Martin Munro <mmunro@LTRR.ARIZONA.EDU>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Calibration loose ends (was Re: [ITRDBFOR] crossdating)
Date: Sun, 29 Aug 2004 11:46:03 -0700
Reply-to: grissino@UTKUX.UTCC.UTK.EDU

This an attempt to tie up the loose ends from an earlier part of the discussion, the idea that calibration of the radiocarbon timescale be considered invalid, pending a better understanding of crossdating. Some of the previous posts seem to imply that measurements of the C-14 half-life depend on the calibration; in fact it can be determined by present-day laboratory measurements without reference to any old material, simply by observing the decay rate in a known quantity of the isotope. Physicists seem happy that beta decay isn't affected by mundane external influences, so the half life should be constant. If the amount of C-14 in a sample depends only on its age and the (constant) half life, a calibration curve from a collection of samples of known true age would be a diagonal straight line; but this would imply that each sample started with the same concentration of C-14. There are many effects that could change this concentration through time: variations in cosmic ray sources, changing solar activity, changes in the upper atmosphere, atmospheric circulation, uptake and release of carbon from large sinks and sources... etc. Given enough correctly dated samples, you can recover the sum of these variations from the form of the calibration curve. In practice, the most important variation appear to be on multi-millennial scales, with smaller fluctuations (wiggles) on century/multi-decadal scales superimposed on this.

Wood from crossdated tree rings provided the known-age reference material used in the calibration curves, and there were two main phases of work, the first of which roughed out the general form of the curve and hinted at the short-period structure, the second of which reconstructed the century-scale variations in detail using higher precision measurements. Contamination of old samples with C-14 of more recent origin is a widely recognized problem, addressed by physical and chemical pre-treatment protocols for the material. A couple of complicating effects that are of more interest from a tree-physiological point of view. Isotopic fractionation occurs along the entire chain of processes between carbon in the environment and its incorporation in the specific components of the wood that end up in the calibration samples. A ring forming in a particular year might

continue to accumulate C-14 in subsequent years. But people who work with C-14 are well aware of various corrections for isotopic fractionation, and the migration of carbon across ring boundaries has been the subject of several empirical investigations, notably using the stepwise change in C-14 concentrations following atmospheric nuclear tests in the 1950s and 60s as a tracer. The more recent phase of calibration work was substantially complete around 15 years ago, and was covered in an extensive series of journal articles and symposia.

Let's suppose we have been provided with a demonstration that crossdating is invalid: what would be the consequences for C-14 calibration? One of the most alarming would be that we would have to come up with a convincing explanation of how independent tree ring chronologies could be in error in precisely the same way---the known-age reference samples are not just from bristlecone pines, and crossdating within the network of oak chronologies is completely independent of the bristlecones. Both are completely self-supporting chains of inferences anchored in living trees and extending back into sub-fossil wood. There are published comparisons of paired calibration curves, with the absolute dates and C-14 concentrations based on oaks in one case, and on bristlecones in the other. My understanding of tree physiology is rudimentary at best, but surely when two such vastly different wood anatomies are involved there must be differences in the physiological constraints on wood formation. If potentially unidentified missing rings are supposed to be the most serious problem with the bristlecone chronologies, the oak chronologies should not be affected in any case, since they almost never include missing rings in this sense (although that's not to say they have no anatomical ambiguities that can confound crossdating). The crossdating error could not be merely a shared systematic bias; not only does the long term trend in the calibration curves derived from the two chronologies share a common non-linear trend, but the short-term fluctuations in C-14 concentration (wiggles) match between the two curves. There are small differences between calibrations derived from different geographical regions, but these have themselves formed the basis for further research and geophysical modeling.

The strengths of the two sets of chronologies are complimentary. Oaks may have almost no missing rings (*sensu stricto*) and provide larger volumes of wood for C-14 analysis, but the individual samples are only a few hundred years long, showing significant variations in growth with increasing pith age, and (particularly in the case of the

sub-fossil wood) there will be uncertainties about the environment in which the tree was growing. Bristlecone pines give a much better chance of finding wood that has grown over periods of many centuries with no marked age-related trends, and there's a compelling continuity between the living trees and the remnant wood lying on the ground nearby.

An account of wood formation from a physiological perspective would undoubtedly be a beautiful thing in its own right, even if it had little to contribute to dendrochronology. Moreover one of my pet peeves is seeing people manipulate data as mere collections of numbers divorced from any underlying model---and in the case of dendrochronology the model has to be biological. But I'd number myself amongst those who can't see why our use of crossdating must await a reasonably complete physiological model of wood formation. By analogy, if the doctors in some traditional society are using a human physiology based on the balance or imbalance of the four humours, but they have a treatment for a particular disease that results in an 80% survival rate, as opposed to a %40 survival rate if it goes untreated, you're obviously better off slurping down their bitter potion first and working out the explanation in current Western physiological terms afterwards (if that's the only treatment option).

So even if at present our understanding of crossdating is largely limited to statistical phenomenology, that may be good enough to live with until something better comes along. That's not to imply that we should be credulous, and automatically accept current practices simply because great authorities have taken the same route: astronomers were at one time expected to work as astrological consultants, casting horoscopes for rulers and interpreting signs in the sky in terms of current political affairs. There's no necessary reason to follow Douglass' crossdating methods any more than we should follow Kepler's example of casting horoscopes---unless they work. Although the seeming effectiveness of crossdating could in principle be invalid, it has been applied so widely that we would need presented with a very strong critique before abandoning it.

I'm not really qualified to discuss crossdating and C-14 calibration from a point of view of someone active in current research, but was fortunate to be sitting on the sidelines of the oak calibration work in the 80s, and just the other day Tom Harlan dropped by with the oldest known absolutely dated bristlecone sample, so will offer this as a kind of correction by proxy until any of the people

who've done the real work care to comment

---Martin.

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: question
Date: Mon Sep 6 11:10:47 2004
Cc: Professor David Taplin <coliemore@hotmail.com>, Ben Santer <santer1@llnl.gov>

Tom,

Ben should have seen the ERA-40 Report # 18. You can forward the JGR paper.

WRT 1, it is difficult to say as it depends who's produced the values. For HadCRUT2v, I think I've convinced the HC that the globe is $(NH+SH)/2$. If Peter Thorne did the calculations then this will be the case.

There is another issue. Sometimes the trends over Jan79-Dec03 are calculated from the 300 months rather than the 25 years. Christy does this, I think.

NCDC's Globe is probably the one domain. I've been doing some work with Russ Vose at NCDC, which he's still to write up. Most of the differences were due to how the globe was calculated. It is more informative to also include NH and SH as well as globe in such tables. I'll forward a plot Tom Peterson produced a week or two ago.

ERA-40 (2)comparisons are discussed in the ERA-40 report # 18 and the JGR submitted paper.

This also has comparisons by continent, which again are more informative. There is a plot in that work from the full globe vs the CRU coverage. I wouldn't believe their tropics.

Also

Antarctica is way off as well - at least where the surface data are located, so I wouldn't have much faith in their values for the unmonitored parts.

On (3) I did some comparisons ages ago with Jim Angell's surface data from sondes. Jim's data was just noisier and I suspect LKS would be also. I've not done anything like this for

ages. The closest would be the ERA-40 comparisons, which is much more extensive than the LKS network.

I might have a chance to do an LKS comparison if Dian sends me the co-ordinates.

Comparisons over 1958-2003 will be much more realistic, but the ERA-40/NCEP degrade prior to the 1960s. LKS would be better here. All sonde data look odd in the late 1950s to the early 1960s. The jump around 1976/77 has always intrigued me. It is bigger in some regions than others - I think it gets more credence because it is large over western North America. Kevin had a paper on this in BAMS in the late 1980s.

Cheers

Phil

At 15:57 04/09/2004, Tom Wigley wrote:

Phil,

On Sept. 13-17 I will be at a meeting at the Met Office to do with a report we are writing on trends in vert temp profiles as part of the US Climate Change Science Program (CCSP). It involves all the usual suspects. Seven chapters, the last of which is equivalent to a summary for policy-makers -- for which I am the lead author. Various people are updating data sets and doing calculations of trends, etc. Some of the surface numbers I found to be a bit disturbing -- so I am asking for your opinion. These are trends per decade for Jan. 1979 thru Dec. 2003

| SOURCE | GLOBE | 30S-30N |
|-----------|-------|---------|
| HadCRUT2v | 0.169 | 0.127 |
| NCDC | 0.151 | 0.146 |
| ERA40 | 0.113 | 0.032 |
| LKS | 0.074 | 0.056 |

(1) CRU and NCDC are consistent within the noise, but I have one question -- how do both calculate GLOBE?

(2) ERA40 is marginally OK (relative to CRU) in GLOBE, but the tropics is alarmingly different. (The diff here accounts for the GLOBE difference.) Why is this? Which is better? Is this discussed in your paper with Adrian?

(3) LKS is the surface data from the corrected LKS radiosonde data set. The difference here must be partly due to coverage issues. But I recall that years ago we saw a difference between surface sonde and CRU data. Have you done a like with like comparison (i.e., selecting the LKS sonde sites and extracting the corresp CRU (and NCDC, and ERA40 -- and (if possible) NCEP) data? This seems to be a pretty basic sanity check on the sonde data -- so, if you have not done this already, could you do it for me please?

I think there is a nice little GRL paper here. For the CCSP we are also giving trends, etc. over 1958-2003. So the real need is for a full time series comparison over this period -- i.e., not just trends. In other words, what I would like you to produce is the monthly time series for the various data sets for the LKS coverage. If you don't know the LKS site locations, I can get these for you.

Re going back to 1958, the sonde trop data have a well known (but not well explained) problem over roughly 1958 to 1964/5. I am curious as to whether this shows up in the LKS surface record. I am also curious about the apparent 1976 jump -- some people have made a lot of noise about this, but I don't see it as a major item in the global surface data. So the Q here is, is is apparent in the restricted coverage of the sonde data?

I hope you can help. I am leaving here on Sept 7 to spend a few days with a friend of mine in Plymouth -- you could contact me thru him (I

am copying this to him so you can see his email).

Thanx,

Tom.

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich Email p.jones@uea.ac.uk

NR4 7TJ

UK

From: Phil Jones <p.jones@uea.ac.uk>
To: wigley@cgd.ucar.edu
Subject: Sahel IJC paper
Date: Mon Sep 6 14:36:38 2004
Cc: santer1@llnl.gov

Tom,

You've probably seen this response to a truly awful paper in IJC. Aiguo did a really good job. Apparently, these two jerks have submitted a response to the comment. Wonder what they will say ? Adrian Chappell still thinks his analysis is correct !

Cheers

Phil

Prof. Phil Jones
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School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: Phil Jones <p.jones@uea.ac.uk>
To: wigley@cgd.ucar.edu
Subject: Re: question
Date: Thu Sep 9 13:52:25 2004
Cc: santer1@llnl.gov

Tom,
Program and the input LKS file. Program is adapted from one I had. Ended up a little convoluted. Should work with any of the 4 CRU temp data files (CRUTEM2(v), HadCRUT2(v)). For the Russian, grid point, changing 4 59 to 4 57 will give a box with data in from 1929.
3rd file is my unix run file - for files to channels.

Cheers
Phil

At 12:20 09/09/2004, D M R Taplin wrote:

Phil,
Thanx. Looks very interesting. I will look more when I get back to Boulder. It would help if you sent the program (just to Boulder). Also what are the numbers listed at the end of the LKS file?
Will you be reading email while away?
Tom.

=====
Professor David Taplin DSc
Coliemore House
Down Thomas Plymouth PL90BQ UK

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>
CC: Professor David Taplin <coliemore@hotmail.com>, Ben Santer <santer1@llnl.gov>
Subject: Re: question
Date: Wed, 08 Sep 2004 13:44:44 +0100

Tom,
Here are some files to look at and think about. John Lanzante has sent me the locations of the 87 stations in the LKS dataset. I associated these with CRU 5 deg grid boxes and calculated NH (based on 54 sites), SH (32) and Global (as one domain), so to get the globe the CRU way you need to average the NH and SH series (all to 3 deg places). The second line in all the results files is the count of stations. I can do this as % area if you want.

The CRU data I used is the file hadcrut2v, so this includes SST anomns over the

ocean.

I can repeat this with the land only file. Used the variance corrected version.

There are 4 files

1. The LKS stations. This is what John sent with the lat/long identifiers for the grid boxes on the front.

2-4 NH, SH and Globe as one domain results.

The first file has a fix in it. This is to pick up the 5 deg square (85-90S, 5W-0) that has

the South Pole data. This square is where I've always put this data.

For the NH there were 54 sites and for the SH 32. Site 9 (WMO ID 21504) is always missing,

even with hadcrut2v. The site is located on an island in the Laptev Sea. There isn't a surface

site anywhere near it. I could move the location and pick up the nearest CRU box, but it will

be over 5 deg of lat and 10 deg of long away. It's somewhat unusual for sonde sites not to have

a surface site near them. I guess it just doesn't report its surface data.

I'm here until Sept 15 then away for much of the time until end of October. I could send you

the program, which should run with crutem2v or the non-variance adjusted versions, which you

could pick up from the CRU web site.

Cheers

Phil

At 15:57 04/09/2004, Tom Wigley wrote:

Phil,

On Sept. 13-17 I will be at a meeting at the Met Office to do with a report we are writing on trends in vert temp profiles as part of the US Climate Change Science Program (CCSP). It involves all the usual suspects. Seven chapters, the last of which is equivalent to a summary for policy-makers -- for which I am the lead author.

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I hope you can help. I am leaving here on Sept 7 to spend a few days with a friend of mine in Plymouth -- you could contact me thru him (I am copying this to him so you can see his email).

Thanx,
Tom.

Prof. Phil Jones
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University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ

UK -----

<< lksdata.out >>

<< lksnh7003v.dat >>

<< lkssh7003v.dat >>

<< lksgl7003v.dat >>

Prof. Phil Jones

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From: Andy Revkin <anrevk@nytimes.com>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: mann's thoughts
Date: Tue, 28 Sep 2004 10:44:44 -0400

<x-flowed>

that is a useful way to look at it.

again, takeaway msg is that mann method can only work if past variability same as variability during period used to calibrate your method.

so it could be correct, but could be very wrong as well.

by the way, von storch doesn't concur with osborn/briffa on the idea that higher past variability would mean there'd likley be high future variability as well (bigger response to ghg forcing).

he simply says it's time to toss hockeystick and start again, doesn't take it further than that.

is that right?

At 09:40 AM 9/28/2004, you wrote:

>Dear Andy,

>
>our schematic figure is attached.

>
>Tim

>
>
>
>Dr Timothy J Osborn
>Climatic Research Unit
>School of Environmental Sciences, University of East Anglia
>Norwich NR4 7TJ, UK

>
>e-mail: t.osborn@uea.ac.uk
>phone: +44 1603 592089
>fax: +44 1603 507784
>web: <http://www.cru.uea.ac.uk/~timo/>
>sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

Andrew C. Revkin, Environment Reporter, The New York Times

229 West 43d St. NY, NY 10036

Tel: 212-556-7326, Fax: 509-357-0965 (via www.efax.com, received as email)

</x-flowed>

From: Stefan Rahmstorf <regentage@gmx.de>
To: Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: [Wg1-ar4-ch06] Ch6-Climate Sensitivity
Date: Fri, 01 Oct 2004 11:49:05 +0200
Reply-to: stefan@pik-potsdam.de
Cc: wg1-ar4-ch06@joss.ucar.edu

Hi co-authors,

here are some thoughts on what to say on climate sensitivity in our chapter - this is an attempt to focus on the main, simple messages for policy makers. (I think we should try retaining those important messages and not lose sight of them amidst all the details, complexity and caveats.)

The main policy-relevant question could be phrased as follows: Does the past climate history tell us how sensitive the climate system is to CO₂?

I submit that the answers to this we get from different time periods are the following.

Deep Time:

Reconstructions are too uncertain (and boundary conditions too different, e.g. continents in different places, different ocean circulation) to draw quantitative conclusions about sensitivity to CO₂, but there is clear evidence that times of high CO₂ in Earth history tend to be ice free (Royer et al. 2004). A second piece of evidence is the Late Paleocene Thermal Maximum, which shows that the climate has responded by warming to a large carbon release into the atmosphere. Just how large this carbon release was is not known, since several origins of the carbon are possible, which have different isotope signature and would thus imply different amounts. But the temperature response was large (6K), and if anything this response would point to a high sensitivity.

Glacial-Interglacial Changes:

We have by now sufficiently good quantitative reconstructions of CO₂ and other forcings as well as temperatures in order to derive useful quantitative estimates of climate sensitivity. LGM was the most recent time in history in which CO₂ concentration differed greatly from pre-industrial values, by as much as it does now. It is the closest test case for response to CO₂ changes that we have.

There are two basic methods to derive climate sensitivity:

(i) Based on data analysis - e.g. Lorius et al. 1991 (concluding sensitivity is 3-4 K).

This method has the caveat that this sensitivity applies to colder climate, which may differ somewhat from that which applies in present climate as the strength of feedbacks is expected to depend on the mean climate (e.g., stronger snow-albedo feedback in colder conditions).

(ii) Based on combining data and models - e.g. Schneider von Deimling et al. 2004. Does not have the above caveat, but depends on models.

Lag of CO₂ behind temperature does not imply a lack of CO₂ effect on climate, since the lag is small (centuries, not millennia).

Holocene, last millennium

??

Overall conclusions

Qualitatively, climate history is at least consistent with the accepted CO2 sensitivity. There is no evidence for much lower or much higher CO2 sensitivity (note that CO2 is not the only forcing). The more recent climate history (as far back as ice core data go) does allow quantitative inferences. The results of these estimates all lie within the IPCC range and provide strong support for this. Paleodata may even allow to reduce this range, since at least one study argues that values above 4K are very likely inconsistent with the reconstructed LGM climate: for high CO2 sensitivity, tropical cooling in the glacial should have been larger.

Cheers,
Stefan

Wg1-ar4-ch06 mailing list

Wg1-ar4-ch06@joss.ucar.edu <http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>

From: Tom Wigley <wigley@cgd.ucar.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: past 1000 yr
Date: Wed, 06 Oct 2004 11:58:16 -0600

<x-flowed>
SEE CAPS

Tim Osborn wrote:

> Hi Tom - I'd be happy to contribute if I have something worth
> contributing! I'm a bit rushed today and away tomorrow, but can
> respond to further emails later in the week.
>
> At 14:31 03/10/2004, Tom Wigley wrote:
>
>> Caspar Ammann and I plan to publish some MAGICC
>> results for the past 100 years.
>
>
> Presume you mean 1000 years, hence relevance of ECHO-H/von Storch.

OOPS! YES.

>
>
>> Part of the reason is the new
>> solar forcing, as in my Science note with Peter Foukal.
>
>
> Yes I saw that. With a brief scan I didn't realise that you were
> presenting a new forcing history, just discussing reasons why
> long-term changes may be lower than previously estimated. But
> presumably you can use such reasoning to develop a new forcing history
> - or, better, a range or even a PDF of such histories. And then
> extend it using 14-C or 10-Be, or a combination?

WE SAY *NO* LOW FREQ FORCING. C-14/Be-10 ARE PROXIES FOR MAGNETIC FIELD
CHANGES. THERE
IS NO ADEQUATE THEORY RELATING THESE TO LUMINOSITY CHANGES -- IN FACT

THEORY SUGGESTS

THEY ARE *NOT* RELATED. SO WE ARE SUGGESTING A DIFFERENT FORCING HISTORY, WITH IMPLICATIONS AS IN THE FIGURE. NO SOLAR-INDUCED LIA, IN ACCORD WITH THE PROXY CLIMATE RECONSTRUXIONS. FURTHER, THERE IS SOME RECENT WORK SUGGESTING THAT PART OF THE C-14/Be-10 CHANGESW ARE DUE TOCHZNGES IN THE *EARTH'S* MAGNETIC FIELD.

>
>
>> So we
>> address both forcing and sensivity uncertainties. In
>> addition, the drift due to incorrect initialization is an issue.
>
>
> Surely not so in MAGICC? But yes, it is in GCMs and particularly so
> in ECHO-G.

OF COURSE WHAT I MEAN IS TO USE MAGICC TO QUANTIFY THE INITIALIZATION 'DRIFT'.

>
>
>> I have not yet read the Storch paper or your comment -- but
>> did you mention this problem?
>
>
> We said that ECHO-G had a redder spectrum than other model simulations
> (there was no room to say that it showed greater fluctuations, but we
> cited the Jones/Mann paper which has an intercomparison figure in
> it). We didn't talk about the reasons for this (drift early on,
> strong solar forcing throughout and no tropospheric aerosols to
> mitigate recent warming) because we'd already said that the simulation
> didn't necessarily represent real climate history.
>
>
>> Also, can you remind me just what was done with the ECHO
>> run?
>
>
> Main problem in terms of introducing "drift" (or "adjustment") was

> that they used a control run with present day CO2 as initial
> conditions. Although they allowed a 70-year spin-up (prior to AD
> 1000) to adjust back to pre-industrial CO2, this doesn't look long
> enough and the adjustment probably goes on for the first 400 years of
> the run - i.e. there is gradually disappearing cooling trend over this
> period. All based on MAGICC runs, but still fairly convincing
> (including non-zero heat flux out of the ocean in ECHO-G itself).

SEE THE STOUFFER PAPER IN CLIM DYN 23, 327 (2004).

>
>
>> If you have something to add on this, you can join as a co-author.
>
>
> I'm not quite sure what you plan, nor the input you need, but
> hopefully I can help.

WHAT I WOULD LIKE IS YOUR BEST ESTIMATE OF THE MAGNITUDE OF THE SPURIOUS
INITIALIZATION EFFECT IN
TERMS OF FORCING.

>
>
> Cheers
>
> Tim
>
>
> Dr Timothy J Osborn
> Climatic Research Unit
> School of Environmental Sciences, University of East Anglia
> Norwich NR4 7TJ, UK
>
> e-mail: t.osborn@uea.ac.uk
> phone: +44 1603 592089
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> web: <http://www.cru.uea.ac.uk/~timo/>
> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
>
>

>

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: More vertical profile plots
Date: Thu Oct 7 10:28:36 2004

Ben,

Thanks for the plots. I gather from Karl that you'll be in Seattle and not at the HC review.

I'll be in Seattle also and am missing the HC review, so we can catch up on things.

Last week was the first LA meeting of AR4. You have likely been contacted by Kevin and also maybe by Brian Soden about writing something on tropopause heights. It would perhaps be useful to send them these figures and maybe also to David Parker.

For our chapter Kevin is co-ordinating the U/A and circulation sections. I'm doing the surface T/P and extremes and the final summary. I've been too busy to think about anything

yet ! We have a mix of abilities in the LAs, but Brian, David P, Dave Easterling and Albert

Klein Tank of KNMI are solid. The Iranian, Argentinian, Romanian, Kenyan don't seem up to too much, but this is life in the IPCC - remember Ebby !

The fact that HadCRUT2v is close to PCM may be fortuitous, but good nonetheless. If you

subsample PCM with CRU coverage, you say the PCM trend will reduce. The paper and report with Adrian shows that if you look at the full ERA-40 surface T data, then the reverse happens.

Not a large increase though. Most comes from the SH, so there are issues of what ERA-40 is doing over the Southern Oceans, Antarctica and Australia are key. I'll be talking about this

work in Seattle.

I don't have any IDAG work to give you - not done a lot. Plan to look at the 1740 event in Europe, when time permits. If you want any of my ppt for your IDAG talk, you can look through in Seattle.

Good to catch up in a weeks time. Hope you and Nick are well. Away next week in Delhi at a GCOS workshop.

Cheers

Phil

At 01:50 07/10/2004, you wrote:

Dear Jerry, Ram, and Jim,

Here are the profiles of zonally-averaged atmospheric temperature change that you requested. As I mentioned in yesterday's email, I've prepared a couple of different versions of these plots. First, there are two different analysis periods: January 1979 through to December 1999, and January 1958 through to

December 1999. Second, temperature changes are expressed in two different ways: in terms of linear trends per decade, and in terms of the total linear changes over the two analysis period. So there are four different vertical profile plots:

```
-rw-r--r-- 1 bsanter climate 194436 Oct 6 16:27 ccsp_vp_lt_1979-1999.ps
-rw-r--r-- 1 bsanter climate 142312 Oct 6 16:27 ccsp_vp_lt_1958-1999.ps
-rw-r--r-- 1 bsanter climate 201997 Oct 6 16:43 ccsp_vp_tlc_1958-1999.ps
-rw-r--r-- 1 bsanter climate 198109 Oct 6 17:04 ccsp_vp_tlc_1979-1999.ps
```

All the relevant information is encoded in the file name: "lt" denotes linear trend, and "tlc" denotes total linear change. Personally, I have a preference for the total linear change plots. If you compare panel f (the PCM ALL forcing case) of the "tlc" plots for 1979-1999 and 1958-1999, the much larger total changes over the longer analysis period are visually obvious. This is not the case if changes are expressed in degrees C/decade.

I note that (as requested by Roger Pielke in Exeter), the plots are appropriately area weighted.

All profiles of zonally-averaged atmospheric temperature change are ensemble means. Each ensemble mean was calculated from four individual realizations.

There is no subtraction of control run drift, which probably is not a significant factor at this point in the perturbation experiments.

I've also updated the two plots that I sent you yesterday, which show global-mean and tropical-mean profiles of atmospheric temperature change. These plots now include observed near-surface temperature trends, estimated from HadCRUT2 and HadCRUTv (the latter is the variance corrected version of HadCRUT2). PCM ALL and HadCRUT near-surface temperature changes are in good agreement, both for global- and tropical averages. I'm pretty sure that in the global-mean case, subsampling PCM ALL results with HadCRUT coverage would yield a slightly warmer PCM ALL 2m temperature trend (in view of the muted warming of 2m temperatures at high southern latitudes in ALL; these areas are not well sampled in HadCRUT).

It would be nice to show these plots of global- and tropical-average changes in Chapter 5. I think they make some useful points.

Hope all of this is helpful,

With best regards,

Ben

(P.S.: I'd like to acknowledge the assistance of Charles Doutriaux and Mike Wehner in producing these plots. Considerable data processing was involved in generating these six figures).

--

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From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Keith Briffa <k.briffa@uea.ac.uk>, wg1-ar4-ch06@joss.ucar.edu
Subject: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Date: Mon, 11 Oct 2004 20:27:35 +0200

<x-flowed>

Hi Keith,

I can take a stab at the THC bit (not strong evidence so far for linkages to multidecadal/century scale changes, but cannot be ruled out) the marine evidence from the North Atlantic (14C chronological control), and some aspects of tropical/high latitude linkages.
Eystein

At 17:00 +0100 11-10-04, Keith Briffa wrote:

>Friends and authors (especially Ricardo, Olga,
>Fortunat, David, Ramesh, Zhang, Dan, Eystein and
>Valerie)
>Now back from travels (until Wednesday when off to Austria for a few days)
>I thought it best to suggest a break down for
>the writing of the data section for the last
>2000 years of the IPCC palaeoclimate chapter.
>Please see the outline produced at the meeting.
>We have 4 IPCC pages . I will write a short
>intro linking to the instrumental data with
>links to Chapters 3-5. I will coach this in a
>general introduction to this section that
>addresses the points listed in the initial notes
>(namely how we use the various high , and few
>low, resolution data to construct regional and
>large-scale temperature variability , and where
>possible, gain insight into hydrologic
>variability. I will say we use models to get
>insight into methodology and to explore regional
>coverage and seasonality issues and we use
>control and forced model runs to look at
>sensitivity and detection issues , but also use
>data to test model variability and sensitivity .

>I can first go at the NH (SH) Spaghetti diagram
>discussion and hopefully you will pick up the
>regional aspects of the temperature and
>precipitation (moisture) variability .
>Rather than me say - I would like you to come
>back with the major areas you will cover , but
>these may best be done in terms of
>climatologically meaningful regions - ie
>relating to the ENSO, NAM, PDO , AAO, monsoon
>areas - then we could fill in the remaining
>regions if significant non overlap in areas is
>apparent (Eurasia, non-monsoon china etc) . We
>do not want a list of every paper ever written ,
>but a selection of (the better) work that you
>feel has regional relevance (and some length
>presumably). The other alternative is just to
>divide up the world to our own regions and then
>discuss the climate indices separately. This
>would likely be easier to do . Let me know what
>you think. Either way , we also should have a
>specific discussion of forcings at high
>resolution , and Fortunat, Valerie could cover
>solar and volcanic , perhaps Eystein discussing
>what evidence there is for THC change . The
>knotty issue of THC versus NAO and the link to
>model theories/models could go here - or
>perhaps later in the section 6.4.3.2 ? Davis
>what say you about this? The same is true of
>ENSO links to terrestrial precipitation patterns
>and temperature?
>I don't like the idea of dealing wit quasi
>periodicities separately , but rather wit the
>regional discussions eg North American drought.
>The question of LIA , MWP will come up in the
>large scale average discussion but you can also
>address it in the regional discussions , but in
>a critical and quantitative way. I would like to
>see the evidence for extremes/abrupt change
>from the regional syntheses and then see if we
>have enough to define and discuss the issue
>separately. Olga could you pick up on the
>glacial variations (perhaps with links to models
>also?)

>
>So come back to me asap to let me know
>impressions and regional/variable focus you all
>wish to pick up. Ricardo will obviously do North
>South linkages as per the PEP1 transect , but
>what about along PEP2 and 3/ WE may have to pick
>this up in the light of the regional data. Can
>you also let me know if/who you might be asking
>to help with writing . Peck , I would still
>rather have Mike Mann in , so what is the story
>here - can I ask him? Suggestions for summary
>Figures still welcome - I would like to have a
>High lat , mid lat , low lat transect type
>figure for temperature , possibly along each PEP
>transect - with longest instrumental data . A
>forcing diagram is also a must - but could
>combine Holocene and "blow up " last 2000 years.

>
>Best wishes

>Keith

>--

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--

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The Bjerknes Training site offers 3-12 months fellowships to PhD students
More info at: www.bjerknes.uib.no/mcts

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Re: comment Von Storch?
Date: Thu Oct 14 16:29:31 2004

Mike,
FYI.

I met this guy in Utrecht last week at Albert Klein Tank's PhD ceremony. It appears from many media reports that people really believe that their run is an ALTERNATE to yours - based

on no proxy data. Even Hans has sent an email around to this effect, but he obviously isn't

making it as clear as I've just done to this Dutch journalist. I think he might be being clear with

fellow scientists and economical with the truth with journalists, i.e. not directing them down the

correct path when he sees them going down the wrong one.

I should see Ray next week in Seattle at a DoE meeting.

Cheers

Phil

Dear Karel,

I have only got back from a meeting this morning. I see you have also had a long reply from

Mike Mann about the von Storch paper.

Basically the von Storch et al paper is a discussion of the methodology used in the Mann,

Bradley Hughes papers from 1998, 1999. It doesn't contain any new nor any observed proxy data. It is entirely a model study. Therefore, it cannot produce a record for the last millennium,

it cannot claim that the Medieval Warm Period was warmer than today, nor that the Little Ice

Age may have been colder than MBH says.

It is really alarming that many media people (including yourself) have been taken in. What the

von Storch et al paper is about is a climate model run - just one simulation. All it uses is

an estimate of past variations in solar forcing and volcanic eruptions and more recently anthropogenic changes in greenhouse gases and sulphate aerosols.

As I said the paper in a methodological critique of MBH, nothing more than that. It IS NOT

an alternative to MBH. It also not based on ANY paleoclimatic data. If you believe it, you are putting everything on the model being correct and that their best guess at the past

history
of forcing as being correct.
Regards
Phil

At 15:28 13/10/2004, you wrote:

Dear professor Jones,
(We met ten days ago in Utrecht, when Albert Klein Tank got his PhD).
I am a science journalist of the Dutch daily newspaper NRC Handelsblad in Rotterdam ([1]www.nrc.nl).
I try to write an article about climate (surface temperature) reconstruction as far back as the year 1000 - the well know Mann, Bradley, Hughes (1998 and 1999) research. The reason is, of course, the publication of the article of Von Storch, Zorita, c.s. in Science-online (30 september). Von Storch claims that the statistical approach of Mann c.s. produced a serious underestimation of the low frequency (long term) oscillations in global temperature. The conclusion could be that the Medieval Warm Period was in fact warmer than today. And the recent warming is - after all - not so special.
Can you in a few words - and for a general public - give a comment on the paper? Does it make sense? It seems pretty convincing to me.
Can you help me?
Waiting for your reply,
sincerely yours,
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e-mail knip@nrc.nl
phone 31-10-4067327

Prof. Phil Jones
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References

1. <http://www.nrc.nl/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: John.Birks@bot.uib.no, masson@lsce.saclay.cea.fr,
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oyvind.lie@bjerknes.uib.no , joos@climate.unibe.ch , juerg@giub.unibe.ch
, Elsa Cortijo <Elsa.Cortijo@lsce.cnrs-gif.fr>, j.holmes@ucl.ac.uk,
harrye@ldeo.columbia.edu, jgoqam@iiqab.csic.es, mschulz@geo.palmod.uni-
bremen.de
Subject: IMPRINT Budget (Work package 1)
Date: Wed Oct 20 13:49:34 2004

Dear Partners in Workpackage 1 of IMPRINT,
today is the deadline by which Eystein requested input as regards the
reworked (and necessarily much shortened), proposal document. We have
also been making some
effort to consolidate the indicative budgets that most of you have
sent to us.

We now need to transfer these figures to Eystein , even though a few
partners have not
supplied numbers to us , though they may have sent them to Eystein
directly.

It is clear that we are now close to 30 partners in Workpackage 1
alone, and have
indicative budget requests totaling well over the nominal 5 million
Euro originally
allocated. In fact , the likely total with all partner requests
included is likely to be
nearer to 10 million!

We have been given a (very unofficial) hint from Brussels that an
"appropriate" total
project request of about 17 million for IMPRINT might be sensible ,
with a final figure ,
if the project ever gets accepted, of 15 million being possibly
awarded (subject of course
to referees' comments and subsequent reorganisation of priorities).
The simple message is that Eystein will now have to make an executive
decision as to the
total amount requested .

If we ever get that far, reorganised budgets will have to be decided
on the basis of very

specific
work plans that will need to be formalised for a second submission -
especially as they relate
to the justification for field work and new data analyses. We also
need to budget for the
involvement of non-partners , possibly using a mixture of workshop and
minor funding awards
to facilitate data collection etc.

It has been made clear that new practical work campaigns would not be
sanctioned across all

Tasks

in Workpackage 1 . Rather, the bulk of work would involve re-
dating/interpretation of
mostly existing data and reconstructions of forcings and climate .
Specific cases will have

to be made to justify sampling and processing of new data.

Thanks to all of you for your help and thanks to Eystein for taking on
the enormous task of

organising this proposal .

Keith and Tim

--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Rob Wilson" <rjwilson_dendro@blueyonder.co.uk>
To: <K.briffa@uea.ac.uk>
Subject: data - Quaternary Science Reviews 19 (2000) 87-105
Date: Thu, 21 Oct 2004 15:53:21 +0100
Reply-to: "Rob Wilson" <rjwilson_dendro@blueyonder.co.uk>

Hi Keith,

When would be a good time tomorrow (or next week) to phone you about the data you have available at your website from your QSR 2000 paper.

I am particularly interesting in using the long chronologies from the Polar Urals (Yamal) and Tornetrask.

This is for Gordon's and Rosanne's NH temp recon update, so I thought I should have a chat with you before using the data.

all the best

Rob

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: MBH
Date: Fri Oct 22 15:13:20 2004
Cc: santer1@llnl.gov

Tom,

Just got the Science attachments for the von Storch et al. paper for Tim and Keith, so I thought you might like to see them. I've just sent a reply to von Storch as he claims his model is a better representation of reality than MBH. How a model that is only given past forcing histories can be better than some proxy data is beyond me, but Hans seems to believe this. The ERA-40 report and JGR paper are relevant here. ERA-40 is not of climate quality. There are differences and trends with CRU data before the late 1970s and again around the mid-1960s that should include other variables that are calculated. It is so bad in the Antarctic that ERA-40 rejects most of the surface obs (because they get little weight) and they don't begin to get accepted until the late 1970s. Conclusion is that

you can't consider ERA-40 for climate purposes. Maybe the next generation, with a considerable efforts in getting all the missing back data in and changes to weights given to surface data might mean the 3rd generation is better.

I shouldn't rabbit on about this as I have to go home to drive with Ruth to Gatwick for our week in Florence. A lot of people criticise MBH and other papers Mike has been involved in, but how many people read them fully - or just read bits like the attached. The attached is a complete distortion of the facts. M&M are completely wrong in virtually everything they say or do. I have sent them countless data series that were used in the Jones/Mann Reviews of Geophysics papers. I got scant thanks from them for doing this - only an email saying I had some of the data series wrong, associated with the wrong year/decade.

I wasted a few hours checking what I'd done and got no thanks for pointing their mistake out to them.

If you think M&M are correct and believable then go to this web site

[1]<http://cgi.cse.unsw.edu.au/~lambert/cgi-bin/blog/>

It will take a while to get around these web pages and you've got to be a bit of nerd and know

the jargon, but it lists all the mistakes McKittrick has made in various papers. I bet there isn't

a link to this on his web site. The final attachment is a comment on a truly awful paper by

McKittirck and Michaels. I can't find the original, but it's reference is in this. The paper didn't consider spatial autocorrelation at all. Fortunately a longer version of the paper did get rejected by IJC - it seems a few papers are rejected !

Point I'm trying to make is you cannot trust anything that M&M write. MBH is as good a way of putting all the data together as others. We get similar results in the work in the Holocene in 1998 (Jones et al) and so does Tom Crowley in a paper in 1999. Keith's reconstruction is strikingly similar in his paper from JGR in 2001. Mike's may have slightly less variability on decadal scales than the others (especially cf Esper et al), but he is using a lot more data than the others. I reckon they are all biased a little to the summer

and none are truly annual - I say all this in the Reviews of Geophysics paper !

Bottom line - there is no way the MWP (whenever it was) was as warm globally as the last 20 years. There is also no way a whole decade in the LIA period was more than 1 deg C on a global basis cooler than the 1961-90 mean. This is all gut feeling, no science, but years of experience of dealing with global scales and variability.

Must get to Florence now. Back in Nov 1.

Cheers
Phil

At 20:46 21/10/2004, you wrote:

Phil,
I have just read the M&M stuff criticizing MBH. A lot of it seems valid to me. At the very least MBH is a very sloppy piece of work -- an opinion I have held for some time.
Presumably what you have done with Keith is better? -- or is it?
I get asked about this a lot. Can you give me a brief heads up? Mike is too deep into this to be helpful.
Tom.

Prof. Phil Jones
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References

1. <http://cgi.cse.unsw.edu.au/~lambert/cgi-bin/blog/>

From: Phil Jones <p.jones@uea.ac.uk>
To: Adrian.Simmons@ecmwf.int, santer1@lnl.gov
Subject: Fwd: Re: K&C (fwd)
Date: Mon Nov 22 09:29:09 2004
Cc: wigley@ucar.edu

Adrian and Ben,

Roger Pielke did send this to me over the weekend, so he's being honest in one respect. I still think he's reading far too much into NCEP1. The bottom panel of their Fig1 shows both CRU and GHCN (-ERA40) having no difference over the period from the late 1960s. If the obs assimilated before 1967 (even in the US) were improved, the apparent drop before might disappear.

Cheers

Phil

Date: Fri, 19 Nov 2004 18:35:58 -0700 (MST)
From: Roger Pielke <pielke@atmos.colostate.edu>
To: p.jones@uea.ac.uk
cc: wigley@cgd.ucar.edu
Subject: Re: K&C (fwd)
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean

Phil-

FYI; thank you for sharing your paper. I have circulated the attached to our CCSP Committee with the permission of Eugenia and Ming, and want to also share with you.

The conclusion from my own work with the NCEP reanalysis is that it is appropriate for trend assessments if integrated metrics are used (thickness for example), and for regions where the regional trend signal is quite large. We have published on both of this issues. One value-added of reanalyses is that since the winds are monitored independently of the temperatures, they provide information on the horizontal layer averaged temperatures in the mid- and high-latitudes, which helps adjust, to some extent, biases in the temperatures.

Also, as we have shown with regional data (e.g. Florida) and others have shown elsewhere (e.g. Andy Pitman for Australia) there is a clear land use change signal on surface temperature. This provides independent evidence that the Kalnay and Cai results should be expected.

Roger

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Roger A. Pielke, Sr., Professor and State Climatologist
1371 Campus Delivery, Department Atmospheric Science,
Colorado State University, Fort Collins, CO 80523-1371,
Phone: 970-491-8293/Fax: 970-491-3314, Email: pielke@atmos.colostate.edu

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and [2]<http://climate.atmos.colostate.edu>

----- Forwarded message -----

Date: Fri, 19 Nov 2004 11:04:42 -0700 (MST)

From: Roger Pielke <pielke@atmos.colostate.edu>

To: _NESDIS NCDC CCSP Temp Trends Lead Authors

<CCSPTempTrendAuthors.NCDC@noaa.gov>, chris.folland@metoffice.gov.uk,
peter.thorne@metoffice.gov.uk

Cc: Eugenia Kalnay <ekalnay@atmos.umd.edu>, Ming Cai <cai@huey.met.fsu.edu>

Subject: Re: K&C (fwd)

Resent-Date: Fri, 19 Nov 2004 11:05:15 -0700

Resent-From: CCSPTempTrendAuthors.NCDC@noaa.gov

Hi All

I requested to Ming Cai and Eugenia Kalnay that they respond to the comments regarding their work. The response is forwarded to you in this e-mail.

This debate, of course, should really take place in the literature. There has been, however, in my view an unfortunate change over time where reviewers who disagree with already published work recommend rejection of subsequent work rather than letting the community view and assess the different perspectives on a science issue. Our report has to make sure it is inclusive, in order to avoid this pitfall.

An unbiased discussion of the K&C results, and ways to resolve the disagreement through hypothesis testing, should be included in the appropriate chapters.

Roger

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1371 Campus Delivery, Department Atmospheric Science,
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VISIT OUR WEBSITES AT: [3]<http://blue.atmos.colostate.edu/>
and [4]<http://climate.atmos.colostate.edu>

----- Forwarded message -----

Date: Fri, 19 Nov 2004 12:16:27 -0500

From: cai <cai@met.fsu.edu>

To: Roger Pielke <pielke@atmos.colostate.edu>

Cc: Ming Cai <cai@met.fsu.edu>, Y. K. Lim <yklim@met.fsu.edu>,
Eugenia Kalnay <ekalnay@atmos.umd.edu>

Subject: Re: K&C

Dear Roger,

Attached is the preliminary summary report on our recent work on the estimate of land-use-change climate impact using the reanalysis. Very fortunately, we had secured a one-year funding from NSF starting last August. Despite a short time period, we have already produced sufficient

results to confirm the robustness of our original work using different datasets that have the state-of-art quality.

Here I just want to add one more comment about Simmons et al. paper. Basically, they claimed that the difference between the ERA40 and CRU is very small and therefore, our method is not applicable if the reanalysis is as good as the ERA40. There are two things that are incorrect in their claims. First of all, if the reanalysis were made to be exactly the same as the observations, by definition, there would be no difference between reanalysis and the surface observations. Since the ERA40 was obtained by directly assimilating the CRU surface observations whereas the NNR didn't use any surface temp. observation, it is natural to expect that the difference between the surface observation and ERA40 is small. Second, Simmons et al. manually reduces the difference between the ERA40 and CRU by setting the mean difference between the ERA40 and CRU from 1987 to 2001 be ZERO. As a result, the difference "LOOKs" very small in recent years. However, the difference from 1961 to 1985 has to be larger (otherwise, they would make an error in their plot). In other words, by doing so, the gap between the ERA40 and CRU appears decreasing in time rather increasing in time as shown in KC and in the new figure 1 in the attached file (which is the same as Simmons et al. paper except we reset the 1960-70 to be zero in order to see how the POSITIVE gap increases in time). If we closely examine their figures, we will see by applying their treatment, the gap between CRU and reanalysis is a NEGATIVE one (e.g., CRU is below ERA40 from 1960 to 1980) and such a NEGATIVE gap decrease in time is equivalent to that the POSITIVE gap increases in time as found in KC from the NNR data (e.g., the CRU becomes more above the ERA40). So Simmons et al's results actually CONFIRM our findings rather discredit our finding. We actually reproduced Simmons et al calculations and confirm that their results are correct (see the second attached figure, which is identical to Fig.1 in our preliminary report except the NEGATIVE gap is used and 1-year running mean was applied as in Simmons et al). But their interpretations are incorrect.

I appreciate if you could also forward the email to the CCSP authors. Let me know if you want to me to reply to Tom and CCSP co-authors directly.

Regards.

Ming

The report:

The replica of one of the key figures in Simmons et al.

On Nov 18, 2004, at 4:53 PM, Roger Pielke wrote:

Tom-

Since we have not seen the paper, we cannot make any judgements on the robustness of that paper in showing that the Kalnay and Cai work is "flawed". I expect to have a summary by Eugenia and Ming tomorrow, however, which will address the published concerns on their work, and

will
forward to the Committee. Please forward us a copy of the Simmons et al
paper.

I also would like a response to my MWR Florida paper where we
specifically show the dominant role of documented land use change in
peninsular Florida in the 20th century on July-August surface air
temperature change. Or Andy Pitman's work who shows a major effect on
temperature trends in south-western Australia due to land use change.
This work, and others like it, support the conclusions of Kalnay and
Cai
on a major role of land surface processes on surface temperature
trends.

How do you reconcile those independent conclusions with the paper you
list above?

Roger

--

+++++

Roger A. Pielke, Sr., Professor and State Climatologist
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Colorado State University, Fort Collins, CO 80523-1371,
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pielke@atmos.colostate.edu

VISIT OUR WEBSITES AT: [5]<http://blue.atmos.colostate.edu/>

and [6]<http://climate.atmos.colostate.edu>

On Thu, 18 Nov 2004, Tom Wigley wrote:

Date: Thu, 18 Nov 2004 14:28:16 -0700

From: Tom Wigley <wigley@cgd.ucar.edu>

To: CCSP Authors <CCSPTempTrendAuthors.NCDC@noaa.gov>

Subject: K&C

Resent-Date: Thu, 18 Nov 2004 14:28:17 -0700

Resent-From: CCSPTempTrendAuthors.NCDC@noaa.gov

Folks,

Roger makes the point that there is no comprehensive assessment of
this

paper.

There is ... It is in a paper that has, I believe, been accepted by

JGR

atmospheres.

A.J. Simmons, P.D.Jones, et al. "Comparison of trends and
low-frequency

variability in CRU,

ERA-40 and NCEP/NCAR".

I think the conclusion is that the K&C paper *is* flawed.

Tom.

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References

1. <http://blue.atmos.colostate.edu/>
2. <http://climate.atmos.colostate.edu/>
3. <http://blue.atmos.colostate.edu/>
4. <http://climate.atmos.colostate.edu/>
5. <http://blue.atmos.colostate.edu/>
6. <http://climate.atmos.colostate.edu/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: v.jones@geog.ucl.ac.uk
Subject: first go
Date: Tue Nov 23 16:01:56 2004
Cc: v.shishov@uea.ac.uk

Viv

attached is the text you sent with some suggestions and comments (track changes must be on).

I am also sending a small piece of text that could be expanded if needed (this to be inserted where you describe the tree-ring input) - but at this stage I think you need to have a look at comments and consider the specifics of the lake and tree sampling (the latter if any).

I thought it best to send these comments rather than plough on doing stuff you don't want. I think the "hook" needs to be the important opportunity to assess recent changes in lake and tree productivity and see if any evidence for response to climate, as well as searching for unprecedented evidence of climate change. I realise this is predominantly a lake project with a link to trees and models, but the links must be more than token. I can provide more background as to where we are with tree-ring work in Euro-Siberia if needed. I think the model stuff also needs specific justification. Is Simon going to contribute here?

Don't get hung up on the "decline or changing sensitivity issue" in trees. This is NOT a great problem in Scandinavia, Ural/Yamal and is anyway a divergence in trend and quite subtle and evident in wood density mostly. We are also of the opinion that it could be partly a statistical processing artifact - we are exploring this now.

If you plough through my comments and suggestions and then return the text with specific requests of what you wish to do I will then try to oblige Thursday

cheers

Keith

--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Martin Todd <mtodd@geog.ucl.ac.uk>
Subject: Re: NERC application
Date: Tue Nov 30 16:34:00 2004

Martin

in response to Nadia's message and our talk - consider the following as regards title and objectives

Title

The precedence of Ecological Responses to 20th Century Climate changes in Arctic Lakes and Trees

Suggested Objectives

We will quantify how the changes in 20th century Arctic climate (including mean and variability) are reflected in recent and past lake sediment records. We will determine the response of lake ecosystem parameters and the relationships with specific climatic controls.

We will define the character of variability in different natural archives contained in dated sediments reaching back over 2000 years. We will generate well-calibrated, high-resolution (decadal to centennial time scales) estimates of past summer climate variability over this time in western Arctic Siberia.

We will compare the lake sediment data with evidence of tree-growth and associated summer climate changes, based on selected updating of an extensive, existing network of chronologies, including long sub-fossil series extending back more than 4000 years in Yamal and Taimyr. These data (with perfect inter-annual dating accuracy) will be reprocessed to provide summer temperatures specifically representative of annual, decadal and centennial timescales.

We will determine (for the first time) the extent to which the independent proxy-based summer climate histories concur or disagree and explore the extent to which they demonstrate the precedence of recent (20th century) climate trends in a multi-millennial context. By comparing this evidence with the output of state-of-the-art GCM experiments, simulating climate changes in the Arctic over the last 500 to 1000 years, we will explore the degree to which recent changes in Arctic lakes (and tree-growth rates) are attributable to anthropogenic as opposed to natural climate changes.

At 13:55 30/11/2004, you wrote:

Hi keith,

The submission deadline for the NERC grant with Viv Jones is imminent. She's getting in a bit of a panic. I wonder whether you have some text already prepared to describe the details of the ECHO-G experiments. I could get the information but will have to dig in the literature. I was hoping you would have a summary paragraph from the SO&P document similar to the one we have written about the HADCM3 exp

Thnaks

Martin

Martin Todd University Lecturer Department of Geography

UCL (University College London)

26 Bedford Way

London WC1 8HR

email m.todd@geog.ucl.ac.uk

--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: New version of Chapter 4
Date: Thu Dec 2 10:01:40 2004
Cc: "Folland, Chris" <chris.folland@metoffice.gov.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Ben Santer <santer1@llnl.gov>

Dear Toms, Chris and Ben,

If large-scale is important (as said by Tom W), I can't see how microclimatic issues that Roger goes on about can be that important. Maybe when you all meet at the delightful Chicago Airport Hilton, you can remind him of spatial degrees of freedom.

Is the NOAA Tsurf used the new Smith and Reynolds (2005) spatially infilled surface dataset? If this is the case maybe Ben could do a plot of NOAA minus HadCRUT2v?

I have a plot that David Parker produced of Smith and Reynolds (2005) over land and Jones and Moberg (2003) land (as smoothed global averages) from 1880. Prior to about 1960 the SR dataset is always about 0.15 warmer than JM. This looks likely due to infilling with 61-90 averages (i.e zeroes) over the Antarctic and some continental interiors of S. America, Africa, western China and Australia (where there are no obs pre early 1950s, 1956 for the Antarctic). SR should be OK for 1979-99 and be very similar to HadCRUT2v.

Cheers

Phil

At 23:31 01/12/2004, Roger Pielke wrote:

Tom-

One issue to sort out with respect to "VTT" remains whether there are unrecognized biases in the surface data. This issue is very much relevant if, as seems the case from Phil Jones's e-mail, the "raw data" that has been used has such large overlap among the different surface analyses. If this is the case, there are not three independent assessments of surface temperature trends. Moreover, unlike the MSU data, there are inhomogeneities associated with the diverse locations of each surface monitoring site (which have microclimate changes over time).

This issue is also very much a tropical issue as this is where large land use/land cover change has occurred in the satellite era (photographs rather than written documentation would really help in this assessment, as we have proposed).

Roger

--

+++++

Roger A. Pielke, Sr., Professor and State Climatologist
1371 Campus Delivery, Department Atmospheric Science,
Colorado State University, Fort Collins, CO 80523-1371,
Phone: 970-491-8293/Fax: 970-491-3314, Email: pielke@atmos.colostate.edu
VISIT OUR WEBSITES AT: [1]<http://blue.atmos.colostate.edu/>
and [2]<http://climate.atmos.colostate.edu>

On Wed, 1 Dec 2004, Tom Wigley wrote:

> Date: Wed, 01 Dec 2004 16:15:01 -0700
> From: Tom Wigley <wigley@cgd.ucar.edu>
> To: "Folland, Chris" <chris.folland@metoffice.gov.uk>
> Cc: Thomas R Karl <Thomas.R.Karl@noaa.gov>,
> Roger Pielke <pielke@atmos.colostate.edu>,
> Phil Jones <p.jones@uea.ac.uk>, carl mears <mears@remss.com>,
> CCSPTempTrendAuthors.NCDC@noaa.gov

> Subject: Re: New version of Chapter 4

>

> Chris et al.,

>

> I do not see this as high priority. We are supposed to be looking at
> *VTT*. Uncerts/diffs in individual data sets are relevant, of course, but
> what is currently missing is a map (maps) of sfc vs trop trend diffs.
> We are meant to be addressing a problem that we have made
> clear at the global and tropix scale -- but just *where* are the problem
> areas? (I think Carl showed us such a map previously -- we need this,
> or similar, or more, in the report since it really is the crux of the
> problem.)

>

> Ideally we need sfc minus MSU LoTrop (A), sfc minus MidTrop
> (UAH (B) and RSS(C)) to at least look at, and decide which is/are best to
> show. I imagine this will have some bearing on Roger Pielke's concerns
> re LULC. If the biggest differences are over the oceans (and from memory
> this is the case, worst in the SH), then sorting this out would arguably
> be more important than sorting out LULC effects. It would be hard to
> argue (albeit not impossible) that teleconnections from LULC in (e.g.)
> North America, or even the Amazon Basin, are responsible for trend diffs
> over the South Pacific

>

> In Ch. 1 there is a correlation map -- this is pretty useless in my
> view, altho
> it would be interesting to compare the correl map with an equiv trend
> diff map.

>

> Ch. 3 has maps of the trends at sfc, mid trop, lo strat -- so we are close
> to trend diff map. But even those who might be brilliant enough to produce
> the trend diff map in their heads will be thwarted, becoz the mid trop map
> in Ch. 3 uses the average of UAH and RSS. Good grief! This really is
> carrying political correctness too far. Please, please John L et al.,
> replace
> the mid trop panel in 3.6.2.3 by separate panels for RSS and UAH.

>

> The next in my list of related wishes is a map of the RSS minus UAH trend
> diffs (D). Eyeballing A, B, C and D together could be interesting.

>

> I would put these things right at the top of my wish list for Chicago.

>

> Tom.

> =====

>

> Folland, Chris wrote:

>

>>Tom

>>

>>Can you get Russ Vose to look at the issues of data overlap and local
>>and regional similarity. My original suggestion was to compare trends
>>over 1958-2003 and 1979-2003 at each grid point in the two data sets and
>>also over larger (regional) areas. This would go to the heart of any
>>differences in the context of this report, is easy to do, and can be
>>plotted on a pair of maps with a third "difference in trend" map for
>>each period. Where differences are large, a more detailed look at the
>>data can be done. It might even show up errors! Even the first analysis

> >on its own should give enough information to sharpen up well the current
> >speculative text and can be done perhaps in parallel with NRC review.
> >
> >Chris
> >
> >Professor Chris Folland
> >
> >Head of Climate Variability Research
> >
> >Global climate data sets are available from [3]<http://www.hadobs.org>
> >
> >Met Office, Hadley Centre, Fitzroy Rd, Exeter, Devon EX1 3PB United
> >Kingdom
> >Email: chris.folland@metoffice.gov.uk
> >Tel: +44 (0)1392 886646
> >Fax: (in UK) 0870 900 5050
> > (International) +44 (0)113 336 1072)<[4]<http://www.metoffice.gov.uk>> >
> >Also: Hon. Professor of School of Environmental Sciences, University of
> >East Anglia
> >
> >
> >

> >-----Original Message-----

> >From: Thomas R Karl [[5]<mailto:Thomas.R.Karl@noaa.gov>]
> >Sent: 01 December 2004 18:23
> >To: Roger Pielke
> >Cc: Phil Jones; Folland Chris; carl mears;
> >CCSPTempTrendAuthors.NCDC@noaa.gov
> >Subject: Re: New version of Chapter 4
> >
> >

> >Phil,

> >I think we need to be careful -- the method of combining the data can
> >matter very much. It is just that despite our different methodologies
> >the results are similar on large scales. I know we could use other
> >methods and the differences are more significant, e.g, first
> >differences, homogenization of ships, etc.
> >

> >Tom

> >Roger Pielke wrote:

> >>Hi Phil

> >>Thanks for the quick feedback. This helps a lot!

> >>With Best Regards

> >>Roger

>>>
>>
>>
>>
>>
>>
>>
>

Prof. Phil Jones
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References

1. <http://blue.atmos.colostate.edu/>
2. <http://climate.atmos.colostate.edu/>
3. <http://www.hadobs.org/>
4. <http://www.metoffice.gov.uk/>
5. <mailto:Thomas.R.Karl@noaa.gov>

From: Phil Jones <p.jones@uea.ac.uk>

To: dkaroly@ou.edu, Kevin Trenberth <trenbert@cgd.ucar.edu>

Subject: Re: Communication with AR4 WGI Chapter 3

Date: Wed Dec 8 11:42:31 2004

Cc: Susan Solomon <solomon@al.noaa.gov>, Martin Manning <Martin.Manning@noaa.gov>, Jean Palutikof <jean.palutikof@metoffice.gov.uk>, Cynthia Rosenzweig <crosenzweig@giss.nasa.gov>

Resending. Apologies! I changed Jean's email incorrectly. This one is now correct.

Phil

David,

I will send you this once we post the ZOD on the WG1 web site in mid-Jan05. Our diagrams are in a state of flux. Most of the temperature and precipitation trend maps are being done

in Asheville and I should be getting them later this week or early next. We will be showing maps

for the whole 20th century, but others will focus on the period since 1979. You might like to consider avoiding duplication by using these - eventually they will be 1979-2005 (poss 2006).

Trends of indices in extremes will likely be similar, but with +/- signs on maps. Nothing has been decided yet, though, and I expect a significant part of our time at LA2 will be taken up

by discussing/improving diagrams in our ZOD.

You can help us by sending comments to WG1 on the relevant parts - which are likely to be almost all.

Cheers

Phil

Cheers

Phil

At 16:47 07/12/2004, David Karoly wrote:

Hi,

As you may be aware, I am an LA for chapter 1 "Assessment of observed changes and responses in natural and managed systems" in the AR4 WGII and I have been identified as one of the points-of-contact for interactions between WGI and WGII. The chapter in which I am involved will depend heavily on inputs from a number of chapters in the WGI report. Hence, I contacting the CLAs of the relevant chapters, including chapters 2, 3, 4, 5, 6, 7, and 9, by email to discuss ways to ensure effective communication between our chapters and to avoid undue overlap between respective chapters in WGI and our chapter in WGII.

Your chapter on "Observations: Surface and atmospheric climate change" is a key chapter in WGI and it is important that what we say in our chapter in WGII follows from and agrees with your chapter. I would be very happy to discuss ways to ensure effective communication between our two chapters.

Specific aspects from your chapter of relevance to our chapter include observed changes in regional temperature and precipitation, both means and extremes. We plan to use a figure in our chapter showing a global map of observed temperature trends over the last 30 years (?) overlaid with locations of significant observed changes in natural and managed systems. We want to make sure that this is based on the same dataset(s) that you will be using to show the observed temperature trends.

In practice, almost everything in your chapter will be relevant to our chapter. I would be grateful if you could send me a copy of your ZOD after it is completed, so that I can make sure that our chapter is consistent with yours. I am happy to send you a copy of our ZOD, if you would like to read it.

I will not be coming to the WGI LA meetings until LA3, when I will be involved as a review editor. It will be important that we have already established effective communication before then.

I look forward to working with you over the next two years to ensure that the IPCC AR4 is the best possible assessment.

Best wishes, David

--

~~~~~  
Dr David Karoly

Williams Chair and Professor of Meteorology

School of Meteorology

University of Oklahoma phone: +1-405-325-6446

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Norman, OK 73019 email: dkaroly@ou.edu

USA [1]<http://weather.ou.edu/~dkaroly/Personal.htm>  
~~~~~

Prof. Phil Jones

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References

1. <http://weather.ou.edu/~dkaroly/Personal.htm>

From: Gavin Schmidt <gschmidt@giss.nasa.gov>
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solomon@al.noaa.gov, jmahlman@ucar.edu, rbierbau@umich.edu
Subject: RealClimate.org
Date: 10 Dec 2004 08:56:42 -0500
Cc: Mike Mann <mann@virginia.edu>, Eric Steig <steig@ess.washington.edu>,
ammann@ncar.edu, rbradley@geo.umass.edu, aclement@rsmas.miami.edu,
rasmus.benestad@met.no, rahmstorf@pik-potsdam.de

Colleagues,

No doubt some of you share our frustration with the current state of media reporting on the climate change issue. Far too often we see agenda-driven "commentary" on the Internet and in the opinion columns of newspapers crowding out careful analysis. Many of us work hard on educating the public and journalists through lectures, interviews and letters to the editor, but this is often a thankless task.

In order to be a little bit more pro-active, a group of us (see below) have recently got together to build a new 'climate blog' website: RealClimate.org which will be launched over the next few days at:

<http://www.realclimate.org>

The idea is that we working climate scientists should have a place where we can mount a rapid response to supposedly 'bombshell' papers that are doing the rounds and give more context to climate related stories or events.

Some examples that we have already posted relate to combatting dis-information regarding certain proxy reconstructions and supposed 'refutations' of the science used in Arctic Climate Impact Assessment. We have also posted more educational pieces relating to the

interpretation of the ice core GHG records or the reason why the stratosphere is cooling. We are keeping the content strictly scientific, though at an accessible level.

The blog format allows us to update postings frequently and clearly as new studies come along as well as maintaining a library of useful information (tutorials, FAQs, a glossary etc.) and past discussions. The site will be moderated to maintain a high signal-to-noise ratio.

We hope that you will find this a useful resource for your own outreach efforts. For those more inclined to join the fray, we extend an open invitation to participate, for instance, as an occasional guest contributor of commentaries in your specific domain, as a more regular contributor of more general pieces, or simply as a critical reader. Every time you explain a basic point of your science to a journalist covering a breaking story, think about sharing your explanation with wider community. RealClimate will hopefully make that easier. You can contact us personally or at contrib@realclimate.org for more information.

This is a strictly volunteer/spare time/personal capacity project and obviously nothing we say there reflects any kind of 'official' position. We welcome any comments, criticisms or suggestions you may have, even if it is just to tell us to stop wasting our time! (hopefully not though).

Thanks,

Gavin Schmidt

on behalf of the RealClimate.org team:

- Gavin Schmidt
- Mike Mann
- Eric Steig
- William Connolley
- Stefan Rahmstorf
- Ray Bradley
- Amy Clement
- Rasmus Benestad
- William Connolley
- Caspar Ammann

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: Some weekend thoughts
Date: Mon Dec 13 09:29:24 2004

Kevin,

Read everything over the weekend, and here are a few comments. Glad I did this yesterday, as not thinking too well at the moment as daughter-in-law in labour for the last 4 hours. No news yet - just waiting !

Haven't made any alterations yet. Here are my thoughts.

3.1 I'll make a few cosmetic changes - mainly to refer to the Appendices a couple of times re significance.

Box 3.3 Reads better, will replace with this one when merge is done.

3.4 3.4.1.5 needs some work. Doesn't seem to read or flow that well.

3.4.2.1 Maybe need to expand on homogeneity tests.

3.4.2.2 4th para seems a little at odds with previous one?

3.4.2.3, 3.4.2.4 OK

3.4.3 Clouds. Needs some more work to develop a clearer message. You're aware of this.

3.4.4 Radiation. Similar comments to the cloud section. I have some specific notes for both. Despite this, probably OK for the ZOD. Maybe all we need to do is to highlight this to the reviewers.

3.5 Section seems overlong. I know you've reduced it a lot ! Contains a number of sentences where English could be improved.

3.5.1. OK

3.5.2 Significance levels for Fig 3.5.1 need some discussion. We'll need to work some on this Figure.

3.5.3 and 3.5.4 OK for the ZOD with a few better sentences.

3.5.5 and 3.5.6 Both sections seem overlong. Again know you've reduced this a lot, but if we need reductions here is a good place.

3.5.7 OK

Box 3.5 OK

3.6 Generally good.

3.6.1 OK

3.6.2 Probably remove the impact para - leave for the moment, though.

3.6.3 OK

3.6.4 I can improve this a little. It isn't all Scandinavian glaciers that are advancing, just those in SW Norway. Those in the north of Sweden are retreating.

3.6.5 OK

3.6.6/ 3.6.7 Basically OK. May need more re ACW and SAM link if we can say

anything.

3.7 This is probably too long, so would be another area for some reduction.

Agree on your suggestions for deletions as repetitive.

3.7.1.1-3.7.1.3 OK though all a little long.

3.7.1.4 This is the one where there is some repetition. Not much on monsoon.

A lot here is already in 3.8 on extremes and the Dai et al (2004) paper is now referred to in 3.3, here and in 3.8. Suggest it should just be in 3.3 and again in 3.9 (it isn't there yet).

Your figures seem in better shape than those in my section. We will likely need to work on the one Dennis is doing. Will need some colour. You're aware of which need more work from your comments. We can leave these in for reviewer and LA thoughts.

Dave has sent me a first go at the figures. Made loads of suggestions. Dave was aware colour choices poor and will be doing more on them today.

Is Chris Landsea the only person you've removed from the CA list so far? It seems so.

I should have time tomorrow onwards to do merging and send out the 3 files to all our LAs. Are you happy with me merging in your refs list? I'll keep the discard ones at end in a separate list. Still hopeful of doing all this by close of play here on Thursday. All day in London on Friday and CRU party today week from 11am onwards. Going for Dec 16 means I will only be able to get some of the Figures in 3.2 and 3.3 properly into the text.

Will send Dave's next Figure versions if they are much better. No point with current one.

Still no news !

Cheers

Phil

At 21:16 10/12/2004, you wrote:

Phil

Attached are the three sections. Please use these for any suggested edits. Of the text, 3.7 is loosest and needs careful comparison with 3.3 to check for inconsistencies. There is model stuff in there that is not quite right or incomplete: I removed some. There is redundant ENSO-related stuff. A lot of the monsoon variability is linked to ENSO and we could say that succinctly but it would decimate what the CAs and Panmao have done. I think we will need to do this in Beijing, but I left it for now. Note the refs has a list of discards at the end.

Suggest we keep this, perhaps in a different file, and if stuff gets deleted with references, then the refs get moved there.

Some of the figures are not quite in order in 3.6 and there is the extra figure that

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: need to chat - important
Date: Mon, 13 Dec 2004 10:55:45 -0500

Hi Keith,

I have to head out around 11:30 AM (40 minutes from now). You can try reaching me at my cell phone after that (434-227-6969)...

Thanks,

Mike

At 08:03 AM 12/13/2004, Michael E. Mann wrote:

Hi Keith,

I'll be working at home this morning. You can call me at: 434-977-7688

Mike

At 07:25 AM 12/13/2004, Keith Briffa wrote:

Mike

could you confirm a telephone number to call you on in 3 hours say

thanks

Keith

--

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References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: email #1: some background info first...

Date: Mon, 13 Dec 2004 11:47:16 -0500

Hi Keith,

Thanks again for your phone call, and the (informal) opportunity to help out where I can. I'm perfectly happy in that role (as an informal contributor and a formal reviewer, for example), if you and Peck, for example, are both comfortable with that.

First, "RealClimate" should be helpful. It deals w/ the skeptic claims, etc. but using the legitimate

peer-reviewed research as a basis for the discussion.

The "hockey stick" overview should be helpful:

[1]<http://www.realclimate.org/index.php?p=7>

as well as itemized responses to the various contrarian propaganda/myths:

[2]<http://www.realclimate.org/index.php?p=11>

and the specific discrediting of the claims of McIntyre and McKittrick, based both on our response to their rejected Nature comment:

[3]<http://www.realclimate.org/index.php?p=8>

and the discussion of the analysis in the Rutherford et al (2004) paper in press in Journal of Climate, that independently discredits them:

[4]<http://www.realclimate.org/index.php?p=10>

In the following emails, I'll attach some other materials (submitted papers) that deal w/ the McIntyre and McKittrick matter, and the von Storch matter,

Please let me know if there is anything we discussed that I forget to provide you. Will also draft an email to the small group (you, me, Scott, Caspar, Gene) about the prospective additional RegEM/Mann et al method model analyses,

cheers,

Mike

Professor Michael E. Mann

Department of Environmental Sciences, Clark Hall

University of Virginia

Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[5]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

References

1. <http://www.realclimate.org/index.php?p=7>

2. <http://www.realclimate.org/index.php?p=11>
3. <http://www.realclimate.org/index.php?p=8>
4. <http://www.realclimate.org/index.php?p=10>
5. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: email #2: paper in review in J. Climate (as a letter), discrediting McIntyre and McKittrick

Date: Mon, 13 Dec 2004 11:47:26 -0500

Keith,

This paper is in review, and can be referred to (just clear w/ Caspar or Gene first) for IPCC draft purposes. They basically show that the McIntyre and McKittrick paper is total crap, and they provide an online version of the Mann et al method (and the proxy data), so individuals can confirm for themselves...

Mike

Professor Michael E. Mann

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University of Virginia

Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\Wahl_MBH_Recreation_JClimLett_Nov22.pdf"

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: email #3: Stendel et al paper (submitted)
Date: Mon, 13 Dec 2004 11:53:16 -0500

Keith,

Attached is the Stendel et al paper (submitted to "Climate Dynamics" last month) and a corrected version of their Figure 3 (using the correct Mann and Jones NH series).

The importance of this paper is that they use the same model as von Storch (higher resolution in fact), and get a temperature history that looks much like the reconstructions/other models. Also, they appear to get the negative NAO pattern in the Maunder Minimum, which von Storch et al do not...

Again, this should be referenceable in the zero order draft, but would be good to contact Martin Stendel first about this...

Mike

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\stendel_et_al_ClimDyn.pdf" Attachment Converted:
"c:\eudora\attach\nh-extend.pdf"

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: email #4: comment (in press in Science) on von Storch et al paper
Date: Mon, 13 Dec 2004 11:56:41 -0500

Keith,

I think the attached comment (in press in "Science") is pretty self-explanatory. It raises the main objections to the von Storch et al paper (some of which you and Tim already had raised, really)...

Mike

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\VonStorchReply04-submitrevised.pdf"

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: email #5: paper in review in J. Climate letters using NCAR forced simulation and RegEM

Date: Mon, 13 Dec 2004 11:56:56 -0500

Hi Keith,

here (w/ the supplementary info also attached) is the paper summarizing the results I showed in Victoria of the RegEM analysis of pseudoproxies in the forced CSM simulation. This is in review as a "letter" in Journal of Climate, and can be referred to as "submitted" in the zero-order draft.

As we discussed, parallel experiments are being done using the MBH98 method, but regardless of those results, this suggests, at least, that the RegEM-based NH reconstructions (e.g. in the Rutherford et al paper you're co-author on) are unlikely to be impacted by the bias discussed by von Storch et al...

Mike

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
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Charlottesville, VA 22903

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137

[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\pseudoproxy-jclimlett1.pdf" Attachment Converted: "c:\eudora\attach\supplementary1.pdf"

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>
Subject: Re: [Wg1-ar4-ch06] Fw: Section on Modes of Variability
Date: Thu, 16 Dec 2004 17:37:03 -0700
Cc: k.briffa@uea.ac.uk, peltier@atmosp.physics.utoronto.ca, Eystein Jansen <eystein.jansen@geo.uib.no>

Hi Ricardo - good to hear from you. Thanks too for the interesting figure. I have some comments on this section (6.5.4) and also for the others' you're helping to lead.

Regarding 6.5.4 - I hope Dick and Keith will have jump in to help you lead, and I can too. I think the hardest, yet most important part, is to boil the section down to 0.5 pages. In looking over your good outline, sent back on Oct. 17 (my delay is due to fatherdom just after this time), you cover ALOT. The trick may be to decide on the main message and use that to guide what's included and what is left out. For the IPCC, we need to know what is relevant and useful for assessing recent and future climate change. Moreover, we have to have solid data - not inconclusive information. My take:

ENSO - coral records sensitive to ENSO (e.g., Urban et al. and Cobb et al - attached) suggest ENSO has changed in response to past forcing change (Cobb et al - updated interp by Mann et al - see recent email attachment) and recent climate change (Urban et al). Ditto for Indian Ocean - not sure if can connect to dipole - I could ask Julie Cole? NAO - lots of papers and what's the consensus? I'm not sure, but I think it is that we can't say for sure what has happened to the NAO - or AO for sure (Keith might no more - recent Ed Cook paper might be the key? - I'm not an expert here). Same thing for PDO (not an expert, but aren't their reconstructions that don't agree - see Cole et al for one - attached). In both these cases, the reconstructions don't always agree. Or do they say the NAO variability has stayed pretty constant?

Tropical Atlantic - Black et al 1999 (attached to prev email) also says 12 year mode (no consensus if dipole is the correct name for what Chang first described - see ref in Black attached) has been constant for 800 years.

Annual modes - does paleo have anything definitive to say yet? I'm a coauthor on a soon to be submitted AO reconstruction paper, but I'm not sure reviewers will go for it - nor does it match D'Arrigo's recent AO reconstruction paper (can't find).

So, the trick is for you to lead us (Dick, Keith, me - maybe Julie - ENSO expert) to produce 0.5 pages of HIGHLY focused and relevant stuff. Can you take another crack at your outline and then tell us what you need? Thanks!

Regarding 6.5.9 - can you help Dan, Ramesh and others to make quick headway on this one - it's totally missing. Thanks!

Regarding 6.3.2.1 - Keith will need help, no doubt - particularly with a good S. Hemisphere perspective (he can override me on this, but since I'm contacting you...) thanks! What do we have for the southern hem? Southern S. America, New Zealand, Tasmania, ice core?

Regarding 6.3.2.2 - what's your opinion of where this section stands?

Thanks - hope you are enjoying summer - although Tucson never gets that cold!

Best, Peck

----- Original Message -----

From: [1]Ricardo Villalba

To:

Sent: Thursday, December 16, 2004 2:55 PM

Subject: Fw: Section on Modes of Variability

Dear IPCC colleagues

Please, find attached a preliminary draft of the proposed figure for the section: Modes of variability. The caption follows. Best regards,

Modes of variability

Figure caption. Coherent modes of climate variability across the Pacific Ocean during the past four centuries. The upper part of this figure compare temperature-sensitive tree-ring records (red triangles) from high-latitude, Western North and South America with a geochemical coral record (yellow triangle) from Raratonga, tropical South Pacific. The series shown from top to bottom are: Spring/Summer Gulf of Alaska temperature reconstruction (1600-1994; Wiles et al., 1998), Sr/Ca coral record from Rarotonga (1726-1996; Linsley et al. 2004) and annual Northern Patagonia temperature reconstruction (1641-1989; Villalba et al., 2003). Correlation coefficients between records are indicated. To facilitate the comparison, the Sr/Ca coral record is shown reversed.

Interdecadal to centennial variability in each time series was isolated by using singular spectrum analysis (SSA; lower part of the figure). For each record, all SSA reconstructed components with mean frequencies longer than 20 years were summed. Correlation coefficients between these long-term modes of variability are also shown.

Thin and thick arrows indicate coincidences in oscillations between the Raratonga and one or two high-latitude records, respectively.

Linsley, B., G. Wellington, D. Schrag, L. Ren, M. Salinger and A. Tudhope, 2004: Geochemical evidence from corals for changes in the amplitude and spatial pattern of South Pacific interdecadal climate variability over the last 300 years. *Climate Dynamics*, 22, 1-11.

Villalba, R., Lara, A., Boninsegna, J.A., Masiokas, M., Delgado, S., Aravena, J.C., Roig, F.A., Schmelter, A., Wolodarsky, A., Ripalta, A. 2003. Large-scale temperature changes across the southern Andes: 20th-century variations in the context of the past 400 years. *Climatic Change*, 59: 177-232.

Wiles, G. C., D'Arrigo, R.D. and Jacoby, G.C., 1998. Gulf of Alaska atmosphere-ocean variability over recent centuries inferred from coastal tree-ring records. *Climatic Change*, 38, 289-306.

Ricardo

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PAGES SSC: [3]<http://www.pages.unibe.ch/>

Attachment converted: Macintosh HD:modes of variation.jpg (JPEG/prvw) (000C0BD1)

Wg1-ar4-ch06 mailing list
Wg1-ar4-ch06@joss.ucar.edu
<http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>

--

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"c:\eudora\attach\Coleetal2002GRL.pdf"

References

1. <mailto:ricardo@lab.cricyt.edu.ar>
2. <mailto:ricardo@lab.cricyt.edu.ar>
3. <http://www.pages.unibe.ch/>

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>, Kevin Trenberth
<trenbert@cgd.ucar.edu>, Peter Ambenje <omash01@yahoo.com>, Roxana
Bojariu <bojariu@b.astral.ro>, David Easterling
<david.Easterling@noaa.gov>, David Parker
<david.parker@metoffice.gov.uk>, Fatemeh Rahimzadeh <rahim_f@irimet.net>,
Jim Renwick <j.renwick@niwa.co.nz>, Matilde Rusticucci
<mati@at.fcen.uba.ar>, Brian Soden <bsoden@rsmas.miami.edu>, Panmao Zhai
<pmzhai@cma.gov.cn>, Albert Klein Tank <Albert.Klein.Tank@knmi.nl>
Subject: Re: [Fwd: Re: [Fwd: Re: "Model Mean Climate" for AR4]]
Date: Mon Dec 20 17:55:56 2004
Cc: richard.wood@metoffice.gov.uk

Kevin,

I will be around tomorrow (so Dec 21) until Dec 23 inclusive. Then
again from Jan 3.

I will be checking email during the break from Dec 28 onwards.

Are you in control of the glossary additions and modifications?

As to change of base period - this seems like a decision for the
whole of WGI. To redo

the global temperature average, I can just move the series up/down,
but this isn't

the correct way to do it. I should talk out a new base period from
all the individual

stations and recalculate anomalies for the oceans. For the oceans
this isn't a

problem, but the land it is a serious problem. Many stations have
good (i.e. near

complete base periods for 1961-90) but I'll lose hundreds, maybe over
a thousand,

stations if I went to 1981-2000.

For both surface temperature and precipitation we don't have
spatially complete datasets

(like models) so it will be quite difficult.

For the circulation indices (like SOI and NAO) based on station
pairs there is a

variance term (SD). Some of the character of the series will change.
We could

easily adjust all these series by simple offsetting but it isn't
doing it properly.

I'm in the throws of a project with the HC checking all the 61-90
normals we have

for series that are incomplete, to ensure we don't have any biases.
This has taken

quite a time and I don't want to waste the effort.

The arguments of Albert and Dave make a lot of sense - continuity
with the TAR etc.

These sort of things can be explained, but then the FOD will not be
compatible with

all the papers we are referring to. This will lead to lots of
confusion. I would like to

stick with 1961-90. I don't want to change this until 1981-2010 is
complete, for 3

reasons : 1) We need 30 years and 81-10 will get all the MSU in nicely, and 2)

I will be near retirement !! 3) is one of perception. As climatologists we are often changing base periods and have done for years. I remember getting a number

of comments when I changed from 1951-80 to 1961-90. If we go to a more recent one

the anomalies will seem less warm - I know this makes no sense scientifically, but

it gives the skeptics something to go on about ! If we do the simple way, they will say

we aren't doing it properly.

Best idea might be to show some maps of 1981-2000 minus 1961-90 to show spatially

where it makes a difference for temp and precip. Showing it is quite small and likely

within the intermodel differences for years which are only nominally 1981-2000. This

might

keep both sides happy.

We also probably need to consider WGII. Also the paleo chapter will find 1981-2000

impossible. 1961-90 is difficult for them but not insurmountable.

Cheers

Phil

PS Fatima has received all the emails - her email only came to me. Not heard from

some of our LAs.

At 15:44 20/12/2004, Kevin Trenberth wrote:

Hi all

I have received comments on this from Albert, David, Dave, and Jim. Some below.

As I commented to Jim, the choice of a base period affects the zero line. In some of

our plots, namely the ones that have series of bars from the zero line to the anomaly

value, thereby infilling between the anomaly and the zero, the zero base value is

greatly emphasized. This is in contrast to a simple time series with points joined,

especially if the zero line is not also drawn. In the latter case, it is simple to move

the axis up or down to fit with the new base period. But it makes a bigger difference

to the bar plots. Now maybe that is a comment on the use and utility of bar plots,

because the relative values do not change.

The choice also affects any anomaly plots for any subperiod. But this is where the

comparison with models is most likely to occur. In this case there is a spatial pattern

to the offset, namely the difference between means for 1961-90 and 1981-2000. We could also derive that difference for certain fields and provide it to modelers to enable comparisons with our plots. For trends over certain subperiod, this makes no difference.

It seems that whatever we do, we will need an extra appendix explaining some of this and perhaps even giving plots of these differences.

In the meantime, let me suggest to those of you making computations, that you consider doing it both ways, rather than having to go back and do it over later.

Regards

Kevin

I agree with Albert, this would make comparisons with the TAR figures difficult.

Dave

Klein Tank, Albert wrote:

Hi Kevin,

My immediate response is that the choice for another base period will probably not

affect our assessment of results, but it will change all figures w.r.t the TAR. This

will be difficult to communicate and will take much more space to explain.

Albert.

----- Original Message -----

Subject: Re: [Fwd: Re: "Model Mean Climate" for AR4]

Date: Mon, 20 Dec 2004 13:06:44 +0000

From: Parker, David (Met Office) [1]<david.parker@metoffice.gov.uk>

To: Kevin Trenberth [2]<trenbert@cgd.ucar.edu>

References: [3]<41C34CDA.3060304@cgd.ucar.edu>

Kevin

It is obviously possible to use 1980-2000 though it would require some data-processing work. The main objection is that anomalies (of temperature) would appear to be reduced relative to previous publications and readers/policymakers could become confused. A minor objection is that 1980-2000 is a bit short. Satellite data are of course in its favour. In due course, 1981-2010 will be ideal!

Regards

David

On Fri, 2004-12-17 at 21:17, Kevin Trenberth wrote:

> All

> Please note the discussion below. Note the proposed base period of

> 1980-2000. Can we get your reactions? If it is decided to use this,

> what difficulties would it create? Other comments?

> Kevin
>
> ----- Original Message -----
> Subject:
> Re: "Model Mean Climate" for AR4
> Date:
> Fri, 17 Dec 2004 14:14:58 -0700
> From:
> Kevin Trenberth
> [4]<trenbert@cgd.ucar.edu>
> To:
> Wood, Richard
> [5]<richard.wood@metoffice.gov.uk>
> CC:
>
> References:
>
> [6]<FCE86FAA6B302A42AF7F9C6255745E3703C5F4@exxmail2.desktop.frd.metoffice.com>
>
> Richard
>
> The current base period being used in Chapter 3 is anomalies
> determined with respect to the 1961-1990 base period. In
> observations there is a strong emphasis on using 30 year periods and
> the more recent one, 1971-2000 is not yet available. We would need to
> discuss whether to try to switch to that. It certainly won't be in
> any ZOD. Otherwise, though, we are placing a lot of emphasis on
> trends from 1979 on. The grounds for this are 1) The 1976-77 shift
> seems to be about when anthropogenic climate change took off: prior to
> then we are under the realm of natural variability (basically a TAR
> result); and 2) 1979 is when a whole bunch of satellite data and
> other analyses (like global reanalyses) become much more reliable and
> global. So 1979 is the closest proxy to 1976/77.
>
> If 1981-2000 is to be used, it will, of course, include some climate
> perceptible climate change that may influence perceptions of
> anomalies. But I agree there is a lot to be said for consistency.
> Moreover, it is manageable for observational data bases. Because of
> the satellite effects on obs it is important to start on or after 1979
> and stop while we still have obs. So for round numbers 1981-2000 makes
> most sense. I think that was the conclusion we came to in Trieste,
> but it is not reflected in any material I have seen yet in our
> chapter.
>
> Phil is not available till after New Year, I believe.
>
> Regards
> Kevin
>
> Wood, Richard wrote:
> > Dear Jerry and other CLAs,
> >

> > Jerry: would you be willing to do this please, once some text is agreed?

> > All: any comments on the proposed text? (esp from observational chapters re meaning periods). An early response would be appreciated as if we send this to PIs it needs to be done as soon as possible.

> >

> >

> > We've just had a meeting of Chapter 8 LAs in San Francisco. One issue that came up was what period of what run to use for the analysis of the 'mean climate' in the AR4 models, for Chapter 8. Clearly we hope there will be a number of diagnostic projects looking at the models over the next few months, and the more uniformly that analysis can be done the better.

> >

> > To cut a long story short, we felt that given the choice it would be most appropriate to define models' 'mean climate' by looking at the 1981-2000 mean from the all forcings 20th Century runs (or the ensemble mean if there is an ensemble). That would be consistent with the base period Chapter 10 is using for the projections. We recognise that there could be all sorts of reasons why that is not appropriate in particular cases, both scientific and practical (e.g. the observational dataset covers another period, or a longer time mean is needed because of particular modes of variability, or there is a problem with model drift or trends). So we wouldn't want to be prescriptive, but all other things being equal we would suggest that as the analysis period. If there are no show-stoppers for this, we were thinking it would be good to send out a brief email to the PIs of the diagnostic projects to request that they bear this in mind in their analysis. Jerry, there were a few other topics that might be raised in such an email and Karl Taylor will be contacting you about those.

> >

> > To be definite, I suggest below some straw-man text that could be sent out.

> >

> > Thanks and best wishes,
> > Richard

> >

> > "Defining model 'mean climate':

> > In defining the 'mean climate state' of a model for comparison
against
> > observations there are number of choices that could be made, e.g. use
> > model 'control runs' (which may have either preindustrial or present
day
> > trace gases), or use the '20th Century all forcings' runs (many of
which
> > are available as ensembles started from varying initial conditions).
For
> > the 20th Century integrations there is also a choice of meaning
period.
> > It is recognised that the optimal choice for a given problem may
depend
> > on a number of factors including the period over which observations
are
> > available, and the need for a non-drifting or non-trending model
> > solution. We also recognise that some projects have already begun
their
> > analysis based on a particular choice. We therefore do not wish to
> > prescribe a solution to this problem and leave it to the judgement of
> > individual projects. However, in cases where there is a choice, we
wish
> > to encourage as much uniformity in the analysis as possible, and
> > therefore propose that other things being equal, model mean climate
is
> > defined based on the 1981-2000 period of the 'all forcings 20th
> > Century' runs (or the ensemble mean where appropriate)."
> >
> >
> > -----
> > Richard Wood
> > Met Office Fellow and Manager Ocean Model Evaluation
> > Met Office Hadley Centre for Climate Prediction and Research
> > FitzRoy Road, Exeter EX1 3PB, UK
> > Phone +44 (0)1392 886641 Fax +44 (0)1392 885681
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[8]<http://www.metoffice.gov.uk>
> >

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| | |
|--|---|
| Kevin E. Trenberth | e-mail: |
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| Climate Analysis Section, NCAR | [10] www.cgd.ucar.edu/cas/ |
| P. O. Box 3000, | (303) 497 1318 |
| Boulder, CO 80307 | (303) 497 1333 (fax) |

Street address: 1850 Table Mesa Drive, Boulder, CO 80303

| | |
|----------------------------------|--|
| Prof. Phil Jones | |
| Climatic Research Unit | Telephone +44 (0) 1603 592090 |
| School of Environmental Sciences | Fax +44 (0) 1603 507784 |
| University of East Anglia | |
| Norwich | Email p.jones@uea.ac.uk |

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References

1. <mailto:david.parker@metoffice.gov.uk>
2. <mailto:trenbert@cgd.ucar.edu>
3. <mailto:41C34CDA.3060304@cgd.ucar.edu>
4. <mailto:trenbert@cgd.ucar.edu>
5. <mailto:richard.wood@metoffice.gov.uk>
6. <mailto:FCE86FAA6B302A42AF7F9C6255745E3703C5F4@exxmail2.desktop.frd.metoffice.com>
7. <mailto:richard.wood@metoffice.gov.uk>
8. <http://www.metoffice.gov.uk/>
9. <mailto:trenbert@ucar.edu>
10. <http://www.cgd.ucar.edu/cas/>

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: A quick question
Date: Tue Dec 21 11:39:09 2004

Kevin,

No idea how Chris Folland got this. Presumably David Parker forwarded it !

Anyway, it doesn't matter. The questions are:

When will you be sending me your signed-off draft?

Will this be the complete doc file of text?

Will you be modifying any of the figures?

On the latter just want to know if I'm keeping track of figs as well as Refs. I've got the two you sent last night.

I'll be off from 5pm on Dec 23. I'll begin reading the draft from Dec 29. Will likely be in at least once on Dec 29-31, but will be checking email from Dec 29.

Cheers

Phil

All

As someone who dealt with these matters in the past, a decision about the climate normals period was regarded as so important that all of WG1 debated it and agreed the outcome. So that should be the route again, I believe, if a change is wanted. From a personal perspective, I tend to agree with Phil that this time we should stick (in general) to 1961-90 normals, and that IPCC 2013 should perhaps change to 1981-2010.

Having said that, we may produce 1981-2000 normals in the next year for SST if we can solve adequately remaining problems (for climate change monitoring) with satellite SSTs. A key goal is monitoring changes in the Southern Ocean. Solutions are likely to include use of some corrected (to bulk SST data) ATSR data. This depends on work elsewhere in the Met Office. However, some less well corrected AVHRR data is needed as well to extend normals adequately back to 1981 in much of the Southern Ocean. This may give a new perspectives on the southern ocean SST changes; are likely to be significantly different in the southern half of the southern ocean from the global average. This is suggested by the lack of reduction of Antarctic sea ice, in contrast to the Arctic, which still persists. Such work may or may not get into IPCC FAR but if it did, it could be a special case. But it would need careful handling for conversion to advice to policy makers.

Chris

Prof. Phil Jones

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UK

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Fwd: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Date: Thu, 23 Dec 2004 14:04:44 -0500

Hey Keith,

I hope your visit w/ your family went well...

I went ahead and tried to make some constructive comments on what you sent (figured it would be nice to get this out of the way before the holidays come round)..

Let me say I think it's shaping up very nicely--looks like it should be a significant improvement on the '01 report. You've handled the various controversies and points of dispute delicately and adeptly, while still driving home in the end the key point (that the evidence appears to point to anomalous late 20th century behavior).

I made a dozen or so minor comments--please make use of them as you see fit.

Lets reconvene on this after the holidays. Thanks again for including me in and giving me an opportunity to comment.

I hope the rest of your holidays go well,

mike

At 01:31 PM 12/22/2004, you wrote:

Mike

don't know what the status of the whole chapter is - but I thought I would send this very first and rough

draft to you anyway - I have to wait and see the whole thing and hear from Peck before doing more.

Just heard my dad is now pretty much bedridden and officially declared blind (diabetes etc) and have to fit in a visit to him and mum (who I have not seen for ages) and spend at least a few days with the kids so there is no way I can work more on this till later

- as I said - really appreciate your input , have a great Christmas and for f..ks sake

keep the right priorities to the fore as the years progress

cheers

Keith

Date: Wed, 22 Dec 2004 18:23:02 +0000

To: Jonathan Overpeck <jto@u.arizona.edu>

From: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: [Wg1-ar4-ch06] IPCC last 2000 years data

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

Bcc: t.m.melvin@uea.ac.uk,Tim Osborn <t.osborn@uea.ac.uk>

Peck and Eystein

I have to break off now for the christmas period

This is unavoidable. I am sending what I have now

even though I am not at all happy with it.

It is obviously only part way there. Getting the data to produce Figures and work out how to design them is going to be time very consuming and I will rely entirely on Tim here to do them

- and the regional input

stuff if wanted will need input from a number of people that I have not been able to contact (see later)

The borehole discussion (contributed to by Henry Pollack) will need batting around and Henry (and Mike , who contributed a section on regional forced changes) will need to be kept on board. There will be loads to say on the simulated temperature histories and Tim will help here also

- but much is unpublished or

even unanalysed (hence Simon and Eduardo will need to contribute eventually). The glacier bit at the end is what Olga sent and I have not had time to work through it.

You two need to give some direction as to how

much you wish to have explicitly looking at the mass of NAO?AO reconstructions , ditto ENSO or PDO and all the

simulations of these - but at this stage not sure where in overall plan all this going. Do we really want a discussion on MWP

and LIA per se ? The regional descriptions , including Southern Hemisphere could be infinite length and I suppose we should only discuss longest or pre assimilated information - but will need specific input here from colleagues if we are to do these regional (including precipitation) sections .

I know Julie and Ed , and presumably Eystein , will be the best people to ask.

I am attaching the current text and placeholder ideas for Figures .

Not feasible to work more on these until know wider priorities re space.

Have had bad experience with ENDNOTE - and Tom Melvin here will forward the biblio file later.

I wanted to do more , but that is all I can manage til after Xmas

Here is wishing you (and your loved ones) all the best

Keith

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[2]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\IPCCFAR_6-3-2-1_ mem23-12-04.doc"

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Phil Jones <p.jones@uea.ac.uk>
To: t.osborn@uea.ac.uk,k.briffa@uea.ac.uk
Subject: Fwd: Re: Fw: Rutherford et al. [2004]
Date: Tue, 04 Jan 2005 11:22:31 +0000

FYI.

Just look at the attachment. Don't refer to it or send it on to anybody yet. I guess you could refer to it in the IPCC Chapter - you will have to some day !

Cheers
Phil

X-Sender: mem6u@multiproxy.evsc.virginia.edu
X-Mailer: QUALCOMM Windows Eudora Version 6.1.1.1
Date: Thu, 30 Dec 2004 09:22:02 -0500
To: Phil Jones <p.jones@uea.ac.uk>
From: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fw: Rutherford et al. [2004]
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean
X-UEA-MailScanner-SpamScore: s

Phil,
I would immediately delete anything you receive from this fraud.
You've probably seen now the paper by Wahl and Ammann which independently exposes McIntyre and McKittrick for what it is--pure crap. Of course, we've already done this on "RealClimate", but Wahl and Ammann is peer-reviewed and independent of us. I've attached it in case you haven't seen (please don't pass it along to others yet). It should be in press shortly. Meanwhile, I would NOT RESPOND to this guy. As you know, only bad things can come of that. The last thing this guy cares about is honest debate--he is funded by the same people as Singer, Michaels, etc...
Other than this distraction, I hope you're enjoying the holidays too...
talk to you soon,
mike
At 09:02 AM 12/30/2004, you wrote:

Mike,

FYI. Just in for an hour or so today as still off until Jan 4.
Not replied to this - too much else with IPCC etc. Not read this in detail - just printed it off.

Have a good New Year's Eve.
Cheers
Phil

From: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Fw: Rutherford et al. [2004]
Date: Wed, 29 Dec 2004 10:08:18 -0500
X-Mailer: Microsoft Outlook Express 6.00.2800.1158
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean

Dear Phil,

I have noticed the following statements in Rutherford et al [2004], in which you are a co-author. As compared with some of your co-authors, I get the impression that, while you feel very strongly about your views, you are also concerned with getting to the bottom of matters and are less concerned with scoring meaningless debating points. In this spirit, I draw your attention to some incorrect statements in Rutherford et al. [2004] concerning our material. There is really a quite serious problem with the PC methods in MBH98 and the comments made in Rutherford et al [2004] are really quite misleading. For the reasons set out below, I request that these comments be removed from the manuscript.

Regards, Steve McIntyre

----- Original Message -----

From: [1]Steve McIntyre
To: [2]David Randall
Cc: [3]Scott Rutherford ; [4]Paul Kushner ; [5]Cindy Carrick ; [6]Ross McKitrick
Sent: Tuesday, December 28, 2004 1:48 PM
Subject: Rutherford et al. [2004]
Dear Dr. Randall,

Recently, at the website [7]www.realclimate.org, Michael Mann publicized a submission by Rutherford et al. to Journal of Climate, entitled Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Method, Predictor Network, Target Season, and Target Domain. This paper contains some untrue statements and mischaracterizations regarding criticisms we (McIntyre and McKitrick) made of Mann et al. (1998) [MBH98] in a 2003 paper and subsequent exchanges under the auspices of Nature. We are writing to request that these untrue statements be removed from the paper before any further processing of the document by Journal of Climate takes place.

First, Rutherford et al. states that McIntyre and McKitrick [2003] used an incorrect version of the Mann et al. (1998) proxy indicator dataset. The history of this matter is

summarized below (all relevant emails and other documentation are available at [8]<http://www.climate2003.com/file.issues.htm> .

In April 2003, we requested from Mann the FTP location of the dataset used in MBH98. Mann advised me that he was unable to recall the location of this dataset and referred the request to Rutherford. Rutherford eventually directed us to a file (pcproxy.txt) located at a URL at Manns FTP site. In using this data file, we noticed numerous problems with it, not least with the principal component series. We sought specific confirmation from Mann that this dataset was the one used in MBH98; Mann said that he was too busy to respond to this or any other inquiry. Because of the many problems in this data set, we undertook a complete new re-collation of the data, using the list of data sources in the SI to MBH98 and using original archived versions wherever possible. After publication of McIntyre and McKittrick [2003], Mann said that dataset at his FTP site to which we had been referred was an incorrect version of the data and that this version had been prepared especially for me; through a blog, he provided a new URL which he now claimed to contain the correct data set. The file creation date of the incorrect version was in 2002, long prior to my first request for data, clearly disproving his assertion that it was prepared in response to my request. Mann and/or Rutherford then deleted this incorrect version with its date evidence from his FTP site.

It is false and misleading for Rutherford et al. to now allege that we used the wrong dataset. We used the dataset they directed us to at their FTP site. More importantly, for our analysis, to avoid the problems with the principal component series, we re-collated the tree ring data identified in MBH98 from ITRDB archives, calculated fresh principal component series; in addition, we re-collated other proxy data from archived versions wherever possible. Thus, our own calculations were not affected by the errors in the supplied file as we did NOT use the incorrect version in our calculations. To suggest otherwise, as is done in Rutherford et al [2004], is highly misleading. To date, no source code or other evidence has been provided to fully demonstrate that the incorrect version (now deleted) did not infect some of Manns and Rutherfords other work. In this respect, we note that the now deleted file pcproxy.txt occurs in a legend in a graphic at Rutherfords website, indicating possible use elsewhere by Rutherford of the incorrect version.

Accordingly, we request that the above claim be removed from the manuscript.

Secondly, Rutherford et al. [2004] argues that the difference between MBH98 results and MM03 results occurs because of our misunderstanding of a stepwise procedure in MBH98 for the calculation of principal component series for tree ring networks. Again, this claim is misleading on its face. While our 2003 paper did not implement the (then undisclosed) stepwise procedure, as soon as this matter was raised in subsequent correspondence in November 2003, we implemented it and we continued to observe the discrepancies in principal component series and final results. The current manuscript ignores a refereed

exchange at Nature in which we specifically clarified (in response to a reviewers question) that we had obtained such results while using the exact stepwise procedure described in MBH98. Mann is aware of this refereed exchange.

The reason for the difference between our results and MBH98 results is primarily due to the fact that the tree ring principal component series in MBH98 cannot be replicated using a conventional principal components method. The MBH98 principal component series can only be replicated by standardizing on a short segment a procedure nowhere mentioned in MBH98 and only recently acknowledged in the SI to the Corrigendum of Mann et al. [Nature 2004] in response to our concerns on the subject expressed to Nature. In effect, MBH98 did not use a conventional centered PC calculation, but used an uncentered PC calculation on de-centered data. The impact of this method is the subject of ongoing controversy, which is well-known to the authors, but the existence of the method in MBH98 is no longer in doubt. In discussions of PC calculations in 2004 exchanged with the authors through Nature, we implemented the stepwise procedures of MBH98 referred to in the present manuscript and demonstrated that important differences remain even with stepwise procedures, as long as the uncentered and decentered methods of MBH98 are used. The differences in PC series resulting from using centered and uncentered series has been fully agreed to by all parties in the Nature exchange, although the parties continue to disagree on the ultimate effect on final NH temperature calculations. Accordingly, the discussion in Rutherford et al. [2004] is very incomplete and misleading in this respect. While we recognize that Mann et al. have argued that they can salvage MBH98-type results using alternative methodologies (e.g. increasing the number of PC series used in the 1400-1450 period), these salvage efforts are themselves a matter of controversy and do not validate the claims being put forward in the Rutherford et al. paper.

Accordingly we ask that this claim also be deleted from the manuscript.

Regards,
Stephen McIntyre and Ross McKittrick

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Attachment Converted: "c:\documents and settings\tim osborn\my documents\eurora\attach\Wahl_MBH_Recreation_JClimLett_Nov22.pdf"

References

1. <mailto:stephen.mcintyre@utoronto.ca>
2. <mailto:randall@atmos.colostate.edu>
3. <mailto:srutherford@rwu.edu>
4. <mailto:j.climate@atmosphysics.utoronto.ca>
5. <mailto:cindy@atmos.colostate.edu>
6. <mailto:rmckitri@uoguelph.ca>
7. <http://www.realclimate.org/>
8. <http://www.climate2003.com/file.issues.htm>
9. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: k.briffa@uea.ac.uk
Subject: Fwd: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Date: Tue, 4 Jan 2005 21:52:47 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, cddhr@giss.nasa.gov

Hi Keith - Happy new year. Hopefully, you had a good holiday. I've had a chance to read your section and hopefully you've had a chance to read what I sent just before the holidays. The purpose of this email is to help get a focus on the finish line (just a few days away) and to get a dialog going that will hopefully help you finish section 6.3.2.1. If you'd like to talk on the phone, just let me know.

Please see my email from right before xmas holidays for original comments. Plus, here are the new ones from both me and David Rind:

0) as leader of this KEY section, we need you to take the lead integrating everything you think should be integrated, editing and boiling it down to just ca 4 pages of final text (e.g., 8 pages of typed text plus figs). This means cutting some material (e.g., forcings and simulations) and perhaps moving glacier record (MUCH boiled down) to a box. See below.

00) note that we can also perhaps move some of the details to the appendix (although we won't write this until after the current ZOD crunch, save an outline of what you might want in there).

1) I like your figure ideas, with the comments:

1a) I don't think you need figure 1d - the SH recons are sketchy since not much data, and it might be better to just discuss in a sentence or three. Any space saved is good too. Not sure about your proposed 1e - have to see it, I guess.

1b) Figure 2 looks interesting. I'm trying to get the latest Arctic recon from Konrad Hughen - it is quite robust and a significant multi-proxy update. Should be published in time, though not sure thing since he's still hot on including his (our) AO recon which is more sketchy

1c) I think we can save space and improve organization if we DO NOT include Fig 3. However, this is open for debate - see David's comments below.

2) I agree with David's comments in general - so see them below. The prickly issue is where to put the forcings and simulated changes. I am close to having the prose from the radiation chapter, including the latest Lean and Co's view on solar - this will make many of the existing simulations involving inferred past solar forcing suspect (I will send in a day or so I hope). This means that we might be best saving space and downplaying this work some. I'm not sure, but wanted to debate it with you. Also, Chap 9 will have simulations in spades, so we can save space by letting them do it. Also, as David points out, we can focus on it elsewhere in our chapter more concisely - leaving you to focus on the VERY important obs record of temp and other changes. Can you tell, I'm still not 100% sure? I'll send another email to you and others about this in a bit.

3) Your section is too long and needs to be condensed. Thus, you need to think through what's most important and what's less so. For example, we need to figure out how to condense the glacier record of change. David thinks it should be a separate section that cuts across time scales (i.e., Holocene and last 2000 years). Perhaps we should try to make it into a box - 3 to 5 short paragraphs and a figure or two. Either way we have to really wack it. What do you think - you and I should be on the same page with Eystein before discussing w/ Olga perhaps. Or you can discuss with her - you're the lead on this section.

4) you're doing an impressive job! Lots to keep track of.

Next, here is what David has offered. Take it all with a grain of salt, but I have read it and he has many good points. On the structural or any other points, I'm happy to discuss on the phone, or you can just debate with him and me on email.

***** From David Rind 1/4/05 *****

6.3 Understanding Past Climate System Change (forcing and response)

6.3.1 Introduction (0.5 pages)

6.3.2 The Current Interglacial

6.3.2.1 Last 2000 years (4 pages)

Figure 1 should be of the last 2000 years, with appropriate caveats, not just since 1860 (which will undoubtedly be in other chapters).

pp. 8-18: The biggest problem with what appears here is in the handling of the greater variability found in some reconstructions, and the whole discussion of the 'hockey stick'. The tone is defensive, and worse, it both minimizes and avoids the problems. We should clearly say (e.g., page 12 middle paragraph) that there are substantial uncertainties that remain concerning the degree of variability - warming prior to 12K BP, and cooling during the LIA, due primarily to the use of paleo-indicators of uncertain applicability, and the lack of global (especially tropical) data. Attempting to avoid such statements will just cause more problems.

In addition, some of the comments are probably wrong - the warm-season bias (p.12) should if anything produce less variability, since warm seasons (at least in GCMs) feature smaller climate changes than cold seasons. The discussion of uncertainties in tree ring reconstructions should be direct, not referred to other references - it's important for this document. How the long-term growth is factored in/out should be mentioned as a prime problem. The lack of tropical data - a few corals prior to 1700 - has got to be discussed. The primary criticism of McIntyre and McKittrick, which has gotten a lot of play on the Internet, is that Mann et al. transformed each tree ring prior to calculating PCs by subtracting the 1902-1980 mean, rather than using the length of the full time series (e.g., 1400-1980), as is generally done. M&M claim that when they used that procedure with a red noise spectrum, it always resulted in a 'hockey stick'. Is this true? If so, it constitutes a devastating criticism of the approach; if not, it should be refuted. While IPCC cannot be expected to respond to every criticism a priori, this one has gotten such publicity it would be foolhardy to avoid it.

In addition, there are other valid criticisms to the PC approach. Assuming that the PC structure stays the same was acknowledged in the Mann et al paper as somewhat risky, given

the possibility of altered climate forcing (e.g., solar). Attempting to reconstruct tropical temperatures using high latitude PCs assumes that the PCs are influenced only by global scale processes. In a paper we now have in review in JGR, and in other papers already published, it is shown that high latitude climate changes can directly affect the local expression of the modes of variability (NAO in particular). So attempting to fill in data at other locations from PCs that could have local influences may not work well; at the least, it has large uncertainties associated with it.

The section from p.18-20 - simulations of temperature change over the last millennium , including regional expressions - should not be in this section. It is covered in the modeling section (several different times), and will undoubtedly be in other chapters as well. And the first paragraph on p. 19 is not right - only by using different forcings have models been able to get similar responses (which does not constitute good agreement). The discussion in the first paragraph of p. 20 is not right - the dynamic response is almost entirely in winter, which would not have affected the 'warm season bias' paleoreconstructions used to prove it. It also conflicts with ocean data (Gerard Bond, personal communication). Anyway, it's part of the section that should be dropped.

pp. 20-28: The glacial variations should be summarized in a coherent global picture. Variations as a function of time should be noted - not just lumped together between 1400 and 1850 - for example, it should be noted where glaciers advanced during the 17th century and retreated during the 19th century, for that is important in understanding possible causes for the Little Ice Age (as well as the validity of the 'hockey stick'). The discussion on the bottom of p.25-27 as to the causes of the variations is inappropriate and should be dropped - note if solar forcing is suspect, every paragraph that relates observed changes to solar forcing will be equally suspect (e.g., see also p. 44, first paragraph).

Bottom of p. 27: Greene et al. (GRL, 26, 1909-1912, 1999) did an analysis of 52 glaciated areas from 30-60N and found that the highest correlation between their ELA variations in the last 40 years was with summer season freezing height and winter season precip. The warm season freezing height was by far more important. Therefore, the relationship of glacier variations to NAO changes (which are important only in winter), as discussed in this paragraph, while perhaps valid for a period of time in southern Norway, is not generally applicable.

p. 34-36 on forcings: note that this is redundant to what is discussed in several later sections (e.g., 6.5.2); and other chapters), and that is true of forcing in general for the whole of section 6.2. I would strongly suggest dropping forcing from section 6.3.2.1, at least, and perhaps giving it its own number, or referring to othersubsections for it. It has a different flavor from the responses, and the section is already very big. Forcing does need to be discussed in the paleoclimate chapter, for reasons of climate sensitivity and explaining observations, but that is what Chapter 6.5 is about.

(In summary - 6.3.2.1 already is taking on one controversy - paleotemperatures, which is needs to do better, It should not have to deal with the forcing problems as well, and especially not in an off-handed way.)

Specific comments: p. 36: 6 ppm corresponds to a temperature response of 0.3 to 0.6°K using the IPCC sensitivity range.

p. 36, last paragraph: one could equally well conclude that the reconstructions are showing temperature changes that are too small. This is the essence of the problem with the last 2000 years: if the reconstructions are right, either there was no solar forcing, or climate sensitivity is very low. If the real world had more variability, either there was solar forcing, or climate sensitivity is high (as is internal variability). I've tried to say this in the climate sensitivity sub-chapter.

pp. 37-41: obviously a lot of overlap, but it shouldn't be hard to combine these.

p. 39, first paragraph: but can the models fully explain what is thought to have happened? Quantification is important here, because many of the same climate/veg models are being used to assess future changes in vegetation.

p. 42 - first full paragraph: what are the implications of the methane drop without a CO2 drop?

p. 43, middle paragraph: obviously should mention solar-orbital forcing in this paragraph.

p. 44, first paragraph: again, assuming a solar forcing

p. 45, first paragraph: overlap with pp. 20-28.

Second paragraph: overlap with p.39, last full paragraph

p. 52 - repeat of p. 43.

***** END From David Rind 1/4/05 *****

Thanks! Cheers, peck

--

Jonathan T. Overpeck
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Attachment Converted: "c:\eudora\attach\BriffaComments.doc"

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Fwd: Re: the Arctic paper and IPCC
Date: Wed, 5 Jan 2005 11:15:53 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith - great (!) to hear from you - hope you had a good holiday.
Your reward (ha) is the attached paper and comment below from Konrad.
He can supply data if needed for a synthetic figure, but we can add
this later once the Science paper he mentions (w/ us a co-authors
among millions, I assume) gets vetted more. Your call.

I'm still not convinced about the AO recon, and am worried about the
late 20th century "coolness" in the proxy recon that's not in the
instrumental, but it's a nice piece of work in any case.

Now, for all the issues you raise on other stuff in your email, I'll
address to you and that crowd.

thanks, Peck

>X-Sieve: CMU Sieve 2.2
>Date: Wed, 05 Jan 2005 10:53:56 -0500
>From: Konrad Hughen <khughen@whoi.edu>
>Organization: WHOI
>X-Accept-Language: en-us, en
>To: Jonathan Overpeck <jto@u.arizona.edu>
>Subject: Re: the Arctic paper and IPCC
>X-Virus-Status: No
>X-Virus-Scanned: by amavid-new at email.arizona.edu

>

>Hey Peck,

>

>Here's a pdf of a draft of Peter's methods paper. The figures will
>be what goes into the Science paper. I've sent the whole thing to
>help explain the figs, but let me know if you guys have questions.
>Also, I have a movie of reconstructed Arctic temp through time. Too
>big to attach but I'll try and get it to you somehow. Pretty cool.
>We're planning to include the movie and supplemental figs
>("robustness" tests, etc.) into the new website Matt's working on.

>

>Good to talk yesterday. I'll get a CV to you today.

>

>-Konrad

>

--

Jonathan T. Overpeck
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</x-flowed>

Attachment Converted: "c:\eudora\attach\ArcticOct16.pdf"

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: Fwd: Re: [Wg1-ar4-ch06] IPCC last 2000 years data

Date: Wed, 5 Jan 2005 12:24:47 -0700

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, cddhr@giss.nasa.gov, Fortunat Joos <joos@climate.unibe.ch>, joos <joos@climate.unibe.ch>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>

Hi Keith and Co - I think David likes a good debates, so the main thing is to consider his comments and respond appropriately. Although the first priority has to be on the ZOD text and display items, maybe you can go back over his comments AFTER the looming deadline and further discuss things with David and others. For now, just work away.

The biggest issue is how to handle forcing and simulations - i.e., where to put different pieces in the chapter. Eystein and I will help the team work through this. More soon, but for now just proceed as you have been proceeding. There is real merit to the concept that your section is about how climate varied over the last 2ka, and what caused these variations. The flip side is that we need to get a clear vision of how this differs from what goes into the other sections. Eystein and I will work more on this asap.

Your plan re: glaciers is good. That's a tough one, but it has to be boiled WAY down. Moreover, my gut is to focus on the extent to which these complicated natural archives (e.g., complicated by ppt change) support or do not support the other proxy evidence/conclusions. This is why I was thinking we might think about a box, and to include the Lonnie perspective in it - e.g., glaciers are now melting everywhere (almost - we know why they are not in those places) in a manner unprecedented in the last xxxx years. Make sense? See what Olga says, and if needbe, I can help focus that stuff more.

Thanks! Peck

>Hi Peck (et al)

>I am considering comments (including David's) re last 2000 years -

>some are valid = some are not . Will try to chop out bits but we

>need this consensus re the forcing and responses bit - I am for

>keeping the forcings in as much as they relate to the specific model

>runs done - and results for last 1000 years as I suspect that they

>will not be covered in the same way elsewhere . David makes couple

>good points - but extent to which forcings different (or

>implementation) perhaps need addressing here. The basic agreement I

>mean is that the recent warming is generally unprecedented in these

>simulations.

>It will take time and input from the tropical ice core /coral people

>to do the regional stuff well . I think the glaciological stuff is a

>real problem - other than just showing recent glacial states (also

>covered elsewhere) - of course difficult to interpret any past
>records without modelling responses (as in borehole data), but this
>requires considerable space . My executive decision would be to ask
>Olga to try to write a couple of paragraphs on limits of
>interpretation for inferring precisely timed global temperature
>changes? What do others think? I only heaved Olga's stuff in at
>last moment rather than not include it - but of course it needs
>considerable shortening. The discussion of tree-ring stuff is
>problematic because it requires papers to be published eg direct
>criticism of Esper et al. We surely do not want to waste space HERE
>going into this esoteric topic? All points on seasonality , I agree
>with , but the explicit stuff on M+M re hockey stick - where is
>this? ie the bit about normalisation base affecting redness in
>reconstructions - sounds nonsense to me ?
>
>I have to consider the comments in detail but am happy for hard
>direction re space and focus. If concensus is no forcings and model
>results here fine with me - Peck and Eystein to rule
>Keith
>

--

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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Parker, David (Met Office)" <david.parker@metoffice.gov.uk>, Neil Plummer <n.plummer@bom.gov.au>
Subject: RE: Fwd: Monthly CLIMATbulletins
Date: Thu Jan 6 08:54:58 2005
Cc: "Thomas C Peterson" <Thomas.C.Peterson@noaa.gov>

Neil,

Just to reiterate David's points, I'm hoping that IPCC will stick with 1961-90.

The issue of confusing users/media with new anomalies from a different base period is the key one in my mind. Arguments about the 1990s being better observed than the 1960s don't hold too much water with me.

There is some discussion of going to 1981-2000 to help the modelling chapters. If we do this it will be a bit of a bodge as it will be hard to do things properly for the surface temp and precip as we'd lose loads of stations with long records that would then have incomplete normals. If we do we will likely achieve it by rezeroing series and maps in an ad hoc way.

There won't be any move by IPCC to go for 1971-2000, as it won't help with satellite series or the models. 1981-2000 helps with MSU series and the much better Reanalyses and also globally-complete SST.

20 years (1981-2000) isn't 30 years, but the rationale for 30 years isn't that compelling. The original argument was for 35 years around 1900 because Bruckner found 35 cycles in some west Russian lakes (hence periods like 1881-1915). This went to 30 as it easier to compute.

Personally I don't want to change the base period till after I retire !

Cheers

Phil

At 09:22 05/01/2005, Parker, David (Met Office) wrote:

Neil

There is a preference in the atmospheric observations chapter of IPCC AR4 to stay with the 1961-1990 normals. This is partly because a change of normals confuses users, e.g. anomalies will seem less positive than before if we change to newer normals, so the impression of global warming will be muted. Also we may wish to wait till there are 30 years of satellite data, i.e until we can compute 1981-2010 normals, which will then be globally complete for some parameters like sea surface temperature.

Regards

David

On Tue, 2005-01-04 at 21:58, Neil Plummer wrote:

> Hi Hama, Tom

> (and David, Blair)
> Re: the issue of using the 1971-2000 normals in CLIMAT rather than
> 1961-1990 normals.
>
> Happy New Year!
> I have copied the relevant text from CCI XIII below, which provides
> reasons for staying with the 1961-90 standard.
> My initial recommendation is the same as Tom's, i.e. stay with the
> standard for now.
>
> I think there are two main factors to consider here - capability and
> demand. While there are clearly advantages with widespread use of
> normals derived using the later period there must be the capacity to
> do so.
>
> Perhaps in the lead-up to CCI-XIV, OPAG 2 can find out the extent of
> the support for the change among users of CLIMAT and OPAG 1 can find
> out more about capabilities. (Note, however, that this is not strictly
> on issue for OPAG 1 according to the ToRs for the ICT and any of the
> ETs. Happy to assist though).
>
> We may use the climate working groups in the Regional Associations to
> assist with surveying members capabilities and could do the same
> regarding the demand question though I think Tom's CCI/CLIVAR ET is
> best placed to give that guidance.
>
> *** David, Blair - Interested in your thoughts on this matter.
>
> Cheers
> Neil
> -----
> From CCI XIII ...
>
> 6.1.2 The Commission noted with satisfaction that
>
> the 19611990 Standard Normals were now complete
>
> and expressed its appreciation to NCDC for assembling
>
> the data as well as to those Members who had contributed
>
> data. It further noted that the 19611990
>
> Standard Normals would remain in use for global purposes
>
> until the next Standard Normals for the period

>
> 19912020 were completed.
>
> 6.1.3 The Commission noted that, in addition to the
>
> 1961 to 1990 WMO Standard Normals, many countries
>
> had produced climatic normals using the 1971 to 2000
>
> period. The Commission also noted the discussion held
>
> among Members on whether the standard 30-year normals
>
> should be accompanied by normals calculated over
>
> a more current period or a shorter period to reflect
>
> recent climate variability. The Commission noted the
>
> usefulness of periods other than the contiguous 30-year
>
> period for certain analyses below the global scale.
>
> However it decided to maintain the Climatological
>
> Standard Normals process, as it provided a common reference
>
> period for climate research and monitoring
>
> worldwide.
>
>
>
> Neil Plummer
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> Email n.plummer@bom.gov.au

>
>
>
>
>
>

> From: Thomas C Peterson [[1]mailto:Thomas.C.Peterson@noaa.gov]
> Sent: Tuesday, 4 January 2005 1:11 AM
> To: H Kontongomde
> Cc: Hans Teunissen; Neil Plummer
> Subject: Re: Fwd: Monthly CLIMATbulletins

>
>

> Thanks for responding, Hama. I agree with you on both
> points. I wonder how many countries produced 71-2000
> Normals? I'll cc Neil Plummer on this as the ET on Observing
> Requirements and Standards for Climate is under his
> leadership.

> Regards,
> Tom

>
>

> H Kontongomde wrote:

> > Dear Tom and Hans,

>

> > Happy New Year! I apologize for responding so late. I was on annual
> > leave since 13 December. The question of which "Normal" between
> > 1961-1990 and 1971-2000 is now frequently asked by many WMO Members.
> > Depending on the practical use of the normal, one of the two Normal can
> > be preferred to the other. However, the policy for CLIMAT messages is
> > to use the 1961-1990 Normals and until CCI change the standard, I would
> > also recommend that our colleagues of Turkey continue to use these 61-90
> > normals. This allows spatial comparisons for the entire globe, because,
> > not all countries have their 1971-2000 averages ready for use.

>

> > However, I think it is time that the CCI Expert Team on Observing
> > Requirements and Standards for Climate clarifies the problem in
> > explaining why the 61-90 Normals should continue to be the standard or
> > why it is time to change.

>

> > I will respond to our colleagues of Turkey.

>

> > Best regards,

>

> > Hama Kontongomde

>

>

> >>>> Hans Teunissen 1/3/2005 12:16:00 PM >>>>

> >>>>

> > Thanks for those suggestions, Tom. I'm not sure if your two questions
> > below were meant to be different (is a word 'change' missing from the
> > first?), but I think I get the gist from the answers. Re the CLIMAT code
> > official standards, I don't think Dick (or GCOS) is really the right
> > person to go to. That would be Hama, or, it seems, OSY (Sasha Karpov)
> > since they arranged the publication of TD-1188. Is that right, Hama? And
> > are you OK to use Tom's suggestion in the reply to Turkey?

> >

> > Hans.

> >

> >

> >>>> "Thomas C Peterson" <Thomas.C.Peterson@noaa.gov> 17.12.04 19:58:42

> >>>>

> >>>>

> > Dear Hans & Hama,

> >

> > As you may remember, I was just in Turkey in October interacting with
> > many people in their climate group. They have a pretty good team.

> >

> > The question as I understand it is not the reliability of their data
> > that are transmitted (e.g., for December 2004) but for the section of
> > the CLIMAT code which shows anomalies to a base period or what quintile
> > the precipitation falls in. Turkey indicates that they think their
> > 1971-2000 Normals are more reliable than their 1961-1990 Normals. I
> > would agree with them that they are probably correct in that. I believe
> > the same could be said about the US Normals.

> >

> > However, as I recall, not all countries redo their Normals every 10
> > years. Many only redo them every 30 years, which, I believe is the WMO
> > Standard. So for this WMO coded transmission (CLIMAT) I expect that
> > they specify the 1961-1990 Normals.

> >

> > 1. Would it make a difference in climate monitoring? Yes for those
> > users who make use of the anomaly values it could make a big difference.
> > More important, probably, than reliability is that the climate changes
> > over a decade and taking 1961-1970 out and substituting in 1991-2000 to
> > the base period calculation may make a big difference in some cases.

> >

> > 2. Would it make a difference in climate monitoring? Probably not as
> > most climate monitoring groups don't use the reported anomalies each
> > month but rather take the observations and use them with Normals they
> > already have in a different file.

> >

> > In sum, if my memory was correct on the coding, I would recommend that
> > they continue to use the official standard even if they have something
> > better out there because it has the potential for making a significant
> > difference and it is important that all groups follow the official
> > standard.

> >
> > Does this sound reasonable? I'm not an expert in the CLIMAT code, so
> > you might want to check with Dick about official standards for CLIMAT
> > before you answer.

> >
> > Regards,

> >
> > Tom

> >
> > Hans Teunissen wrote:
> > Hama: This one looks like it's definitely a concern for CCI/WCD. From
> > theGCOS side, it seems just an issue of what's to be in the GSN archive
> > -1971 to 2000 (reliable) or 1961 to 1990 (possibly unreliable). My
> > votewould be for the former, but I don't know what CCI policy would be.
> > Tom,do you agree re the GSN archive? (I see 6 stations for Turkey are
> > inthere now, some with very long records; not sure what implication
> > ofthis proposal really would be for those...are you?) Or would you
> > preferto try to salvage some of the older data there (at NCDC)? Could
> > you letus know? I then suggest that Hama respond for the WMO/CCI
> > 'system'. Doesthat sound OK? I'll be away from tomorrow until 3 January.
> > Best wishes for the Holidays and the New Year, Hans.

> > =====Dr.

> > Hans W. Teunissen
> > Tel:+41.22.730.8086Global Climate Observing System (GCOS) Fax:
> > +41.22.730.8052c/o World Meteorological Organization
> > E-mail:HTeunissen@wmo.int7 bis, Ave. de la PaixCP 2300, CH-1211
> > Geneva

> >
> 2Switzerland=====

> >
> >
> > Subject:
> > Fwd: Monthly CLIMATbulletinsFrom:
> > "Alexander Karpov" <AKarpov@wmo.int>Date:
> > Fri, 17 Dec 2004 11:52:43 +0100To:
> > "Hans Teunissen" <HTeunissen@wmo.int>
> > Dear Hans,As per attached query, I am kindly relying on your expertise
> > how to best navigate the solisitor.Best regards,Sasha *zden Dokuyucu
> > <odokuyucu@meteor.gov.tr> 17/12/04 08:58:21 >>> Dear
> > colleagues,First of all I want to say that, I find out your e-mail
> > addresses from the Web site of WMO. Please excuse me if this question

> > doesn't concern you. But if you know who concern this matter, could you
> > forward him/her this mail to get answer. I will be very glad if you pay
> > attention me. Thanks. We are a group of people who has been working in
> > the division of Climate Section, which is the sub department of
> > Agricultural Meteorology in Turkish State Meteorological Service. This
> > department is responsible for collecting all climate data from the
> > observing stations, recording and transmitting them via the
> > telecommunication system to the data collecting centre and archiving them
> > properly. This division is also responsible for transmitting monthly
> > CLIMAT bulletins to the WMO's relevant service. On behalf of Turkey, we
> > consider the climate data, which include the period of between 1971 and
> > 2000 years, are more trustworthy because of the development in
> > technological, telecommunicational and training fields. Our experiences
> > are supporting this situation. We want to ask you, does it any effect on
> > global monitoring system, if we use the period of years 1971-2000
> > instead of 1961-1990 in transmitting monthly CLIMAT REPORTS. We would be
> > very pleasure if you could get us more information. Yours Sincerely.
> > Ozden DOKUYUCU Engineer Agricultural Meteorology and Climatology Analysis
> > Department Turkish State Meteorological Service P.O. Box: 401 Ankara,
> > Turkey Telephone : +90-312-3022446 Fax
> > : +90-312-3612371 e-mail : odokuyucu@meteor.gov.tr
> > -- Thomas C. Peterson, Ph.D. Climate Analysis Branch National Climatic
> > Data Center 151 Patton Avenue Asheville, NC 28801 Voice:
> > +1-828-271-4287 Fax: +1-828-271-4328
> >

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UK

References

1. <mailto:Thomas.C.Peterson@noaa.gov>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: solomina@gol.ru
Subject: Fwd: Re: Fwd: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Date: Thu Jan 6 10:11:10 2005
Cc: jto@u.arizona.edu,Eystein Jansen <eystein.jansen@geo.uib.no>

Olga
am sending this to get you in this loop re the discussion for slimming down the 2000 year section Basically , IN THIS BIT - the decision is to reduce the glacier evidence to a very much smaller piece , coached in the sense of how the glacier evidence is problematic for interpreting precise and quantitative indications of the extent of regional or Hemispheric Warmth (and even cold) - issues of translating tongue position or volume into specific temperature and precipitation forcing . Hence , I am having to remove the stuff you sent and am asking if you could consider trying to write a brief section dealing with the issues I raise ? I also attach some initial comments by David Rind (on the full first draft of the chapter sent round by Eystein) for consideration Sorry about this - but presumable (as you suggested earlier) some of this can go in the 10K bit. You can shout at me (and the others) later!
cheers
Keith

X-Sender: jto@jto.inbox.email.arizona.edu
Date: Wed, 5 Jan 2005 12:24:47 -0700
To: Keith Briffa <k.briffa@uea.ac.uk>
From: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Fwd: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, cddhr@giss.nasa.gov,
Fortunat Joos <joos@climate.unibe.ch>, joos <joos@climate.unibe.ch>,
"Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

X-Virus-Scanned: by amavisd-new at email.arizona.edu
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean

Hi Keith and Co - I think David likes a good debates, so the main thing is to consider his comments and respond appropriately. Although the first priority has to be on the ZOD text and display items, maybe you can go back over his comments AFTER the looming deadline and further discuss things with David and others. For now, just work away. The biggest issue is how to handle forcing and simulations - i.e., where to put different pieces in the chapter. Eystein and I will help the team work through this. More soon, but for now just proceed as you have been proceeding. There is real merit to the concept that your section is about how climate varied over the last 2ka, and what caused these variations. The flip side is that we need to get a clear vision of how this differs from what goes into the other sections. Eystein and I will work more on this

asap.

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Hi Peck (et al)

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I have to consider the comments in detail but am happy for hard direction re space and focus. If concensus is no forcings and model results here fine with me - Peck and Eystein to rule

Keith

--

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References

1. <http://www.geo.arizona.edu/>
2. <http://www.ispe.arizona.edu/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Phil Jones <p.jones@uea.ac.uk>
To: Susan.Solomon@noaa.gov, Susan Solomon <Susan.Solomon@noaa.gov>, Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: Re: After the FOD
Date: Thu Jan 6 15:13:31 2005
Cc: martin.manning@noaa.gov

Susan,

Thanks for the quick reply. Kevin might have thoughts, but I'll give it some thought over the next few months. It isn't crucial till well after our second meeting.

Kevin can relay our thoughts on references next week, and we can come up with specific suggestions here if these need to be discussed with WG2 and WG3 before all the second lead author meetings. I know we can reduce our number of references with more work, but I suspect we will be requested at the time of the FOD and SOD (and maybe the ZOD) to consider many others. A lot of NMSs, University Depts. and Research Institutes measure success as seeing their work cited by IPCC ! I reviewed KNMI this time last year and they did exactly this. This shouldn't be a measure, but we will likely be under pressure to cite many more papers for this reason.

Cheers

Phil

At 13:58 06/01/2005, Susan Solomon wrote:

Phil,

Happy new year to you too. It's good to hear that your chapter is progressing well. I'll see Kevin next week at the AMS meeting and perhaps we can discuss its high points, along with the more basic issue of references, etc.

You've raised a number of concerns that are always an issue not only for IPCC but also for other assessments and even for our own individual key papers at times. But you have made no suggestions as to how to deal with them.

Could you please let me know if you have any suggestions to put forward?

Thanks,

Susan

>> Susan,

> Happy New Year !

> I'm working hard on the Chapter that Kevin has put sterling efforts on over
> the Christmas break. It'll be with you by Jan 14, hopefully earlier.

>
> I've been talking to Keith Briffa here and there is a lot of email
>traffic

> from the skeptics about the last 1K years. Also Senator Inhofe's speech
> from Jan 4 is doing the rounds.

>
> I know you've probably thought all this through, but there will be
> a number of key issues in AR4. Likely candidates that I'm aware of
> are the MSU issue (where we seem to be making some progress)
> and the last 1K years (where we might be but as this is about paleo
> it does take time).

>
> Well the issue is, once the FOD goes out to all -in say Sept/Oct 05 -

> what will stop the drafts getting onto web sites, in the media etc - and
> the whole thing blowing up then instead of being properly aired in 2007.
> I know we won't have an SPM, but those that want will say - they are
> only referring to papers that endorse their views and they are not
> referring to scientists with contrary ones. AR4 will get a bad press
> only half way through it's development.
>
> I know you will have phrases like 'draft only' and 'not for distribution'
> but can we really police this.
>
> Once the ZOD is in, Kevin and me will be sending you some ideas
> about referencing - formats, abbreviations, smaller fonts etc. We currently
> have about 3 times what we allowed for (7 pages of 70).
>
> Cheers
> Phil
>
>
>
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From: "olgasolomina" <olgasolomina@yandex.ru>
To: k.briffa@uea.ac.uk
Subject: IPCC glaciers
Date: Sun, 9 Jan 2005 10:02:19 +0300 (MSK)
Reply-to: olgasolomina@yandex.ru
Cc: jto@u.arizona.edu, eystein.jansen@geo.uib.no, Valerie.Masson@cea.fr,
ricardo@lab.cricyt.edu.ar

Hi Keith,

May I have your part of the text (2ka) to have a look, please. As far as I understand we decided to have glacier fluctuations separately in a frame. In this case, shall we keep glacier variations in the Holocene or we will extract it to place in this frame? I will contact Georg Kaser (ch 04) to see what they already have to comment on glacier/climate links. They must have treated this problem already. Besides it is more natural to consider it using the instrumental data. In this case we will deal with the paleo problem only, i.e. the dating of moraines, the erased traces of old advances, the use of lacustrine deposits to reconstruct the glacier erosion (size), the reconstruction of former ELAs, the sizes of retreated glacier etc. Shall we discuss the accumulation reconstructed from the ice cores or it will be just the problem of glacier front variations?

Another possibility is to have a common frame with the ch 04: How glaciers reflect climate and what they say about the climate in the Holocene (last 2ka).

I need the answers before I begin.

Please notice the change of my e-mail address. I will check both addresses a while, but have to move to a new one olgasolomina@yandex.ru

Regards,
olga

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: [Wg1-ar4-ch06] comments to 6.3.2.1 (mainly for Keith)
Date: Mon, 10 Jan 2005 14:40:27 -0700
Cc: Keith Briffa <k.briffa@uea.ac.uk>, cddhr@giss.nasa.gov, rahmstorf@pik-potsdam.de, joos <joos@climate.unibe.ch>

<x-flowed>

I agree; Keith should have the room, and section 6.5.8 should be compatible - has Fortunat followed the discussion between David/Stefan. Can you guys (David, Stefan, Keith, and Fortunat) ensure this?

Thanks, Peck

>Hi,
>interesting discussion on an important topic. If space is the
>limiting factor we may have to evaluate whether to cut back on less
>central issues elsewhere in the chapter. We will to a large extent be
>judged on how we tackle the hockey stick, sensitivity, unprecedented
>20th century warming issues in view of palaeo, and if a slight
>expansion is what it takes to do this properly, then I am
>sympathetic to that (without having heard Peck on the issue).

>Cheers,

>Eystein

>

>

>

>At 16:32 +0000 10-01-05, Keith Briffa wrote:

>>thanks David

>>have to say that it is very difficult to say much in the minimal
>>space - and we really need a page to discuss the problems in the
>>reconstruction and and interpretation of the various forcings in
>>different models - I am just going to put this down in an over
>>abbreviated way and ask for specific corrections for you and Stefan
>>et al. The detail perhaps depends on what the final Figure looks
>>like and Tim is trying to put it together but lots of weird and
>>interesting stuff / questions arise as we do - especially relating
>>to past estimates of solar irradiance used by different people. At

>>15:29 10/01/2005, David Rind wrote:

>>>(I tried to send this earlier and it got hung up; apologies if it
>>>eventually gets through and you get a second version.)

>>>

>>>Well, yes and no. If the mismatch between suggested forcing, model
>>>sensitivity, and suggested response for the LIA suggests the
>>>forcing is overestimated (in particular the solar forcing), then
>>>it makes an earlier warm period less likely, with little
>>>implication for future warming. If it suggests climate sensitivity
>>>is really much lower, then it says nothing about the earlier warm
>>>period (could still have been driven by solar forcing), but

>>>suggests future warming is overestimated. If however it implies
>>>the reconstructions are underestimating past climate changes, then
>>>it suggests the earlier warm period may well have been warmer than
>>>indicated (driven by variability, if nothing else) while
>>>suggesting future climate changes will be large.

>>>
>>>This is the essence of the problem.

>>>
>>>David

>>>
>>>
>>>
>>>
>>>
>>>

>>>At 9:28 AM +0000 1/10/05, Keith Briffa wrote:

>>>>THanks Stefan

>>>>At 21:13 07/01/2005, Stefan Rahmstorf wrote:

>>>>>Keith,

>>>>>

>>>>>some comments added in the text for the past millennium, plus I
>>>>>wrote some extra sentences on the implications of the dispute
>>>>>(repeated below).

>>>>>Hope it is useful,

>>>>>Stefan

>>>>>

>>>>>>Note that the major differences between the proxy
>>>>>>reconstructions and between the model simulations for the past
>>>>>>millennium occur for the cool periods in the 17th-19th
>>>>>>Centuries; none of these reconstructions or models suggests
>>>>>>that there was a warmer period than the late 20th Century in
>>>>>>the record.

>>>>>>

>>>>>>>A larger amplitude of preindustrial natural climate variability
>>>>>>>does not imply a smaller anthropogenic contribution to 20th
>>>>>>>Century warming (which is estimated from 20th Century data, see
>>>>>>>Chapter XXX on attribution), nor does it imply a smaller
>>>>>>>sensitivity of climate to CO₂, or a lesser projected warming
>>>>>>>for the future.

>>>>>

>>>>>

>>>>>--

>>>>>Stefan Rahmstorf

>>>>><<http://www.ozean-klima.de>>www.ozean-klima.de

>>>>>www.realclimate.org

>>>>>

>>>>>Wg1-ar4-ch06 mailing list

>>>>>Wg1-ar4-ch06@joss.ucar.edu

>>>>><http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>

>>>>>

>>>>>--

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Valerie.Masson@cea.fr

Subject: Re: Glaciers Ch 6

Date: Mon, 10 Jan 2005 17:20:06 -0700

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, trond.dokken@bjerknes.uib.no, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>

V - well said. Eystein and I will be working on your Holo section - more tomorrow. thx, Peck

>2 comments

>

>- the various NH T reconstr use polar records : to my knowledge only
>use of melt index that itself does not calibrate properly in Mann's
>reconstruction. I sent you Keith winter d18O from Vinther 2003 which
>provides a reconstruction of NAO changes (I think this is the more
>detailed calibration study for Greenland isotopes).

>On a decadal time scale calibration studies for Antarctica (Vostok
>and Law Dome, inland vs coastal sites) using available instr records
>(50 years) show correct decadal scale temperature signals. Even at
>places with subannual resolution like Law Dome I think that you
>cannot use the isotopes on a yearly basis but only decadal scale.

>

>- tropical glaciers : works conducted here on Andean ice cores
>together with modelling of isotopes in a GCM all showed a consistent
>decadal variability on the 20th century, most of which interpreted
>to be related to precip change (see for instance Hoffmann et al,
>Science, "Taking the pulse of the tropical water cycle", Science,
>2003). For more ancient past periods it is thought that part of the
>signal is due to T (and vertical lapse rate change), part to
>precip.I would not like to cosign any text claiming for a T
>reconstruction based on Andean ice cores.

>

>

>Keith Briffa wrote:

>

>>I agree with suggestion - there is the problem of the isotopic
>>analyses from tropical (and to some extent polar) ice cores still .
>>I am not happy simply to show these in a Figure relating to the
>>large-scale temperature changes - because we are not sure of the
>>extent to which they can be interpreted as such . The various NH
>>reconstructions use some polar isotope records but looking at plots

>>of the tropical records throws up some strange behavior over the
>>last 2000 years . I am not happy to write about these as Valerie
>>and Olga are better qualified and because I would like to see more
>>formal calibration against even short temperature records . I have
>>therefore , not as yet explicitly said anything about these
>>tropical records. I will sendthe latest text and latest draft
>>Figure 1 later today

>>

>>At 10:03 09/01/2005, Jansen@geo.uib.no wrote:

>>

>>>Dear Olga,

>>>My suggestion would be, and I believe this is echoed by Peck, is
>>>that the box

>>>we produce comes in the overall Holocene sub-chapter, thus to avoid
>>>repetition. The figure should mainly give syntheses of the glacier extent
>>>variations through the Holocene, if possible, or a fraction of it
>>>if data only

>>>exists e.g. for the last few millennia, for those regions where there is a
>>>reliable data set. Then with text explaining what we think drove these
>>>variations. I think it should be a box in Ch6, and could also include the
>>>recent trends I have just talked with Atle and he is able to contribute
>>>curves for Scandinavia and the Alps into a figure before the end of the week
>>>(in a couple of days). He feels putting something together for North America
>>>and perhaps New Zealand is feasible, but he cannot do this before the ZOD
>>>deadline. Perhaps you might be able? If we get something for the
>>>tropics from

>>>Lonnie and Ellen and what you have, I will be able to put this together in a
>>>figure for the box via assistance here. We can in such a figure leave space
>>>open for curves we anticipate including for the First Draft.

>>>It might be a good idea to in this figure also include the recent,
>>>instrumental evidence for the same regions, akin to what will be in Ch4, and
>>>of course, in the next iteration come back to possible joint Ch4
>>>and 6 figure.

>>>

>>>How does this sound?

>>>

>>>Cheers,

>>>Eystein

>>

>>

>>--

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>
>
>Attachment converted: Macintosh HD:masson 5.vcf (TEXT/txt) (000C2383)

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</x-flowed>

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Valerie.Masson@cea.fr, masson@dsm-mail.saclay.cea.fr
Subject: Re: Urgent - pls respond FAST
Date: Tue, 11 Jan 2005 11:57:13 +0100
Cc: Jonathan Overpeck <jto@u.arizona.edu>, Jean-Claude Duplessy <Jean-Claude.Duplessy@lsce.cnrs-gif.fr>, raynaud@lgge.obs.ujf-grenoble.fr, cddhr@giss.nasa.gov, rahmstorf@pik-potsdam.de, dolago@uonbi.ac.ke, Keith Briffa <k.briffa@uea.ac.uk>, Fortunat Joos <joos@climate.unibe.ch>

<x-flowed>

Valerie,

Thanks for putting together the chapter so well. I think it is quite comprehensive now. I have made a few changes in the enclosed document and also added a comment (pops up if you mark the yellow field).

I tend to like the questions, and think it highlights the relevance elements of the chapter. The missing references I have suggested, we can take care of in the final editorial process from our side.

As for figures one figure showing the evidence for Holocene warmt and the abrupt character of the 5-4ka cooling, perhaps with a low latitude data set that shows another evolution would be good to have, as you indicate, but we cannot bombard the chapter with wiggly lines, so the most characteristic examples would be best.

If you need high lat.ocean data I can provide, or perhaps NorthGrip 0-18 is best?

Cheers,

Eystein

Cheers,

Eystein

At 11:13 +0100 11-01-05, Valerie Masson-Delmotte wrote:

>Valerie Masson-Delmotte wrote:

>

>>I tried the question style for the Holocene
>>section... Any feedback would be appreciated
>>together with missing references (Fortunat).
>>Valerie.

>>

>>Jonathan Overpeck wrote:

>>

>>>Hi all leads and seconds of our Chap 6.5
>>>Synthesis sections. Fortunat came up with a
>>>interesting way to highlight what's important
>>>and why in his section 6.5.3, and Eystein and
>>>I would like feedback from you - particularly
>>>the leads - on whether this approach would
>>>work for each of your subsections.

>>>

>>>He used a question and answer style. If people
>>>do not like this then the question at the
>>>beginning of the paragraphs can of course be
>>>easily dropped and replaced by a statement.
>>>BUT, what do you say about using this
>>>convention throughout 6.5??? Note that some
>>>sections might have much more text per unit
>>>question.

>>>

>>>Please respond asap. Thanks, Peck and Eystein

>>

>>

>>

>

>

>

>Attachment converted: Sauvignon blanc:Holocene-VMD3.doc (WDBN/MSWD)
(004575F7)

>Attachment converted: Sauvignon blanc:masson 8.vcf (TEXT/ttxt)
(004575F8)

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Attachment Converted: "c:\eudora\attach\Holocene-VMD3_ej_com.doc"

From: Jonathan Overpeck <jto@u.arizona.edu>
To: derzhang@cma.gov.cn
Subject: Re: Re: [Wg1-ar4-ch06] URGENT - Deadline approaching
Date: Wed, 12 Jan 2005 10:21:10 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, r.ramesh@prl.ernet.in,
dolago@uonbi.ac.ke, Jean-Claude Duplessy <Jean-Claude.Duplessy@lsce.cnrs-
gif.fr>, Keith Briffa <k.briffa@uea.ac.uk>

Hi Prof. Zhang: thanks for your email and good to hear about your book. I will send the reference file to the LAs for them to incorporate as appropriate. You will also be editing the ZOD when it's complete, or of specific sections before then if you ask the appropriate leader of a section of interest (see previous listserv email with this list in case you don't remember from Italy).

Regarding 6.5.9, I will cc this to Dan and Ramesh so you can coordinate with them directly. This is the process we have adopted for all subsections so we don't waste time with the CLA's having to relay messages. Go direct...

I will also CC to Jean-Claude and Keith, so they make sure they have checked your input.

Many thanks, Peck

+@Dear Peck:
...

As regards Section 6.5.9 I shall do my utmost to help Den and Ramesh. But the assistance is to come only after I have read through their draft . Only in that way can I form an ideal "it must be relevant to policy makers" . I have been accustomed to write about scientific facts. Now I am confronted with a new problem how to serve the purpose of another style. Otherwise my suggestions would be of no use.

I had sent paragraphs to Jean-Claude for Section 6.2.2 and to Keith Briffa for section 6.3.2.1. last Nav..

With best wishes,

--

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: where I am !!!! !
Date: Wed Jan 12 14:01:35 2005

Eystein

in theory - it is supposed to be finished. I would just remove the two sections I suggested (or certainly move the regional simulation stuff into Ricardo's section. How does end note cope with references that are not published?

Keith

At 13:26 12/01/2005, you wrote:

Hi Keith,

I am in transit back to Bergen where there is a strong storm at present, but just a query to ask what you think a realistic time frame for your part. I will be reading through it on the way. If you have problems getting the references in, this is something we can help with, if you just write in text author name, year and paper, then we download from the ISI base and enter into End Note here, just to help you prioritising the text and figures.

Thanks for all your efforts. This is a critical part of the Chapter and the most complex and it seems to progress well, despite the strains.

Cheers,
Eystein

Basically, I need to send this to you to because there comes a point when I am just not able to read it objectively.

I would really like you both - and David and Stefan (I am ccing to them only) to look at it. Obviously it has grown too much, but the information in here is in my opinion all important.

I suggest removing the regional simulations stuff from the end (as David said earlier!) but feel this should be somewhere - also (sorry Eystein) perhaps the ocean section should go? I have dropped the proposed Figure 2 - after wasting a lot of time on it - there are too many problems with getting and understanding data - and then making any sensible conclusion on the basis of it. We really must have the two Figures left though - or some variants (these need borehole curves including and some way of indicating envelope of uncertainty around all reconstructions - perhaps as gray shading of different darkness depending on how many confidence limits overlap).

I would really appreciate a dispassionate look by all of you at the conclusions drawn after the the description of both Figures - in the light of the discussion we had about interpreting these Figures. I am really happy if you and David and Stefan (and Fortunat?) consider what is worth and not worth trying to say re the implications of these Figures, beyond the TAR. I can not tell if what I am saying is balanced (I know

Esper reconstruction is very hairy and ECHO-G run has much too great long-term variability - but no evidence PUBLISHED to support this - yet at least). Is what I say about the implications of the reconstructions banal?

I have been battling with teaching today and fucked up course scheduling by the administration that has outraged some students. Tomorrow I must take daughter back for new term in Cambridge - and now must work on proposal for Russian who leaves Thursday and needs to submit before then.

Do have a look and trim , cross reference as needed. The nightmare with these references continues also and I will have to get someone to help out here - incidentally our secretary has gone absent for a month . I will be back in hopefully by tomorrow afternoon . The conclusions (bullets?) should be very brief - but can not see them yet - suggestions welcome

I can try to do something for the methods but would rather you just told me exactly what is needed. I will then work on this Thursday and likely happy to accept what you say re this text. I know I have not contributed to the discussing on other sections - very frustrating - but must wait til after ZOD . Sorry

Keith

--

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References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.bjerknes.uib.no/mcts>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, Keith Briffa
<k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Eystein Jansen
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rahmstorf@pik-potsdam.de, cddhr@giss.nasa.gov
Subject: Urgent - FINAL review/edits of 6.5.8 Sensitivity
Date: Wed, 12 Jan 2005 16:55:36 -0700
Cc: raynaud@lgge.obs.ujf-grenoble.fr, Jean-Claude Duplessy <Jean-
Claude.Duplessy@lsce.cnrs-gif.fr>

Hi all on the list above... Some of you have received this already
straight from David, but
some other key people have not. Eystein and I would appreciate it very
much if you would
please read/comment/and edit the attached section 6.5.8 (Sensitivity)
NO LATER THAN
THURSDAY NOON, Eastern time (6PM GMT).

Please send responses to all on the address list ABOVE, plus Peck.

Thanks, Peck

X-Sieve: CMU Sieve 2.2
X-Sender: drind@4dmail.giss.nasa.gov
Date: Wed, 12 Jan 2005 13:29:53 -0500
To: joos <joos@climate.unibe.ch>
From: David Rind <drind@giss.nasa.gov>
Subject: Re: Fwd: Re: Fwd: 6.5.8 Sensitivity
Cc: David Rind <drind@giss.nasa.gov>,
Jonathan Overpeck <jto@u.arizona.edu>,
Dominique Raynaud <raynaud@lgge.obs.ujf-grenoble.fr>,
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Jean-Claude Duplessy <Jean-Claude.Duplessy@lsce.cnrs-gif.fr>,
rahmstorf@pik-potsdam.de, cddhr@giss.nasa.gov
X-Virus-Scanned: amavisd-new at email.arizona.edu
X-Spam-Status: No, hits=-2.272 required=7 tests=BAYES_00,
HTML_20_30,
HTML_MESSAGE, MIME_SUSPECT_NAME
X-Spam-Level:

Dear Fortunat (and others),

Here is the revised section 6.5.8. I've put in most of your changes
(and also most of
those suggested by Stefan, particularly with regards to clarifying
the sign of the
radiative forcing). Most importantly, I've removed the table - I
agree it seems to imply
a solidity that is really not there. The one thing I have not done
is condense it
greatly (of course!). The real reason for going into such detail,
rather than just

saying, "well, the forcing and response are uncertain, so we can't conclude anything",
is I think it's important to show that paleoclimate scientists have gone to some effort
to try to deduce climate sensitivity from the paleorecord, the parameter that is
probably of most interest to IPCC. In that respect the details are important, as are the
magnitudes of uncertainty represented in the different studies. Obviously, at any point
in the proceedings the section can be shortened, but I thought it useful to start with
this level of quantification, and show paleoclimate has this similarity with the rest of
IPCC in addition to more qualitative concepts.

I've responded to your individual comments below.

At 6:15 PM +0100 1/11/05, joos wrote:

Dear David,
Here my comments on the updated climate sensitivity section. Please apologize if I formulate my comments straight away, but I need to leave
very soon. Many of my comments might have to do with presentation. Your main conclusions in paragraph f are fine.
My view is that it would be ideal to address the issue from a probabilistic view point. this is of course not always possible.
1) Maunder Minimum section:
Several studies using Monte Carlo approaches show that almost any climate sensitivity is possible when taking into account
uncertainties
in radiative forcing input data as well as observational records over
the 20 century as constraints. See the Paris report for more information.
The uncertainty does not only arise from indirect aerosol effect,
but
also form the whole range of forcing agents that all have an uncertainty
attached. E.g. Reto Knutti did some evaluation of his results where
he
assumed that the aerosol forcing is exactly know (No error) -> even
then
climate sensitivity remains unconstrained. Clearly, uncertainty is growing when going further back in time than the last century as
done
here. Then, the numbers provided in the table are useless, as you
now
state in the last sentence of the revised text.
2) Other sections:
I think similar concerns also hold for the other sections. For
example,

the LGM global cooling is very uncertain. I have just heard yesterday a

talk by Ralph Schneider who showed how different SST reconstructions (Alkenone, Cd/Ca, MAT, radiolare etc) disagree. global SST cooling might be anywhere between 0 and 4 K or so. Of course, CLIMAP and the recent GLAMAP update provide a reasonable estimate. However, the point is that uncertainties are huge.

The table is a very focused and stand alone thing for the reader. It gives the impression that climate sensitivity for different period can be well evaluated. However, this is not the case.

3) My conclusion:
- The table should be dropped. I have quite a strong feeling here, as it seems to me that the number in the table are very hard to defend and should not be made prominent.

The table and reference to it has been dropped.

- The whole section should be condensed considerably. Your main conclusions in paragraph f are fine.

Well, removing the table will shorten this section!

Further comments:

1) section d) 1. para: solar forcing reduction estimate range up to 0.65% for MM e.g. Reid, 97 and Bard et al.

Correction made, and reference added (and I also corrected the numbers as Stefan suggested, although the upper number is actually larger given the Reid estimate).

2) section d, last para equilibrium
The statement that transient effects are not important is very hard to defend:

2a) The warming and forcing up to today is considered. Certainly, we are now far from equilibrium (a lag of 30 years or so).

2b) the volcanic forcing is very pulse like and I do not see how the equilibrium concept holds here. It can only be evaluated in a transient way.

3c) The MM is probably not in equilibrium climate, as solar forcing has likely varied over the MM as indicated by radiocarbon, although sunspots

were not present

I've removed the word "transient" but I have justified the equilibrium aspect of the sentence with a reference (we investigated that issue by running from 1500 through the Maunder Minimum, and seeing what the prior changes in solar forcing did to the Maunder Minimum cooling - the effect, as noted in the reference, was small in our model).

3) section b) end of 1. para: How should such a 'general climate sensitivity' be defined?

For now I've simply suggested what should also be factored in; I don't know that it's our place to come up with a new definition per se, although if IPCC is interested, we could try!

4), section c) Somewhat a mix of model and observations. end of 2 para:

It is not clear which forcing was operating in these different models (at least it is not stated in the text) and hence one can not directly imply a climate sensitivity in the way done here. For this the forcing that went into the model simulations must be known.

I looked at each of the references and saw what forcing they actually used - they were all very similar except for one which used current orbital parameters (not really important). This comment is now included.

Hope this is useful and looking foreward to further debate the issue.

Thanks for the comments!

David

ps - Jonathan, the attached Endnote library includes the references we discussed yesterday, as well as all the ones relevant for this section.

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Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences
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Attachment Converted: "c:\eudora\attach\newest_6.5_2.8.doc" Attachment
Converted:
"c:\eudora\attach\IPPC_2007_1_Rind_Copy"

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk
Subject: methods - section 6.2.2
Date: Wed, 12 Jan 2005 22:57:53 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, joos <joos@climate.unibe.ch>, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>

<x-flowed>

Hi all: Keith and Tim asked for specific requests in terms of what you could do for section 6.2.2. I'm hoping Valerie and Fortunat have already made enough progress that they can ask, but here's my take:

1. you have lots of methodology material in your 6.3.2.1, and this is good. It would be good to refer to this from the earlier, more general 6.2.2
2. the goal of 6.2.2 is to give the reader more confidence in paleo and to get them to read on with confidence that what they read will be of use
3. I suspect that the format V and F will be working around will be one that can first highlight chronological issues (that we can date some proxies very well, and that's what we focus on in this chapter primarily). It would be good to have the usual comforting comments about tree rings and other annual proxies.
4. The, it would be good to have the basics on how proxies reflect climate, and how we know we understand the relationship. That it is useful even if the proxy is responding to things other than climate. Seasonality, etc. Include brief overview of calibration, verification. you know the drill.
5. keep it short and not too detailed. Use lots of references - including to the most recent stuff.
6. I'm sure we'll end up modifying/improving later after we figure out what to do with the appendix
7. Need to work fast, very fast, but hopefully V and F have made real progress already.

Thanks!! Peck

--

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk
Subject: Comments on 6.3.2.1
Date: Wed, 12 Jan 2005 22:58:01 -0700
Cc: rahmstorf@pik-potsdam.de, drind@giss.nasa.gov, Eystein Jansen <eystein.jansen@geo.uib.no>, joos <joos@climate.unibe.ch>

<x-flowed>

Keith, Tim (and friends- please read below and provide your comments THURS too) - just finished reading your draft and my primary reaction is one of great relief and admiration. You've done an excellent job. I'm sure things will look different in the end, but for the ZOD, this lays things out just fine.

That said, here are comments. More are in the attached draft w/ track changes

1. still need to see the figs - ok to state what still has to be done (as you have)
2. regarding the ocean section, I think some of it should stay in - both as a placeholder for other relevant stuff, and because it is important. See attached. It would be good if EYSTEIN would look at my comments for this section and provide the needed minor help - we need the punchline/bullet - how does the 20th century compare with the previous part of the record (you say it shows the warming, but then don't go the next step.
3. THIS IS THE ONLY COMMENT THAT WILL TAKE MORE THAN A FEW MINUTES - can we get THE word on the MWP in before hydro? Heck, I'd even support a small (smaller than the other ones) box. There is lots of debate about the MWP,. and we need to weigh in. Was it global, hemispheric, regional only (e.g., Europe and N. Atlantic - can then refer back to it in ocean section)? Was it one synchronous warm event or a bunch of shorter regionally asynchronous events? Warmer than 20th? Late 20th? (think you answered this, but need to nail it!). Cite the cast of papers you've already discussed, plus Bradley et al Science 03.
4. what you say is balanced, and it's ok to note in the text where you anticipate serious improvement w/ more published paper support - e.g., Esper (you're doing a paper on this, no?) and ECHO-G.
5. have to have boreholes on Figs too - that would be more important now than uncertainty estimates around all recons - the latter is harder, but in any case, say what you intend to add after ZOD.
6. see text - minor edits
7. I can make draft bullets from what you sent

Guys - it was worth the wait. Hope you can take advantage of the relatively minor edits required and help some with other sections as asked for.

--

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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Peck your comments...
Date: Thu, 13 Jan 2005 09:53:07 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith -

- 1) ok on the refs - send tomorrow
- 2) glad you're keen for the box - it can't be too long - maybe shot for ca. 400 words? After the ZOD is done, I'm sure we can tune to the correct balance of info. A fig is ok if it's compelling. The box will either be 6.1 or 6.2 depending on whether you refer to it in your section before or after the glacier box. I'm guessing it'll be 6.1 and come first, but it's your call. Think of a title for the box - something like "Box 6.1: The Medieval Warm Period" or maybe something more catchy. Can't be too glib.
- 3) glad you have some borehole in there. Of course, you'll be at the front of the line for dealing with the grief we get no matter what choice we make. So the key is to go with what can be best justified. Your section has this nice balance already.

Thanks for getting Tim (and you as time permits) to work on those other sections - VERY important too. But, your section is the most important.

thx, Peck

>...are really welcome. Am now incorporating them , plus doing some
>editorial bits - though will wait on Eystein to send replacement
>ocean bit . Having to get one of my people to do the references but
>not likely these will arrive til tomorrow. The main point to discuss
>is your comment on the MWP . I like the idea of a box. This IS
>sufficiently important to warrant it - in the context that most
>people say "it was warm/warmer than now then so disproves anthro
>effect - we should address this explicitly. I will have a go - but
>need to know how many words and Figure(s) allowed. We can simply
>just refer to this box in a couple of places in existing text. Sorry
>about Figures - now got some (2) borehole lines in (but may need
>more - reluctant to use Huang and Pollack original though because
>obviously much too cold on basis of simple regional averaging

>biases. Will send latest version (without box on MWP) tonight my
>time.
>Keith
>
>--
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--

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</x-flowed>

From: David Rind <drind@giss.nasa.gov>
To: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
Subject: Re: 6.5.8 revisions
Date: Thu, 13 Jan 2005 17:00:26 -0500
Cc: David Rind <drind@giss.nasa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, FortunatJoos@email.arizona.edu

Here are my responses to Stefan's comments. While I could have made each of these points in the document itself, it is already sufficiently long that Jonathan had me cut it before most of you guys saw it.

At 8:53 PM +0100 1/13/05, Stefan Rahmstorf wrote:

Hi folks,
on the topic of climate sensitivity. I just lost a long mail on it due to a software crash, so sorry if I'm brief now.

I think it makes no sense for the purpose of the IPCC to discuss a climate sensitivity to orbital forcing - if such a thing can be defined at all. The first-order idea of orbital forcing is that in annual global mean it is almost zero - and in any case the large effect orbital forcing has on climate has very little to do with its global mean value. Hence, we'll confuse people by discussing it in this way, and even citing numbers for it. For the purpose of IPCC, I think climate sensitvity should refer to climate sensitivity wrt. greenhouse gases.

The point here is that climate can be forced by other factors than simply a global, annual average radiation change, which is the metric now being used. The orbital forcing induced changes are wonderful examples of this, hence the paleoclimate chapter is a perfect place to discuss it. Variations in seasonal and latitudinal forcing clearly have had a major impact on climate, including forcing of ice ages, yet the annual average radiative change is small. The importance of this with respect to IPCC is that other climate forcings can also affect the seasonal and latitudinal distribution of radiation - aerosols, land surface changes, and even solar radiation (considering cloud cover distributions) - hence they too may have a disproportionate influence compared to their annual global average magnitude.

What is said in this subsection is simply that this one metric clearly fails with respect to the major variations in paleoclimate, and as a general rule, there should be room for an expanded concept (which may then have utility for current and future climate forcing as well).

Also, it is questionable to discuss climate sensitivity for uncoupled models, especially for glacial times - Ganopolski et al. (Nature 1998) have shown that glacial climate looks very different with mixed layer ocean vs. coupled. I think for a 2007 IPCC report we shouldn't be discussing old uncoupled runs when coupled model results are available. (And it is a little odd that the above paper, the first coupled model simulation of glacial climate, cited over 150 times so far, is ignored here in the discussion of the last glacial maximum - if you do a search on the Google Scholar engine for the key words "Last Glacial Maximum", you'll find it's the second-most cited paper on this topic after the Petit et al. Vostok data paper.)

In fact, most if not all of climate sensitivity measurements have been done for what Stefan calls "uncoupled models", atmospheric models coupled to mixed layer ocean models. The results from all prior IPCC reports give sensitivities from precisely these types of models - for the basic reason that almost no one has ever run a coupled model for 2CO2 to equilibrium. The other disadvantage of coupled models in this regard is that their control run, if simulated long enough, often does not reproduce the current climate in important respects - one is then getting a climate sensitivity with respect to something far removed from the current climate, so what good is it? The fact that models coupled to a dynamic ocean and those coupled to mixed layer oceans may get different responses - and one can see from the numbers that the responses are actually fairly similar in general - can be related to the ocean dynamics changes; as the text notes, that is considered a feedback in this subsection, and therefore an appropriate part of the climate sensitivity calculation.

I still think it makes no sense to say that climate sensitivity depends on the sign of

the forcing. Talking about greenhouse gases: whether you will do an experiment going from 280 ppm to 300 ppm, or the other way round from 300 ppm to 280 ppm, should give you the same climate sensitivity. Perhaps you mean that going from 280 to 300 will give a different result compared to going from 280 to 260, but then you're really comparing different mean climates. I think this "directionality" of climate sensitivity is not a good concept.

It's not the forcing per se that's the issue here, it's the feedbacks that potentially can alter the climate sensitivity to the sign of the forcing.

It has been suggested in the past that climate sensitivity is larger to cooling perturbations than to warming ones, and we ourselves have found that result in some earlier model runs. The standard reason given is that with a cooling climate perturbation, sea ice can expand further equatorward, to cover a broader area, and intersect more solar radiation - therefore providing a more positive feedback to the cooling. In a warming climate, the sea ice retreats and intersects less radiation - but the sunlight-weighted area is smaller in the regions it is retreating to, so its positive feedback to the warming is not as large.

However - water vapor works the opposite way. Given the exponential dependence of water vapor on temperature, in a warming climate the added temperature would allow for a greater water vapor change (increase) than would occur with a cooling climate of the same magnitude. Hence the water vapor feedback should be greater in a warming climate.

So the answer is - nobody knows. Jim Hansen did a survey of people at GISS recently to see what the general opinion was for a paper he's working on (and sending around). Since paleoclimates have suffered both positive and negative forcings (in the examples given in this section), and since we don't know the answer to this question, we can't really say whether the sign of the forcing is important or not. So I've left it as an open question, with the possibility that it might matter.

Relating forcing to response, the sensitivity from the models is then on the order of $0.6^{\circ}\text{C}/\text{Wm}^{-2}$ (or higher, depending on the model used); the sensitivity from the observations, if taken at face value, would be considerably less.

I still don't understand how you get this conclusion. This would mean: if you take models with those estimated forcings and run them, they should show a big mismatch with the proxy data. As far as I can tell from the diagram by Mike Mann attached, combining models and data, only the Von Storch simulation (not shown on this one) does show such a mismatch. (And that uses 1.5 times the Lean solar forcing.)

If you look at the various model simulations done for this time period, the only way the models can reproduce the "observed" cooling relative to the present is by using only a subset of the forcings. When you use all the forcings, you get a much higher number. You can do the math yourself: with a "best-guess" radiative forcing change of 2.4Wm^{-2} , models with a sensitivity of $0.6^{\circ}\text{C}/\text{Wm}^{-2}$ will get a temperature change of some 1.5°C , which over the course of 300 years shows up in GCMs. For example: Cubasch et al (1997), using just solar forcing in the ECHAM 3 model came up with cooling of 0.5°C ; if you add a reasonable response to the approximately $1.5\text{--}2\text{ W/m}^2$ forcing from trace gases plus aerosols, you get an additional 1°C cooling (given the sensitivity stated above). Counteracting that could be land surface changes - but counteracting that are undoubtedly the reduced pre-industrial tropospheric ozone, plus any additional volcanic cooling (a la Crowley). So assuming those sort of cancel, we have a 1.5°C cooling for the MM time period from solar plus anthropogenic, similar to what we get in the GISS model (as noted in our 2004 paper). That can be compared with the Mann et al reconstruction - and you can see from your figure that for the 1700 time period relative to the 1990s, the cooling is about 0.5°C . Similarly, Fischer-Bruns et al. (2002) with the ECHAM 4 model, using solar forcing of -0.1% for the MM, and volcanic forcing greater than today (like Crowley) got a cooling of 1.2°C . The Zorita et al study also got a large magnitude cooling when using all the forcings. BTW, neither ECHAM 3 nor ECHAM 4 has a large climate sensitivity - it is of the order of

0.6C/Wm-2, as referred to in the comment above. Note that none of these models are shown in your accompanying figure, and all are GCM studies.

How did the Crowley and Bauer studies that are shown in the figure (using EB or EMIC models) get the smaller cooling magnitudes indicated there? Only by using a subset of the forcings - Crowley basically threw out the solar changes (and had a lower sensitivity model), Bauer et al. used a large aerosol effect and still needed a large deforestation warming to bring her results in line with the Mann et al. reconstruction (in fact, it was done specifically for that reason). None of these runs used the tropospheric ozone reduction that we have evidence did occur. My impression is that these studies took the observations as given and were asking the question of what forcings would be needed to reproduce them. That is an interesting question, but it obviously does not validate the observations.

The specific comment you refer to above relates to the discussion in the previous paragraphs, which detail the radiative forcings and all the different model responses. It is a fair representation of the current status, however unsettling that is. But in the current incarnation of this subsection, we do not use it to imply a low climate sensitivity - we simply say that given the uncertainties in forcing and response, we cannot use this time period to better understand climate sensitivity. And I think that's accurate.

David

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From: Keith Briffa <k.briffa@uea.ac.uk>

To: jto@u.arizona.edu,David Rind <drind@giss.nasa.gov>, joos@climate.unibe.ch,Eystein Jansen <eystein.jansen@geo.uib.no>

Subject: near final 6.3.2.1

Date: Thu Jan 13 19:03:36 2005

Guys

here is the latest draft of 6.3.2.1 (only waiting on slight edits on ocean bit from Eystein and ENDNOTE reffs to be sorted. Have agreed with Peck and Eystein to do a Medieval Warm Box tomorrow and insert a sentence or two on lack of info for SH .Figures of course need work -

particularly sorting out how to represent uncertainty around all reconstructions in Fig 1 and represent totality ion Fig 2d. Also some forcing data still missing - may have to wait til after ZOD (will also need to put in other borehole curve(s) but data not to hand).

Having virus troubles with by email (and our system randomly blocking some files) - sorry so don't know whether David has seen this at all (re his comments on Figures - which are now embedded as GIFs and attached separately as 2 files in case go wrong again.

As I type just got Stefan's message and comments and Goose paper- will look at tonight and incorporate tomorrow.

David - I know it is received wisdom that volcanos only force climate for 1 to 2 years - but in our SOAP transient models this is not the case where several large eruptions occur (co- incidentally often in sunspot minima periods - see the actual magnitude of radiative forcing in Figure 2 (and these effects are directly transmitted as continually propagating coolings in ocean in HADCM3 and ECHO-G for up to decades i believe. Anyway - I am happy with your conclusions and agree that these are not "negative". I would rather just pick a cool period and not label it as MM (or late MM) as this is a solar definition as such should be defined according to solar proxy data (and hence choice of shorter period seems unsupported). If you just say a date range without the label , I think it avoids the issue.

Sorry for garbled writing but rushing - I like your bit (in case this did not come across) thanks all for now

Keith

--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: comments on Briffa, last millennium
Date: Thu, 13 Jan 2005 19:15:25 +0100
Cc: Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>

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Dear Keith,

you've done a great job on the touchy subject of the last millennium, which is central to our whole chapter.

My comments to that are threefold:

(1) If you could shorten the text somewhat, it could become more powerful

(2) Some small edits & comments are in the attached doc

(3) I propose some improvements to the figures as follows.

- Fig 1a the land temps seem to go off plot, temperature scale needs to be extended

- we need a break between panels a and the rest, since it's a different time scale on the x axis

- Fig 1c also has one curve going off the top

- Panels 1b-d might run the time axis up to 2010 or so, else the important rise at the end is hidden in the tick-marks and less obvious than it should be

- the legends need to say what the baseline period (zero line of y-axis) is (hard to find this in the axis label)

- this baseline should be the same for all curves, i.e. 1961-1990. Fig 2d says 1901-1960 - it's not ideal to have a different one, as compared to Fig 1. Also, is it true? Surely the Storch curve is not shown relative to this baseline, it's way above it. Aligning it like this could lead to the dangerous misunderstanding that Storch suggests a much warmer medieval time compared to everyone else, which of course is not the case.

I hope this helps.

Cheers, Stefan

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Stefan Rahmstorf
www.ozean-klima.de
www.realclimate.org

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Attachment Converted: "c:\eudora\attach\Briffa_ed_sr .doc"

Attachment Converted: "c:\eudora\attach\goosse_et_al_2005.pdf"

From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
To: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Box 6.1: The Medieval Warm Period
Date: Thu, 13 Jan 2005 19:47:04 +0100
Cc: t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, drind@giss.nasa.gov, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>, joos <joos@climate.unibe.ch>

<x-flowed>
Hi friends,

good idea for a box. Just want to make sure you're aware of the attached paper by Goosse et al., which may be helpful in illustrating what we all know, but what here is shown in a citeable way: local climate variations are dominated by internal variability (redistribution of heat), only very large scale averages can be expected to reflect the global forcings (GHG, solar) over the past millennium.

Stefan

--
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</x-flowed>

Attachment Converted: "c:\eudora\attach\goosse_et_al_20051.pdf"

From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
To: David Rind <drind@giss.nasa.gov>
Subject: Re: 6.5.8 revisions
Date: Thu, 13 Jan 2005 20:53:13 +0100
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck
<jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen
<eystein.jansen@geo.uib.no>, FortunatJoos@email.arizona.edu

Hi folks,
on the topic of climate sensitivity. I just lost a long mail on it due
to a software crash,
so sorry if I'm brief now.

I think it makes no sense for the purpose of the IPCC to discuss a
climate sensitivity to
orbital forcing - if such a thing can be defined at all. The first-
order idea of orbital
forcing is that in annual global mean it is almost zero - and in any
case the large effect
orbital forcing has on climate has very little to do with its global
mean value. Hence,
we'll confuse people by discussing it in this way, and even citing
numbers for it. For the
purpose of IPCC, I think climate sensitivity should refer to climate
sensitivity wrt.
greenhouse gases.

Also, it is questionable to discuss climate sensitivity for uncoupled
models, especially
for glacial times - Ganopolski et al. (Nature 1998) have shown that
glacial climate looks
very different with mixed layer ocean vs. coupled. I think for a 2007
IPCC report we
shouldn't be discussing old uncoupled runs when coupled model results
are available. (And
it is a little odd that the above paper, the first coupled model
simulation of glacial
climate, cited over 150 times so far, is ignored here in the
discussion of the last glacial
maximum - if you do a search on the Google Scholar engine for the key
words "Last Glacial
Maximum", you'll find it's the second-most cited paper on this topic
after the Petit et al.
Vostok data paper.)

I still think it makes no sense to say that climate sensitivity
depends on the sign of the
forcing. Talking about greenhouse gases: whether you will do an
experiment going from 280
ppm to 300 ppm, or the other way round from 300 ppm to 280 ppm, should
give you the same
climate sensitivity. Perhaps you mean that going from 280 to 300 will
give a different
result compared to going from 280 to 260, but then you're really
comparing different mean
climates. I think this "directionality" of climate sensitivity is not
a good concept.

Relating forcing to response, the sensitivity from the models is then on the order of $0.6 \text{ }^{\circ}\text{C} / \text{Wm}^{-2}$ (or higher, depending on the model used); the sensitivity from the observations, if taken at face value, would be considerably less.

I still don't understand how you get this conclusion. This would mean: if you take models with those estimated forcings and run them, they should show a big mismatch with the proxy data. As far as I can tell from the diagram by Mike Mann attached, combining models and data, only the Von Storch simulation (not shown on this one) does show such a mismatch.

(And that uses 1.5 times the Lean solar forcing.)

Stefan

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Stefan Rahmstorf

[1]www.ozean-klima.de

[2]www.realclimate.org

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eutora\attach\millennium.jpg"

References

1. <http://www.ozean-klima.de/>
2. <http://www.realclimate.org/>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

Subject: the new "warm period myths" box

Date: Thu, 13 Jan 2005 21:45:38 -0700

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>

<x-flowed>

Hi Keith and Tim - since you're off the 6.2.2 hook until Eystein hangs you back up on it, you have more time to focus on that new Box. In reading Valerie's Holocene section, I get the sense that I'm not the only one who would like to deal a mortal blow to the misuse of supposed warm period terms and myths in the literature. The sceptics and uninformed love to cite these periods as natural analogs for current warming too - pure rubbish.

So, pls DO try hard to follow up on my advice provided in previous email. No need to go into details on any but the MWP, but good to mention the others in the same dismissive effort. "Holocene Thermal Maximum" is another one that should only be used with care, and with the explicit knowledge that it was a time-transgressive event totally unlike the recent global warming.

Thanks for doing this on - if you have a cool figure idea, include it.

Best, peck

--

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Professor, Department of Atmospheric Sciences

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</x-flowed>

From: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
To: David Rind <drind@giss.nasa.gov>
Subject: Re: 6.5.8 revisions
Date: Fri, 14 Jan 2005 12:20:47 +0100
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck
<jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen
<eystein.jansen@geo.uib.no>, FortunatJoos@email.arizona.edu

Hi David,
thanks for the detailed response. I'll try to be brief.
On the orbital forcing you write:

The point here is that climate can be forced by other factors than simply a global, annual average radiation change, which is the metric now being used.

I think we all agree on this point. My concern is only about how to present it in the section. I think that giving a climate sensitivity wrt. global mean orbital forcing is confusing to the uninitiated, e.g. your statement in the section:

This high climate sensitivity ($2\text{C}/\text{Wm}^{-2}$) is occurring in an atmospheric model (ECHAM-1) whose sensitivity to doubled CO_2 is about $0.6\text{C}/\text{Wm}^{-2}$.

I really think we should not give a number like $2\text{C}/\text{Wm}^{-2}$ as "climate sensitivity" to global-mean orbital forcing and contrast it to that to doubled CO_2 . It gives out the message to people that climate sensitivity is all over the place and ill defined. That's not the case. Climate sensitivity is a well-defined concept for a globally uniform forcing like CO_2 forcing, but nobody expects any clear relation between the global mean part of orbital forcing and the climate response.

On uncoupled models:
I agree that for $2\times\text{CO}_2$ runs, you will get very similar climate sensitivity with uncoupled and coupled models, because there is no large change in ocean heat transport between equilibrium $1\times$ and $2\times$ CO_2 states (as confirmed by doing this in coupled models). The mixed layer boundary condition used in the uncoupled models simply assumes a fixed, prescribed ocean heat transport, which turns out to be a valid approximation in this case.

My concern was and is specific to the discussion for LGM climate, where this is not a valid approximation, as we know both from proxy data and from model results that ocean circulation and heat transport was very likely quite different in the LGM compared to

today. In our Nature 98 LGM simulation, we get 50% difference in the response of the

Northern Hemisphere mean temperature, between the uncoupled "mixed layer" experiment and

the one that includes the ocean model. 50% is a first-order difference, and hence I think

that all the evidence we have today, points to the "constant heat transport" approximation

breaking down when applied to the LGM. The IPCC report should not draw conclusions about

climate sensitivity from LGM experiments that have made this approximation, as I think

those would be hard to defend. I must say I'm starting to get a little concerned about the

chapter discussing 1980s papers for no other apparent reason than them being authored by

Rind, while leaving out important more recent, widely recognised advances in the field.

I attach the Schneider et al. paper I announced earlier, submitted to Science today and

arguable the most comprehensive study on deriving climate sensitivity from LGM data

constraints that has been done so far.

On the directionality of the climate sensitivity:

of course I understand the reasons, the ice feedback and water vapor feedback etc., I've

written about those myself in the past - again this is only a difference in how best to

present the same, undisputed facts. You make the argument that when going to a colder

climate, sensitivity is different from when going to a warmer climate. That is undisputed.

But that in my view has nothing to do with the "direction" of the experiment, but with the

fact that sensitivity in a colder climate is different from sensitivity in a warmer

climate. I explained with the ppm example because I thought that's simple. A

"directionality" would be, if going from 280 to 300 ppm would give a different equilibrium

response compared to going from 300 to 280. But that's not what you're talking about. Your

talking about going from 280 to 260 (say), as compared to going from 280 to 300. That of

course gives different results, because the difference 280-260 applies to a colder climate

than the difference 300-280 (no matter in which "direction" you derive this).

Stefan

--

Stefan Rahmstorf

[1]www.ozean-klima.de

[2]www.realclimate.org

Attachment Converted: "c:\eudora\attach\schneider_jan05.pdf"

References

1. <http://www.ozean-klima.de/>
2. <http://www.realclimate.org/>

From: David Rind <drind@giss.nasa.gov>
To: Stefan Rahmstorf <rahmstorf@pik-potsdam.de>
Subject: Re: 6.5.8 on climate sensitivity and last millennium
Date: Fri, 14 Jan 2005 14:23:47 -0500
Cc: David Rind <drind@giss.nasa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, Fortunat Joos@email.arizona.edu

Here are my responses to the comments concerning 6.5.8d. With respect to Stefan's main concern: I too am sensitive to the possible mis-use of words that appear in a cavalier manner in the text. I think the way to avoid that is to be as precise as possible about what is being said. I also feel that hand-waving should be minimized - just because there are uncertainties, does not mean IPCC will throw up its hands. Thus the attempt to quantify these numbers are precisely as they will be done in other IPCC chapters. Again, the responses are in red, and the text alterations (or in this case, some entire text) are in blue.

I'm not working on this topic myself so I'm by no means an expert. But I am still quite concerned with the wording in 6.5.8 on the last millennium. First, to avoid misunderstandings, I would like to suggest again to describe forcings and climate changes going forward in time, rather than going backwards in time. Even colleagues here that I discuss it with misunderstand the present version with backwards reasoning - it leads to phrases like "deforestation warming" (used by David in his last mail), although deforestation caused cooling - backwards in time you can see this as a warming, but should you call it "afforestation warming" if you look back in time? I suggest to use the physical, forwards, time arrow in the discussion.

In all the other sections of 6.5.8 we discuss the temperature change and the radiative forcing relative to the present - when it was colder than the present, the temperatures were indicated to be colder, and the radiative forcing more negative. To alter that for this section alone would cause added confusion. I have therefore in each case tried to make it perfectly clear what is being said. In particular, I agree that in the case of deforestation the terminology does become confusing so the text has been changed to be more

communicative; it now reads,

Warming of 0.35°C due to the existence of vegetation and forests that have since been cut down was found by (Bauer et al., 2003) ...

The section states:

If one takes mid-range estimates of solar and anthropogenic forcings, and assumes that volcanic, tropospheric ozone and land albedo changes cancel out, the resulting radiative forcing change is $\sim -2.4 \text{ Wm}^{-2}$.

I don't think we should give a "mid-range" of the forcing like this; the assumption that ozone, land albedo and volcanic changes cancel is hard to justify in any case. For the forcing we need to give a range in my opinion, not one number. If we give a range, it will become clear that the forcing is too uncertain for drawing conclusions on climate sensitivity from this time period.

The problem with giving a maximum range for this time period is the same as giving one for the 20th century - the inclusion of the potential indirect effects of aerosols means you can wipe out all climate forcing entirely. It becomes a 'reductio ad absurdum'. The issue in particular for the Late Maunder Minimum time period, and the specific reason for including it, is that it potentially says something about SOLAR forcing.

In writing this section, we are not simply doing a core dump of everything people have done, we are supposed to use our brains to assess the likely situation. Having already provided the range of uncertainty, we can give a 'best estimate' for the various forcings that we can use in a meaningful way if we are careful - and which show the importance of the uncertainty in the solar forcing. I do agree that what existed in the text especially for the third paragraph needed improvement. Therefore, after several talks with people here, I've altered (especially) the first and third paragraphs accordingly. Rather than just stating the conclusion that climate sensitivity can't be well defined, the paragraphs now show quantitatively that is the case. The specificity, I believe, gives people a real

feeling for the uncertainties, and in the way it is done here, especially the uncertainty in the solar forcing and actual climate response. (This rewrite obviates the need for a direct response to several of Stefan's other comments.)

(d) Last 1000 years

We concentrate here on the Late Maunder Minimum time period in which sunspots were generally missing (approximately 1675-1715), but outside of the estimated solar irradiance change, the discussion is applicable for the pre-industrial climate in general. The primary forcings relative to today are (1) a decrease in various greenhouse gases, with a forcing of approximately $-2.4 \pm 0.25 \text{ Wm}^{-2}$ (not including tropospheric ozone changes); (2) reduced tropospheric sulfate aerosols, whose direct effect is estimated by IPCC (2001) as $+0.4 \pm 0.3 \text{ Wm}^{-2}$ with an indirect effect ranging from $+0.5$ to $+2 \text{ Wm}^{-2}$ (3) a solar forcing reduction estimated as ranging from -0.12 to -1.56 Wm^{-2} (0.05% to 0.65%) ((Hoyt and Schatten, 1993); (Lean, 2000); (Foukal and Milano, 2001); (Reid, 1997)); and (4) volcanic aerosol forcing either similar to today ((Robertson, 2001)), lower than today ((Robock and Free, 1996)), or higher ((Crowley, 2000)). Large uncertainties therefore exist for all of the forcings except the trace gas values (again excluding tropospheric ozone). The cooling effects are offset to small degree by land albedo changes, estimated to contribute $+0.4 \text{ Wm}^{-2}$ ((Hansen et al., 1998)). Reduced tropospheric ozone has been estimated to cause an additional forcing of -0.3 to -0.8 Wm^{-2} (Mickley et al., 2001), while increased stratospheric ozone produced a positive forcing of -0.09 to -0.25 Wm^{-2} (IPCC, 2001). If one takes the most widely used or mid-range estimates of solar (-0.5 Wm^{-2} from (Lean, 2000)) and anthropogenic forcings (-2.4 Wm^{-2} from reduced trace gases, other than tropospheric ozone; $+0.5 \text{ Wm}^{-2}$ from reduced sulfate aerosols), land albedo changes (0.4 Wm^{-2}), decreased tropospheric ozone (-0.35 Wm^{-2} (IPCC, 2001)) and increased stratospheric ozone ($+0.15 \text{ Wm}^{-2}$ (IPCC, 2001)), the net radiative forcing for this time is estimated as -2.2 Wm^{-2} . [For this exercise we ignore the effects of volcanoes, the indirect effects of sulfate aerosols, and the effects of carbon and organic aerosols.]. Including these additional components (except

for volcanic aerosols for which even the sign of the change is not well known), Hansen (personal communication) calculates a value close to -2 Wm^{-2} . How cold was this time period? Different reconstructions (Fig. X1) provide different estimates of cooling, ranging from -0.45°C ((Mann et al., 1999), annual value for the Northern Hemisphere), to about -0.7°C ((Esper et al., 2002) for 20-90°N in the growing season, and (Briffa and Osborne, 2002) (from borehole temperature records). Model studies (Fig. X2) for this time period have generally employed significant solar reductions (-0.2% to -0.4%), which by themselves have resulted in cooling of about -0.5°C ((Cubasch et al., 1997); (Bauer et al., 2003); (Rind et al., 2004)). Utilizing a forcing of -1.5 to -2 Wm^{-2} from the combined influence of preindustrial trace gases and aerosols results in additional cooling of about -1 to -1.5°C ((Fischer-Bruns et al., 2002); (Rind et al., 2004); (Zorita et al., 2004)). If volcanic aerosols were actually more extensive during this time period, then additional cooling would arise from this factor as well (on the order of -0.4°C found by (Hegerl et al., 2003) using the (Crowley, 2000) reconstruction). Warming of 0.35°C due to the existence of vegetation and forests that have since been cut down was found by (Bauer et al., 2003), on the same order but of opposite sign to the tropospheric ozone forcing (Mickley et al., 2004). Adding these effects from model simulations together produces a total cooling on the order of -1 to -1.5°C or greater, significantly larger than any of the paleo-estimates. For the ~ 50 year time period associated with the Maunder Minimum, without large forcing trends, the model results are essentially in radiative balance, and while the influence of past solar variations could still be in acting, in at least one study they were shown to be unimportant (Rind et al., 2004).

The climate sensitivity from the GCMs used for these studies is on the order of $0.6^\circ\text{C}/\text{Wm}^{-2}$ (or higher, depending on the model used). To calculate the sensitivity from the observations, we first use the estimated forcing of -2.2 Wm^{-2} and recognize that $\sim 0.85 \text{ Wm}^{-2}$ of this is unresolved (Hansen, personal communication) due to the rapid trace gas changes of the last few decades. Therefore, only 1.35 Wm^{-2} of the radiative forcing should have

been expressed in the system. Were this to have resulted in a temperature change of about -0.5°C (as in the Mann et al reconstruction), it would imply a climate sensitivity of 0.37 Wm^{-2} , i.e. at the low end of the IPCC range for doubled CO_2 response. Using the higher estimated cooling of -0.7°C results in a climate sensitivity of 0.52 Wm^{-2} . Alternatively, if the uncertain solar forcing change was at the estimated minimal value (-0.12 Wm^{-2}), then the radiative forcing change would be reduced accordingly, and climate sensitivity for the two reconstructions increases to 0.5 Wm^{-2} and 0.7 Wm^{-2} (near 3°C for doubled CO_2) respectively, for the different temperature reconstructions. This exercise can be carried on ad infinitum; considering the actual uncertainty in many of the forcings, and in the actual temperature response of the climate system, we conclude that we cannot properly constrain climate sensitivity for this time period (and to some extent the results are similar for other preindustrial time periods compared to the present).

As an aside: if one uses the minimal estimate of solar forcing in the example presented, one gets a range of temperature response to $2\times\text{CO}_2$ of $2-3^{\circ}\text{C}$, not too much different from that concluded in the paper Stefan just sent around (which was 2.5 to 3°C).

Then you state the Mann et al. data are 0.5°C below the 1990s in the Maunder Minimum. I can see they are 0.4°C below the reference level (I believe this is 1961-1990). The mean of the 1990s is 0.3°C above this level (I calculated this from the Jones data) - so I find that the Mann data are in fact 0.7°C below the 1990s in the MM. The difference between model expectation for 2.4 W/m^2 and the actual found in the Mann data is almost gone then. Add to that the possibility that the Mann data may somewhat underestimate the variability, and I do not see any significant discrepancy between models and data, which we should mention and which we could defend as real - even for "best guess" sensitivity and forcing, let alone considering the uncertainty in those.

The easiest way to see this is to note that the Mann et al reconstruction has the late 1600s slightly warmer than the late 1800s. It is widely acknowledged that the late 1800s

were 0.6C colder than today (taking into account the heat island effect) (and the radiative forcings, a la IPCC 2001, are all with respect to the 1990s.) That puts the late 1600s at less than 0.6C colder, close to the value indicated in the text.

David

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//

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//

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, oyvind.paasche@bjerknes.uib.no

Subject: Keith's box

Date: Mon, 17 Jan 2005 11:16:32 -0700

<x-flowed>

Hi all - attached is Keith's MWP box w/ my edits. It reads just great
- much like a big hammer. Nice job.

Please insert after Eystein has had his say. thx, Peck

--

Jonathan T. Overpeck

Director, Institute for the Study of Planet Earth

Professor, Department of Geosciences

Professor, Department of Atmospheric Sciences

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Attachment Converted: "c:\eudora\attach\MWP-KRBJto.doc"

From: Malcolm Hughes <mhughes@ltrr.arizona.edu>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fwd: Your concerns with 2004GL021750 McIntyre
Date: Fri, 21 Jan 2005 10:47:40 -0700
Cc: Tom Wigley <wigley@cgd.ucar.edu>, rbradley@geo.umass.edu,
t.osborn@uea.ac.uk, wigley@ucar.edu, phil Jones <p.jones@uea.ac.uk>,
keith Briffa <k.briffa@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>

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Michael E. Mann wrote:

> Hi Malcolm,

>

> This assumes that the editor/s in question would act in good faith.

> I'm not convinced of this.

>

> I don't believe a response in GRL is warranted in any case. The MM
> claims in question are debunked in other papers that are in press and
> in review elsewhere. I'm not sure that GRL can be seen as an honest
> broker in these debates anymore, and it is probably best to do an end
> run around GRL now where possible. They have published far too many
> deeply flawed contrarian papers in the past year or so. There is no
> possible excuse for them publishing all 3 Douglass papers and the Soon
> et al paper. These were all pure crap.

>

> There appears to be a more fundamental problem w/ GRL now,
> unfortunately...

>

> Mike

>

> At 08:47 PM 1/20/2005, mhughes@ltrr.arizona.edu wrote:

>

>> Mike - I found this sentence in the reply from the GRL

>> Editor-in-Chief to be

>> interesting:

>> "As this manuscript was not written as a Comment, but rather as

>> a full-up scientific manuscript, you would not in general be asked to

>> look it over."

>> Does it not then follow that if you were to challenge their "work" in

>> a "full-

>> up scientific manuscript", but not as a "Comment" it, too, should be

>> reviewed

>> without reference to MM?

>> Maybe the editor-in-chief should be asked if this is the case, or
simply

>> challenged by a submission?

>> Cheers, Malcolm

>> Quoting "Michael E. Mann" <mann@virginia.edu>:

>>

>> >

>> >

>> > Thanks Tom,

>> >

>> >

>> > Yeah, basically this is just a heads up to people that something
>> might be
>> > up here. What a shame that would be. It's one thing to lose "Climate
>> > Research". We can't afford to lose GRL. I think it would be
>> > useful if people begin to record their experiences w/ both Saiers
and
>> > potentially Mackwell (I don't know him--he would seem to be
>> complicit w/
>> > what is going on here).
>> >
>> >
>> > If there is a clear body of evidence that something is amiss, it
>> could be
>> > taken through the proper channels. I don't that the entire AGU
>> hierarchy
>> > has yet been compromised!
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>> > The GRL article simply parrots the rejected Nature comment--little
>> > substantial difference that I can see at all.
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>> >
>> > Will keep you all posted of any relevant developments,
>> >
>> >
>> > mike
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>> >
>> > At 04:30 PM 1/20/2005, Tom Wigley wrote:
>> >
>> > Mike,
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>> > This is truly awful. GRL has gone downhill rapidly in recent years.
>> > I
>> >
>> > think the decline began before Saiers. I have had some unhelpful
>> >
>> > dealings with him recently with regard to a paper Sarah and I have
>> >
>> > on glaciers -- it was well received by the referees, and so is in
>> > the
>> >
>> > publication pipeline. However, I got the impression that Saiers was
>> >
>> > trying to keep it from being published.
>> >
>> >
>> > Proving bad behavior here is very difficult. If you think that
>> > Saiers
>> >
>> > is in the greenhouse skeptics camp, then, if we can find
>> > documentary

>> >
>> > evidence of this, we could go through official AGU channels to get
>> >
>> > him ousted. Even this would be difficult.
>> >
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>> > How different is the GRL paper from the Nature paper? Did the
>> >
>> > authors counter any of the criticisms? My experience with Douglass
>> >
>> > is that the identical (bar format changes) paper to one previously
>> >
>> > rejected was submitted to GRL.
>> >
>> >
>> > Tom.
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>> > =====
>> >
>> >
>> > Michael E. Mann wrote:
>> >
>> > Dear All,
>> >
>> >
>> > Just a heads up. Apparently, the contrarians now have an
>> > "in" with GRL. This guy Saiers has a prior connection w/ the
>> > University of Virginia Dept. of Environmental Sciences that causes
me
>> > some unease.
>> >
>> >
>> > I think we now know how the various Douglass et al papers w/
>> Michaels and
>> > Singer, the Soon et al paper, and now this one have gotten published
in
>> > GRL,
>> >
>> >
>> > Mike
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>> >
>> >
>> > Subject: Your concerns with
>> > 2004GL021750 McIntyre
>> >
>> > Date: Thu, 20 Jan 2005 14:42:12 -0600
>> >
>> > X-MS-Has-Attach:
>> >
>> > X-MS-TNEF-Correlator:
>> >
>> > Thread-Topic: Your concerns with 2004GL021750 McIntyre

>> >
>> > Thread-Index: AcT/MITTfwM54m4OS32mJvW4BluE+A==
>> >
>> > From: "Mackwell, Stephen"
>> > <mackwell@lpi.usra.edu>
>> >
>> > To:
>> > <mann@virginia.edu>
>> >
>> > Cc: <cjr@egs.uct.ac.za>,
>> > <james.saiers@yale.edu>
>> >
>> > X-OriginalArrivalTime: 20 Jan 2005 20:42:12.0740 (UTC)
>> > FILETIME=[84F55440:01C4FF30]
>> >
>> > X-UVA-Virus-Scanned: by amavisd-new at fork7.mail.virginia.edu
>> >
>> > X-MIME-Autoconverted: from base64 to 8bit by
>> multiproxy.evsc.Virginia.EDU
>> > id j0KKgLO11138
>> >
>> >
>> > Dear Prof. Mann
>> >
>> > In your recent email to Chris Reason, you laid out your concerns
that I
>> > presume were the reason for your phone call to me last week. I have
>> > reviewed the manuscript by McIntyre, as well as the reviews. The
editor
>> > in this case was Prof. James Saiers. He did note initially that the
>> > manuscript did challenge published work, and so felt the need for an
>> > extensive and thorough review. For that reason, he requested
>> > reviews from
>> > 3 knowledgeable scientists. All three reviews recommended
>> > publication.
>> >
>> > While I do agree that this manuscript does challenge (somewhat
>> > aggressively) some of your past work, I do not feel that it takes a
>> > particularly harsh tone. On the other hand, I can understand your
>> > reaction. As this manuscript was not written as a Comment, but
>> > rather as
>> > a full-up scientific manuscript, you would not in general be asked
to
>> > look it over. And I am satisfied by the credentials of the
reviewers.
>> > Thus, I do not feel that we have sufficient reason to interfere in
the
>> > timely publication of this work.
>> >
>> > However, you are perfectly in your rights to write a Comment, in
which
>> > you challenge the authors' arguments and assertions. Should you
>> > elect to
>> > do this, your Comment would be provided to them and they would be

>> offered
>> > the chance to write a Reply. Both Comment and Reply would then be
>> > reviewed and published together (if they survived the review
process).

>> > Comments are limited to the equivalent of 2 journal pages.

>> >

>> > Regards

>> >

>> > Steve Mackwell

>> >

>> > Editor in Chief, GRL

>> >

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>> > Professor Michael E. Mann

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>

Hi Mike - of course we shouldn't make that assumption. If the issues are being dealt with elsewhere in the peer-reviewed literature soon (in time for IPCC to be aware of them) then there would be no reason for a riposte in GRL. Even so, it might be worth putting the hypothetical case to the Editor-in-Chief to test his response. Cheers, Malcolm
</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: FOIA
Date: Fri Jan 21 15:20:06 2005
Cc: Ben Santer <santer1@llnl.gov>

Tom,

I'll look at what you've said over the weekend re CCSP.

I don't know the other panel members. I've not heard any more about it since agreeing a week ago.

As for FOIA Sarah isn't technically employed by UEA and she will likely be paid by Manchester Metropolitan University.

I wouldn't worry about the code. If FOIA does ever get used by anyone, there is also IPR to consider as well.

Data is covered by all the agreements we sign with people, so I will be hiding behind them. I'll be passing any requests onto the person at UEA who has been given a post to deal with them.

Cheers

Phil

At 14:35 21/01/2005, Tom Wigley wrote:

Phil,

Thanks for the quick reply.

The leaflet appeared so general, but it was prepared by UEA so they may have simplified things. From their wording, computer code would be covered by the FOIA. My concern was if Sarah is/was still employed by UEA. I guess she could claim that she had only written one tenth of the code and release every tenth line.

Sorry I won't see you, but I will not come up to Norwich until Monday.

Let me fill you in a bit (confidentially). You probably know the panel members. We were concerned that the chair would be a strong person. It is Jerry Mahlman -- about the best possible choice. Richard Smith is the statistician -- also excellent. Dave Randall, too -- very good. As token skeptic there is Dick Lindzen -- but at least he is a smart guy and he does listen. He may raise his paper with Gianitsis that purports to show low climate sensitivity from volcanoes. I will attach our paper that proves otherwise, in press in JGR.

Preparing the report has been a good and bad experience. I think I had the worst task with the Exec. Summ. -- it tied up most of my time for the past 3 months. The good has been the positive

interactions between most of the people -- a really excellent bunch. I have been very impressed by Carl Mears and John Lanzante. At meetings, John Christy has been quite good -- and there were good and positive interactions between John and Roy and the RSS gang that helped clarify a lot. Outside the meeting, in the email world, he has been more of a pain. He has made a lot of useful suggestions for the ExSumm -- but he keeps accusing the AOGCMers of faking their models (not quite as bluntly as this). In the emails there are some very useful exchanges from Jerry Meehl, Ramaswamy and Ben detailing the AOGCM development process. We will be writing a BAMS article on this in the summer -- much of what happens in model development is unknown to the rest of the community. The 'faking' idea prompted me to write a tongue in cheek note -- also attached. As far as I know, John will not raise this particular issue in his dissentin views.

To accommodate dissenting views, the report will have a "dissenters' appendix", with responses. You will get this at some stage -- the deadline for dissenters to produce is Jan 31, and we will not finish our rebuttals until mid Feb. The dissenters are John C, and (far worse) Roger Pielke Sr. All of the rest of us disagree with these persons' dissenting views. Roger has been extremely difficult -- but the details are too complex to put in an email. On the other hand he has made a number of useful contributions to the ExSumm and other chapters. Suffice to say that he has some strange ideas (often to do with the effects of landuse change) that are interesting but still, in my view, speculative -- but testable.

We have yet to see the dissents -- and it would not be ethical for me to say any more than I have already.

Best wishes,

Tom.

Phil Jones wrote:

Tom,

I hope the VTT panel doesn't prove a meeting too many at this time. It is currently scheduled for Feb 23-25 and I only get back from an 8 day workshop in Pune on Feb 20.

The IPCC Chapter with Kevin is now with WGI in Boulder. We did put you down as one of our potential reviewers. Don't know whether you'll have time or whether WGI will select you - regional balance etc.

Next week I'll be in Reading and Exeter, so

won't be in CRU. Have to be at an RMS Awards meeting then something on Reanalysis, then I have to collect some data from the archives in Exeter for a small project we have. It is easier for me to get this than explain to someone how to do it. So I'll miss you - not back till Thursday night.

On the FOI Act there is a little leaflet we have all been sent. It doesn't really clarify what we might have to do re programs or data. Like all things in Britain we will only find out when the first person or organization asks. I wouldn't tell anybody about the FOI Act in Britain. I don't think UEA really knows what's involved.

As you're no longer an employee I would use this argument if anything comes along. I think it is supposed to mainly apply to issues of personal information - references for jobs etc.

Sorry I'll miss you next week. If you're in on Sunday perhaps you could come round to our new house in Wicklewood. Phone number is still the same as 01953 605643. Keith and Sarah know where it is even if they did get lost the first time they came.

Cheers

Phil

At 02:59 21/01/2005, you wrote:

Phil,
Tom Karl told me you will be on the VTT review panel. This is very good news.

Unfortunately I will not be at the meeting on the 23rd -- I will be in midair half way across the Pacific to spend a couple of weeks in Adelaide.

I got a brochure on the FOI Act from UEA. Does this mean that, if someone asks for a computer program we have to give it out?? Can you check this for me (and Sarah).

I will be at CRU next Mon, Tue, Wed in case Sarah did not tell you.

Thanks,

Tom.

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From: "PJ Valdes, Geographical Sciences" <P.J.Valdes@bristol.ac.uk>
To: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: EU
Date: Fri, 21 Jan 2005 17:21:02 -0000
Cc: oyvind.paasche@geo.uib.no

<x-flowed>

Keith,

It is purely a matter of resources, and since Simon will be doing the millennial stuff with the Hadley model within IMPRINT, and I think that probably my resources will be best focussed in some of the other work packages. But it is possible and I will try to do it if the opportunity arises.

Cheers

Paul

--On 21 January 2005 17:12 +0000 Keith Briffa <k.briffa@uea.ac.uk> wrote:

> Great Paul

> but I still do not see , if we do get funded, why you can not do some
> runs (in keeping with the wider hemisphere isotope records) that fit with
> your wishes within IMPRINT.

>

> At 15:16 21/01/2005, PJ Valdes, Geographical Sciences wrote:

>> Keith and Eystein,

>>

>> Thanks for your comments. Without modelling MILLENNIUM is a very much
>> weaker project. I admit that I am attracted to doing something with them
>> because I have wanted to get more involved in the last 1000 years, and
>> it would be a good opportunity to run our new isotope enabled version
>> of the Hadley model.

>>

>> However, IMPRINT is a much stronger project overall and and I also
>> prefer the broader range of timescales offered by IMPRINT (although
>> whether we have ended up being too broad is another issue). Given this
>> and the other things discussed, I will decline the offer from Danny

>> Carroll

>>

>> Best Wishes

>> Paul

>>

>> --On 20 January 2005 22:24 +0100 Eystein Jansen

>> <eystein.jansen@geo.uib.no> wrote:

>>

>>> Hi Keith and Paul,

>>>

>>> I think Millennium might be a problem, but if the project does not
>>> employ a hierarchy of models and have a comprehensive modelling
>>> component it is hard to see how it fits the work program of the call.
>>> We discussed this kind of situation in one of our first meetings and
>>> agreed that we on an institutional basis should not be involved in
>>> competing projects, and I think we need to re-emphasise this agreement
>>> in our London meeting. I also gave Valerie the same opinion as some of
>>> the people in her lab had been asked to join the McCarroll proposal
>>> This said, it is clear that we have work to do with Imprint, we need to
>>> scrutinize budgets and the size of the partnership, look at how we best
>>> focus the science and give enough funds to the critical aspects. I do
>>> hope that the Imprint partners remain loyal to the project and that we
>>> keep it as intended: the best paleoscientists in Europe joined
>>> together. Best regards,

>>> Eystein

>>>

>>>

>>>

>>>

>>> At 13:31 +0000 20-01-05, Keith Briffa wrote:

>>>> Paul

>>>> there is no doubt that Danny's project presents
>>>> something of a problem for us. As far as I
>>>> understand ,yes, it and IMPRINT are the only two
>>>> contenders. I know (confidentially) that they
>>>> have been criticised for not having any
>>>> modelling . Danny approached Hans von Storch
>>>> (and presumably others) , but Hans decided not
>>>> to go with them . At the outset of our
>>>> deliberations regarding IMPRINT , we did discuss
>>>> the possibility that we would impose an
>>>> exclusivity clause on participants - asking them
>>>> to agree not to subscribe to any other project
>>>> (I think Rick Battarbee had been involved in
>>>> another project that did this) . Hence at least
>>>> several of us , in the early (HOLCLIM) stage
>>>> agreed to this - but it was never reinstated

>>>> after the project expanded to its present size.
>>>> Personally , I worry that we are too large and
>>>> possibly could be seen as not focused enough -
>>>> but this is then hard to square with the recent
>>>> referees' comments suggesting our geographic
>>>> scope was too narrow! On paper , I believe the
>>>> whole formulation and partnership of IMPRINT is
>>>> superior to MILLENNIUM , but that did not stop
>>>> me being interested when Danny asked me, some
>>>> time ago , if I would also them. Like you , I do
>>>> not wish to cut off possible fingers in possible
>>>> pies - but I felt that I could not be formally
>>>> included in both .

>>>> The problem is that one has no idea which way
>>>> the anonymous referees will view the judging
>>>> criteria. Surely , in terms of scientific scope
>>>> , our project is superior (though how well it
>>>> ever works and how well we integrate in practise
>>>> is any ones bet).

>>>> The bottom line as I see it is that as only one
>>>> project can be funded , MILLENNIUM should still
>>>> be seen as competition - with you as part of it
>>>> , it would be much stronger competition.

>>>> As for the funding - I know things are
>>>> ill-defined at best at present. I do not think
>>>> anything should be seen as rigid - though we
>>>> certainly have too large a group .

>>>>

>>>> Don't know if this helps

>>>> Keith

>>>>

>>>> At 12:47 20/01/2005, you wrote:

>>>>> Keith,

>>>>>

>>>>> I've just tried to phone you but you were not in your office.

>>>>>

>>>>> I have been contacted by Danny Carroll and
>>>>> invited to join his EU project MILLENNIUM. I
>>>>> gather that this project has also passed the
>>>>> first hurdle and, according to Danny, there are
>>>>> only two such projects so I assume that
>>>>> MILLENNIUM is directly competing against
>>>>> IMPRINT.

>>>>>

>>>>> The modelling he wants me to do is different to
>>>>> anything I will be doing for IMPRINT so there
>>>>> is no scientific reason why I shouldn't say yes
>>>>> to him, and of course it would also allow me to
>>>>> keep a foot in both camps! However there are
>>>>> clear political/strategic issues to consider
>>>>> and I rate IMPRINT higher on my agenda, even
>>>>> though (judging from the IMPRINT indictative
>>>>> money which was very low for Bristol despite
>>>>> having Colin, Sandy and myself involved) it
>>>>> seems likely that the IMPRINT resources will be
>>>>> very limited.

>>>>>

>>>>> Before I respond to him, I wanted to know if
>>>>> you (or anyone else at UEA) are involved in
>>>>> MILLENNIUM. From what I can see, it is very
>>>>> close to your interests. If you are not, was
>>>>> this because you wanted to focus entirely on
>>>>> IMPRINT.

>>>>>

>>>>> Don't misinterpret this email. As I said, I do
>>>>> see IMPRINT higher than MILLENNIUM. However, I
>>>>> would just like more info before deciding how
>>>>> best to respond to Danny.

>>>>>

>>>>> Cheers
>>>>> Paul

>>>>>

>>>>> -----

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>>>>> University of Bristol Email: P.J.Valdes@bristol.ac.uk
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>>>>> -----

>>>>>

>>>>> --

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>>>> <http://www.cru.uea.ac.uk/cru/people/briffa/>

>>>>
>>>>
>>>> --
>>>>

>>> Eystein Jansen
>>> Professor/Director
>>> Bjerknes Centre for Climate Research and
>>> Dep. of Earth Science, Univ. of Bergen
>>> Allégaten 55
>>> N-5007 Bergen
>>> NORWAY

>>> e-mail: eystein.jansen@geo.uib.no Phone: +47-55-583491 - Home:
>>> +47-55-910661 Fax: +47-55-584330

>>> -----
>>> The Bjerknes Training site offers 3-12 months fellowships to PhD
>>> students More info at: www.bjerknes.uib.no/mcts

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>>> -- ---
>>>

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>>
>> -----

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>
> --

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</x-flowed>

From: "Stephen Juggins" <Stephen.Juggins@newcastle.ac.uk>
To: "Eystein Jansen" <eystein.jansen@geo.uib.no>, <imprint-ssc@bjerknes.uib.no>
Subject: Imprint vs. Millennium
Date: Fri, 28 Jan 2005 12:53:52 -0000
Cc: <oyvind.paasche@geo.uib.no>, "Erick Larson" <Erick.Larson@fa.uib.no>

Hi Eystein

I received these comments below from our research office. This outlines the Newcastle approach.

In one case at least it is clear that the idea that groups would not join another consortium as agreed by the ssc had not been passed on to partners outside those discussions. To apply this retrospectively could be seen as unfair - this is obviously how Millennium interpret it. One option that would avoid a split and limit any wider damage or bad feeling would be to get partners to sign a confidentiality agreement now. This would restrict or stop the flow of information between consortia, which, after all, is the main cause for concern.

Cheers, Steve

-----Original Message-----

From: Alan Tuck [mailto:Alan.Tuck@newcastle.ac.uk]
Sent: 28 January 2005 11:40
To: Tony Stevenson
Subject: RE: Question on ethics

Sharp practice certainly. Not necessarily unethical I would have thought.

In a number of cases we have been asked by coordinators to sign up to an exclusivity agreement whereby we will not take part in other consortia who are applying under the same call.

However, we have resisted this saying that we cannot restrict the activities of other academics on the campus, although we have been prepared to sign up to such an agreement that would limit the activities of the particular PI and his/her immediate research group. That way, all of those involved are fully aware of the commitment and its implications. Of course, if they are not happy about this we would not sign up but that in turn would probably mean exclusion from the consortium.

Additionally, and this applies to any collaboration during the preparatory stage, we would recommend that a confidentiality agreement were put in place; this at least would limit the onward transmission of information that could help another grouping.

In this instance I guess that we are where we are.

As it was not established at the outset that a party could only be

involved with one group it may be difficult to move to that position now, not so much because of issues with the other Coordinator but more importantly because it could jeopardise ongoing relationships with fellow collaborators who would be made to choose sides. There again, as these are the probably the very parties who have operated as split personalities there is the question of working with them again.

In any event, it may still be sensible to try to implement a confidentiality agreement so that access to information is restricted and not used to help the other consortium's cause.

Of course, there is the other option of possibly joining forces. The result could be an even stronger application.

Alan

Steve Juggins
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Newcastle upon Tyne Fax: +44 (0)191 222 5421
NE1 7RU, UK Mobile: +44 07740054905
<http://www.campus.ncl.ac.uk/staff/Stephen.Juggins/>

> -----Original Message-----

> From: Tett, Simon [mailto:simon.tett@metoffice.gov.uk]
> Sent: 28 January 2005 09:23
> To: Michael Diepenbroek; simon.tett@metoffice.gov.uk; Eystein
> Jansen; imprint-ssc@bjerknes.uib.no
> Cc: oyvind.paasche@geo.uib.no; Erick Larson
> Subject: RE: [Fwd: URGENT]

>

> One issue to stress in the proposal is that we are trying to
> build a new community. One that unites parts of the broad
> paleo community with (part of) the climate modelling community.
> Simon

>

> Dr Simon Tett Managing Scientist, Data development and applications.
> Met Office Hadley Centre (Reading Unit)
> Meteorology Building, University of Reading Reading RG6 6BB
> Tel: +44 (0)118 378 5614 Fax +44 (0)118 378 5615
> Mobex: +44-(0)1392 886886
> E-mail: simon.tett@metoffice.gov.uk <http://www.metoffice.gov.uk>
> Global climate data sets are available from <http://www.hadobs.org>

>

>

> -----Original Message-----

> From: Michael Diepenbroek [mailto:mdiepenbroek@pangaea.de]
> Sent: 27 January 2005 17:21
> To: simon.tett@metoffice.gov.uk; 'Eystein Jansen';
> imprint-ssc@bjerknes.uib.no
> Cc: oyvind.paasche@geo.uib.no; 'Erick Larson'
> Subject: AW: [Fwd: URGENT]

>
>
> Simon, a forced merge could definitely happen if the
> commission feels that it is worth to have a paleo IP. The
> other outcome could be that they get the impression that the
> community is devived and thus this IP might fail to have the
> wanted impact. The result could be that there is no IP in the
> end. Michael
>
> Dr. Michael Diepenbroek
> WDC-MARE / PANGAEA - www.pangaea.de
>
> _____
> MARUM - Institute for Marine Environmental Sciences
> University Bremen
> POP 330 440
> 28359 Bremen
> Phone ++49 421 218-7765, Fax ++49 421 218-9570
> IP Phone ++49 421 57 282 970
> e-mail mdiepenbroek@pangaea.de
>
>
>
> > -----Urspr"ngliche Nachricht-----
> > Von: Tett, Simon [<mailto:simon.tett@metoffice.gov.uk>]
> > Gesendet: Donnerstag, 27. Januar 2005 15:20
> > An: Eystein Jansen; imprint-ssc@bjerknes.uib.no
> > Cc: oyvind.paasche@geo.uib.no; Erick Larson
> > Betreff: RE: [Fwd: URGENT]
> >
> >
> > Hi Eystein,
> > 1) Institutions (assuming they are sufficiently
> controlling)
> > should not be involved in two proposals. It feels unethical
> to me -- a
> > lot of time and effort goes into putting the proposal together.
> Someone
> > doing this is trying to benefit without being sufficiently
> committed.
> >
> > 2) You are right -- we are including this as a condition of
> being part
> > of the Imprint partnership. Institutions could choose to
> drop out of
> > Imprint or Millennium. Note we do need to be somewhat
> pragmatic. There
> > are institutions that we really need.
> >
> > 3) It is only bullying if we have a greater degree of power than
> > Millennium and use that power to punish. For example it would be
> > bullying if I said I would never work with anyone involved in
> > Millennium. As nobody is saying such a thing I think it
> would be crazy
> > to say we are bullying...

> >
> > 4) I talked to my director. He supports my position but notes some
> > nuances. For example if the two projects were competing for
> the same
> > call but had some very different foci. His example was hot
> spots. You
> > could have one proposal about East Europe and another about
> the Med.
> > Their would not be such a direct clash there.
> >
> > to summarise. I think our position should be "you can only
> be in one
> > competing project. Please choose which one."
> >
> > Eystein it might be worth you taking to Danny -- if only to smooth
> > things over. One possible outcome of the two proposals
> going in is a
> > forced merge. If that happens we need to have reasonable
> relationships.
> >
> > Simon
> >
> > Dr Simon Tett Managing Scientist, Data development and
> applications.
> > Met Office Hadley Centre (Reading Unit)
> > Meteorology Building, University of Reading Reading RG6 6BB
> > Tel: +44 (0)118 378 5614 Fax +44 (0)118 378 5615
> > Mobex: +44-(0)1392 886886
> > E-mail: simon.tett@metoffice.gov.uk <http://www.metoffice.gov.uk>
> > Global climate data sets are available from <http://www.hadobs.org>
> >
> >
> > -----Original Message-----
> > From: Eystein Jansen [mailto:eystein.jansen@geo.uib.no]
> > Sent: 27 January 2005 12:18
> > To: imprint-ssc@bjerknes.uib.no
> > Cc: oyvind.paasche@geo.uib.no; Erick Larson
> > Subject: [Fwd: URGENT]
> >
> >
> > FYI, see below what happened after Valerie said
> > that LSCE was not going to participate
> > inMillennium.
> > My opinion is as follows:
> > We should do as planned.
> > We will ask people to choose which project to be
> > part of. My opinion is that it is not ethical to
> > participate in two competing proposals for the
> > same topic. This creates concerns about
> > confidentiality and concerns that proprietary
> > information might be transferred between
> > projects.
> > Most people would see that this is not a good
> > position to be in and see that it creates

> > conflicts of interest.
> > We cannot force anybody to withdraw, but we have
> > the right to decide who is part of our project
> > and the responsible person at each institution
> > have the right to choose whether the institution
> > joins a bid or not.
> > This is not bullying, and we have come across
> > this problem because we have found out about this
> > in our own partner institutions, which of course
> > needs to know which projects they are part of.
> > I don't think we should force this, it is not
> > worth it, but we should make our point clear, and
> > try to convince those concerns that it is best to
> > choose.
> >
> > Any comments are appreciated.
> >
> > Eystein
> >
> >
> > >Envelope-to: Jansen@geo.uib.no
> > >Date: Thu, 27 Jan 2005 12:52:04 +0100
> > >From: Valerie Masson-Delmotte <Valerie.Masson@cea.fr>
> > >Reply-To: Valerie.Masson@cea.fr
> > >Organization: LSCE
> > >X-Accept-Language: en-us, en
> > >To: Jansen@geo.uib.no
> > >Subject: [Fwd: URGENT]
> > >X-Miltered: at dsm-mail with ID 41F8D587.000 by
> > >Joe's j-chkmail (<http://j-chkmail.ensmp.fr>)!
> > >X-checked-clean: by exiscan on alf
> > >X-Scanner: 275dbee6d499691adc2db0ba5dbafa18
> > ><http://tjinfo.uib.no/virus.html>
> > >X-UiB-SpamFlag: NO UIB: 1.1 hits, 11.0 required
> > >X-UiB-SpamReport: spamassassin found;
> > > 0.1 -- hvorfor herfra?
> > > 0.2 -- HTML included in message
> > > 0.9 -- Message is 40% to 50% HTML
> > >
> > >Dear Eystein,
> > >
> > >You may have thought that I was more diplomatic
> > >than I really am. Sorry about this trouble and
> > >wishing that it would create no more trouble.
> > >Valerie.
> > >
> > >
> > >Return-Path: <D.McCarroll@swansea.ac.uk>
> > >Received: from nenuphar.saclay.cea.fr (nenuphar.saclay.cea.fr
> > >[132.166.192.7])
> > > by dsm-mail.saclay.cea.fr
> > >(8.12.11/jtppda-5.4) with ESMTP id j0RB1UBU030794
> > > for <masson@lsce.saclay.cea.fr>; Thu, 27 Jan
> > 2005 12:47:30

> > +0100
> > >Received: from araneus.saclay.cea.fr (araneus.saclay.cea.fr
> > [132.166.192.110])
> > > by nenuphar.saclay.cea.fr
> > >(8.12.10/8.12.10/CEAnet-internes.4.0) with ESMTTP
> > >id j0RB1V99004140
> > > for <masson@lsce.saclay.cea.fr>; Thu, 27 Jan 2005 12:47:31
> +0100
> > (MET)
> > >Received: from sainfoin.extra.cea.fr (unverified) by
> > araneus.saclay.cea.fr
> > > (Content Technologies SMTPRS 4.3.17) with ESMTTP
> > >id
> > ><T6ec09f0a1284a6c06e548@araneus.saclay.cea.fr>;
> > > Thu, 27 Jan 2005 12:47:30 +0100
> > >Received: from mhs.swan.ac.uk (mhs.swan.ac.uk [137.44.1.33])
> > > by sainfoin.extra.cea.fr
> > >(8.12.10/8.12.10/CEAnet-Internet.4.0) with ESMTTP
> > >id j0RB1Sab008971;
> > > Thu, 27 Jan 2005 12:47:30 +0100 (MET)
> > >Received: from [137.44.41.18] (helo=ccs-maill1.singleton.swan.ac.uk)
> > > by mhs.swan.ac.uk with esmtp (Exim 4.43)
> > > id 1Cu87R-0003P8-PD; Thu, 27 Jan 2005 11:47:25 +0000
> > >Received: by ccs-maill1.singleton.swan.ac.uk with
> > >Internet Mail Service (5.5.2656.59)
> > > id <DJ8KFQ1Y>; Thu, 27 Jan 2005 11:46:50 -0000
> > >Message-ID:
> > ><840186FCFC231A4980595D19685DDE4A0129CB6D@lsntex3.clyne.swan.ac.u
> > k>
> > >From: "McCarroll D." <D.McCarroll@swansea.ac.uk>
> > >To: William Austin <wena@st-andrews.ac.uk>,
> > > Anders Rindby
> > > <anders@coxsys.se>,
> > > "Andreas J. Kirchhefer"
> > > <Andreas.Kirchhefer@ib.uit.no>,
> > > Andreas Luecke <a.luecke@fz-juelich.de>,
> > > Barbara Wohlfarth <barbara@geo.su.se>,
> > > Brazdil Rudolf
> > > <brazdil@sci.muni.cz>,
> > > Brigitta Ammann <Brigitta.Ammann@ips.unibe.ch>,
> > > Christian Bigler <christian.bigler@eg.umu.se>,
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> > > <christian.kamenik@ips.unibe.ch>,
> > > "Davies Siwan."
> > > <Siwan.Davies@swansea.ac.uk>,
> > > Emilia Gutierrez <emgutierrez@ub.edu>,
> > > "Froyd C." <C.Froyd@swansea.ac.uk>,
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> > > <M.H.Gagen@swansea.ac.uk>,
> > > Gerd Helle <g.helle@fz-juelich.de>,
> > > Gudrun Larsen <glare@raunvis.hi.is>,
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> > <buentgen@wsl.ch>,
> > > Valerie Daux <Valerie.Daux@cea.fr>,
> > > Valerie Masson-Delmotte <masson@dsm-mail.saclay.cea.fr>
> > >Subject: URGENT
> > >Date: Thu, 27 Jan 2005 11:46:42 -0000
> > >MIME-Version: 1.0
> > >X-Mailer: Internet Mail Service (5.5.2656.59)
> > >Content-Type: multipart/alternative;
> > > boundary="---- =_NextPart_001_01C50465.A49F468B"
> > >X-SA-Exim-Mail-From: D.McCarroll@swansea.ac.uk

> > >X-Miltered: at dsm-mail with ID 41F8D4D2.001 by
> > >Joe's j-chkmail (<http://j-chkmail.ensmp.fr>)!
> > >X-Spam-Checker-Version: SpamAssassin 2.64 (2004-01-11) on
> > dsm-mail.cea.fr
> > >X-Spam-Level: **
> > >X-Spam-Status: No, hits=2.8 required=4.0 tests=BAYES_44,HTML_60_70,
> > > HTML_MESSAGE,NIGERIAN_SUBJECT1 autolearn=no version=2.64
> > >
> > >27th January
> > >
> > >Dear Millennium partners
> > >
> > >I have been informed by one of our partners that
> > >the other IP proposal (IMPRINT) has decided that
> > >institutions should
> > >not be in both applications (IMPRINT and MILLENNIUM) and that they
> > >want Millennium partners to choose either one or the
> > >other. I am advised that they may issue a
> > >dictate to this effect very soon.
> > >
> > >It is my view that they have absolutely no right
> > >to do this. The Millennium application is
> > >confidential, and they have no right to ask
> > >anyone if they are part of the proposal or not.
> > >They certainly have no right to dictate that an
> > >institution can only be part of one proposal.
> > >
> > >I suggest that if any of you are contacted by
> > >IMPRINT and asked about Millennium you either
> > >ignore the message or politely tell them that EU
> > >proposals are confidential. They should not be
> > >allowed to bully anyone in this way or to
> > >undermine our project.
> > >
> > >Personally I think that there is absolutely no
> > >problem with institutions or even individuals
> > >being in both projects. The aim of an Integrated
> > >Project is to bring together the best
> > >scientists, so it is not a surprise that the
> > >best scientists appear in more than one
> > >application. If they are forced to choose then
> > >it inevitably means that some of the best groups
> > >will not get funded. That is not in the
> > >interests of the EU or of science.
> > >
> > >I will contact the leaders of IMPRINT today and
> > >try to encourage them to re-think this strategy.
> > > It is not necessary to make the community
> > >divide in this way. If they go ahead I will
> > >immediately contact the Commission and make a
> > >formal complaint.
> > >
> > >Apart from this small problem everything is
> > >going very well and we are on target to produce

From: Keith Briffa <k.briffa@uea.ac.uk>

To: dirk.verschuren@gfz-potsdam.de

Subject: Re: Dirk

Date: Fri Jan 28 16:15:49 2005

Cc: Stephen.Juggins@newcastle.ac.uk, Valerie Masson-Delmotte <masson@lsce.saclay.cea.fr>, eystein.jansen@geo.uib.no, Sandy Tudhope <sandy.tudhope@ed.ac.uk>, dan.charman@plymouth.ac.uk

Dear Dirk

good news re your not dropping out . We are happy to have you and if you can do what you can in the time available this would be good. Valerie and I will send a general message Monday am to all WP1 folk to say what is needed now, but we thought it best to get back to you straight away re specific points raised in Steve's message.

First, I hope you will be responsible with Dan (and help from Sandy Tudhope) for co-ordinating Task 1.4 of WP1 following the concept as we saw it in the preliminary proposal. Of course you would focus on North African (and north and south of this area) work - on the collection, comparison, integration, interpretation of the high and lower resolution records that relate to hydrology. I see Dan as taking the strain regarding the more Northern areas - with obvious attention to wetlands and Sandy helping with dynamic links (and ENSO?). Of course there are other records and there will be a need to restrict "new" collection/laboratory analyses to very specific , justified (and accepted by SC) situations , but the high resolution core(s) you told me of would be relevant. I suggest you think in terms of a person to work on this AND data compilation - perhaps a (cheap) postdoc for 3 years , and money for internal WP1 meetings - say 250KEuro ?

FOR NOW - we need you to liaise with Dan and Sandy to produce what you can for the Task 1.4(see attached old version of proposal to start from) . We will need a "state of the Art" Scientific objectives and approach details . Your whole Task 1.4 section can only be 1 page A4 single spaced max.

AFTER LONG DISCUSSION IN LONDON- it was decided that this task would NOW NOT INCLUDE the paleoflood work - and Eystein will be communicating with Bennitto to (regretfully) to inform him that we have had to remove his contribution (please do not contact him until Eystein has done this). We will not put a specific focus on floods (though of course some work can be done using existing European flood data), because of Rudolf Brazdil , and we hope , he will accept to be part of WP1 but put some of his requested funds into WP6 . Hence you 3 can concentrate more on the concept of large scale hydrologic variability ,monsoon changes , north south linkages etc. The problem with ENSO persists. I know you Sandy want to focus entirely on this, but we could compromise perhaps and you do part this and part Europe? It was decided that we will (somewhere) include data/model comparison with US droughts , but this does not require effort on our part other than minor data compilation of existing records [Eystein, we therefore need to ensure Cook is one of the associated americans]. We will put together an appendix of preliminary records to be used in each task - just to show impressive new potential integration (but not a priority for now).

You do not need to sign any forms officially at this stage - just get approval presumably from your department internally . If we ever get there, forms will be handled at contract negotiation.

So get in touch with each other (resend ideas , do not assume your previous emails went to each other), get exchanging ideas and draft what you can .

ON monday , the specific letter to all people will come round , with requested timeline , task, deliverables re budget and precise format of Science writing that we need to assemble the proposal. Then Valerie and I will have to look at the whole thing in the context of our total 3.7 M budget.

IT WILL ALL SEEM WORTH IT IN 2006

All the very best Keith and Valerie

Keith's home number is 441953 851013

mobile 0776 9732 685

At 12:37 28/01/2005, Stephen Juggins wrote:

Hi Keith, copy to Eystein, Oyvind

Just had a long chat with Dirk. It's OK, he's not in Millenium!

The reason he was pulling out is over committment this year. Anyway, I managed to persuade him to change his mind - the project won't start until Jan 2006 at the very earliest, so any input won't be needed until next year. He was also unsure what to ask for - I suggested he should cost in a post-doc for 3 years and 2 meetings per year, plus some "data workshops". Keith - can you give him some guidance on costing these so they are in line with what others are asking for. I told him that you would look at the overall budges for WP1 and adjust if necessary to meet the target.

His only short term problem is revising any text for the proposal - he leaves for Kenya next Thursday. I realised that Eystein has only sent the documents to the ssc people so Keith, can you forward these to Dirk and let him know exactly what you need from him for the text and budgets.

Finally, Dirk was worried that he wouldn't be able to get any paperwork & signatures from his Uni but as I understood from the meeting yesterday this was not needed. Is this right? If there are any forms to fill in we had better get these to him asap.

Cheers, Steve

Steve Juggins

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[1]<http://www.campus.ncl.ac.uk/staff/Stephen.Juggins/>

--

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[2]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.campus.ncl.ac.uk/staff/Stephen.Juggins/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: imprint-ssc@bjerknes.uib.no
Subject: RE:
Date: Mon, 31 Jan 2005 12:17:44 +0100
Cc: mschulz@palmod.uni-bremen.de, stocker@climate.unibe.ch

<x-flowed>

Hi, just for clarification as we continue on the St.2 proposal (you'll get the mailing tomorrow with documents, scheduling etc. as planned). The merger of ICON into Imprint was discussed several times in the preparatory phase of Imprint (before name was decided) in meetings we had in London early last year. However a number of the present WP leaders did not take part in these early deliberations, hence this is the reason for the lack of a collective memory of the background.

Reasons for including it:

1. Good science, on a topic of high relevance (abrupt climate change) focussed and with emphasis on aspects dealing with predictability of such changes rather than mapping out their distribution and impact (as has been done before).
3. Important to avoid competing proposals within Europe to avoid the paleo-community being marginalised.

Cheers,
Eystein

At 09:18 +0000 31-01-05, Tett, Simon wrote:

>Hi Rainer,
> Until our recent meeting in London I was
>not aware of the history and do not recall any
>discussion about blending ICON into the project.
>I expect that is a decision Eystein made.
>However, I am very glad that the work is part of
>the IP. I think it will allow much better
>science to be done.
>

>Simon

>

>Dr Simon Tett Managing Scientist, Data development and applications.

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>E-mail: simon.tett@metoffice.gov.uk <http://www.metoffice.gov.uk>

>Global climate data sets are available from <http://www.hadobs.org>

>

>

>-----Original Message-----

>From: rainer.zahn@icrea.es [mailto:rainer.zahn@icrea.es]

>Sent: 31 January 2005 08:45

>To: imprint-ssc@bjerknes.uib.no;

>eystein.jansen@geo.uib.no;

>oyvind.paasche@geo.uib.no; Erick.Larson@fa.uib.no

>Cc: mschulz@palmod.uni-bremen.de; stocker@climate.unibe.ch

>Subject:

>

>

>Simon,

>

>I couldn't agree more on the issue of having the science focussed in

>Imprint. I am surprised though that the background behind having WP3 and

>Task 4.6 in Imprint does not appear to be common knowledge within Imprint.

>Thought the merger has been discussed and agreed upon by the consortium.

>

>We will move forward with our WP and see that we get the Holocene part in

>WP3/4.6 strengthened so as to make fit with the timescales of the rest of

>the planned work.

>

>As a note on the side, you may have noted in the comments of the independent

>assessor that Eystein contracted in for advice that he mentions WP3

>specifically for its clarity and relevance. While I tend to agree I am also

>aware that he probably is not the specialist to assess the issue of

>relevance and significance. Yet, in the WP3 description we are asking a set

>of clear-cut questions, which to me doesn't seem the case for other WPs that

>leave an unfortunate impression of confusion. Beyond the needed scientific

>focus mentioned on several occasions in London and your email, clarity is an

>issue that does not seem to be equally distributed throughout the proposal.

>So as much as I do sympathise with the discussion about the sense or

>non-sense of have WP3 in Imprint, I am convinced to my heart that we need to

>improve profoundly on the quality of our WP descriptions if Imprint is to
>stand a chance of being considered for funding.

>

>Cheers, Rainer

>

> Rainer Zahn, Professor de Recerca
> Institució Catalana de Recerca i Estudis Avançats, ICREA

>i Universitat Autònoma de Barcelona

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--

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The Bjerknes Training site offers 3-12 months fellowships to PhD students
More info at: www.bjerknes.uib.no/mcts

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: For your eyes only
Date: Thu Feb 3 13:11:46 2005

Mike,

It would be good to produce future series with and without the long instrumental series and maybe the documentary ones as well. The long measurements can then be used to validate the low-freq aspects at least back to 1750, maybe earlier with the documentary. There are some key warm decades (1730s, some in the 16th century) which the Moberg reconstruction completely misses and gives the impression that all years are cold between 1500 and 1750.

Away Feb 6-10 and 12-20 and 22-25 (last in Chicago - on the panel to consider the vertical temp work of CCSP).

Cheers

Phil

Cheers

Phil

At 15:26 02/02/2005, you wrote:

Thanks Phil,

Yes, we've learned our lesson about FTP. We're going to be very careful in the future what gets put there. Scott really screwed up big time when he established that directory so that Tim could access the data.

Yeah, there is a freedom of information act in the U.S., and the contrarians are going to try to use it for all its worth. But there are also intellectual property rights issues, so it isn't clear how these sorts of things will play out ultimately in the U.S.

I saw the paleo draft (actually I saw an early version, and sent Keith some minor comments). It looks very good at present--will be interesting to see how they deal w/ the contrarian criticisms--there will be many. I'm hoping they'll stand firm (I believe they will--I think the chapter has the right sort of personalities for that)...

Will keep you updated on stuff...

talk to you later,

mike

At 09:41 AM 2/2/2005, Phil Jones wrote:

Mike,

I presume congratulations are in order - so congrats etc !
Just sent loads of station data to Scott. Make sure he documents everything better this time ! And don't leave stuff lying around on ftp sites - you never know who is trawling

them. The two MMs have been after the CRU station data for years. If they ever hear there

is a Freedom of Information Act now in the UK, I think I'll delete the file rather than send

to anyone. Does your similar act in the US force you to respond to enquiries within 20 days? - our does ! The UK works on precedents, so the first request will test it.

We also

have a data protection act, which I will hide behind. Tom Wigley has sent me a worried email when he heard about it - thought people could ask him for his model code. He has retired officially from UEA so he can hide behind that. IPR should be relevant here,

but I can see me getting into an argument with someone at UEA who'll say we must adhere to it !

Are you planning a complete reworking of your paleo series? Like to be involved if you are.

Had a quick look at Ch 6 on paleo of AR4. The MWP side bar references Briffa, Bradley, Mann, Jones, Crowley, Hughes, Diaz - oh and Lamb ! Looks OK, but I can't see it getting past all the stages in its present form. MM and SB get dismissed. All the right

emphasis is there, but the wording on occasions will be crucial. I expect this to be the

main contentious issue in AR4. I expect (hope) that the MSU one will fade away. It seems

the more the CCSP (the thing Tom Karl is organizing) looks into Christy and Spencer's series, the more problems/issues they are finding. I might be on the NRC review panel, so will keep you informed.

Rob van Dorland is an LA on the Radiative Forcing chapter, so he's a paleo expert by GRL standards.

Cheers

Phil

At 13:41 02/02/2005, you wrote:

Phil--thought I should let you know that its official now that I'll be moving to Penn State next Fall.

I'll be in the Meteorology Dept. & Earth and Environmental Systems Institute, and plan to head up a center for "Earth System History" within the institute. Will keep you updated,

Mike

Prof. Phil Jones

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References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: Re: [Fwd: Re: Zero order draft of Chapter 3, AR4, IPCC]
Date: Fri Feb 4 17:23:32 2005

Kevin,

I was concerned about splitting too, and suggested as a way of getting through the work a little quicker. Pairs will also work as long as we choose the right ones. Agree we need to separate the major from minor, so anything that can be done there in April will be good.

I suspect the comments from the nominated reviewers will all have to answered in a formal way - as a dry run for the FOD and SOD.

On the figures we need to compare notes on these in a few weeks and assign particular people to them. We both worked with Dave on the set of trends. They may not be perfect, but they are better than some of the others. I think we will need to do more of this.

Giving responsibility for a handful to some of the LAs is a possibility. We'll need to give clear instructions though and expect loads of iterations. I can deal with 3.2 with David and the HC if we can agree on what and how we want them. Most of the other sections require much more thought. I'll work on this.

I agree 100% with you on the TC section. This will get scrutinized by many more now. I'll report back on the CCSP review. Apart from Lindzen the panel seem pretty good. So, I'll gauge what the key issues appear to be in the panel's minds. Agree that we shouldn't treat it's conclusions as gospel (otherwise why are we bothering), but treat it as a very very major review article.

Must go home now. Have a good trip back to NZ.

Cheers

Phil

At 16:39 04/02/2005, you wrote:

Phil I tried to attach the ppt with all the figures: but it is too big for your server??

Kevin

----- Original Message -----

Subject: Re: Zero order draft of Chapter 3, AR4, IPCC

Date: Fri, 04 Feb 2005 09:36:00 -0700

From: Kevin Trenberth [1]<trenbert@cgd.ucar.edu>

To: Phil Jones [2]<p.jones@uea.ac.uk>

References: [3]<42024852.7060406@cgd.ucar.edu>

[4]<6.1.2.0.0.20050204144545.03dd6830@pop.uea.ac.uk>

Hi Phil

Not sure how to handle all this. Recall how it was done for GCOS: I don't think that worked. The official version requires each comment to have name etc on it so it can be carved up. The CAs won't do that, so I think we have to treat each CA separately, or at best broken up by section. I can try to get my admin to work on it if we have clear guidelines.

I am also concerned about splitting: There are a lot of things that can be done by LAs working in pairs. In previous IPCCs we broke up into sections. Two people worked on each section in parallel. Lots of things can be done that way. But there are some major things that we have to build a consensus on of all of us. I now have a particular interest in making sure the hurricanes are done well. I also am concerned about the UA-MSU etc and clearly you and I should both be engaged there. So sorting out the fairly minor from major points will be a key task.

I am not taken by our set of figures. If I look at them and try to create a story e.g. by ppt, I think they are lacking. I am attaching the ones I have assembled.

I am away next week in Hawaii at the Chapman conference (AGU). Then I am briefly back and then I am gone and out of touch in New Zealand on personal time 20 Feb to 3 March.

Kevin

Phil Jones wrote:

Kevin,

At least two of the CAs have already begun reading the ZOD. I hope your clear message is followed by all the CAs. Glad you sent the pdf and not the doc version. Tracked changes would be a nightmare.

With all these comments, I presume we'll both assemble all the CA comments. WGI will get comments from our nominated (and their) referee's. I presume WGI will somehow collate these, so for example, all comments on section 3.7 or 3.7.1 will be together.

Is

there a way we can collate all the CA comments similarly? I guess we can decide this later when some more have come in. I reckon we'll have to split the group in Beijing

if we are to get through all the comments in the 3.5 days, so separating them would prove useful. Would an email to WGI be useful to see if they can do it for us? Just a thought !

As you saw, I've reminded our LAs with responsibility for linking with other chapters look at that chapter as well.

No chance so far to look at the CCSP (vertical temp trends) - 6 sections each of 40-70 pages !!

Away from today Feb 6-10 in Madrid (EU project meeting) , 12-20 in Pune (extremes workshop - the last one in the current round, for South Asia) and 22-25 at O'Hare Hilton for the CCSP report.

Only here 11th and 21st. Should have email contact in Madrid and Chicago, but Pune may be hit and miss. Still, not much need for too much contact at this time.

I'll give the diagrams and other issues some thought whilst away. Albert will be in Pune.

Have a good few weeks and I hope the Landsea issue has subsided.

Cheers

Phil

At 15:50 03/02/2005, you wrote:

Dear CA

The zero order draft of Chapter 3 of the WG1 IPCC AR4 report is now available. Your contribution has helped us put together this draft, and we thank you very much. However, it is NOT yet the first draft; we recognize that it is incomplete in some places (for instance where some CAs did not come through, or through oversight), and we have not even reviewed it fully ourselves, given the tight timetable. So we are seeking constructive comments and your assistance on developing the first draft. What is most helpful is for you to suggest new text and references, and explicit changes. Not "such and such" is bad or needs fixing. We can not promise to use the new text because there are 60 CAs who may well suggest different things. We also have to limit page numbers, so we especially welcome suggestions for shortening. If you care to rewrite a section more succinctly, then we will gladly consider it. The figures are all preliminary and will be thoroughly examined in Beijing in May, so suggestions of improved or more recent figures are welcomed. We also welcome copies of any papers submitted or referred to.

I am sending this out in two parts. This part has the text attached as a pdf. It is order 1 MB. The second part includes the figures, many in color, and it is 3.7 MB. We need you comments by 1 April 2005 at the latest. If you prefer to focus only on the section in which your contribution appeared, then that is fine, but you are welcome to comment on other parts as well. If you can not comment or prefer not to for some reason or another, a message to that effect would also be welcomed so we can track responses.

Please send your comments, preferably in word, with your name on each page, and clear identification of section, page and line number or figure number. You may like to make a comment, followed by explicit suggestion for addition or change. Please do justify and argue why the change is needed. Please send comments to Kevin Trenberth and Phil Jones, who will assemble them.

Many thanks for your help

Kevin Trenberth

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Phil Jones

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References

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: chris.folland@metoffice.gov.uk
Subject: Fwd: Re: FW: "hockey stock" methodology misleading
Date: Tue Feb 8 16:44:17 2005

X-Sender: mem6u@multiproxy.evsc.virginia.edu
X-Mailer: QUALCOMM Windows Eudora Version 6.1.1.1
Date: Fri, 04 Feb 2005 16:04:57 -0500
To: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu,
tom crowley <tom@ocean.tamu.edu>, tom crowley <tom@ocean.tamu.edu>,
mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu,
Keith Briffa <k.briffa@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>

From: "Michael E. Mann" <mann@virginia.edu>
Subject: Fwd: Re: FW: "hockey stock" methodology misleading
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean
X-UEA-MailScanner-SpamScore: s
sorry, forgot to attach the paper...
mike

Date: Fri, 04 Feb 2005 15:54:15 -0500
To: Phil Jones <p.jones@uea.ac.uk>, rbradley@geo.umass.edu, Tom Crowley, Tom Crowley,
mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, Keith Briffa <k.briffa@uea.ac.uk>
From: "Michael E. Mann" <mann@virginia.edu>
Subject: Fwd: Re: FW: "hockey stock" methodology misleading

Date: Fri, 04 Feb 2005 15:52:53 -0500
To: Andy Revkin <anrevk@nytimes.com>
From: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: FW: "hockey stock" methodology misleading

Hi Andy,

The McIntyre and McKittrick paper is pure scientific fraud. I think you'll find this reinforced by just about any legitimate scientist in our field you discuss this with. Please see the RealClimate response:

[1]<http://www.realclimate.org/index.php?p=111>
and also:

[2]<http://www.realclimate.org/index.php?p=114>

The Moberg et al paper is at least real science. But there are some real problems with it (you'll want to followup w/ people like Phil Jones for a 2nd opinion).

While the paper actually reinforces the main conclusion of previous studies (it also finds the late 20th century to be the warmest period of the past two millennia), it challenges various reconstructions

using tree-ring information (which includes us, but several others such as Jones et al, Crowley, etc). I'm pretty sure, by the way, that a very similar version of the paper was rejected previously by Science. A number of us are therefore very surprised that Nature is publishing it, given a number of serious problems:

Their method for combining frequencies is problematic and untested:

A. they only use a handful of records, so there is a potentially large sampling bias.

B. worse, they use different records for high-frequencies and low-frequencies, so the bias isn't even the same--the reconstruction is apples and oranges.

C. The wavelet method is problematic. We have found in our own work that you cannot simply combine the content in different at like frequencies, because different proxies have different signal vs. noise characteristics at different frequencies--for some records, their century-scale variability is likely to be pure noise. They end up therefore weighting noise as much as signal. For some of the records used, there are real age model problems. The timescale isn't known to better than +/- a couple hundred years in several cases. So when they average these records together, the century-scale variability is likely to be nonsense.

D. They didn't do statistical verification. This is absolutely essential for such reconstructions (see e.g. the recent Cook et al and Luterbacher et al papers in Science). They should have validated their reconstruction against long-instrumental records, as we and many others have. Without having done so, there is no reason to believe the reconstruction has any reliability. This is a major problem w/ the paper. It is complicated by the fact that they don't produce a pattern, but just a hemispheric mean--that makes it difficult to do a long-term verification. But they don't attempt any sort of verification at all! There are some decades known to be warm from the available instrumental records (1730s, some in the 16th century) which the Moberg reconstruction completely misses--the reconstruction gives the impression that all years are cold between 1500 and 1750. The reconstruction would almost certainly fail cross-validation against long instrumental records. If so, it is an unreliable estimate of past changes.

We're surprised the Nature Reviewers didn't catch this.

E. They also didn't validate their method against a model (where I believe it would likely fail). We have done so w/ our own "hybrid frequency-domain" method that combines information separately at low and high-frequencies, but taking into account the problem mentioned above. This is described in:

Rutherford, S., Mann, M.E., Osborn, T.J., Bradley, R.S., Briffa, K.R., Hughes, M.K., Jones, P.D., [3]Proxy-based Northern Hemisphere Surface Temperature Reconstructions: Sensitivity to Methodology, Predictor Network, Target Season and Target Domain, Journal of Climate, in press (2005).

In work that is provisionally accepted in "Journal of Climate" (draft attached), we show that our method gives the correct history using noisy "pseudoproxy" records derived from a climate model simulation with large past changes in radiative forcing. Moberg et al have not tested their method in such a manner.

F. They argue selectively for favorable comparison w/ other work:

(1) Esper et al: when authors rescaled the reconstruction using the full instrumental

record (Cook et al, 2004), they found it to be far more similar to Mann et al, Crowley and Lowery, Jones et al, and the roughly dozen or so other empirical and model estimates consistent w/ it. Several studies, moreover [see e.g.: Shindell, D.T., Schmidt, G.A., Mann, M.E., Faluvegi, G., [4]Dynamic winter climate response to large tropical volcanic eruptions since 1600, Journal of Geophysical Research, 109, D05104, doi: 10.1029/2003JD004151, 2004.] show that extratropical, land-only summer temperatures, which Esper et al emphasises, are likely to be biased towards greater variability--so it's an apples and oranges comparison anyway.

(2) von Storch et al: There are some well known problems here: (a) their forcing is way too large (Foukal et al in Science a couple months back indicates maybe 5 times too large), DKMI uses same model, more conventional forcings, and get half the amplitude and another paper submitted recently by the Belgium modeling group suggests that some severe spin-up/initialization problems give the large century-scale swings in the model--these are not reproducible.

(3) Boreholes: They argue that Boreholes are "physical measurements" but many papers in the published literature have detailed the various biases in using continental ground surface temperature to estimate past surface air temperature changes--changing snow cover gives rise to a potentially huge bias (see e.g. : Mann, M.E., Schmidt, G.A., [5]Ground vs. Surface Air Temperature Trends: Implications for Borehole Surface Temperature Reconstructions, Geophysical Research Letters, 30 (12), 1607, doi: 10.1029/2003GL017170, 2003).

Methods that try to correct for this give smaller amplitude changes from borehole temperatures:

Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., [6]Optimal Surface Temperature Reconstructions using Terrestrial Borehole Data, Journal of Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003]

[[7]Correction(Rutherford and Mann, 2004)]

Most reconstructions and model estimates still "sandwich" the Mann et al reconstruction. See e.g. figure 5 in: Jones, P.D., Mann, M.E., [8]Climate Over Past Millennia, Reviews of Geophysics, 42, RG2002, doi: 10.1029/2003RG000143, 2004.

Ironically, MM say our 15th century is too cold, while Moberg et al say it's too warm. Hmmm....

To recap, I hope you don't mention MM at all. It really doesn't deserve any additional publicity. Moberg et al is more deserving of discussion, but, as outlined above, there are some real problems w/ it. I have reason to believe that Nature's own commentary by Schiermeier will actually be somewhat critical of it.

I'm travelling and largely unavailable until Monday. If you need to talk, you can possibly reach me at 434-227-6969 over the weekend.

I hope this is of some help. Literally got to run now...

mike

At 02:14 PM 2/4/2005, Andy Revkin wrote:

Hi all,

There is a fascinating paper coming in Nature next week (Moberg of Stockholm Univ., et al) that uses mix of sediment and tree ring data to get a new view of last 2,000 years. Very warped hockystick shaft (centuries-scale variability very large) but still pronounced 'unusual' 1990's blade.

i'd like your reaction/thoughts for story i'll write for next thursday's Times.

also, is there anything about the GRL paper forthcoming from Mc & Mc that warrants a response?

I can send you the Nature paper as pdf if you agree not to redistribute it (you know the embargo rules).

that ok?

thanks for getting in touch!

andy

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References

1. <http://www.realclimate.org/index.php?p=111>
2. <http://www.realclimate.org/index.php?p=114>
3. <http://www.realclimate.org/RuthetalJClim2004.pdf>
4. <ftp://holocene.evsc.virginia.edu/pub/mann/Shindelletal-jgr04.pdf>
5. <ftp://holocene.evsc.virginia.edu/pub/mann/gissgst03.pdf>
6. <ftp://holocene.evsc.virginia.edu/pub/mann/borehole-jgr03.pdf>
7. <http://holocene.evsc.virginia.edu/shared/articles/JGRBoreholeCorrection04.pdf>
8. <ftp://holocene.evsc.virginia.edu/pub/mann/JonesMannROG04.pdf>
9. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
10. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
11. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
12. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Michael E. Mann" <mann@virginia.edu>

To: Phil Jones <p.jones@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>, "Eugene R" <wahle@alfred.edu>, Scott Rutherford <srutherford@rwu.edu>

Subject: Re:

Date: Sat, 12 Feb 2005 17:44:06 -0500

Cc: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk

sorry. text revised yet again. no more changes until I receive comments from everyone.

thanks...

mike

At 12:03 PM 2/11/2005, Phil Jones wrote:

Mike,

Keith and Tim are here next week, but very busy with a proposal to the EU.

So you may have to hassle them a bit, or hang on for a week or two.

Nature dragged in the IPCC angle which annoyed me. I tried to explain to him how IPCC works. IPCC won't be discussing this in Beijing in May - except as part of Chapter 6. Hans von Storch will likely regret some of the words he's said.

FYI, just as NCAR have put up a web site to give the whole story re Chris Landseas's 'resignation' from a CA in the atmos. obs. chapter (to help Kevin Trenberth out), KNMI are doing the same re Rob van Dorland and that Dutch magazine. The chief scientist at KNMI has got involved as Rob didn't say the things attributed to him. I'll find out more on this in Pune as a guy from KNMI will be there.

Several other CAs on our chapter pulled out, or just didn't do anything. Their stories never got run.

Dick's report was good and my bit in Nature came across well.

Say hi to all there and wish Steve well.

Cheers

Phil

At 16:19 11/02/2005, Michael E. Mann wrote:

Phil--thanks, that's great. Really happy to hear that everyone is on board with this. I'm at a symposium honoring Steve Schneider out at Stanford right now. Lots of folks here--as I talk this over w/ them, and see Dick Kerr's coverage of this, etc. I realize it's not so bad--I was afraid this would be spun as bolstering the contrarians, but it hasn't. In large part due to quotes from you and others pointing out that the study actually reinforces the key conclusions, etc., and the fact Dick Kerr showed Keith and Tim's plot showing the scattering of multiple reconstructions, etc. which takes the focus off "Mann" a bit...

Nonetheless, I *am* convinced their methodology is suspect, as the analysis I sent shows. So I will really appreciate input from Keith, Tim, and you to make sure the language and wording are appropriate and fair...

I will revise as I get input from various people, with an aim to having this submission-ready in about 10 days (so you can have one final look after you return, and before you have to head out again).

looking forward to getting people's comments, feedback, etc.

thanks again,

mike

At 08:05 AM 2/11/2005, Phil Jones wrote:

Mike et al,

I've talked to Keith and Tim here and it seems best if we all come in with you on this response. What you have done is basically fine. We can discuss specific wording later.

My problem is that I'm off tomorrow to Pune till Feb 20 and email may be sporadic or non-existent. So can you discuss revised drafts with Keith and Tim, but keep me on - lower down as I'm away. I'm here on Feb 21 then off to Chicago to review the vertical temperature report for the NRC/NAS Feb 22-25.

Keep me on the emails in case email works well in Pune.

Cheers

Phil

At 23:35 10/02/2005, Michael E. Mann wrote:

Dear Caspar, Gene, Scott, Phil,

I am attaching a response I've drafted to the Moberg et al paper (attached for those of you who haven't seen it). The message is pretty clear and simple--their method overemphasizes the low-frequency variability. To demonstrate this, I've made use of stuff from Mann and Jones, and from the Mann/Rutherford/Wahl/Ammann J. Climate letter on Pseudoproxies. So I would welcome any of you to be co-authors on this--just let me now if you're interested. I've been in touch w/ Keith (he and Tim are potentially working on their own independent response--waiting to hear further).

This is a very rough draft, so comments much appreciated.

Looking forward to hearing back,

Mike

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[3]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>
Attachment Converted: "c:\eudora\attach\MobergComment2.doc"

References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: WSJ
Date: Mon, 14 Feb 2005 11:37:07 -0500
Cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>

A good comparison of all of the reconstruction constructive by William Connelly, which makes it clear that the take-home point is robust, is available here:

[1]http://en.wikipedia.org/wiki/Image:1000_Year_Temperature_Comparison.png
mike

At 10:58 AM 2/14/2005, Tom Wigley wrote:

Mike,

I'm sorry we had no time to talk at Stanford.

Here is the answer to the LIA bounce back idea ...

For 20th century warming to be a bounce back, the heat must come from somewhere. The only source consistent with the bounce back idea is the ocean.

The Levitus data show that heat has been going INTO the ocean, not coming out of it.

This is really obvious, but I have never seem it stated anywhere.

Re WSJ. They say ...

"Statistician Francis Zwiers of Environment Canada, a government agency, says he now agrees that Dr. Mann's statistical method "preferentially produces hockey sticks when there are none in the data."

Dr. Mann, while agreeing that his mathematical method tends to find hockey-stick shapes, says this doesn't mean its results in this case are wrong. Indeed, Dr. Mann says he can create the same shape from the climate data using completely different math techniques."

It is a bit worrying that Francis agrees with M&M -- but it seems that you do too.

My questions are:

(1) Do other reconstructions (not including Lonnie Thompson's of course) suffer from this standardization problem?

(2) You have stated that simply averaging the data together gives the same result. Has this elementary method been published?

(2a) I note that the PC1 amplitude time series invariably correlates highly with the (non-areally-weighted) 'area average'. So this brings up the issue of whether you use some area weighting in your PCA -- as we

invariably do when doing PCA of gridded data?

(3) From what I can see without reading their full GRL paper, M&M think that the RE statistic has an odd sampling distribution. It is easy to show this by Monte Carlo simulation -- have you done this (i.e., in the abstract, as a statistical exercise, not for the specific case of MBH98, etc.)?

Tom.

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References

1. http://en.wikipedia.org/wiki/Image:1000_Year_Temperature_Comparison.png
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Michael E. Mann" <mann@virginia.edu>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>, Stephen H Schneider
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k.briffa@uea.ac.uk, p.jones@uea.ac.uk
Subject: Fwd: RE: WSJ article
Date: Wed, 16 Feb 2005 17:56:01 -0500

Interesting that Antonio R. doesn't (or at least claims not to)
recognize a lack of balance
in the article.

Please treat this email as confidential. I don't believe that sending
a letter to the
editor myself would be the best avenue. But perhaps someone else is
interested in pursuing
this?
Mike

Subject: RE: WSJ article
Date: Wed, 16 Feb 2005 17:43:10 -0500
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: WSJ article
Thread-Index: AcUUAig6ON4Ck5ANQ20foGmU0QNsvAAAEqMA
From: "Regalado, Antonio" <Antonio.Regalado@wsj.com>
To: "Michael E. Mann" <mann@virginia.edu>
X-OriginalArrivalTime: 16 Feb 2005 22:43:10.0610 (UTC)
FILETIME=[E423A720:01C51478]
X-UVA-Virus-Scanned: by amavisd-new at fork11.mail.virginia.edu

Hi Mike,

On the personal stuff, I'd go with your first impressions, rather
than the perceptions of
others. This isn't a one-sided story. Anyway, I certainly want to
find out who is right
here and so I am open to writing more as the papers come out and the
facts become
clearer, just as I have written in the past about the Soon and
Balliunias business (p.
A3not bad) and about paleo-climate (p. 1 story in 2002 about Gary
Comers funding,
feature story on Lonnie Thompson's melting glaciers), etc. Would it
surprise you to
hear that anytime I write a story which seems to favor global
warming I am also deluged
by accusations of bias and demands for corrections etc.?
Regarding Moberg, I think the issue you are raising is a question of
emphasis and not a
matter for a correction. The specific sentences you're thinking of
(Indeed, new research

from Stockholm University on historical temperatures suggests past fluctuations were nearly twice as great as the hockey stick shows. That could mean the 20th-century jump isn't quite so anomalous.) seem to me be not only factual but precisely to the point of what the mainstream of science is discussing vis a vis MBH, which was the topic of that part of my story. For instance, in the Anderson/Woodhouse commentary that accompanied Moberg in the same issue of Nature, they too stress the increased variability just as I did and they make no mention of the late 1990s. And as per my email Monday, my article does also say that other reconstructions also indicate that the 20th Century was unusually warm and that the punch line is the same. Im sure youre fully sick of writing letters, but this may be right opportunity for a letter to the editor from you or someone who you can second. The person to send a letter to is [1]Karen.Pensiero@wsj.com. If you want, CC: me and my editor, [2]Elyse.tanouye@wsj.com. Or even an editorial on the broader topic of where the science is at. I can give you the name for who to send an editorial to if you want it. It is probably worth pointing out that no amount of debate can change the facts buried in those tree rings, etc.. Yes, I will continue to write about climate. The next topic is impacts. What do you think is the best story there? Id like to write about current impacts rather than only projected ones as these will be more tangible for the reader. Also, since the Arctic has been well covered Id be interested in impacts at lower latitudes. Antonio

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References

1. <mailto:Karen.Pensiero@wsj.com>
2. <mailto:Elyse.tanouye@wsj.com>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>
Subject: Re: B8 - REMINDER
Date: Mon Feb 21 14:27:10 2005
Cc: Valerie.Masson@cea.fr

we need to sort out budget - I have received no response from Eystein re rethinking - can not judge other WPs but suspect too much going into modelling /simple modelling . We would rather inflate request now and rethink (with wider evidence) later. We need another million from other WPs .

Keith

At 14:04 21/02/2005, you wrote:

Dear All,

I still miss the B8 section from

WP1 (Keith)

WP4 (Simon)

WP6 (Eduardo)

WP7 (Johann)

WP8 (Viv)

As you know very well time is running short. Please send me the missing B8 no later than Wednesday (23 February). If you cannot meet this already overdue deadline please let me know.

For details, see below.

Cheers,

Øyvind

B.8 Detailed implementation plan - first 18 months

MAX 40 PAGES

This section describes in detail the work planned to achieve the objectives of the proposed project up to its first 18 months in operation. The recommended length, excluding the forms specified below, is up to 15 pages. An introduction should explain the structure of this 18-month detailed implementation plan and how the plan will lead the participants to achieve the objectives aimed for by that time. It should also identify significant risks and contingency plans for these. The plan must be broken down into work packages (WPs) which should follow the logical phases of the project during this period, and include management of the project and assessment of progress and results to this point. Essential elements of the plan are:

- a) Detailed implementation plan introduction - explaining the structure of this plan and the overall methodology used to achieve the objectives of the first 18 months. Include a version of the form A3 which is used in Part A of the proposal, but covering just the first 18 months

b) Work planning, showing the timing of the different WPs and their tasks (Gantt chart or similar)

WP and Task leaders: Provide input (Max 4 pages per WP) with detail of plans including milestones and key deliverables

c) Graphical presentation of the components, showing their interdependencies (Pert diagram or similar)

d) Detailed work description broken down into work packages: Work package list (use work package list form below);

Deliverables list (use Deliverables list form below);

Description of each work package (use work package description form below, one per work package):

Note: The number and structure of work packages used must be appropriate to the complexity of the work and the overall value of the proposed project. Each work package should be a major subdivision of the proposed project and should also have a verifiable end-point (normally a deliverable or an important milestone in the overall project).

The planning should be sufficiently detailed to justify the proposed effort and allow progress monitoring by the Commission - the day-to-day management of the project by the consortium may require a more detailed plan.

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Fwd: Re: Canadians and the Millennium
Date: Mon Feb 21 15:35:44 2005

Mike,

FYI only - here is a reply from Francis. He's still onsite,
just stuck learning French.

Cheers

Phil

X-Mailer: QUALCOMM Windows Eudora Version 6.2.1.2

Date: Mon, 21 Feb 2005 07:14:34 -0800

To: Phil Jones <p.jones@uea.ac.uk>

From: Francis Zwiers <Francis.Zwiers@ec.gc.ca>

Subject: Re: Canadians and the Millennium

Cc: "francis.zwiers@ec.gc.ca" <francis.zwiers@ec.gc.ca>

X-UEA-MailScanner-Information: Please contact the ISP for more information

X-UEA-MailScanner: Found to be clean

Hi Phil,

At 02:29 21/02/2005, you wrote:

Francis,

Been away for the last week and off again tomorrow for the rest of this week. I was surprised to see comments from you in WSJ saying that McIntyre and McKittrick were likely right and the Mann reconstruction is wrong. I hope it is a case of misreporting !

Well, this isn't what I said, and its also not what is reported in the WJS article. The article quotes me as saying that the technique preferentially produces hockey sticks (actually, I **think** I said that it preferentially produces PC1s with hockey stick shapes, but that's a distinction that may have escaped the reporter - or I may have miss-spoken). In any case, this does not mean that the general form of the reconstruction (illustrating the unusual nature of the 20th century) is wrong - and I went to pains in the interview to also make that point.

The nearest composite reconstruction to MM in the 15th century is MBH98. All the others have the 15th century cooler than MBH98. There is no way MM are right in the 15th century. Also Moberg et al (2005) has too much long-term variability.

Sorry for the short email, I have loads of others to go through before

the end of today. We can discuss in more detail at Duke !

Unfortunately, I won't be at Duke because I'm still stuck in a particular type of Canadian purgatory called french training.

Cheers, Francis

Cheers

Phil

Prof. Phil Jones

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Meteorological Service of Canada

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NR4 7TJ

UK

References

1. <http://www.cccma.bc.ec.gc.ca/>

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Fwd: CCNet: PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA
Date: Mon Feb 21 16:28:32 2005
Cc: "raymond s. bradley" <rbradley@geo.umass.edu>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>

Mike, Ray and Malcolm,

The skeptics seem to be building up a head of steam here ! Maybe we can use this to our advantage to get the series updated !

Odd idea to update the proxies with satellite estimates of the lower troposphere rather than surface data !. Odder still that they don't realise that Moberg et al used the Jones and Moberg updated series !

Francis Zwiers is still inside. He said that PC1s produce hockey sticks. He stressed that the late 20th century is the warmest of the millennium, but Regaldo didn't bother with that. Also ignored Francis' comment about all the other series looking similar to MBH.

The IPCC comes in for a lot of stick.

Leave it to you to delete as appropriate !

Cheers

Phil

PS I'm getting hassled by a couple of people to release the CRU station temperature data. Don't any of you three tell anybody that the UK has a Freedom of Information Act !

X-Sender: f023@pop.uea.ac.uk

X-Mailer: QUALCOMM Windows Eudora Version 6.1.0.6

Date: Mon, 21 Feb 2005 15:40:05 +0000

To: p.jones@uea.ac.uk

From: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Fwd: CCNet: PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA

Subject: CCNet: PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA

Date: Mon, 21 Feb 2005 15:02:37 -0000

X-MS-Has-Attach:

X-MS-TNEF-Correlator:

Thread-Topic: pressure grows on climate modellers to relase secret data

Thread-Index: AcUXiV64e/f3Ii8uQSa0X88pndSQgQAI2O1w

From: "Peiser, Benny" <B.J.Peiser@livjm.ac.uk>

To: "cambridge-conference" <cambridge-conference@livjm.ac.uk>

X-UEA-MailScanner-Information: Please contact the ISP for more information

X-UEA-MailScanner: Found to be clean

CCNet 22/2005 - 21 February 2005

PRESSURE GROWING ON CONTROVERSIAL RESEARCHER TO DISCLOSE SECRET DATA

This should have produced a healthy scientific debate. Instead, Mr. Mann tried to shut down debate by refusing to disclose the mathematical algorithm by which he arrived at his conclusions. All the same, Mr. Mann was forced to publish a retraction of some of his initial data, and doubts about his statistical methods have since grown.

--The Wall Street Journal, 18 February 2005

But maybe we are in that much trouble. The WSJ highlights what Regaldo and McIntyre says is Mann's resistance or outright refusal to provide to inquiring minds his data, all details of his statistical analysis, and his code. So this is what I say to Dr. Mann and others expressing deep concern over peer review: give up your data, methods and code freely and with a smile on your face.

--Kevin Vranes, Science Policy, 18 February 2005

Mann's work doesn't meet that definition [of science], and those who use Mann's curve in their arguments are not making a scientific argument. One of Pournelle's Laws states "You can prove anything if you can make up your data." I will now add another Pournelle's Law: "You can prove anything if you can keep your algorithms secret."

--Jerry Pournelle, 18 February 2005

The time has come to question the IPCC's status as the near-monopoly source of information and advice for its member governments. It is probably futile to propose reform of the present IPCC process. Like most bureaucracies, it has too much momentum and its institutional interests are too strong for anyone realistically to suppose that it can assimilate more diverse points of view, even if more scientists and economists were keen to join up. The rectitude and credibility of the IPCC could be best improved not through reform, but through competition.

--Steven F. Hayward, The American Enterprise Institute, 15 February 2005

(1) HOCKEY STICK ON ICE

The Wall Street Journal, 18 February 2005

(2) SCIENCE AND OPEN ALGORITHMS: "YOU CAN PROVE ANYTHING WITH SECRET DATA AND ALGORITHMS"

Jerry Pournell, 18 February 2005

(3) OPEN SEASON ON HOCKEY AND PEER REVIEW

Science Policy, 18 February 2005

(4) CLIMATE CHANGE SCIENCE: TIME FOR TEAM "B"?

The American Enterprise Institute, 15 February 2005

(5) BRING THE PROXIES UP TO DATE!

Climate Audit, 20 February 2005

(6) CARELESS SCIENCE COSTS LIVES

The Guardian, 18 February 2005

(7) RE: MORE TROUBLE FOR CLIMATE MODELS

Helen Krueger <hkrueger@sbcglobal.net>

(8) HOW TO HANDLE ASTEROID 2004 MN4

Jens Kieffer-Olsen <dstdba@post4.tele.dk>

(9) AND FINALLY: EUROPE FURTHER FALLING BEHIND IN TECHNOLOGY AND RESEARCH

EU Observer, 10 February 2005

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(1) HOCKEY STICK ON ICE

The Wall Street Journal, 18 February 2005

[1]http://online.wsj.com/article_email/0,,SB110869271828758608-IdjeoNmlah4n5yta4GHaqyIm4,00.html

On Wednesday National Hockey League Commissioner Gary Bettman canceled the season, and we guess that's a loss. But this week also brought news of something else that's been put on ice. We're talking about the "hockey stick."

Just so we're clear, this hockey stick isn't a sports implement; it's a scientific graph. Back in the late 1990s, American geoscientist Michael Mann published a chart that purported to show average surface temperatures in the Northern Hemisphere over the past

1,000 years. The chart showed relatively minor fluctuations in temperature over the first 900 years, then a sharp and continuous rise over the past century, giving it a hockey-stick shape.

Mr. Mann's chart was both a scientific and political sensation. It contradicted a body of scientific work suggesting a warm period early in the second millennium, followed by a "Little Ice Age" starting in the 14th century. It also provided some visually arresting scientific support for the contention that fossil-fuel emissions were the cause of higher temperatures. Little wonder, then, that Mr. Mann's hockey stick appears five times in the Intergovernmental Panel on Climate Change's landmark 2001 report on global warming, which paved the way to this week's global ratification -- sans the U.S., Australia and China -- of the Kyoto Protocol.

Yet there were doubts about Mr. Mann's methods and analysis from the start. In 1998, Willie Soon and Sallie Baliunas of the Harvard-Smithsonian Center for Astrophysics published a paper in the journal *Climate Research*, arguing that there really had been a Medieval warm period. The result: Messrs. Soon and Baliunas were treated as heretics and six editors at *Climate Research* were made to resign.

Still, questions persisted. In 2003, Stephen McIntyre, a Toronto minerals consultant and amateur mathematician, and Ross McKittrick, an economist at Canada's University of Guelph, jointly published a critique of the hockey stick analysis. Their conclusion: Mr. Mann's work was riddled with "collation errors, unjustifiable truncations of extrapolation of source data, obsolete data, geographical location errors, incorrect calculations of principal components, and other quality control defects." Once these were corrected, the Medieval warm period showed up again in the data.

This should have produced a healthy scientific debate. Instead, as the *Journal's* Antonio Regalado reported Monday, Mr. Mann tried to shut down debate by refusing to disclose the mathematical algorithm by which he arrived at his conclusions. All the same, Mr. Mann was forced to publish a retraction of some of his initial data, and doubts about his statistical methods have since grown. Statistician Francis Zwiers of Environment Canada (a government agency) notes that Mr. Mann's method "preferentially produces hockey sticks when there are none in the data." Other reputable scientists such as Berkeley's Richard Muller and Hans von Storch of Germany's GKSS Center essentially agree. We realize this may all seem like so much academic nonsense. Yet if there really was a Medieval warm period (we draw no conclusions), it would cast some doubt on the contention that our SUVs and air conditioners, rather than natural causes, are to blame for apparent global warming.

There is also the not-so-small matter of the politicization of science: If climate scientists feel their careers might be put at risk by questioning some orthodoxy, the inevitable result will be bad science. It says something that it took two non-climate scientists to bring Mr. Mann's errors to light.

But the important point is this: The world is being lobbied to place a huge economic bet -- as much as \$150 billion a year -- on the notion that man-made global warming is real. Businesses are gearing up, at considerable cost, to deal with a new regulatory environment; complex carbon-trading schemes are in the making. Shouldn't everyone look very carefully, and honestly, at the science before we jump off this particular cliff?

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(2) SCIENCE AND OPEN ALGORITHMS: "YOU CAN PROVE ANYTHING WITH SECRET DATA AND ALGORITHMS"

Jerry Pournell, 18 February 2005

[2]<http://www.jerrypournelle.com/view/view349.html#hockeystick>

Science and Open Algorithms: You can prove anything with secret data and algorithms.

There is a long piece on the global "hockey stick" in today's Wall Street Journal that explains something I didn't understand: Mann, who generated the "hockey stick" curve purporting to show that the last century was unique in all recorded history with its sharp climb in temperature, has released neither the algorithm that generated his curve nor the data on which it was based.

I had refrained from commenting on the "hockey stick" because I couldn't understand how it was derived. I've done statistical analysis and prediction from uncertainty much of my life. My first job in aerospace was as part of the Human Factors and Reliability Group at Boeing, where we were expected to deal with such matters as predicting component failures, and deriving maintenance schedules (replace it before it fails, but not so long before it fails that the costs including the cost of the maintenance crew and the costs of taking the airplane out of service are prohibitive) and other such matters. I used to live with Incomplete Gamma Functions and other complex integrals; and I could not for the life of me understand how Mann derived his famous curve. Now I know: he hasn't told anyone. He says that telling people how he generated it would be tantamount to giving in to his critics.

More on this after my walk, but the one thing we may conclude for sure is that this is not science. His curve has been distributed as part of the Canadian government's literature on why Canada supports Kyoto, and is said to have been influential in causing the "Kyoto Consensus" so it is certainly effective propaganda; but IT IS NOT SCIENCE. Science deals with repeatability and openness. When I took Philosophy of Science from Gustav Bergmann at the University of Iowa a very long time ago, our seminar came to a one-sentence "practical definition" of science: Science is what you can put in a letter to a colleague and he'll get the same results you did. Now I don't claim that as original for it wasn't even me who came up with it in the seminar; but I do claim Bergmann liked that formulation, and it certainly appealed to me, and I haven't seen a better one-sentence practical definition of science. Mann's work doesn't meet that definition, and those who use Mann's curve in their arguments are not making a scientific argument.

One of Pournelle's Laws states "You can prove anything if you can make up your data." I will now add another Pournelle's Law: "You can prove anything if you can keep your algorithms secret."

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(3) OPEN SEASON ON HOCKEY AND PEER REVIEW

Science Policy, 18 February 2005

[3]http://sciencepolicy.colorado.edu/prometheus/archives/climate_change/000355open_season_on_hocke.html

By Kevin Vranes

The recent 2/14 WSJ article ("Global Warring..." by Antonio Regaldo) addresses the debate that most readers of this site are well familiar with: the Mann et al. hockey stick. The WSJ is still asking - and trying to answer - the basic questions: hockey stick or no hockey stick? But the background premise of the article, stated explicitly and implicitly throughout, is that it was the hockey stick that led to Kyoto and other climate policy. Is it?

I think it's fair to say that to all of us in the field of climatology, the notion that Kyoto is based on the Mann curve is utter nonsense. If a climatologist, or a policy advisor charged with knowing the science well enough to make astute recommendations to his/her boss, relied solely on the Mann curve to prove definitively the existence of anthropogenic warming, then we're in deeper trouble than anybody realizes. (This is essentially what Stephan Ramstorf writes in a 1/27 RealClimate post.) And although it's easy to believe that national and international policy can hinge on single graphs, I

hope we give policy makers more credit than that.

But maybe we are in that much trouble. The WSJ highlights what Regaldo and McIntyre says is Mann's resistance or outright refusal to provide to inquiring minds his data, all details of his statistical analysis, and his code. The WSJ's anecdotal treatment of the subject goes toward confirming what I've been hearing for years in climatology circles about not just Mann, but others collecting original climate data.

As concerns Mann himself, this is especially curious in light of the recent RealClimate posts ([link](#) and [link](#)) in which Mann and Gavin Schmidt warn us about peer review and the limits therein. Their point is essentially that peer review is limited and can be much less than thorough. One assumes that they are talking about their own work as well as McIntyre's, although they never state this. Mann and Schmidt go to great lengths in their post to single out Geophysical Research Letters. Their post then seems a bit ironic, as GRL is the journal in which the original Mann curve was published (1999, vol 26., issue 6, p. 759), an article which is now receiving much attention as being flawed and under-reviewed. (For that matter, why does Table 1 in Mann et al. (1999) list many chronologies in the Southern Hemisphere while the rest of the paper promotes a Northern Hemisphere reconstruction? Legit or not, it's a confusing aspect of the paper that should never have made it past peer review.)

Of their take on peer review, I couldn't agree more. In my experience, peer review is often cursory at best. So this is what I say to Dr. Mann and others expressing deep concern over peer review: give up your data, methods and code freely and with a smile on your face. That is real peer review. A 12 year-old hacker prodigy in her grandparents' basement should have as much opportunity to check your work as a "semi-retired Toronto minerals consultant." Those without three letters after their name can be every bit as intellectually qualified, and will likely have the time for careful review that typical academic reviewers find lacking.

Specious analysis of your work will be borne out by your colleagues, and will enter the debate with every other original work. Your job is not to prevent your critics from checking your work and potentially distorting it; your job is to continue to publish insightful, detailed analyses of the data and let the community decide. You can be part of the debate without seeming to hinder access to it.

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(4) CLIMATE CHANGE SCIENCE: TIME FOR TEAM "B"?

The American Enterprise Institute, 15 February 2005

[4]http://www.aei.org/publications/pubID.21974/pub_detail.asp

By Steven F. Hayward

The Intergovernmental Panel on Climate Change (IPCC) is currently working on its fourth assessment report. Despite the IPCC's noble intent to generate a scientific consensus, a number of factors have compromised the research and drafting process, assuring that its next assessment report will be just as controversial as previous reports in 1995 and 2001. Efforts to reform this large bureaucratic effort are unlikely to succeed. Perhaps the time has come to consider competition as the means of checking the IPCC's monopoly and generating more reliable climate science.

As the Intergovernmental Panel on Climate Change (IPCC) moves toward the release of its fourth assessment report (fourth AR) in 2007, the case of Chris Landsea offers in microcosm an example of why the IPCC's findings are going to have credibility problems. Last month Landsea, a climate change scientist with the U.S. National Oceanic and Atmospheric Administration (NOAA), resigned as a participant in the producing the report. Landsea had been a chapter author and reviewer for the IPCC's second assessment report in 1995 and the third in 2001, and he is a leading expert on hurricanes and related extreme weather phenomena. He had signed on with the IPCC to update the state of

current knowledge on Atlantic hurricanes for the fourth report. In an open letter, Landsea wrote that he could no longer in good conscience participate in a process that is "being motivated by pre-conceived agendas" and is "scientifically unsound." [1] Landsea's resignation was prompted by an all too familiar occurrence: The lead author of the fourth AR's chapter on climate observations, Kevin Trenberth, participated in a press conference that warned of increasing hurricane activity as a result of global warming. [2] It is common to hear that man-made global warming represents the "consensus" of science, yet the use of hurricanes and cyclones as a marker of global warming represents a clear-cut case of the consensus being roundly ignored. Both the second and third IPCC assessments concluded that there was no global warming signal found in the hurricane record. Moreover, most climate models predict future warming will have only a small effect--if any--on hurricane strength. "It is beyond me," Landsea wrote, "why my colleagues would utilize the media to push an unsupported agenda that recent hurricane activity has been due to global warming." [3] Landsea's critique goes beyond a fit of pique at the abuse of his area of expertise. The IPCC, he believes, has become thoroughly politicized, and is unresponsive to criticism. "When I have raised my concerns to the IPCC leadership," Landsea wrote, "their response was simply to dismiss my concerns." [4]

Landsea's frustration is not an isolated experience. MIT physicist Richard Lindzen, another past IPCC author who is not participating in the fourth report, has written: "My experiences over the past 16 years have led me to the discouraging conclusion that we are dealing with the almost insoluble interaction of an iron triangle with an iron rice bowl." (Lindzen's "iron triangle" consists of activists misusing science to get the attention of the news media and politicians; the "iron rice bowl" is the parallel phenomenon where scientists exploit the activists' alarm to increase research funding and attention for the issue. [5]) And Dr. John Zillman, one of Australia's leading climate scientists, is another ex-IPCC participant who believes the IPCC has become "cast more in the model of supporting than informing policy development." [6] And when the IPCC is not ignoring its responsible critics like Landsea and Lindzen, it is demonizing them. Not long ago the IPCC's chairman, Dr. Rajendra Pachauri, compared eco-skeptic Bjorn Lomborg to Hitler. "What is the difference between Lomborg's view of humanity and Hitler's?" Pachauri asked in a Danish newspaper. "If you were to accept Lomborg's way of thinking, then maybe what Hitler did was the right thing." [7] Lomborg's sin was merely to follow the consensus practice of economists in applying a discount to present costs for future benefits, and comparing the range of outcomes with other world problems alongside climate change. It is hard to judge what is worse: Pachauri's appalling judgment in resorting to *reductio ad Hitlerum*, or his abysmal ignorance of basic economics. In either case, it is hard to have much confidence in the policy advice the IPCC might have. [...]

Time for "Team B"?

The time has come to question the IPCC's status as the near-monopoly source of information and advice for its member governments. It is probably futile to propose reform of the present IPCC process. Like most bureaucracies, it has too much momentum and its institutional interests are too strong for anyone realistically to suppose that it can assimilate more diverse points of view, even if more scientists and economists were keen to join up. The rectitude and credibility of the IPCC could be best improved not through reform, but through competition....

FULL PAPER at [5]http://www.aei.org/publications/pubID.21974/pub_detail.asp

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(5) BRING THE PROXIES UP TO DATE!

Climate Audit, 20 February 2005

[6]<http://www.climateaudit.org/index.php?p=89#more-89>

Steve McIntyre

I will make here a very simple suggestion: if IPCC or others want to use "multiproxy" reconstructions of world temperature for policy purposes, stop using data ending in 1980 and bring the proxies up-to-date. Let's see how they perform in the warm 1990s - which should be an ideal period to show the merit of the proxies. I do not believe that any responsible policy-maker can base policy, even in part, on the continued use of obsolete data ending in 1980, when the costs of bringing the data up-to-date is inconsequential compared to Kyoto costs.

I would appreciate comments on this note as I think that I will pursue the matter with policymakers.

For example, in Mann's famous hockey stick graph, as presented to policymakers and to the public, the graph used Mann's reconstruction from proxies up to 1980 and instrumental temperatures (here, as in other similar studies, using Jones' more lurid CRU surface history rather than the more moderate increases shown by satellite measurements). Usually (but not always), a different color is used for the instrumental portion, but, from a promotional point of view, the juxtaposition of the two series achieves the desired promotional effect. (In mining promotions, where there is considerable community experience with promotional graphics and statistics, securities commission prohibit the adding together of proven ore reserves and inferred ore reserves - a policy which deserves a little reflection in the context of IPCC studies).

Last week, a brand new multiproxy study by European scientists [Moberg et al., 2005] was published in Nature. On the very day of publication, I received an email from a prominent scientist telling me that Mann's hockeystick was yesterday's news, that the "community" had now "moved on" and so should I. That the "community" had had no opportunity to verify Moberg's results, however meritorious they may finally appear, seemed to matter not at all.

If you look at the proxy portion of the new Moberg graphic, you see nothing that would be problematic for opponents of the hockey stick: it shows a striking Medieval Warm Period (MWP), a cold Little Ice Age and 20th century warming not quite reaching MWP levels by 1979, when the proxy portion of the study ends. (I'm in the process of examining the individual proxies and the Moberg reconstruction is not without its own imperfections.) In the presentation to the public - see the figure in the Nature article itself, once again, there is the infamous splice between reconstruction by proxy (up to 1980) and the instrumental record thereafter (once again Jones' CRU record, rather than the satellite record).

One of the first question that occurs to any civilian becoming familiar with these studies (and it was one of my first questions) is: what happens to the proxies after 1980? Given the presumed warmth of the 1990s, and especially 1998 (the "warmest year in the millennium"), you'd think that the proxy values would be off the chart. In effect, the last 25 years have provided an ideal opportunity to validate the usefulness of proxies and, especially the opportunity to test the confidence intervals of these studies, put forward with such assurance by the multiproxy proponents. What happens to the proxies used in MBH99 or Moberg et al [2005] or Crowley and Lowery [2000] in the 1990s and, especially, 1998?

This question about proxies after 1980 was posed by a civilian to Mann in December at realclimate. Mann replied:

Most reconstructions only extend through about 1980 because the vast majority of tree-ring, coral, and ice core records currently available in the public domain do not extend into the most recent decades. While paleoclimatologists are attempting to update many important proxy records to the present, this is a costly, and labor-intensive

activity, often requiring expensive field campaigns that involve traveling with heavy equipment to difficult-to-reach locations (such as high-elevation or remote polar sites). For historical reasons, many of the important records were obtained in the 1970s and 1980s and have yet to be updated. [my bold]

Pause and think about this response. Think about the costs of Kyoto and then think again about this answer. Think about the billions spent on climate research and then try to explain to me why we need to rely on "important records" obtained in the 1970s. Far more money has been spent on climate research in the last decade than in the 1970s. Why are we still relying on obsolete proxy data?

As someone with actual experience in the mineral exploration business, which also involves "expensive field campaigns that involve traveling with heavy equipment to difficult-to-reach locations", I can assure readers that Mann's response cannot be justified and is an embarrassment to the paleoclimate community. The more that I think about it, the more outrageous is both the comment itself and the fact that no one seems to have picked up on it.

It is even more outrageous when you look in detail at what is actually involved in collecting the proxy data used in the medieval period in the key multiproxy studies. The number of proxies used in MBH99 is from fewer than 40 sites (28 tree ring sites being U.S. tree ring sites represented in 3 principal component series).

As to the time needed to update some of these tree ring sites, here is an excerpt from Lamarche et al. [1984] on the collection of key tree ring cores from Sheep Mountain and Campito Mountain, which are the most important indicators in the MBH reconstruction: "D.A.G. [Graybill] and M.R.R. [Rose] collected tree ring samples at 3325 m on Mount Jefferson, Toquima Range, Nevada and 11 August 1981. D.A.G. and M.R.R. collected samples from 13 trees at Campito Mountain (3400 m) and from 15 trees at Sheep Mountain (3500 m) on 31 October 1983."

Now to get to Campito Mountain and Sheep Mountain, they had to get to Bishop, California, which is hardly "remote" even by Paris Hilton standards, and then proceed by road to within a few hundred meters of the site, perhaps proceeding for some portion of the journey on unpaved roads.

The picture below illustrates the taking of a tree ring core. While the equipment may seem "heavy" to someone used only to desk work using computers, people in the mineral exploration business would not regard this drill as being especially "heavy" and I believe that people capable of operating such heavy equipment can be found, even in out-of-the way places like Bishop, California. I apologize for the tone here, but it is impossible for me not to be facetious.

There is only one relatively remote site in the entire MBH99 roster - the Quelccaya glacier in Peru. Here, fortunately, the work is already done (although, needless to say, it is not published.) This information was updated in 2003 by Lonnie Thompson and should be adequate to update these series. With sufficient pressure from the U.S. National Science Foundation, the data should be available expeditiously. (Given that Thompson has not archived data from Dundee drilled in 1987, the need for pressure should not be under-estimated.)

I realize that the rings need to be measured and that the field work is only a portion of the effort involved. But updating 28 tree ring sites in the United States is not a monumental enterprise nor would updating any of the other sites.

I've looked through lists of the proxies used in Jones et al. [1998], MBH99, Crowley and Lowery [2000], Mann and Jones [2003], Moberg et al [2005] and see no obstacles to bringing all these proxies up to date. The only sites that might take a little extra time would be updating the Himalayan ice cores. Even here, it's possible that taking very short cores or even pits would prove adequate for an update and this might prove

easier than one might be think. Be that as it may, any delays in updating the most complicated location should not deter updating all the other locations.

As far as I'm concerned, this should be the first order of business for multiproxy studies.

Whose responsibility is this? While the costs are trivial in the scheme of Kyoto, they would still be a significant line item in the budget of a university department. I think that the responsibility here lies with the U.S. National Science Foundation and its equivalents in Canada and Europe. The responsibilities for collecting the proxy updates could be divided up in a couple of emails and budgets established.

One other important aspect: right now the funding agencies fund academics to do the work and are completely ineffective in ensuring prompt reporting. At best, academic practice will tie up reporting of results until the publication of articles in an academic journals, creating a delay right at the start. Even then, in cases like Thompson or Jacoby, to whom I've referred elsewhere, the data may never be archived or only after decades in the hands of the originator.

So here I would propose something more like what happens in a mineral exploration program. When a company has drill results, it has to publish them through a press release. It can't wait for academic reports or for its geologists to spin the results. There's lots of time to spin afterwards. Good or bad - the results have to be made public. The company has a little discretion so that it can release drill holes in bunches and not every single drill hole, but the discretion can't build up too much during an important program. Here I would insist that the proxy results be archived as soon as they are produced - the academic reports and spin can come later. Since all these sites have already been published, people are used to the proxies and the updates will to a considerable extend speak for themselves.

What would I expect from such studies? Drill programs are usually a surprise and maybe there's one here. My hunch is that the classic proxies will not show anywhere near as "loud" a signal in the 1990s as is needed to make statements comparing the 1990s to the Medieval Warm Period with any confidence at all. I've not surveyed proxies in the 1990s (nor to my knowledge has anyone else), but I've started to look and many do not show the expected "loud" signal e.g. some of the proxies posted up on this site such as Alaskan tree rings, TTHH ring widths, and theories are starting to develop. But the discussions so far do not explicit point out the effect of signal failure on the multiproxy reconstruction project.

But this is only a hunch and the evidence could be otherwise. The point is this: there's no need to speculate any further. It's time to bring the classic proxies up to date.

=====

(6) CARELESS SCIENCE COSTS LIVES

The Guardian, 18 February 2005

[7]<http://www.guardian.co.uk/comment/story/0,3604,1417224,00.html>

Dick Taverne

In science, as in much of life, it is believed that you get what you pay for. According to opinion polls, people do not trust scientists who work for industry because they only care about profits, or government scientists because they suspect them of trying to cover up the truth. Scientists who work for environmental NGOs are more highly regarded. Because they are trying to save the planet, people are ready to believe that what they say must be true. A House of Lords report, Science and Society, published in 2000, agreed that motives matter. It argued that science and scientists are not value-free, and therefore that scientists would command more trust "if they openly declare the values that underpin their work".

It all sounds very plausible, but mostly it is wrong. Scientists with the best of

motives can produce bad science, just as scientists whose motives may be considered suspect can produce good science. An obvious example of the first was Rachel Carson, who, if not the patron saint, was at least the founding mother of modern environmentalism. Her book *The Silent Spring* was an inspiring account of the damage caused to our natural environment by the reckless spraying of pesticides, especially DDT.

However, Carson also claimed that DDT caused cancer and liver damage, claims for which there is no evidence but which led to an effective worldwide ban on the use of DDT that is proving disastrous. Her motives were pure; the science was wrong. DDT is the most effective agent ever invented for preventing insect-borne disease, which, according to the US National Academy of Sciences and the WHO, prevented over 50 million human deaths from malaria in about two decades. Although there is no evidence that DDT harms human health, some NGOs still demand a worldwide ban for that reason. Careless science cost lives.

Contrast the benefits that have resulted from the profit motive, a motive that is held to be suspect by the public. Multinationals, chief villains in the demonology of contemporary anti-capitalists, have developed antibiotics, vaccines that have eradicated many diseases like smallpox and polio, genetically modified insulin for diabetics, and plants such as GM insect-resistant cotton that have reduced the need for pesticides and so increased the income and improved the health of millions of small cotton farmers. The fact is that self-interest can benefit the public as effectively as philanthropy.

Motives are not irrelevant, and unselfish motives are rightly admired more than selfish ones. There are numerous examples of misconduct by big companies, and we should examine their claims critically and provide effective regulation to control abuses of power and ensure the safety of their products. Equally, we should not uncritically accept the claims of those who act from idealistic motives. NGOs inspired by the noble cause of protecting our environment often become careless about evidence and exaggerate risks to attract attention (and funds). Although every leading scientific academy has concluded that GM crops are at least as safe as conventional foods, this does not stop Greenpeace reiterating claims about the dangers of "Frankenfoods". Stephen Schneider, a climatologist, publicly justified distortion of evidence: "Because we are not just scientists but human beings as well ... we need to ... capture the public imagination ... So we have to offer up scary scenarios, make simplified dramatic statements, and make little mention of any doubts we have."

But in the end motives are irrelevant to the validity of science. It does not matter if a scientist wants to help mankind, get a new grant, win a Nobel prize or increase the profits of her company. It does not matter whether a researcher works for Monsanto or for Greenpeace. Results are no more to be trusted if the researcher declares his values and confesses that he beats his wife, believes in God, or is an Arsenal supporter. What matters is that the work has been peer-reviewed, that the findings are reproducible and that they last. If they do, they are good science. If not, not. Science itself is value-free. There are objective truths in science. We can now regard it as a fact that the Earth goes round the sun and that Darwinism explains the evolution of species. A look at the history of science makes it evident how irrelevant the values of scientists are. Newton's passion for alchemy did not invalidate his discovery of the laws of gravitation. To quote Professor Fox of Rutgers University: "How was it relevant to Mendel's findings about peas that he was a white, European monk? They would have been just as valid if Mendel had been a Spanish-speaking, lesbian atheist."

· Lord Taverne is chair of Sense About Science and author of *The March of Unreason*, to be published next month

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===== LETTERS =====

(7) RE: MORE TROUBLE FOR CLIMATE MODELS

Helen Krueger <hkrueger@sbcglobal.net>

Dear Dr. Peiser,

I just want to let you know how much I am enjoying being included in your list so that I can benefit from your astute handling of alarmist information personally and with my students.

Thank you so much!

Regards,

Helen A. Krueger

Educational Consultant

Phone: 203-426-8043

FAX: 203-426-3541

=====

(8) HOW TO HANDLE ASTEROID 2004 MN4

Jens Kieffer-Olsen <dstdba@post4.tele.dk>

Dear Benny Peiser,

In CCNet 18/2005 - 11 February 2005 you brought an interesting article on the possible breakup of NEA 2004 MN4 in the year 2029:

> But there's another reason for concern. According to Dan
> Durda, another SWRI astronomer, 2004 MN4 is likely to be
> a "rubble-pile" asteroid, consisting of material only
> loosely held together by gravity. Because the asteroid
> will pass us at just 2.5 times Earth's diameter, tidal
> forces could tear it apart. The result would be a trail
> of rocks drifting slowly apart with the passage of time.
> One or more of these might hit Earth in the more distant
> future, creating a spectacular fireball as it burns up
> in the atmosphere.

> --Bill Cooke, Astronomy Magazine, 10 February 2005

First of all, a 300m asteroid could break into 100 pieces each larger than the Tunguska impactor. Secondly, the years for which a TS rating of 1 already exist for the object are NOT in the distant future, but 6, 7, and 8 years later.

That reminds us that neither the Torino nor the Palermo scale takes into account the possibility of such a MIRV'ed approach. Furthermore, the Palermo scale is designed to take into account the lead time. Even if 2004 MN4 were not to break up, the lead time to virtual impact in 2029 would be down to one sixth of the time to-day. In other words, if the post-2029 orbit is not being resolved before then, we may as well up the PS rating accordingly. If my math is correct, we should add 0.78 to its Palermo Scale rating, ie. $\log_{10}(6)$, for a total of -0.65.

Yours sincerely

Jens Kieffer-Olsen, M.Sc.(Elec.Eng.)

Slagelse, Denmark

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(9) AND FINALLY: EUROPE FURTHER FALLING BEHIND IN TECHNOLOGY AND RESEARCH

EU Observer, 10 February 2005

[8]<http://www.euobserver.com/?aid=18382&print=1>

By Lucia Kubosova

BRUSSELS / EUOBSERVER - Europeans are still failing to show world leadership in technology and research, a new report shows.

The paper, published on Thursday (10 February) has evaluated the EU research and development programmes and their impact on Europe's knowledge-base and potential for innovation.

While it argues that EU funds for the programmes make a "major contribution", it suggests that more resources, industry participation and simplified administration are needed for them to have a greater effect in future.

"We have somehow lost momentum", said Erkki Ormala, chair of the panel issuing the report.

"The EU is falling behind. And we are now under pressure not only compared to our traditional rivals like the US or Japan, but also China, India or Brazil. We are facing a much tougher competition in talent and knowledge than we are used to".

Research Commissioner Janez Potocnik considers the paper's results as a reason for doubling the funds in his portfolio within the next budgetary period of 2007-2013.

"We don't want to achieve our economic growth by lowering the social or environmental standards. So to compete globally, we need to focus on knowledge", Mr Potocnik said to journalists, adding that the EU programmes should "make a bridge between practical innovation and research".

The report has listed several possible solutions for tackling outlined setbacks.

It argues that the EU must attract and reward the best talent, mobilise resources for innovation and boost cooperation between governments, businesses and universities in research.

It supports the idea of setting up a European Research Council to promote excellence and encourages more industry involvement, mainly on the part of small and medium-sized enterprises (SMEs).

However, SME representatives complain that their ideas about EU research and innovation funding are not taken into consideration.

"It's not about how big the budget is for SMEs and their involvement in such projects.

It is rather about the allocation of the funds. Most of them are granted for huge long-term projects which cost millions of euro and they can hardly attract smaller companies", according to Ullrich Schroeder, from UEAPME, the main umbrella organisation.

He argues that while several reports have already pointed out that SMEs must be more involved if the "Lisbon agenda" goal of 3 percent of GDP to be invested in research and development in the EU by 2010 is to be achieved, in reality they are not as well supported as huge transnational companies.

"It is not that the EU member states invest much less in universities than the US, but the greatest difference is that European SMEs are only investing 8% of the US amount, and it is simply not enough".

Mr Schroeder also said that while "there is a lot of rhetoric from politicians, that the SMEs should get involved, innovate and compete, when they come up with good projects, they are not sufficiently supported".

"The European Commission is more concerned about big companies and hightech areas, while innovation is needed also in more down-to earth sectors", Mr Schroeder told the EUobserver.

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8. <http://www.euobserver.com/?aid=18382&print=1>
9. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Valerie Masson-Delmotte <Valerie.Masson@cea.fr>
To: Hugues Goosse <hgs@astr.ucl.ac.be>
Subject: Re: B parts
Date: Tue, 22 Feb 2005 10:53:29 +0100
Reply-to: Valerie.Masson@cea.fr
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, imprint-ssc@bjerknes.uib.no, erick.larson@fa.uib.no, Beatriz Balino <beatriz.balino@bjerknes.uib.no>, loutre@astr.ucl.ac.be, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>
Dear Eystein,

Congratulations for a very convincing draft.

Please find attached the suggestions by Hubertus Fischer and myself for the parts B1 to B3.

Valerie.

</x-flowed>

Attachment Converted: "c:\eudora\attach\masson54.vcf"

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re:
Date: Thu, 24 Feb 2005 12:45:10 -0500
Cc: Phil Jones <p.jones@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>,
Caspar Ammann <ammann@ucar.edu>, "Wahl, Eugene R" <wahl@alfred.edu>,
Scott Rutherford <srutherford@rwu.edu>

Thanks Keith,
I've made these changes and a few very minor changes just to improve the grammar in places, etc. Also, I'm embarrassed to say that Scott's name was accidentally left out of the author list, so I've included that back in. There was one bit about the high-pass filtering and low-pass filtering which you changed, based on I think some minor confusion about what I meant. I've fixed that. I'm assuming that Tim will be ok w/ the attached, final version, so I'm going to go ahead and submit to Nature now. We'll have ample opportunity for revision at a later stage. Lets cross our fingers. Thanks again everyone,
Mike

At 11:01 AM 2/24/2005, Keith Briffa wrote:

Sorry Mike - still dashing - but attached shows some slight wording changes - only early and late - missed Track changes so just compare - sorry to mess up - otherwise go with this for now and lets see reaction
Keith
t 00:40 22/02/2005, you wrote:

Dear Phil et al,
All of the suggested changes have been made, and some others additional changes have been made for clarification, including descriptions of updated versions of the figures
(Scott: can you get to me pdf versions of figures 1 and 3 that have the correct "degrees" symbol on the y axis? Also--we need an updated url for the pseudoproxy data at fox.rwu.edu as noted! thanks in advance for getting back to me ASAP on these)
Changes indicated in yellow highlighting.
Will try to prepare a final draft for submission once I've heard back from Keith, Tim, and anyone else who has any remaining comments. I've also attached a draft cover letter to go to Nature along w/ the submission.
Thanks,
Mike

At 09:14 AM 2/21/2005, Phil Jones wrote:

Mike,

Here's a few modifications to the text. Keith and Tim are pretty happy with it

as well, but the'll reply as soon as they have some time.

Off again tomorrow to Chicago. Back in next week.

Happy for you to submit this as soon as you have their and other comments.

Cheers

Phil

At 22:44 12/02/2005, Michael E. Mann wrote:

sorry. text revised yet again. no more changes until I receive comments from everyone.

thanks...

mike

At 12:03 PM 2/11/2005, Phil Jones wrote:

Mike,

Keith and Tim are here next week, but very busy with a proposal to the EU.

So you may have to hassle them a bit, or hang on for a week or two.

Nature dragged in the IPCC angle which annoyed me. I tried to explain to

him how IPCC works. IPCC won't be discussing this in Beijing in May - except

as part of Chapter 6. Hans von Storch will likely regret some of the words he's said.

FYI, just as NCAR have put up a web site to give the whole story re Chris Landseas's

'resignation' from a CA in the atmos. obs. chapter (to help Kevin Trenberth out), KNMI

are doing the same re Rob van Dorland and that Dutch magazine. The chief scientist

at KNMI has got involved as Rob didn't say the things attributed to him. I'll find

out more on this in Pune as a guy from KNMI will be there.

Several other CAs on our chapter pulled out, or just didn't do anything. Their

stories

never got run.

Dick's report was good and my bit in Nature came across well.

Say hi to all there and wish Steve well.

Cheers

Phil

At 16:19 11/02/2005, Michael E. Mann wrote:

Phil--thanks, that's great. Really happy to hear that everyone is on board with this.

I'm at a symposium honoring Steve Schneider out at Stanford right now. Lots of folks

here--as I talk this over w/ them, and see Dick Kerr's coverage of this, etc. I realize

its not so bad--I was afraid this would be spun as bolstering the contrarians, but it hasn't. In large part due to quotes from you and others pointing out that the study actually reinforces the key conclusions, etc., and the fact Dick Kerr showed Keith and Tim's plot showing the scattering of multiple reconstructions, etc. which takes the focus off "Mann" a bit...
Nonetheless, I *am* convinced their methodology is suspect, as the analysis I sent shows. So I will really appreciate input from Keith, Tim, and you to make sure the language and wording are appropriate and fair...
I will revise as I get input from various people, with an aim to having this submission-ready in about 10 days (so you can have one final look after you return, and before you have to head out again).
looking forward to getting people's comments, feedback, etc.
thanks again,
mike

At 08:05 AM 2/11/2005, Phil Jones wrote:

Mike et al,
I've talked to Keith and Tim here and it seems best if we all come in with you on this response. What you have done is basically fine. We can discuss specific wording later.
My problem is that I'm off tomorrow to Pune till Feb 20 and email may be sporadic or non-existent. So can you discuss revised drafts with Keith and Tim, but keep me on - lower down as I'm away. I'm here on Feb 21 then off to Chicago to review the vertical temperature report for the NRC/NAS Feb 22-25.
Keep me on the emails in case email works well in Pune.

Cheers

Phil

At 23:35 10/02/2005, Michael E. Mann wrote:

Dear Caspar, Gene, Scott, Phil,
I am attaching a response I've drafted to the Moberg et al paper (attached for those of you who haven't seen it). The message is pretty clear and simple--their method overemphasizes the low-frequency variability. To demonstrate this, I've made use of stuff from Mann and Jones, and from the Mann/Rutherford/Wahl/Ammann J. Climate letter on Pseudoproxies. So I would welcome any of you to be co-authors on this--just let me now

if you're interested. I've been in touch w/ Keith (he and Tim are potentially working on their own independent response--waiting to hear further). This is a very rough draft, so comments much appreciated. Looking forward to hearing back,
Mike

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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[6]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

Attachment Converted: "c:\eudora\attach\MobergComment-final.doc"

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6. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Phil Jones <p.jones@uea.ac.uk>
To: Gabi Hegerl <hegerl@duke.edu>, Tom Crowley <tcrowley@duke.edu>, Gabi Hegerl <hegerl@duke.edu>, myles <m.allen1@physics.ox.ac.uk>, Tim Barnett <tbarnett-ul@ucsd.edu>, Nathan Gillett <gillett@ocean.seos.uvic.ca>, "Stott, Peter" <peter.stott@metoffice.com>, David Karoly <dkaroly@rossby.metr.ou.edu>, Reiner Schnur <schnur@dkrz.de>, Karl Taylor <taylor13@llnl.gov>, francis <francis.zwiers@ec.gc.ca>
Subject: Future Directions
Date: Tue Mar 1 08:40:42 2005

Dear All,

I've knocked Chris off this reply. There is a meeting of the CCDD program next week

in Asheville. I guess Chris wants something for this. I'm on the panel, so if you want to

add to

what Gabi and Tom have put together then let me know and I'll feed that in additionally to

what is already there.

From being at the review last week of the vertical temperature trends panel, the

issue of

reducing forcing uncertainties is important. A number of people think that agreement in

the

20th century is all doing to model tuning due to uncertain forcing with sulphates. How to

counter this is one area. One of my own pet areas is trying to reduce uncertainties in the

paleo record for the last millennium, but again this is one of convincing people that we

really

know what has happened. So much is being made of the paleo records, but are they that

important to detection when most of the work is going on with the 20th century records. Is

the

pre-20th century really that important when it comes to D&A?

Cheers

Phil

At 20:45 28/02/2005, Gabi Hegerl wrote:

Hi IDAG people,

Chris Miller needs some input on where detection is going and what should be funded,

appended is a list Tom and I sent him as rapid response, but it sounds like they are

still

in the process of thinking about

this, so please reply (soon) if you have additions/comments (Chris, only thought of

sending

this now, I hope results will be still helpful)

Gabi

1) extending detection to other fields, esp. U.S. possible variables are circulation, anything hydrological (drought, average rainfall), climate extremes, storms, all this is getting more feasible as observational data get better, reanalyses get more reliable (although trend sstill questionable), and models get better and have higher resolution

2) compiling "showable" scorecard of what has been detected in the system already

3) abrupt changes - Tom thinks the relevance has been overstated of past changes in the thermohaline circulation (because of proximity of massive amounts of ice/freshwater). However, I think it would still be useful to find a fingerprint of predictors for thermohaline shutdown (from waterhosing experiments), and establish how early warning signs can be detected. Another aprupt change that could be dealt with are events such as the mega drought cycles in the western U.S., which our preliminary work indicates does not correspond with multidecal peaks in warmth for zonal average temperatures.

4) using paleoclimate data for understanding regional responses to known forcings, such as pulse of volcanism in early 19th century. tests of a model's predictability on regional scales. this however would require ensemble runs and a fair amount of legwork, so probably would be best as a proposal than as an IDAG project.

5) more surface temperature detection as already donw, to keep analyzing 20th century from models as model diagnostic and evaluating how to get most model performance information out of this diagnostic. For this, updates of forcing estimates, particularly reduced sulfate aerosol uncertainties would be useful.

----- Original Message -----

Subject: Re: Directions in D&A
Date: Tue, 22 Feb 2005 10:51:56 -0500
From: Chris Miller <christopher.d.miller@noaa.gov>
Reply-To: christopher.d.miller@noaa.gov
Organization: NOAA
To: Gabi Hegerl <hegerl@duke.edu>
References: <4216317A.7020700@noaa.gov>
<421A4F67.1040201@duke.edu>

Gabi, I'm looking for some quick thoughts, which probably means just you and Tom.

Obviously, the rest of IDAG would have ideas but it would take some time to poll them (I could see it as an agenda item at the IDAG meeting). If you had a couple highlight items by Thursday morning, that would be helpful as I have an internal meeting where this will be discussed.
Thanks again, Chris
Gabi Hegerl wrote:

Chris, by when do you need this? From the whole IDAG or just, eg from me and Tom?
Gabi
Chris Miller wrote:

Tom, Gabi, As you are probably aware, one of the recurring challenges for federal program managers is to indicate to upper management what the science priorities in the future should be. NOAA is more future-looking than it has been in the past and we are now being called upon more frequently to respond to this question. A simplistic answer would be "more of the same" since we are doing such good work now. This could be part of the answer, but not the whole answer. NOAA is interested in new science thrusts, new observational programs or analyses, new institutional arrangements, etc. (the "new is better syndrome"). I would appreciate it if you could take a few minutes to think about this issue and send me a few bullets on where you think the community should be going on D&A, for both continuing and new investments (from the perspective of the work that IDAG has been involved in to date).
Thanks for your help and look forward to the next IDAG mtg.
Chris

-- ~~~~~
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#### References

1. <http://www.env.duke.edu/faculty/bios/hegerl.html>
2. <http://www.env.duke.edu/faculty/bios/hegerl.html>
3. <http://www.env.duke.edu/faculty/bios/hegerl.html>

From: "olgasolomina" <olgasolomina@yandex.ru>  
To: jto@u.arizona.edu, eystein.jansen@geo.uib.no, Valerie.Masson@cea.fr, k.briffa@uea.ac.uk  
Subject: Glacier box - comments and suggestions  
Date: Sun, 6 Mar 2005 18:14:37 +0300 (MSK)  
Reply-to: olgasolomina@yandex.ru

Dear Valerie, Keith, Eystein and Peck,

Here are my comments on the glaciers box and suggestions for some improvements. I apologize that I am commenting the text that I was supposed to write myself, but we all know the reason - it was done in a rush and I had very limited access to the literature in the fall. I spent two weeks in Lamont (just came back) and had opportunity to read more. I want to say that I very much appreciate the help and contribution from all people who saved the situation to get the draft for the ZOD, and I hope that we can sharpen it further now.

In general my main concerns are the following:

1. We are focusing on the continuous records, which is one of the main achievements of the last years, indeed. But the real continuous records come from Scandinavia only - even the Alps are mostly based on moraine datings (wood etc.). The records from FJL and Brooks Range are not continuous, they are just the same as in any other place in the World, presented as continuous curves. So, two potential strategies can be suggested - to forget the rest of the World and keep the picture Scandinavia and Alps only or add more discontinuous records drawn as curves. I would go for the second solution for obvious reason to keep the global perspective. I attach more curves that I got from publications + I asked Tom Lowell and Wibjorn Karlen to make something of this kind for NZealand and Africa. I suggest to focus in detail (with dates etc.) on the Scandinavian records (as we did in our text), but briefly discuss the general picture of Holocene glacier variations referring to the updated picture. I need your opinion before changing the graphics (see comments and suggestions in "Box comments SO" file)

2. During a good half of the Holocene the glaciers were SMALLER than now. I attach here the figure with the same axes as at the Valerie's picture (warmest/wettest periods), and the detailed comments on it. To be scientifically correct we probably can shade these periods for the regions that we are presenting at our figure (see a separate file "smaller than now"). What is unusual about the modern retreat is the RATE, though we do not know much about the rate of the former retreat (again because of the lack of continuous records).

3. I changed the introduction. I believe that it is really important to keep the general perspective and mention the exceptions, namely glacier advances (at a Holocene global scale) reflect mostly temperature, therefore a kind of global synchronicity can be visible, though occasionally precipitation may trigger certain advances - maritime and tropical regions are likely to experience it more often than the rest of the World). Two papers justifying this point of view appeared recently (Oerlemans, 2005, Mayewski et al., 2004) and deserve attention. I

included the refs in the updated text. I am ready to discuss further this part, but I believe that we need changes here!

I am aware that this will require rather big changes in the text and figure, but I hope we are still at the stage when we can change, can we?

I will come soon with comments on the whole text and suggestions for the links to Ch4 (cryosphere), but I am really concern about those glaciers in the box, you knowà

Regards,  
olga

Thanks! Peck

>

>>Hello,

>>

>>Thanks a lot for the remaind. I (eventually!) got access to the  
>>literature (in Columbia University where I am now) and will come  
>>soon with comments and improvement of the etxt - at list concerning  
>>the glaciers in the Holocene and last two millennia.

>>

>>Regards,

>>olga

>>

>>

>>>Hi all - We have heard from a good number of you, but also have not  
>>>heard from some of you. Please note the deadline for the first round  
>>>of post-ZOD feedback was yesterday, and more is due next week. If you  
>>>have not sent your material, or contacted us yet, please do so as  
>>>soon as possible. A small delay is ok, but we need to hear from you  
>>>in any case - please respond if you have not already done so.

>>>

>>>Thanks, Peck and Eystein

>>>

>>>>Date: Wed, 9 Feb 2005 11:15:25 -0700

>>>>To: wgl-ar4-ch06@joss.ucar.edu,betteotto-b

>>>>From: Jonathan Overpeck <jto@u.arizona.edu>

>>>>Subject: The next round of work is upon us - IMPORTANT

>>>>Cc:

>>>>Bcc:

>>>>X-Attachments: :Macintosh HD:370627:Glossary WgI TARChap6.doc:

>>>>

>>>>Greetings Chap 6 Lead Authors:

>>>>

>>>>By now, the rush up to the ZOD is hopefully but a fond memory, and  
>>>>you're ready to get back into the thick of IPCC chapter work. Both  
>>>>Chapter 6 and the other chapters are now on the WG 1 website for all  
>>>>of you to enjoy and critique. See your email from the WG1 TSU for  
>>>>information on how to get ZOD chapters.

>>>>

>>>>As you read our chapter, you will no doubt be thinking - "it's

>>>>really too bad we did so much at the last minute, and that the ZOD  
>>>>is so rough." The science is in there, and you all did a great job,  
>>>>but in the future, we won't have the luxury of sending an incomplete  
>>>>draft to the TSU. The purpose of this email is to set a deliberate  
>>>>pace to ensure that our FOD is as perfect and polished as possible.  
>>>>Anything short of this will look bad to our colleagues, and will  
>>>>cost us more work in the official post-FOD IPCC review process.  
>>>>PLEASE MEET ALL DEADLINES below.

>>>>

>>>>Please read all of this communication and NOTE the deadlines - we  
>>>>are asking that you all respond quickly on a couple issues.

>>>>

>>>>\*\*\*1) Due as soon as you read this email - we would like to  
>>>>consider a pre-May LA meeting involving all, or a sub-set of LAs,  
>>>>and would like to know when you are available to meet for 2 days  
>>>>(plus travel to/from US East Coast). The purpose would be to get  
>>>>much further ahead with the FOD and to be able make the most of the  
>>>>Beijing LA2 meeting in May. Remember how frustrating the Trieste  
>>>>meeting was due to the lack of time. Please let us know if you are  
>>>>available to meet April 12,13 (Tues/Wed) and April 19,20 (Tues/Wed).  
>>>>We will pick the dates that work best. Funding would be handled in  
>>>>the usual IPCC manner.

>>>>

>>>>

>>>>\*\*\*2) Due February 24, 2005 - each person should read ALL of the  
>>>>Chapter 6 ZOD. As you do this, please compiling a list of all the  
>>>>issues/tasks you think need to be dealt with and completed before  
>>>>the FOD. For example:

>>>>

>>>>o what important issues or disagreements remain unresolved and what  
>>>>needs to be done to resolve them?

>>>>o what work is needed to make the text better?

>>>>o what key (relevant) science is missing?

>>>>o what key references are missing or need to be updated?

>>>>o are there key display items that need to be deleted or added?

>>>>o what work is needed to make final draft display items?

>>>>

>>>>Each LA should provide the above information to PECK and EYSTEIN on  
>>>>a section-by-section basis by February 24. Please let us know NOW if  
>>>>you can't meet this deadline.

>>>>

>>>>

>>>>\*\*\*3) Due March 3, 2005 - (we have to meet a key IPCC deadline)  
>>>>-Now that we have our ZOD, we have been requested to provide input  
>>>>for the official IPCC AR4 Glossary. Please see the attached glossary  
>>>>document, and follow the instructions included at the top of that  
>>>>file. THIS IS JUST AS IMPORTANT AS OUR OTHER WORK. Each LA should  
>>>>provide this information TO PECK AND EYSTEIN by March 3. Please let  
>> >>us know NOW if you can't meet this deadline.

>>>>

>>>>

>>>>\*\*\*4) Due March 10, 2005 - in Trieste, we assigned Chapter Liaisons  
>>>>for each of the other WG1 chapters. This liaison list is attached  
>>>>below. Please note that some of you are liaisons for more than one



>>>> Dick  
>>>> Stefan  
>>>> David  
>>>>  
>>>>Chapter 9. Attribution David  
>>>> Valerie  
>>>> Keith  
>>>>  
>>>>Chapter 10. Projections David  
>>>> Stefan  
>>>>  
>>>>Chapter 11. Regional Dan  
>>>> Ramesh  
>>>> Zhang  
>>>> Overpeck  
>>>>  
>>>>--  
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>>>>Professor, Department of Atmospheric Sciences  
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>>>><http://www.ispe.arizona.edu/>  
>>>>  
>>>>--  
>>>>Dr.Olga Solomina



From: Susan Solomon <Susan.Solomon@noaa.gov>  
To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>  
Subject: Re: Fwd: last millennium  
Date: Tue, 15 Mar 2005 12:50:06 -0700

<x-flowed>

Dear Peck,  
Thanks for your message. I'll look forward to hearing what you and your colleagues think.  
Susan

At 9:26 AM -0700 3/15/05, Jonathan Overpeck wrote:  
>Hi Susan - thanks for sending these along with some interesting  
>ideas. I'll cc this email to Keith Briffa, along with Eystein, to  
>see if the three of us could chat about the issues. Personally, I  
>think the idea of showing the instrumental data near the paleo sites  
>is excellent - but we have to see what Keith thinks since it would  
>be his (and CA Tim Osborn's) job to do this. But, it makes lots of  
>sense. I also like having the composite (average) lines (paleo and  
>instrumental) for the simple reason that they connects back to all  
>the other reconstructions, and thus make the point that these other  
>recons are not so "misleading" after all.

>  
>Funny coincidence - Julie and I have been working on the coral trend  
>story, and just yesterday decided to do what you are suggesting in  
>terms of instrumental data. I'm learning that the coral data are  
>trickier than I thought, but this is a good way of figuring out what  
>we really can or cannot say with these time series.

>  
>More soon, thanks again, Peck

>  
>>X-Sieve: CMU Sieve 2.2  
>>X-Sender: ssolomon@mailsrvr.al.noaa.gov  
>>Date: Mon, 14 Mar 2005 15:40:35 -0700  
>>To: Jonathan Overpeck <jto@u.arizona.edu>  
>>From: Susan Solomon <Susan.Solomon@noaa.gov>  
>>Subject: last millennium  
>>Cc: Martin Manning <Martin.Manning@noaa.gov>  
>>X-Virus-Scanned: amavisd-new at email.arizona.edu  
>>X-Spam-Status: No, hits=0.001 required=7 tests=BAYES\_50  
>>X-Spam-Level:

>>  
>>Hi Jonathan,  
>>Here's some cool plots that Tom Crowley whipped up, as per our  
>>phone discussion. He indicated that it was OK to send to you.  
>>  
>>It seems to me that showing these records explicitly will address a  
>>lot of the issues in the temperature records for the last  
>>millennium. One might or might not choose to try to construct the  
>>composites (see slide 2 versus 3 in the attached). To be totally

>>consistent, it would be nice to show individual records for the  
>>twentieth century near the sites of the tree ring/cores as well,  
>>rather than just the mean over that period. If one did that, the  
>>resulting diagram would avoid any averaging (is it really needed to  
>>make the point?). A remaining issue would be the calibration of the  
>>paleo proxies and how that affects the spread (or lack thereof, in  
>>the overlap period).

>>  
>>What do you think?

>>Susan

>>  
>>  
>>--

>>\*\*\*\*\*

>>Please note my new email address for your records:

>>  
>>Susan.Solomon@noaa.gov

>>\*\*\*\*\*

>>  
>  
>  
>--

>Jonathan T. Overpeck  
>Director, Institute for the Study of Planet Earth  
>Professor, Department of Geosciences  
>Professor, Department of Atmospheric Sciences

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>http://www.geo.arizona.edu/  
>http://www.ispe.arizona.edu/

>  
>Attachment converted: Discovery:crowley.mwp.mar.14.ppt (SLD8/PPT3) (000F0F48)

--  
\*\*\*\*\*

Please note my new email address for your records:

Susan.Solomon@noaa.gov

\*\*\*\*\*

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>  
To: "Michael E. Mann" <mann@virginia.edu>, ray <rbradley@geo.umass.edu>  
Subject: Re: BBC E-mail: New row on climate 'hockey stick'  
Date: Thu Mar 17 13:54:17 2005

Mike,

On Horizon, I'm supposed to be called in a few minutes by someone. Not sure who yet. This program is generally good. They did something on global dimming a few months ago and now want to do something on the truth about global warming, IPCC and skeptics.

That's all I know so far. Person's name is Paul Olding. Should be calling at 2pm, so 5 minutes time.

Cheers

Phil

At 13:21 17/03/2005, Michael E. Mann wrote:

HI Phil,

I agree-like all of these sources (e.g. boreholes, tree-rings, etc.) each one has its own potential weaknesses--in this case, I think cold-season precip could be playing a greater role w/ the mid-latitude glaciers than Oerlemans cares to admit. Not clear that should give a systematic bias towards underestimating temperature variations though, which is the argument you'd need to make if you're a boreholer.

The important thing is that it is entirely independent of everything else that has come before, and looks remarkably like the Bradley and Jones/Mann et al/Jones et al/Crowley & Lowery/Mann & Jones type reconstructions. Somehow the word hasn't really gotten out on this.

I've got a call in from a different BBC reporter today, Ben Dempsey, who seems much better. He's doing something for "Horizon" on climate change.

Do you know anything about this?

Thanks,

mike

At 08:02 AM 3/17/2005, Phil Jones wrote:

Mike,

Reporter was Paul Rincon ("Paul Rincon-NEWSi" <Paul.Rincon@bbc.co.uk>). No-one seems to have picked up on Oerleman's paper yet. You did send me that earlier, so I should have told him about that.

Sarah Raper here has some doubts about Oerleman's work, but it does reproduce the curve very well. Need to be objective though in interpreting it.

Cheers

Phil

At 12:48 17/03/2005, Michael E. Mann wrote:

Hi Phil,

Yes, BBC has been disappointing in the way they've dealt with this--almost seems to be a contrarian element there.

Do you remember the name of the reporter you spoke to?

Thanks,

Mike

p.s. Interesting that they also don't seem to be aware of the Oerleman's paper, which reproduces the "Hockey Stick" using completely independent data and method (glacial mass balance). I've attached in case you haven't seen...

At 03:26 AM 3/17/2005, Phil Jones wrote:

Ray,

I tried to convince the reporter here there wasn't a story, but he went with it anyway.

At least he put in a quote from me that there are loads of other series that show similar-ish series to MBH and MJ. Had to mention the Moberg et al series to achieve this.

The reporter said he'd not seen Moberg et al., and it wasn't flagged up by Nature to them at the appropriate time. Odd ! Then why are you running with this GRL paper as there are 10s issued each week. Well, it turns out, not surprisingly, that MM have issued numerous press releases themselves - using their networks.

Waterhouse is at Anglian Polytechnic Uni (APU) - it's in Cambridge and Chelmsford. Keith said what does John Waterhouse know about paleo - my thoughts also ! We've worked with John several years ago on an isotopes in trees project, that didn't produce much. APU is OK when it comes to counselling studies. Ruth works for them teaching at Yarmouth !

His quote is typical of many I get to here. Pity the reporter didn't mention this to me.

My response would have been what is the point of doing any more paleo work, if we are constrained by the answer we are allowed to get. If we don't have the MWP and LIA then we are wrong. We have orders of magnitude more data than when these came into vogue in the 1960s, but we still are expected to find them.

Cheers

Phil

Cheers

Phil

At 17:20 16/03/2005, you wrote:

ray saw this story on BBC News Online and thought you should see it.

\*\* Message \*\*

Anglia Polytechnic?!!!!

**\*\* New row on climate 'hockey stick' \*\***

New controversy has erupted over one of the most provocative symbols of the global warming debate: the so-called "hockey stick" graph.

< [1]<http://news.bbc.co.uk/go/em/fr/-/2/hi/science/nature/4349133.stm> >

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## References

1. <http://news.bbc.co.uk/go/em/fr/-/2/hi/science/nature/4349133.stm>
2. <http://www.bbc.co.uk/dailyemail/>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
4. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Phil Jones <p.jones@uea.ac.uk>  
To: Ben Santer <santer1@llnl.gov>  
Subject: Re: Stuff....  
Date: Mon Mar 21 10:08:32 2005

Ben,

I will be at Duke. Get to the airport about 6.30pm on the 29th. Looking forward to seeing you there.

I should have signed off on the CCSP report by Easter. We have to get everything done by March 28. We had a conf. call last Friday.

I can see the argument about an assessment and 'new information'. It is a similar thing in IPCC. Glad to hear you're going to submit it for a paper, because I think it is important. It will unlikely change some peoples views, though.

Just had a long call with Chris Folland. He says that the next CCSP vtt meeting is going to be scheduled for Chicago for the week we should be doing the HC review ! Hope you're still going to come to Exeter. You should have less to do than all the other chapters !

See you on the 29th late or more likely for breakfast on the 30th.

Cheers

Phil

At 23:16 18/03/2005, you wrote:

Dear Phil,

Sorry about the delay in replying to your email. I picked up a chest infection while I was at the IPCC meeting in Hawaii, and it proved to be very persistent. I think a weekend's rest will do me good.

It was great to see you in Chicago, even though the meeting itself was quite difficult to sit through. As may have been apparent, Roger and I really rub each other the wrong way. Working with him on this CCSP Report has been a very unpleasant experience. I am taking your advice, and trying to write up the "amplification factor" stuff that I showed in Chicago. I presented this in Hawaii, and it sparked a lot of discussion. Just between you and me, Susan Solomon argued quite forcefully that this new information should NOT go into the CCSP Report, and that we should not be performing science in support of an assessment. She was concerned that the CCSP Report might be subject to unjustified criticism if key conclusions of the Report relied on unpublished work. I have considerable sympathy with this view. It does seem important to get this work submitted to a peer-reviewed publication as soon as possible, and then worry later about whether the material should or should not appear in CCSP.

Are you going to the Duke IDAG meeting? If so, I look forward to seeing you there.

Best regards to you and Ruth,

Ben

--

---

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From: Phil Jones <p.jones@uea.ac.uk>  
To: "Brohan, Philip" <philip.brohan@metoffice.gov.uk>  
Subject: Re: HADCRUT various  
Date: Mon Apr 4 09:50:24 2005  
Cc: Peter Thorne <peter.thorne@metoffice.gov.uk>

Philip,

I'm not unhappy at all. If I am it is more about HadCRUT2 and 3.

I read through the report to DEFRA and will be sending some comments later today. I also commented on what Harry has written as a report for you. I've left those comments with him as he's away this week and I'm off April 6-15.

It is a bit odd with HadCRUT2 that the problem has surfaced now and my old mask hasn't made any difference.

Cheers

Phil

At 15:33 01/04/2005, Brohan, Philip wrote:

Phil.

I've just had a chat with Peter Thorne about HadCRUT2 and 3, and I get the impression that you are concerned, so we thought I should clarify what is going on. In particular I want to assure you that we are not trying to change the system without your approval.

To make things quite clear, we have two HadCRUT systems here:

1) Peter is running HadCRUT2. This is our operational system which produces the new data every month that we send to you and everyone. This is a fixed system, it does exactly what you agreed with Peter a couple of years ago. We don't plan to change it at all.

We did, unfortunately, make a mistake while running the system; we think a land-mask file was changed. This is what Peter's recent messages have been about. We're still not quite sure how this happened, but whatever fix we apply will be to restore the system to the original, agreed state.

2) I am coordinating HadCRUT3. This currently encompasses Harry's work on the data, Simon's work on blending, John Kennedy's work on variance correction, and my work on errors and gridding. Some combination of this work will become the new dataset.

I have a clear picture of what I think should form the new dataset. However, we won't produce HadCRUT3 unless you (and all the other contributors) agree. If I can't persuade you of the value of a change, it won't happen. In particular, I see the land station data as entirely under your control, both now and in the future.

If I (or Peter) misread the vibes and you were not worrying about any of this, please don't start. There are not serious problems with either system.

Have fun,

Philip.

Prof. Phil Jones

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From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

Subject: Re: last millennium - responding to Susan

Date: Mon, 4 Apr 2005 23:08:47 -0700

Cc: Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith and Tim - sorry for the delay in responding. I think the issues you raise are worth discussing, but we can do that in Beijing, and hopefully with Susan. She is keen on the idea, and my gut says it's a good idea to include such "crowley" plots somewhere - at least in the appendix, for example. But, let's talk in person.

In the meantime, we really need your comments on the ZOD - including what you feel has to be done with your section, but also with the others. We have comments from most others, and are expecting the external review comments soon, so please send yours ASAP so they can be included in this important stage.

Thanks! Best, peck

>Jonathan

>

>I am slowly getting teaching duties behind me and certainly turning  
>my attention back to IPCC. I have spoken wit Phil re the  
>observations chapter and we have discussed the need to show pre 20th  
>instrumental data in our chapter in a manner that is relevant to the  
>comparison with more recent instrumental (ie N.Hemisphere or global  
>mean) records , and the possibility of showing ensembles of regional  
>temperature records , and composites in a way that possibly bares on  
>the discussions with Susan. We are still considering this question ,  
>but certainly there needs to be some "frozen grid" curves as flagged  
>in the ZOD.

>I am not sure of the context of the discussion you are having with  
>Susan , or the logic for what Tom Crowley is trying to do with the  
>ensemble curves of various palaeo-series.

>

>I flagged clearly at the outset that I would like to do some  
>regional comparisons of various data/reconstructions . This required  
>more time and input than was achievable for the ZOD. I still think  
>this is desirable though. Similarly , there is far too little in the  
>current version about moisture variability in the last 2000 years

>and too little on the S.Hemisphere in general. It was always clear  
>that there would be much more discussion on the scaling issue and  
>specific reference to work that will explore the effect of regional,  
>seasonal and methodological differences in aggregation and scaling  
>(including timescale dependent effects). The problem is that the  
>work on much of this is not yet done or published. It should be  
>immediately apparent that our greatest enemy , acting against a  
>thorough exposition of these issues , is the lack of sufficient  
>allotted space.

>  
>Now , returning to the Crowley Figures , I do not see how not  
>showing an integrated and "appropriately" scaled record helps to  
>clarify the picture on the precedence of recent warming in any clear  
>way. On the contrary , it merely confuses the issue by omitting to  
>tackle the knotty problem of expressing an underlying mean  
>large-scale signal , that emerges from the regional noise only  
>through aggregation of demonstrably appropriate palaeo-records .  
>This aggregation should allow quantification (with appropriate  
>uncertainty) of the extent of warming and provide clearly defined  
>target for comparison with model simulations.

>  
>If it thought appropriate , yes we can show individual records , but  
>just normalising them over a common base ignores the different  
>sensitivities and regional distribution issues . I am not convinced  
>this selective presentation clarifies anything. I would be happy  
>for this discussion to opened to the rest of the author team.

>  
>best wishes

>  
>Keith

>  
> At 16:28 15/03/2005, you wrote:

>>Hi Keith - I can't remember when you said you'd be able to get back  
>>into the IPCC fray, but I hope it is soon. Please let me and  
>>Eystein know what you think regarding the email I just cc'd to you.  
>>We should respond to Susan asap. Hope things are going well.  
>>Thanks, Peck

>>--  
>>Jonathan T. Overpeck  
>>Director, Institute for the Study of Planet Earth  
>>Professor, Department of Geosciences  
>>Professor, Department of Atmospheric Sciences  
>>

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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>  
To: "Parker, David (Met Office)" <david.parker@metoffice.gov.uk>, Kevin  
Trenberth <trenbert@ucar.edu>  
Subject: Re: Chapter 3.4.1  
Date: Tue Apr 19 16:12:38 2005  
Cc: David Parker <david.parker@metoffice.gov.uk>, Brian Soden  
<bsoden@rsmas.miami.edu>, Susan Solomon <Susan.Solomon@noaa.gov>, Martin  
Manning <Martin.Manning@noaa.gov>, "'David R. Easterling'"  
<david.easterling@noaa.gov>

Kevin,

I plan to look through your 3.4.1 draft tomorrow or later this week. At the same time I also plan to have a go at section 3.2. David has sent me some new figures and there are two new papers to add in. I am having difficulty finding some quality time at the moment, but hope this will come later this week.

I did read all the CCSP report. The review group are having a conf call tomorrow on this, but they have chosen your afternoon, so I can't take part. There were 6

reviewers of the review and one other almost wrote as much as you. Most were positive on the review saying that the report authors have a lot to do, particularly for Chapters 1 and 6. How all this pans out is impossible to tell. The next meeting of the authors is being scheduled for the week after Beijing.

I agree some of their figures are useful, but I too doubt whether we will have

much useful for the FOD we have to write. We will likely be doing them in parallel - which is hardly ideal.

I wouldn't send our 3.4.1 to Tom at this time - at least wait till Brian, David and I

have been through yours. Also I wouldn't want Tom passing it on to the CCSP VTT

authors. I think they will have a lot of hard thinking when they get the NRC review, to

worry too much about what we're doing. We do need to have our chapter and their

report meshing at some time, but this might have to wait till the SOD (by which time their report might be finished).

Cheers

Phil

At 17:35 18/04/2005, Parker, David (Met Office) wrote:

Kevin

Thanks. You have saved me some work because on my journey back from Geneva I also studied the comments on 3.4.1 (on paper) and was considering making an electronic revised section. I came to the conclusion that 3.4.1 should say that there are 2 schools of thought

about Fu et al and other aspects of the temperatures-aloft issue:  
the jury is still out. That would be a assessment (as opposed to a  
review) of the current state of the science. Fu may not be correct as he  
seems to imply upper tropospheric warming rates well outside the error-  
bars implied by the radiosondes (though I am aware of their problems  
too). I have not yet read your attachment but will consider it in the next  
few days.  
I looked at the surface temperature comments too and feel it may be  
best to wait until in Beijing, as most comments are about what diagrams  
to choose. I could try to re-order the urban warming section as  
reviewers suggest, but we may still wish to contact Tsutsumi (who didn't reply  
to my email a couple of months ago) to write something.

Regards

David

On Mon, 2005-04-18 at 17:13, Kevin Trenberth wrote:

> Hi Phil and David, and Brian

>

> I believe you three are probably closest to the satellite

temperature

> record issue and so I am sending this to you. I have thoroughly

gone

> over all the comments we received and I have prepared a revised

3.4.1

> which is attached. This is the cleaned up version. The actual

> version has tracking turned on but the changes are so extensive

that

> they are very hard to follow. As you know, I have read the entire

> CCSP report and commented extensively on it. I know Phil was on

the

> review team and David was there as a lead author. However David

and

> Phil may not be as familiar with the whole report.

>

> Obviously this remains a controversial topic. Many of the

comments we

> received were diametrically opposed to one another. The rhetoric

was

> disappointing (especially from Peter Thorne). In fact Peter's

> comments are mostly not useful and reveal very strong biases

against

> Fu and reanalyses. Previously, you'll recall that David provided

most

> of the text and I edited it and updated it with the Fu material in

a

> somewhat ad hoc fashion that got almost everyone mad. Probably a  
good  
> thing to do in retrospect, as this next version will look so much  
> better. Note that I have done nothing with the appendices at this  
> point, so that needs to be addressed. I have taken out all the  
> tables??  
>  
> You will see even in the current text that I have 2 sections I  
would  
> like to delete.  
>  
> While individual comparisons of radiosonde station data with  
> collocated satellite data (Christy and Norris, 2004) suggest that  
the  
> median trends of radiosonde temperatures in the troposphere are  
> generally very close to UAH trends and a little less than RSS  
trends,  
> trends at individual radiosonde sites vary and root mean square  
> differences of UAH satellite data with radiosondes are substantial  
> (Hurrell et al., 2000). Moreover, as noted in 3.4.1.1, comparisons  
> with radiosonde data are compromised by the multiple problems with  
the  
> latter, and there are diurnal cycle influences on them over land.  
In  
> the stratosphere, radiosonde trends are more negative than both  
MSU  
> retrievals, especially RSS. [DELETE THIS?]  
>  
> The problem here is the rhetoric of Christy et al. In his  
> contribution Christy justifies the UAH record by saying that  
"median  
> trends agree with those of sondes". But he actually sent to us  
his  
> Fig. 2 showing the lack of agreement in general. It is only the  
> median that agrees, the agreement with sondes individually is not  
good  
> and this is just for trends. [Hence the median depends on the  
> selection of stations]. It is even worse if rms differences are  
> examined (as in Hurrell et al 2000). The only reason to include  
this  
> is to rebut Christy's claim. For most other readers it has no  
> business being there. Your suggestions appreciated. Maybe this  
> should go in the appendix?  
>  
> You will see that I have stolen 2 figures from the CCSP report. I  
> made up the 3rd figure from data provided from the CCSP report  
plus  
> extra material (only the global is in the current draft). It  
would  
> also be nice to include a spatial map of trends at the surface and  
for  
> the troposphere (T2 corrected as from Fu) but no such figure  
exists

> anywhere, yet. We can get trends from RSS and UAH for T2. It  
would  
> be good to have access to the originals so we can modify them and  
> clean up the terminology. {On that score, I don't think the CCSP  
> terminology is tenable given the new retrievals of Fu et al (2005)  
and  
> ours, using T2, T3, and T4 is much easier).  
>  
> At present the CCSP report is not very useful to us. Some figures  
are  
> useful. It may become so, but I actually have my doubts, given  
the  
> vested interests of the authors.  
>  
> I am tempted to send this to Tom Karl in his role as editor of our  
> chapter, and of course he is head of the CCSP effort, but I would  
NOT  
> want him to use it for CCSP (except that it might highlight the  
> differences in assessments). What do you think? Via Tom we might  
get  
> better access to the figures and updates? Also I'll cc David  
> Easterling.  
> This would be the main basis for FOD.  
>  
> Ideally also it is desirable to get the figures updated thru 2004,  
but  
> can we?  
>  
> Please read this version and let me know what you think? (Please  
be  
> kind, I have put in a LOT of work on this)  
>  
> Best regards  
> Kevin  
>  
> --  
> \*\*\*\*\*  
> Kevin E. Trenberth e-mail:  
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> Boulder, CO 80307 (303) 497 1333 (fax)  
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> Street address: 1850 Table Mesa Drive, Boulder, CO 80303  
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Global climate data sets are available from [2]http://hadobs.org

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References

1. <http://www.cgd.ucar.edu/cas/>
2. <http://hadobs.org/>

From: Phil Jones <p.jones@uea.ac.uk>  
To: Peter Lemke <plemke@awi-bremerhaven.de>, Kevin Trenberth <trenbert@ucar.edu>  
Subject: Re: WG1 LA2 meeting - Overlap cluster A  
Date: Wed Apr 20 10:49:38 2005  
Cc: Martin Manning <mmanning@al.noaa.gov>, Susan Solomon <ssolomon@al.noaa.gov>, ipcc-wg1@al.noaa.gov, k.briffa@uea.ac.uk

Dear All,

In addition to Kevin's comments and from a quickish look through parts of Chapters 4, 6 and 9, here are a few suggestions.

First for best use of time, I would suggest that Cluster B gets broken into two parts. Basically separating off the overlap with the paleo and instrumental record including borehole temperatures and glacier length changes from the sea ice/SST, snow/temperature. OHC/SST, salinity/precip and SLR etc. The latter can be dealt with by Chs 5, 3 and 4. The former is really for 6, 3 and 4.

Issues for 3 and 6 are the interface of the instrumental and paleo records, particularly how the early 19th century is dealt with. This period of instrumental records is believed by many in the paleo community not to exist, but in Europe and a few other regions it exists back in good order to the late 18th century. The 19th century is, I believe, the key to resolving much of the discussion about the millennium. Much more should be made of this period when comparisons with long forced GCM runs are analyzed. Europe may be a small continent, but the 200-250 year 'perfect proxy' records (which have all seasons!) need to be studied more. As any conclusions relate to Ch 6, the main text should be there, with perhaps a box on the early instrumental period in Ch 3.

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Chapter 9 has an interest in both these issues.

Finally, there is one other issue. Do we want to consider having a web site (distributed?) where the data for some selected time series can be downloaded from - not just the smoothed/plotted series, but on the original timescale as well. This possibly comes back also to a consistent way of smoothing time series.

Cheers

Phil

At 08:11 20/04/2005, Peter Lemke wrote:

Dear Martin,

I am also willing to co-chair the cluster B. (As always) Kevin has done a very good job in listing the most important issues.

Therefore, I have nothing to add at the moment. I will think about this on the weekend.

Best regards,

Peter

Kevin Trenberth schrieb:

Hi Martin

Yes I will do this.

Firstly on cluster A:

I/we have an issue which is: what about changes in radiative forcing from water vapor (or feedback if you prefer), it is of order  $1 \text{ W m}^{-2}$ .

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\*Consistency of:\*

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- 2) overlap and redundancy
- 3) where to place integrated assessment?
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Please see the draft of 3.9.

So in terms of the agenda, the main points are:

- 1) Ensuring consistency among variables across chapters
- 2) Agreement on which chapter and what person will handle what, and in particular, that 3.9 will have a look ahead aspect to the chapters that follow.

The above points could all be briefly on the table with the focus on cross-chapter issues.

Desirable to circulate draft section 3.9 (1 page).

Peter may wish to add or change this?

Regards

Kevin

Martin Manning wrote:

Dear Kevin and Peter

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This is to ask if you would be prepared to jointly co-chair the session on Overlap Cluster B dealing with "Consistency in covering observed climate change" and which will involve discussion among chapters 3, 4, 5, 9 and 11. The attached program lists, on the

last page, overlap / consistency areas that have been mentioned in the ZOD.

We would really be most grateful for your assistance in this, and if you agree, we would like to ask that you each to specify what in your view would be the 2 or 3 most important issues to resolve during the overlap cluster session. We will then use your input to draw up a specific agenda and circulate agendas for all overlap clusters to all CLAs prior to the meeting. We hope in this way that we can reach a shared understanding of the most important overlap and consistency issues and the corresponding key decisions that will have to be made in Beijing.

I would be grateful if you could let me know whether you are able to help us with this by Wednesday 20th.

Regards  
Martin

--

\*Recommended Email address: mmanning@al.noaa.gov

\*\*\* Please note that problems may occur with my @noaa.gov address

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NOAA Aeronomy Laboratory Phone: +1 303 497 4479  
325 Broadway, DSRC R/AL8 Fax: +1 303 497 5628  
Boulder, CO 80305, USA

-- \*\*\*\*\*

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-----

## References

1. <http://www.cgd.ucar.edu/cas/>
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From: Jonathan Overpeck <jto@u.arizona.edu>  
To: Kevin Trenberth <trenbert@ucar.edu>  
Subject: Re: [Fwd: Re: WG1 LA2 meeting - Overlap cluster A]  
Date: Wed, 20 Apr 2005 15:28:30 -0700  
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, olgasolomina@yandex.ru

Kevin - ah yes, good fun. Talked w/ Susan about some of this, and we're hoping that Keith Briffa might be able to participate in "Cluster B" while the rest of our chap 6 team discusses things that bore Keith. I'll forward this to relevant chap 6 folks. Thx, Peck

Jon  
FYI wrt Beijing and overlap issues with chapter 6. You may find some exchanges of interest as well.

Kevin

----- Original Message -----

Subject: Re: WG1 LA2 meeting - Overlap cluster A  
Date: Wed, 20 Apr 2005 17:12:41 +0100  
From: Phil Jones <p.jones@uea.ac.uk>  
To: Kevin Trenberth <trenbert@ucar.edu>  
References: <5.2.0.9.2.20050418185815.0303d0d0@mailsrvr.al.noaa.gov>  
<42654140.2080509@ucar.edu> <42660091.9060600@awi-bremerhaven.de>  
<6.1.2.0.0.20050420101527.01d3f508@pop.uea.ac.uk> <42667322.4070101@ucar.edu>

Kevin,

Right on ! Assumes precip doesn't change - i.e. it's constant. Difficult to do much more for some regions, but could do a lot better for the Alps. Ch 4 has swallowed this hook, line and sinker and it is really a Ch 6 issue. Ch 6 wasn't even aware of it. Can't decide who on Ch 4 knew about it as Oerlemans isn't there and the Swiss Glacier people didn't know about the paper 2 weeks ago when I saw them.

I like the curve as does Mike Mann, but its not for any scientific reason.

Any jury is still out on whether this is right, but I'm glad someone has tried the approach. It is a quantification of what people have assumed, but there likely isn't enough detail in the paper to show how it was done.

I've not seen this paper in a proper issue of Science yet. As such I've not been able to get the supporting material.

This paper is totally independent of all other paleo work. It is much better science than Mobeg et al. in Nature in February. Susan has been sending a few emails to Ch 6 about how to display the various millennium series - some of which she's not thought through.

Just be glad we haven't got paleo in our chapter !

Cheers

Phil

At 16:20 20/04/2005, you wrote:

Hi Phil

I had not read Oerleman's paper, I have now. Some things don't make sense to me: chanes

in precip not included and the time series (esp N America) Also magnitude of implied early 20Th C warming. What is your take?

Kevin

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-- \*\*\*\*\*

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Jon  
FYI wrt Beijing and overlap issues with chapter 6. You may find some exchanges of  
interest as well.  
Kevin  
----- Original Message -----

Subject: Re: WG1 LA2 meeting - Overlap cluster A Date: Wed, 20 Apr 2005 17:12:41 +0100  
From: Phil Jones [1]<p.jones@uea.ac.uk> To: Kevin Trenberth [2]<trenbert@ucar.edu>  
References: [3]<5.2.0.9.2.20050418185815.0303d0d0@mailsrvr.al.noaa.gov>  
[4]<42654140.2080509@ucar.edu> [5]<42660091.9060600@awi-bremerhaven.de>  
[6]<6.1.2.0.0.20050420101527.01d3f508@pop.uea.ac.uk> [7]<42667322.4070101@ucar.edu>

Kevin,  
Right on ! Assumes precip doesn't change - i.e. it's constant. Difficult to do  
much more for some regions, but could do a lot better for the Alps. Ch 4 has swallowed  
this hook, line and sinker and it is really a Ch 6 issue. Ch 6 wasn't even aware of it.  
Can't decide who on Ch 4 knew about it as Oerlemans isn't there and the Swiss Glacier

people didn't know about the paper 2 weeks ago when I saw them.

I like the curve as does Mike Mann, but its not for any scientific reason.

Any jury is still out on whether this is right, but I'm glad someone has tried the  
approach. It is a quantification of what people have assumed, but there likely isn't  
enough detail in the paper to show how it was done.

I've not seen this paper in a proper issue of Science yet. As such I've not been  
able to get the supporting material.

This paper is totally independent of all other paleo work. It is much better science  
than Mobeg et al. in Nature in February. Susan has been sending a few emails to  
Ch 6 about how to display the various millennium series - some of which she's not  
thought through.

Just be glad we haven't got paleo in out chapter !  
Cheers

Phil

At 16:20 20/04/2005, you wrote:

Hi Phil

I had not read Oerleman's paper, I have now. Some things don't make sense to me: changes in precip not included and the time series (esp N America) Also magnitude of implied early 20th C warming. What is your take?

Kevin

Phil Jones wrote:

Dear All,

In addition to Kevin's comments and from a quickish look through parts of Chapters 4, 6 and 9, here are a few suggestions.

First for best use of time, I would suggest that Cluster B gets broken into two parts.

Basically separating off the overlap with the paleo and instrumental record including borehole temperatures and glacier length changes from the sea ice/SST, snow/temperature.

OHC/SST, salinity/precip and SLR etc. The latter can be dealt with by Chs 5, 3 and 4. The former is really for 6, 3 and 4.

Issues for 3 and 6 are the interface of the instrumental and paleo records, particularly

how the early 19th century is dealt with. This period of instrumental records is believed

by many in the paleo community not to exist, but in Europe and a few other regions it exists back in good order to the late 18th century. The 19th century is, I believe, the key

to resolving much of the discussion about the millennium. Much more should be made of this period when comparisons with long forced GCM runs are analyzed. Europe may be a small continent, but the 200-250 year 'perfect proxy' records (which have all seasons!) need

to be studied more. As any conclusions relate to Ch 6, the main text should be there, with

perhaps a box on the early instrumental period in Ch 3.

Somewhat related to the above, Ch 4 has a section on the recent Oerlemans (2005) work

- attached for reference. Mike Mann sent me a figure (see jpg) comparing this with most other

reconstructions of parts of the millennium. It seems that this piece of work should be with

all the others in Ch 6 and not Ch 4. When producing plots like this getting the right base level

is crucial - not just for Oerlemans' series, but also for the boreholes. Also, the degree of

smoothing and the y-scale used can easily determine the takeaway message.

Chapter 9 has an interest in both these issues.

Finally, there is one other issue. Do we want to consider having a web site (distributed?) where the data for some selected time series can be downloaded from - not just the smoothed/plotted series, but on the original timescale as well. This possibly comes back also to a consistent way of smoothing time series.

Cheers

Phil

At 08:11 20/04/2005, Peter Lemke wrote:

Dear Martin,

I am also willing to co-chair the cluster B. (As always) Kevin has done a very good job in listing the most important issues.

Therefore, I have nothing to add at the moment. I will think about this on the weekend.

Best regards,

Peter

Kevin Trenberth schrieb:

Hi Martin

Yes I will do this.

Firstly on cluster A:

I/we have an issue which is: what about changes in radiative forcing from water vapor (or feedback if you prefer), it is of order  $1 \text{ W m}^{-2}$ .

So this relates to water vapor changes in chapter 3.

Cluster B: Consistency in observed climate change: atmosphere, ocean, cryosphere. This may also extend to paleo, chapter 6.

Issues:

\*Consistency of:\*

- \* sea ice with SST
- \* snow cover with snowfall and temperature
- \* glacier melting and permafrost changes vs temperatures
- \* borehole temperatures, glacier changes and paleo record
- \* overlap between paleo record and instrumental record
- \* salinity vs precipitation
- \* ocean heat content with SST and surface fluxes
- \* sea level rise as an integrator: ocean expansion, melting of land ice, increased water storage on land, and changes in TOA radiation (presumably led by Chapter 5.)

Issues consist of use of consistent temperature and precipitation records (don't use NCEP surface temperatures as in Ch 4 CQ).

Points of contention:

- 1) consistency
- 2) overlap and redundancy
- 3) where to place integrated assessment?

- \* sea level: Chapter 5
- \* snow, ice, temperature chapter 3 section 3.9
- \* paleo record vs instrumental chapter 6
- \* overall view including sea level chapter 3, in 3.9
- \* T increase (land, SST, subsurface ocean), snow retreat, sea ice retreat, thinning, freezing season shorter, glacier melt, sea level rise.
- \* Precip changes, drought, salinity, ocean currents, P-E, snowfall.

Please see the draft of 3.9.

So in terms of the agenda, the main points are:

- 1) Ensuring consistency among variables across chapters
- 2) Agreement on which chapter and what person will handle what, and in particular, that 3.9 will have a look ahead aspect to the chapters that follow.

The above points could all be briefly on the table with the focus on cross-chapter issues.

Desirable to circulate draft section 3.9 (1 page).

Peter may wish to add or change this?

Regards

Kevin

Martin Manning wrote:

Dear Kevin and Peter

Please find attached our current program for the second Lead Author meeting on May 10 - 12. We will shortly be sending out some more details on the plans for the meeting and in particular would like to clarify what needs to be done in the Overlap Cluster meetings shown in the program on Wednesday 11th.

This is to ask if you would be prepared to jointly co-chair the session on Overlap Cluster B dealing with "Consistency in covering observed climate change" and which will involve discussion among chapters 3, 4, 5, 9 and 11. The attached program lists, on the last page, overlap / consistency areas that have been mentioned in the ZOD.

We would really be most grateful for your assistance in this, and if you agree, we would like to ask that you each to specify what in your view would be the 2 or 3 most important issues to resolve during the overlap cluster session. We will then use your input to draw up a specific agenda and circulate agendas for all overlap clusters to all CLAs prior to the meeting. We hope in this way that we can reach a shared understanding of the most important overlap and consistency issues and the corresponding key decisions that will have to be made in Beijing.

I would be grateful if you could let me know whether you are able to help us with this by Wednesday 20th.

Regards

Martin

--

\*Recommended Email address: [8]mmanning@al.noaa.gov

\*\*\* Please note that problems may occur with my @noaa.gov address

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Professor, Department of Atmospheric Sciences  
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<http://www.geo.arizona.edu/>  
<http://www.ispe.arizona.edu/>

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## References

1. <mailto:p.jones@uea.ac.uk>
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3. <mailto:5.2.0.9.2.20050418185815.0303d0d0@mailsrvr.al.noaa.gov>
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5. <mailto:42660091.9060600@awi-bremerhaven.de>
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From: trenbert@ucar.edu

To: "Martin Manning" <mmanning@al.noaa.gov>

Subject: Re: WG1 LA2 meeting - Overlap cluster A

Date: Wed, 20 Apr 2005 19:46:31 -0600 (MDT)

Cc: "Phil Jones" <p.jones@uea.ac.uk>, "Peter Lemke" <plemke@awi-bremerhaven.de>, "Susan Solomon" <ssolomon@al.noaa.gov>, ipcc-wg1@al.noaa.gov, k.briffa@uea.ac.uk

Martin I think you are right: the paleo instrumental issue is likely to involve mainly Briffa from Chap 6 and Phil from our chapter, so they might well spin off at some point. Are there others Phil?

Kevin

> Dear Kevin and Phil

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> As you say Chapter 6 was not implicated in the cluster B overlap issues  
> based on the author notes we received with the ZOD. You may want to cover  
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> could consider dealing with that more efficiently in a small group  
> separately from the cluster meeting. So the choice is up to you.

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> If it would be helpful, the TSU could start to compile a list of small  
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> practical timetable for lunch time meetings. But we would need advice on  
> the specific individuals who should be involved in each case and all I am  
> offering is a "dating service" that would distribute a suggested list of  
> times and names that we could possibly update in real time during the  
> meeting in Beijing.

>

> Regards

> Martin

>

> At 09:07 AM 4/20/2005, Kevin Trenberth wrote:

>>Hi Martin

>>I agree with what Phil says, but I note that cluster B does not actually  
>>have chapter 6 as part of it. So the question is whether chapter 6 will  
>>be involved?. If so then we may well want to split into 2 parts. Last  
>>night I had a quick look at Chap 9 and I am concerned about redundancy  
>> and

>>overlap and conflicts: they are doing some similar things with  
>>observations but maybe different obs, and coming to different conclusions  
>>e.g. wrt things like dimming.

>>Kevin

>>

>>Phil Jones wrote:

>>>

>>> Dear All,

>>> In addition to Kevin's comments and from a quickish look through  
>>> parts of Chapters  
>>> 4, 6 and 9, here are a few suggestions.

>>>

>>> First for best use of time, I would suggest that Cluster B gets  
>>> broken into two parts.  
>>> Basically separating off the overlap with the paleo and instrumental  
>>> record including  
>>> borehole temperatures and glacier length changes from the sea ice/SST,  
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>>> The former is really for 6, 3 and 4.

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>>>>Best regards,

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>>>>>Yes I will do this.

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>>>>>So this relates to water vapor changes in chapter 3.

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>>>>>Points of contention:

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>>>>>Please see the draft of 3.9.

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>>>>>So in terms of the agenda, the main points are:

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>>>>>>>Kevin E. Trenberth

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> Boulder, CO 80305, USA

From: Phil Jones <p.jones@uea.ac.uk>  
To: trenbert@ucar.edu, "Martin Manning" <mmanning@al.noaa.gov>  
Subject: Re: WG1 LA2 meeting - Overlap cluster A  
Date: Thu Apr 21 08:57:05 2005  
Cc: "Peter Lemke" <plemke@awi-bremerhaven.de>, "Susan Solomon"  
<ssolomon@al.noaa.gov>, ipcc-wg1@al.noaa.gov, k.briffa@uea.ac.uk

Martin,

You are right, it should just be the two of us and as Keith is just across the corridor we can have the meeting beforehand or on the way together. If you add this though to your list of possible meetings you might find that some others are interested. This meeting of 3 and 6 can occur at the same time as 3 and 4, so during Cluster B. There does need to be some discussion between 4 and 6 though to decide where Oerlemans work is best located within AR4.

There is also the issue of Ch 9 as Kevin mentioned. As with Ch 4 using an NCEP temperature series for the Arctic, there might be issues with some other chapters using observed datasets which Ch 3 might think inappropriate or saying things about them that differ from what we do. Hopefully all these sorts of issues which get flagged when the overviews of the whole of AR4 get discussed (and also at LA3 and LA4).

Cheers  
Phil

At 02:46 21/04/2005, trenbert@ucar.edu wrote:

Martin I think you are right: the paleo instrumental issue is likely to involve mainly Briffa from Chap 6 and Phil from our chapter, so they might

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>> and  
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conclusions  
>>e.g. wrt things like dimming.  
>>Kevin  
>>

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From: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
To: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Fwd: Input for Chapter 6 in AR4  
Date: Thu, 21 Apr 2005 16:04:30 +0200

<x-flowed>

>Hi Keith,

got this paper from Jens Hesselbjerg. Interesting with respect to the von Storch story.

Eystein

>A few comments in English:

>We have used a different version of the MPI coupled modeling system from that described by von Storch et al. to simulate the last 500 years. The model we have used has a different ocean component (OPYC in stead of HOPE) and a higher resolution in the atmosphere (T42 in stead of T31 - by many considered to be a substantial improvement in terms of representing synoptic behavior). Moreover, we have used different reconstructions of the external forcing. All these differnces leads to somewhat differnt behaviours compared to von Storch, and yet the model does seem to depict many of the observed major climatic events. Details are given in the paper.

>  
>venlig hilsen  
>Jens Hesselbjerg Christensen

>  
>  
>

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</x-flowed>

Attachment Converted: "c:\eudora\attach\stendel\_et\_al\_ClimDyn\_final.pdf"

From: Jonathan Overpeck <jto@u.arizona.edu>  
To: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Re: Fwd: Re: WG1 LA2 meeting - Overlap cluster A  
Date: Thu, 21 Apr 2005 20:37:06 -0700  
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Phil Jones <p.jones@uea.ac.uk>

<x-flowed>

Hi Keith and Phil - Thanks. I read this to say that the issue of pre-1860 instrumental data is figured out ok? Plan outlined below sounds good if ok with you both.

Best, Peck

>Peck

>FYI

>Phil and have have talked about the need t adress (even if briefly)  
>the pre 1860 climate data - and both feel that the overlap with the  
>paleo records (see our 1st Figure) in the 2000 year section , is one  
>place to address this - though more needs to be done about the  
>regional bias in these data

>  
>>X-Sender: f028@pop.uea.ac.uk

>>X-Mailer: QUALCOMM Windows Eudora Version 6.1.2.0

>>Date: Thu, 21 Apr 2005 08:57:05 +0100

>>To: trenbert@ucar.edu,"Martin Manning" <mmanning@al.noaa.gov>

>>From: Phil Jones <p.jones@uea.ac.uk>

>>Subject: Re: WG1 LA2 meeting - Overlap cluster A

>>Cc: "Peter Lemke" <plemke@awi-bremerhaven.de>,

>> "Susan Solomon" <ssolomon@al.noaa.gov>,"ipcc-wg1@al.noaa.gov",

>> k.briffa@uea.ac.uk

>>

>>

>> Martin,

>> You are right, it should just be the two of us and as Keith is

>>just across the corridor

>> we can have the meeting beforehand or on the way together. If you

>>add this though to

>> your list of possible meetings you might find that some others are

>>interested. This

>> meeting of 3 and 6 can occur at the same time as 3 and 4, so

>>during Cluster B. There

>> does need to be some discussion between 4 and 6 though to decide  
>>where Oerlemans  
>> work is best located within AR4.  
>> There is also the issue of Ch 9 as Kevin mentioned. As with Ch  
>>4 using an NCEP  
>> temperature series for the Arctic, there might be issues with some  
>>other chapters  
>> using observed datasets which Ch 3 might think inappropriate or  
>>saying things about  
>> them that differ from what we do. Hopefully all these sorts of  
>>issues which get flagged  
>> when the overviews of the whole of AR4 get discussed (and also at  
>>LA3 and LA4).

>>  
>> Cheers  
>> Phil

>>  
>>  
>>At 02:46 21/04/2005, trenbert@ucar.edu wrote:

>>>Martin I think you are right: the paleo instrumental issue is likely to  
>>>involve mainly Briffa from Chap 6 and Phil from our chapter, so they might  
>>>well spin off at some point. Are there others Phil?  
>>>Kevin

>>>  
>>>  
>>>> Dear Kevin and Phil

>>>>  
>>>> As you say Chapter 6 was not implicated in the cluster B overlap issues  
>>>> based on the author notes we received with the ZOD. You may want to cover  
>>>> the point raised by Phil and in particular where the long instrumental  
>>>> records fit, but as this seems to involve only a small number of LAs you  
>>>> could consider dealing with that more efficiently in a small group  
>>>> separately from the cluster meeting. So the choice is up to you.

>>>>  
>>>> If it would be helpful, the TSU could start to compile a list of small  
>>>> group meetings requested by CLAs and look for some way of setting up a  
>>>> practical timetable for lunch time meetings. But we would need advice on  
>>>> the specific individuals who should be involved in each case and all I am  
>>>> offering is a "dating service" that would distribute a suggested list of  
>>>> times and names that we could possibly update in real time during the  
>>>> meeting in Beijing.

>>>>  
>>>> Regards

>>>> Martin

>>>>

>>>> At 09:07 AM 4/20/2005, Kevin Trenberth wrote:

>>>>>Hi Martin

>>>>>I agree with what Phil says, but I note that cluster B does not actually  
>>>>>have chapter 6 as part of it. So the question is whether chapter 6 will  
>>>>>be involved?. If so then we may well want to split into 2 parts. Last  
>>>>>night I had a quick look at Chap 9 and I am concerned about redundancy  
>>>>> and

>>>>>overlap and conflicts: they are doing some similar things with  
>>>>>observations but maybe different obs, and coming to different conclusions  
>>>>>e.g. wrt things like dimming.

>>>>>Kevin

>>>>>

>>

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>--

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--

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<http://www.geo.arizona.edu/>

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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>

To: mann@virginia.edu

Subject: Fwd: CCNet: DEBUNKING THE "DANGEROUS CLIMATE CHANGE" SCARE

Date: Wed Apr 27 09:06:53 2005

Mike,

Presumably you've seen all this - the forwarded email from Tim. I got this email from McIntyre a few days ago. As far as I'm concerned he has the data - sent ages ago. I'll tell him this, but that's all - no code. If I can find it, it is likely to be hundreds of lines of

uncommented fortran ! I recall the program did a lot more than just average the series.

I know why he can't replicate the results early on - it is because there was a variance correction for fewer series.

See you in Bern.

Cheers

Phil

Dear Phil,

In keeping with the spirit of your suggestions to look at some of the other multiproxy publications, I've been looking at Jones et al [1998]. The methodology here is obviously more straightforward than MBH98. However, while I have been able to substantially emulate your calculations, I have been unable to do so exactly. The differences are larger in the early periods.

Since I have been unable to replicate the results exactly based on available materials, I would appreciate a copy of the actual data set used in Jones et al [1998] as well as the code used in these calculations.

There is an interesting article on replication by Anderson et al., some distinguished economists, here [1]<http://research.stlouisfed.org/wp/2005/2005-014.pdf> discussing the issue of replication in applied economics and referring favorably to our attempts in respect to MBH98.

Regards, Steve McIntyre

X-Mailer: QUALCOMM Windows Eudora Version 6.2.0.14

Date: Tue, 26 Apr 2005 13:28:53 +0100

To: Phil Jones <p.jones@uea.ac.uk>,"Keith Briffa" <k.briffa@uea.ac.uk>

From: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Fwd: CCNet: DEBUNKING THE "DANGEROUS CLIMATE CHANGE" SCARE  
Keith and Phil,

you both feature in the latest issue of CCNet:

#### (4) GLOBAL WARMING AND DATA

Steve Verdon, Outside the Beltway, 25 April 2005

[2]<http://www.outsidethebeltway.com/archives/10200>

A new paper ([3]<http://research.stlouisfed.org/wp/2005/2005-014.pdf>) from the St. Luis Federal Reserve Bank has an interesting paper on how important it is to archive not only the data but the code for empirical papers. While the article looks mainly at economic research there is also a lesson to be drawn from this paper about the current state of research for global warming/climate change. One of the hallmarks of scientific research is that the results can be replicable. Without this, the results shouldn't be considered valid let alone used for making policy.

Ideally, investigators should be willing to share their data and programs so as to encourage other investigators to replicate and/or expand on their results.<sup>3</sup> Such behavior allows science to move forward in a Kuhn-style linear fashion, with each generation seeing further from the shoulders of the previous generation.<sup>4</sup> At a minimum, the results of an endeavor-if it is to be labeled "scientific"-should be replicable, i.e., another researcher using the same methods should be able to reach the same result. In the case of applied economics using econometric software, this means that another researcher using the same data and the same computer software should achieve the same results.

However, this is precisely the problem that Steven McIntyre and Ross McKittrick have run into since looking into the methodology used by Mann, Hughes and Bradley (1998) (MBH98), the paper that came up with the famous "hockey stick" for temperature reconstructions. For example, this post here shows that McIntyre was prevented from accessing Mann's FTP site. This is supposedly a public site where interested researchers can download not only the source code, but also the data. This kind of behavior by Mann et. al. is simply unscientific and also rather suspicious. Why lock out a researcher who is trying to verify your results...do you have something to hide professors Mann, Bradley and Huges? Not only has this been a problem has this been a problem for McIntyre with regards to MBH98, but other studies as well. This post at Climate Audit shows that this problem is actually quite serious.

Crowley and Lowery (2000)

After nearly a year and over 25 emails, Crowley said in mid-October that he has misplaced the original data and could only find transformed and smoothed versions. This makes proper data checking impossible, but I'm planning to do what I can with what he sent. Do I need to comment on my attitude to the original data being "misplaced"?

Briffa et al. (2001)

There is no listing of sites in the article or SI (despite JGR policies requiring citations be limited to publicly archived data). Briffa has refused to respond to any requests for data. None of these guys have the least interest in some one going through their data and seem to hoping that the demands wither away. I don't see how any policy reliance can be made on this paper with no available data.

Esper et al. (2002)

This paper is usually thought to show much more variation than the hockey stick. Esper has listed the sites used, but most of them are not archived. Esper has not responded to any requests for data. '

Jones and Mann (2003); Mann and Jones (2004)

Phil Jones sent me data for these studies in July 2004, but did not have the weights used in the calculations, which Mann had. Jones thought that the weights did not matter, but I have found differently. I've tried a few times to get the weights, but so far have been unsuccessful. My surmise is that the weighting in these papers is based on correlations to local temperature, as opposed to MBH98-MBH99 where the weightings are based on correlations to the temperature PC1 (but this is just speculation right now.) The papers do not describe the methods in sufficient detail to permit replication.

Jacoby and d'Arrigo (northern treeline)

I've got something quite interesting in progress here. If you look at the original 1989 paper, you will see that Jacoby "cherry-picked" the 10 "most temperature-sensitive" sites from 36 studied. I've done simulations to emulate cherry-picking from persistent red noise and consistently get hockey stick shaped series, with the Jacoby northern treeline reconstruction being indistinguishable from simulated hockey sticks. The other 26 sites have not been archived. I've written to Climatic Change to get them to intervene in getting the data. Jacoby has refused to provide the data. He says that his research is "mission-oriented" and, as an ex-marine, he is only interested in a "few good" series.

Jacoby has also carried out updated studies on the Gaspé series, so essential to MBH98. I've seen a chronology using the new data, which looks completely different from the old data (which is a hockey stick). I've asked for the new data, but Jacoby-d'Arrigo have refused it saying that the old data is "better" for showing temperature increases. Need I comment? I've repeatedly asked for the exact location of the Gaspé site for nearly 9 months now (I was going to privately fund a re-sampling program, but Jacoby, Cook and others have refused to disclose the location.) Need I comment?

Jones et al (1998)

Phil Jones stands alone among paleoclimate authors, as a diligent correspondent. I have data and methods from Jones et al 1998. I have a couple of concerns here, which I'm working on. I remain concerned about the basis of series selection - there is an obvious risk of "cherry-picking" data and I'm very unclear what steps, if any, were taken to avoid this. The results for the middle ages don't look robust to me. I have particular concerns with Briffa's Polar Urals series, which takes the 11th century results down (Briffa arguing that 1032 was the coldest year of the millennium). It looks to me like the 11th century data for this series does not meet quality control criteria and Briffa was over-reaching. Without this series, Jones et al. 1998 is high in the 11th century. Note that none of this actually "disproves" the global warming hypothesis. However, it does raise very, very serious questions in my opinion. We are talking about enacting policies to curb global warming that could cost not billions, but trillions of dollars. Shouldn't we at least be allowed to see the source code, the data and ask for replication at a minimum? I think the answer is simple: YES!!

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## References

1. <http://research.stlouisfed.org/wp/2005/2005-014.pdf>
2. <http://www.outsidethebeltway.com/archives/10200>
3. <http://research.stlouisfed.org/wp/2005/2005-014.pdf>
4. <http://www.cru.uea.ac.uk/~timo/>
5. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

From: Phil Jones <p.jones@uea.ac.uk>  
To: Ben Santer <santer1@llnl.gov>  
Subject: HC  
Date: Fri Apr 29 10:30:20 2005

Ben,

Tom was here yesterday. He said you were going to the CCSP meeting for a day in Chicago, then flying on to the UK for the HC meeting May 18-19 (and 17th evening). Do you still want to come on up to Norwich afterwards?

Glad to hear from Tom you've been writing up your CCSP chapter and extending it significantly. He gave me a brief summary. I signed off yesterday on the CCSP report. You should be getting it through Tom Karl later today, or by Monday. As I did Ch 5, if you want to check anything with me feel free to. I wasn't able to stop some comments being put in by Lindzen, but Tom has a paper as does Myles which are enough to ignore his and the Douglass papers.

Cheers

Phil

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From: Phil Jones <p.jones@uea.ac.uk>  
To: Kevin Trenberth <trenbert@ucar.edu>  
Subject: Re: ppt for LA2  
Date: Thu May 5 08:08:55 2005

Apologies

Phil

Kevin,

Finally gotten around to putting thoughts down. Mostly on the challenges slides at the start. Maybe you would have said these things.

1. As well as suggesting the model chapters rank models (I don't think they will go with this - even though it is what we should be doing, and there are a whole raft of issues as to how to do it) should we also be dismissing observational papers that are clearly wrong (or a distortion of the facts and emphasizing the wrong issues).

In some parts of our chapter, we omit the poor papers. Just stressing that we are doing an assessment and not a review. An assessment is our expert view of the science at the present.

For space limitations we must omit many papers, but we must do this objectively. In the NRC review I made the point that most of the papers reviewed were the author's own. It is difficult and we must not fall into that trap. All this again comes back to assessment/review.

With 3.4.1 we mustn't get caught up in having to agree with the CCSP VTT report. We're either doing OUR assessment or we might as well give up.

Gone on for long enough on that one.

2. I think we both believe we should be saying somewhere what we should be measuring (how accurately, where and with what). If we don't say this somewhere, AR5 will be in a worse state. Susan is against this, but I think on this point she's wrong. IPCC has a lot of clout - much more than GCOS and/or WMO. It should be saying something about what we should be doing.

3. Minor point, just land warming more than ocean, not much more.

4. I guess you've expanded on linear trends enough

5. The CCSP diagrams are good, but I'm not keen on running means. I guess though they wouldn't be too different with a better smoother.

6. I guess you'll raise map projections. Could add in the new one Dave has done for precip to show the 30E edge.

The additional slides. Most of these are from a talk I have to give in Bern next month.

They relate

mostly to issues with Ch 6. Maybe you can add a couple of them. They relate to the issues of:

- making full use of the instrumental records to compare with proxy records

- changes in seasonality

- was the few hundred years before 1850 always colder than the post 1920 period.



## References

1. <http://www.cgd.ucar.edu/cas/>

From: Phil Jones <p.jones@uea.ac.uk>

To: Aiguo Dai <adai@cgd.ucar.edu>, Kevin Trenberth <trenbert@ucar.edu>

Subject: Re: more on section 3.7 and Marengo

Date: Thu May 5 08:45:53 2005

Cc: Jim Renwick <j.renwick@niwa.co.nz>, Panmao Zhai <pmzhai@cma.gov.cn>, Matilde Rusticucci <mati@at.fcen.uba.ar>, "David R. Easterling" <david.easterling@noaa.gov>

Kevin et al,

The diagram looks too good to me. CRU's data are reasonable over Brazil for some of the period, but poor in others, particularly recently. So we would have difficulty in updating this because of station numbers and quality. We could try using the GPCC dataset. They have huge numbers of stations for Brazil, but only for specific regions and periods, so likely problems there also.

We have a couple of papers in submission to J. Hydrology on flows in the subcatchments of the Parana river, which are well reproduced by rainfall, evaporation and a catchment model. Agree with your concerns about the Amazon flows not agreeing with the rainfall. Do the NAR and SAR regions fully encompass the enormous catchment though.

Cheers

Phil

At 17:36 03/05/2005, Aiguo Dai wrote:

One can use the Chen et al. and CRU to produce similar type of plots to validate Marengo's result.

He did use the CRU rainfall data set, but not for this particular plot.

Aiguo

Kevin Trenberth wrote:

Hi all

As you know we got some manuscripts from Jose Marengo to be considered in our chapter, and he is a LA on another chapter and will be in Beijing. He has offered to be CA.

My question concerns how good his data are? I asked Aiguo Dai to comment:

=====

One of the interesting results from Marengo's work is that he found the Northern and Southern Amazonia have opposite phase of decadal rainfall variations (see attached Fig. from Marengo 2004, Ther. Appl. Climatol.): In the northern Amazonia, rainfall is above normal during ~1945-1975 and below normal during ~1976-1998; and it is opposite in the southern Amazonia. He suggested warmer SST in central and eastern Pacific contributed to the dry conditions in the northern Amazonia during 1976-1998.

As noted in Betts et al. (2005, JHM, in press), Marengo's basin integrated rainfall index does not correlate well with Amazon river flow during the recent decades (worse than Chen et al.). This large multidecadal signal seems, however, robust.

=====

Certainly the attached figure is striking. Are we sure it is not due to changes in the way observations are made? Do other datasets replicate this? The lack of relation with

river flow is a substantial concern. Matilde, can you provide informed commentary? If the figure is good then maybe we should include it?

Kevin

--

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National Center for Atmospheric Research   Fax : 303-497-1333  
P.O. Box 3000, Boulder, CO 80307, USA    [1]www.cgd.ucar.edu/cas/adai/  
Street Address: 1850 Table Mesa Drive, Boulder, CO 80305, USA

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---

## References

1. <http://www.cgd.ucar.edu/cas/adai/>

From: "Polychronis Tzedakis" <P.C.Tzedakis@leeds.ac.uk>  
To: "Rainer Zahn" <rainer.zahn@uab.es>, "Thomas Stocker"  
<stocker@climate.unibe.ch>, "Atte Korhola" <atte.korhola@helsinki.fi>  
Subject: RE: commission performance alpha 5  
Date: Wed, 11 May 2005 16:25:11 +0100  
Cc: <Eystein.Jansen@geo.uib.no>, <Imprint-partner@bjerknes.uib.no>,  
<beatriz.balino@bjerknes.uib.no>, <atle.nesje@geo.uib.no>,  
<oyvind.lie@geo.uib.no>, <john.birks@bio.uib.no>,  
<Carin.Andersson@bjerknes.uib.no>, <trond.dokken@bjerknes.uib.no>,  
<ulysses.ninnemann@geo.uib.no>, <Astrid.Bardgard@fa.uib.no>,  
<richard.telford@bjerknes.uib.no>

Dear all,

First of all a big hand for Eystein and all those who put in so much time into this task. Very disheartening to hear the outcome.

I have much sympathy with what Rainer Zahn has said, especially on the Brussels front and the client relationships that are cultivated with EU officials.

I think that in addition to a letter to the EU, I would suggest that perhaps an editorial in NATURE or something similar, outlining the growing degree of scepticism amongst scientists regarding the transparency of the EU funding process might be in order.

Chronis Tzedakis

-----Original Message-----

From: Rainer Zahn [mailto:rainer.zahn@uab.es]  
Sent: Wed 5/11/2005 2:47 PM  
To: Thomas Stocker; Atte Korhola  
Cc: Eystein.Jansen@geo.uib.no; Imprint-partner@bjerknes.uib.no;  
beatriz.balino@bjerknes.uib.no; atle.nesje@geo.uib.no;  
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ulysses.ninnemann@geo.uib.no; Astrid.Bardgard@fa.uib.no;  
richard.telford@bjerknes.uib.no  
Subject: commission performance alpha 5

dear Eystein, dear Imprint consortium,

I am sure I will not make many friends with what follows below. Firstly, it surely is sad and disheartening to see our proposal going down. and there are many issues involved some of which have been named in the recent emails. But then there are those issues left that have not been named but which I consider relevant if we are to make progress on the EU FWP front. Some of these issues may and will touch a personal nerve here and there, but let's face some of the unpleasant realities much rather than sitting back and keeping going with business as usual, a business that soon may go out of existence.

First, I am not convinced that Imprint was the best we could have done. On my side I was surprised to no small extent during our London meeting to see that those from the modeling community and other groups present obviously had no idea why our palaeo-component (a derivative of the planned ICON IP) was part of Imprint, and they were not overly favourable to listen and expand their views. So in a sense, even within our own consortium there was, perhaps still is a lack of insight and understanding as to what a palaeo-component is about and will have to offer. In the end I am now left with the impression that ICON would have stood a good chance to survive on its own.

Second, as a member of the Imprint consortium I still find it difficult today to sort through this proposal and its various components, tasks, topics, milestones, deliverables etc. Which only tells me how ever so more difficult it must have been for outsiders i.e., reviewers to sift through the bits and pieces and comprehend what this is about. But I also feel that this has to do with the concept of IPs at large as it is not an easy task to compose an IP consortium of the dimension and wide range of expertise envisioned by the commission. The outcome of the whole process in my view confirms the notion that the concept of IPs has fundamentally (and to a large degree predictably) failed. This concept reflects a substantial lack of insight on the side of those who were, presumably still are involved in designing research policies in the commission about what science is about and how it works. Those parties should not be where they are, and they certainly should not be involved in setting up FWP7

This is what I have to say about our proposal.

As for the Commission's performance it is not my impression they are living up to their own standards that they have set up for the quality of proposals requested. In particular the proposal evaluation process is ridiculous and lacks any degree of substance. For instance, the reviews that I did receive in response to my RTN proposal (submitted last year) are mediocre at best, meaningless and useless in detail, beyond anything I would consider expert insight, simply a waste of time and tax payers' money. They are an insult to anybody who did contribute to and put work and effort into that proposal. As for the Imprint proposal we now are faced with the prospect that the only IP proposal, Millennium, that is competing with Imprint from the outset was received more favourably than our own proposal. With this I could live were it not for the fact that in Millennium everything is named as a strategy and work plan that we were

being advised to not do. This speaks a language of its own and to me reflects a fundamental lack of enthusiasm, professionalism and competence with those who give advice and organize the evaluation process.

Obviously,

the vision set out by our programme manager(s) never made it to the reviewers who seemed to follow quite different guidelines, if any.

Lastly, from what I can see around me, particularly in the Mediterranean club, it appears more important and beneficial to spend time in Brussels wiping door handles and leaving a professorial - directorial impression rather than composing upbeat cutting edge science proposals. It is ever so

disheartening that within the FWP our success seems to depend more on who we know than the quality we present. Last time when programme managerial posts in the commission were reshuffled the primary concern around here was

that "we now lose our contacts". This is wrong, a disgrace to our community.

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and more so, about the chaotic way proposals are being solicited and then turned down on grounds that so very obviously have nothing to do with the science presented. There is also the notion that within the commission climate and paleo-work has fallen from grace, for reasons not known to many. Which brings me back to the point that perhaps we do not have the right programme managers in place to fend our cause.

I am prepared to write a firm letter to the commission, or to contribute to

such letter, about the issues impinging on the poor performance of the commission. I rather do that before turning entirely into a full-grown Euroseptic.

Rainer

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From: Denis-Didier.Rousseau@uni-bayreuth.de  
To: <Eystein.Jansen@geo.uib.no>, <Imprint-partner@bjerknes.uib.no>  
Subject: [Fwd: RE: commission performance alpha 5]  
Date: Thu, 12 May 2005 04:48:04 +0200 (MEST)

Dear all IMPRINT colleagues,  
Being away from Europe, this was a very bad news that I got this morning listening about the rejection of IMPRINT. Eystein did a great job by being able to gather the European paleo community under a common umbrella and he deserves a lot of our consideration.

Concerning now the review process, I have been involved several times in Brussels and I have been able to see the evolution of the evaluating panel session after session.

I am not please with this evaluation and I already addressed my comments to Andre Berger. It is not normal that entering the room where you are supposed to meet the other "panelists" you would not know those who are supposed to be representative of your community, this is my first comment.

Second, the way the referees are selected is somehow strange and involve a political issue which is very sensitive as I'm sure you will understand that a country fair representation is not enough in our field which better involves expertise.

Third and last, having set a consortium of the leading Europe institutions and scientists, how can you expect appropriate expertise? I have been approached to join the evaluating panel but refused as being an IMPRINT member to respect some ethic. If, what I wish, we all didi that way, they one can sincerely expect the worst as I already experienced in a recent past.

Forth, complaining to the commission is a waste of time as these administrative people, even if this is you right, will always provide you with arguments to justify the decision. I complain once to the director of the programme who just retun me that the referees of my proposal were relevant, what I know was not the case unfortunately. However I totally support the initiative to question the commission on the way the evaluations are performed, but also how the referees are selected.

Fifth, you all are waiting for the reviews. I agree with Rainer that the comments that are provided are useless and in somehow offending the PIs. This is mostly due to the review process and this again must be changed.

Furthermore what we receive is the consensus report which passed in the European officers hands to be cleaned of any aggressive sentences or words, and must remain politically correct. So effectively these reports are useless. It would be interesting to get also the individual reports on which the consensus one has been established and would better show the real work of every referee, and we would be very surprised sometimes.

Finally to follow Thomas, Rainer and Eric, I would suggest to continue what has been launched with IMPRINT which is to my sense unique in gathering all the European paleo community under the same umbrella. May be the proposal was too broad, but this was following the commission's aim. The "Millenium" proposal benefited of several consecutive EU supports which apparently helped a lot. Their lobbying seem to have ben very efficient, not only in Brussels but in the journals and meetings. The Utrecht initiative was a good one which must stop today. We have the opportunity to gather regularly at least once during the EGU that we all are attending, why not using such opportunity to reinforce the initiative during such meeting?

All the very best to all of you

cheers

denis

----- Urspr&uuml;ngliche Nachricht -----  
Betreff: RE: commission performance alpha 5  
Von: "Polychronis Tzedakis" <P.C.Tzedakis@leeds.ac.uk>  
Datum: Mit, 11.05.2005, 17:25  
An: "Rainer Zahn" <rainer.zahn@uab.es>,  
"Thomas Stocker" <stocker@climate.unibe.ch>,  
"Atte Korhola" <atte.korhola@helsinki.fi>

Dear all,  
First of all a big hand for Eystein and all those who put in so much time into this task. Very disheartening to hear the outcome.

I have muych sympathy with what Rainer Zahn has said, especially on the Brussels front and the client relationships that are cultivated with EU officials.

I think that in addition to a letter to the EU, I would suggest that perhaps an editorial in NAture or something similar, outlining the growing degree of scepticism amongst scientists regarding the transparency of the EU funding process might be in order.

Chronis Tzedakis

-----Original Message-----

From: Rainer Zahn [mailto:rainer.zahn@uab.es]

Sent: Wed 5/11/2005 2:47 PM

To: Thomas Stocker; Atte Korhola

Cc: Eystein.Jansen@geo.uib.no; Imprint-partner@bjerknes.uib.no;

beatriz.balino@bjerknes.uib.no; atle.nesje@geo.uib.no;

oyvind.lie@geo.uib.no; john.birks@bio.uib.no;

Carin.Andersson@bjerknes.uib.no; trond.dokken@bjerknes.uib.no;

ulysses.ninnemann@geo.uib.no; Astrid.Bardgard@fa.uib.no;

richard.telford@bjerknes.uib.no Subject: commission performance alpha 5

dear Eystein, dear Imprint consortium,

I am sure I will not make many friends with what follows below. Firstly, it surely is sad and disheartening to see our proposal going down. and there are many issues involved some of which have been named in the recent emails. But then there are those issues left that have not been named but which I consider relevant if we are to make progress on the EU FWP front. Some of these issues may and will touch a personal nerve here and there, but let's face some of the unpleasant realities much rather than sitting back and keeping going with business as usual, a business that soon may go out of existence.

First, I am not convinced that Imprint was the best we could have done. On my side I was surprised to no small extent during our London meeting to see that those from the modeling community and other groups present obviously had no idea why our palaeo-component (a derivative of the planned ICON IP) was part of Imprint, and they were not overly favourable to listen and expand their views. So in a sense, even within our own consortium there was, perhaps still is a lack of insight and understanding as to what a palaeo-component is about and will have to offer. In the end I am now left with the impression that ICON would have stood a good chance to survive on its own.

Second, as a member of the Imprint consortium I still find it difficult

today to sort through this proposal and its various components, tasks, topics, milestones, deliverables etc. Which only tells me how ever so more difficult it must have been for outsiders i.e., reviewers to sift through the bits and pieces and comprehend what this is about. But I also feel that this has to do with the concept of IPs at large as it is not an easy task to compose an IP consortium of the dimension and wide range of expertise envisioned by the commission. The outcome of the whole process in my view confirms the notion that the concept of IPs has fundamentally (and to a large degree predictably) failed. This concept reflects a substantial lack of insight on the side of those who were, presumably still are involved in designing research policies in the commission about what science is about and how it works. Those parties should not be where they are, and they certainly should not be involved in setting up FWP7

This is what I have to say about our proposal.

As for the Commission's performance it is not my impression they are living up to their own standards that they have set up for the quality of proposals requested. In particular the proposal evaluation process is ridiculous and lacks any degree of substance. For instance, the reviews that I did receive in response to my RTN proposal (submitted last year) are mediocre at best, meaningless and useless in detail, beyond anything I would consider expert insight, simply a waste of time and tax payers' money. They are an insult to anybody who did contribute to and put work and effort into that proposal. As for the Imprint proposal we now are faced with the prospect that the only IP proposal, Millennium, that is competing with Imprint from the outset was received more favourably than our own proposal. With this I could live were it not for the fact that in Millennium everything is named as a strategy and work plan that we were being advised to not do. This speaks a language of its own and to me reflects a fundamental lack of enthusiasm, professionalism and competence with those who give advice and organize the evaluation process. Obviously, the vision set out by our programme manager(s) never made it to the reviewers who seemed to follow quite different guidelines, if any.

Lastly, from what I can see around me, particularly in the Mediterranean club, it appears more important and beneficial to spend time in Brussels wiping door handles and leaving a professorial - directorial impression rather than composing upbeat cutting edge science proposals. It is ever so disheartening that within the FWP our success seems to depend more on who we know than the quality we present. Last time when

programme managerial posts in the commission were reshuffled the primary concern around here was that "we now lose our contacts". This is wrong, a disgrace to our community.

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Rainer

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<<http://www.icrea.es/pag.asp?id=Rainer.Zahn>><http://www.icrea.es/pag.asp?id=Rainer.Zahn>

From: Phil Jones <p.jones@uea.ac.uk>  
To: Katarina Kivel <kivel@stanford.edu>  
Subject: Re: Stephen Schneider's request for review of Wahl-Ammann paper on MBH Robustness for Climatic Change  
Date: Fri May 13 16:47:39 2005

Katerina,

I will be able to review this, despite just coming back from IPCC.

Cheers

Phil

At 20:04 12/05/2005, you wrote:

Dear Phil,

Attached is a letter from Steve Schneider requesting review of the above referenced paper, which is also sent as an attachment (ms and four figures).

Please acknowledge receipt and let us know if you need a hard copy.

Regards,

Katarina

Katarina Kivel

Assistant Editor, CLIMATIC CHANGE

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NR4 7TJ

UK

---

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: Eystein Jansen <eystein.jansen@geo.uib.no>  
Subject: Re: [Fwd: Re: URGENT : IMPRINT en RTN ?]  
Date: Tue May 17 17:03:25 2005

Eystein

We have now heard from Hans Brelen that Millennium will definitely be funded . This means that the very worst case scenario has been realised - because it means that the EU are not likely to call for any palaeoclimate in the next funding round.

I have to say that though there is normally an unfortunate element of randomness in the refereeing of EU proposals , that to a large extent is unfortunate but inevitable, I believe strongly that the system has let us down very badly in this case. It is clear that we, the IMPRINT community were misled ; first by Ib Troen's direction (given publicly in Utrecht) that we should produce a proposal which was of the scale to unify the whole Palaeoclimate community , with a specific role to bring data and modelling foci to bear on the issue of climate predictability; that we should be careful to not to over-emphasise the collection of new data but rather work mostly to consolidate and jointly interpret existing data , and that we should formulate a scheme were these fed directly into a hierarchy of modelling that would address model viability and issues of probability of future climate and its causes.

Secondly, We were misled by the accepting , on the basis of the published call, that the EU required IP proposals of ambitious scope , large enough to move the science of European palaeoclimate forward as a whole and with relevance to globally important issues, with aims clearly beyond the scope of "slightly bigger STREPS" . On reading the cursory referees' responses to our proposal , I am also moved to express my own opinion that they are an insult to the community of researchers that constitute IMPRINT , and an indictment of the failure of the referees to address their assessment to the generally publicized aims of the IP concept. To describe the whole proposal as "too complicated", and to state that there is " no value" in the first four workpackages , and most of all to rate the quality of the consortium as 4 out of 5 , all require explicit justification well beyond the few lines with which we are presented.

While I have no ill will at all regarding the competing proposal Millennium , I feel that the extended IMPRINT community can justifiably ask very serious questions regarding the apparent lack of equitable assessment of the two proposals in the light of the published call requirements - the efforts of the IMPRINT consortium over recent months at least deserve answers as to how , for the sake of 0.5 of a mark , that proposal will be funded when it clearly did not address the scope of the original call - in terms of community integration, emphasis on wider data consolidation, scope of model hierarchy, and specific addressing of the data/model integration towards the issue of climate sensitivity/predictability.

Expressing these concerns should not be considered "sour grapes " . They are not and I congratulate the MILLENNIUM team on having succeeded . Rather these comments are justified because the review process has not taken account of the scope of the IP concept, and the need to invoke a research plan with the necessary breadth and expertise (and proven

managerial ability - as can be gauged by the assessment of the CARBO OCEAN coordination plan) , and because the success of the much more limited MILLENNIUM project has already been cited by European officials as justification for the lack of any need to fund palaeoclimate research in the next call - effectively cutting off the wider paleoclimate community from EU research support for the next few years.

I believe we are justified in questioning the operation of the IP concept , beyond the EU administration, which has , in my opinion has done a serious dis-service to our community and palaeoclimate in general.

At 08:26 16/05/2005, Valérie Masson-Delmotte wrote:

Dear Eystein and Keith,

I hope that you had a good trip back from Beijing. On our side it was a bit hectic (3 hours delay in Amsterdam, arrival at midnight in Paris and lost of Pascale's luggage without ability to find where it was lost!).

I have just received this suggestion from a CEA EC representative that there is a RTN Marie Curie call for september 8th which has a lot of funding - 220 Meuros. You can apply for up to 6 M euros for a series of PhD thesis and postdocs around a real research network (up to 3-4 contracts per participant).

I think that it is an excellent idea... if you and your people, Eystein, would be ready to put some more energy in the proposal.

It would require to re think about the scientific perimeter and the key partners maybe.

Sincerely

Valerie.

Return-Path: <Jean.jouzel@cea.fr>

Received: from muguet.saclay.cea.fr (muguet.saclay.cea.fr [132.166.192.6])  
by dsm-mail.saclay.cea.fr (8.12.11/jtpda-5.4) with ESMTTP id j4G6I6mU023329  
for <masson@dsm-mail.saclay.cea.fr>; Mon, 16 May 2005 08:18:06 +0200

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by muguet.saclay.cea.fr (8.12.10/8.12.10/CEAnet-internes.4.0) with ESMTTP id  
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for <masson@dsm-mail.saclay.cea.fr>; Mon, 16 May 2005 08:18:07 +0200 (MEST)

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(Content Technologies SMTPRS 4.3.17) with ESMTTP id

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Mon, 16 May 2005 08:18:07 +0200

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by agrione.extra.cea.fr (8.12.11/8.12.11) with ESMTTP id j4G6FXcJ010248;

Mon, 16 May 2005 08:15:33 +0200

(envelope-from jouzel@dsm-mail.saclay.cea.fr)

Received: from shiva.jussieu.fr (shiva.jussieu.fr [134.157.0.129])

by cirse.extra.cea.fr (8.12.10/8.12.10/CEAnet-Internet.4.0) with ESMTTP id

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Mon, 16 May 2005 08:18:05 +0200 (MEST)

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by shiva.jussieu.fr (8.12.11/jtpda-5.4) with ESMTTP id j4G6I069096644

; Mon, 16 May 2005 08:18:03 +0200 (CEST)

X-Ids: 165

Mime-Version: 1.0

X-Sender: jzipsl@mailhost.ipsl.jussieu.fr (Unverified)

Message-Id: <v04220801beae642fdb0b@[134.157.81.184]>

In-Reply-To:

<C10DEAFD7469D611878C00B0D0F37B8B012424B2@sophia.saclay.cea.fr>

References: <C10DEAFD7469D611878C00B0D0F37B8B012424B2@sophia.saclay.cea.fr>

Date: Mon, 16 May 2005 07:57:43 -0700

To: CAMINADE Jean Pierre <CAMINADE@dsmdir.cea.fr>

From: Jean Jouzel <Jean.jouzel@cea.fr>

Subject: Re: URGENT : IMPRINT en RTN ?

Cc: masson@dsm-mail.saclay.cea.fr

Content-Type: multipart/alternative;

boundary="===== \_-1095865763==\_ma====="

X-Greylist: Sender IP whitelisted, not delayed by milter-greylis-1.7.2

(shiva.jussieu.fr [134.157.0.165]); Mon, 16 May 2005 08:18:05 +0200 (CEST)

X-Antivirus: scanned by sophie at shiva.jussieu.fr

X-Miltered: at dsm-mail with ID 42883B1E.000 by Joe's j-chkmail

([1]http://j-chkmail.ensmp.fr)!

X-Miltered: at shiva.jussieu.fr with ID 42883B18.001 by Joe's j-chkmail

([2]http://j-chkmail.ensmp.fr)!

X-CEA-Source: externe

X-CEA-DebugSpam: 13%

X-CEA-Spam-Report: The following antispam rules were triggered by this message:

Rule	Score	Description
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DATE_IN_FUTURE_06_12	1.300	Date: is 6 to 12 hours after Received: date
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X-CEA-Spam-Hits: DATE\_IN\_FUTURE\_06\_12 1.3, \_\_CT 0, \_\_CTYPE\_HAS\_BOUNDARY 0, \_\_CTYPE\_MULTIPART 0, \_\_CTYPE\_MULTIPART\_ALT 0, \_\_HAS\_MSGID 0, \_\_MIME\_VERSION 0, \_\_SANE\_MSGID 0

X-Spam-Checker-Version: SpamAssassin 2.64 (2004-01-11) on dsm-mail.cea.fr

X-Spam-Level:

X-Spam-Status: No, hits=-2.9 required=4.0 tests=BAYES\_00,DATE\_IN\_FUTURE\_06\_12 autolearn=no version=2.64

Cher Jean - Pierre,

Excuse-moi de réagir un peu tardivement (je reviens de Chine).

Mais surtout merci pour ce courrier et l'aide proposée ; je pense vraiment que cela vaudrait le coup de le relancer sous la forme RTN et que l'obtention de post-docs correspond bien à l'idée d'imprint (exploitation des données, modélisation).

Pour faire avancer les choses je mets copie à Valérie Masson - Delmotte une des chevilles ouvrières d'IMPRINT au LSCE. Je suggère à valérie de te contacter directement.

Bien amicalement Jean

Bonjour Jean,

J'ai appris ce matin au GTN environnement qu'IMPRINT n'avait pas été accepté.

Avez-vous pensé à le relancer sous la forme d'un (ou de plusieurs) RTN-Marie Curie (Research Training Network) pour l'appel du 8 septembre qui est richement doté (220 MEuros ! du jamais vu !); le montant demandé peut aller jusqu'à 6 MEuros, pas très loin d'IMPRINT.

Il s'agit de proposer une série de post-docs et de thèses articulés autour d'un véritable projet de recherche; environ 3 à 4 CDD pour chaque participant.

La DSM a une expérience dans ce domaine (Greencycles rien qu'au LSCE); on peut t'aider à te monter un projet taillé sur mesures.

Aujourd'hui je ne vois que cette solution car manifestement la ligne "modélisation climat" ne repassera pas au 4ème appel et je ne vois rien d'autre d'aussi bien "doté" dans le paysage du FP6 (qui est sur sa fin).

Cordialement

JPC

<[3]http://promos.hotbar.com/promos/promodll.dll?RunPromo&El=&SG=&RAND=25607&partner=hotbar>

Jean Jouzel

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e-mail : jouzel@lsce.saclay.cea.fr

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Phone: +44-1603-593909

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[4]http://www.cru.uea.ac.uk/cru/people/briffa/

## References

1. <http://j-chkmail.ensmp.fr/>
2. <http://j-chkmail.ensmp.fr/>
3. <http://promos.hotbar.com/promos/promodll.dll?RunPromo&El=&SG=&RAND=25607&partner=hotbar>
4. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: Eystein Jansen <eystein.jansen@geo.uib.no>  
Subject: IMPRINT  
Date: Tue May 17 17:24:34 2005  
Cc: Ib Troen

Eystein

We have now heard that Millennium will definitely be funded . This means that the very worst case scenario has been realised - because it means that the EU are not likely to call for any palaeoclimate in the next funding round.

I have to say that, though there is normally an element of randomness in the refereeing of EU proposals , that to a large extent is unfortunate but inevitable, I believe strongly that the system has let us down very badly in this case.

It is clear that we, the IMPRINT community were misled ; first by Ib Troen's direction (given publicly in Utrecht) that we should produce a proposal which was of the scale to unify the whole Palaeoclimate community , with a specific role to bring data and modelling foci to bear on the issue of climate predictability; that we should be careful to not to over-emphasise the collection of new data but rather work mostly to consolidate and jointly interpret existing data , and that we should formulate a scheme where these are fed directly into a hierarchy of modelling experiments that would address causes of climate change, model viability and issues of probability of future climate and its causes.

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Expressing these concerns should not be considered "sour grapes " . They are not and I congratulate the MILLENNIUM team on having succeeded . They will do valuable research.

Rather these comments are justified because the review process has not taken account of the scope of the IP concept, and the need to invoke a research plan with the necessary breadth and expertise (and proven managerial ability - as can be gauged by the assessment of the CARBO OCEAN coordination plan) , and because the success of the much more limited MILLENNIUM project has already been cited by European officials as justification for the lack of any need to fund palaeoclimate research in the next call - effectively cutting off the wider palaeoclimate community from EU research support for the next few years. I believe we are justified in questioning the operation of the IP concept , and questioning it in fora beyond the circle of EU administration, which has , in my opinion has done a serious dis-service to our community and palaeoclimate in general. At the very least , the "goalposts" regarding IP proposals seem to have been moved and the time of many researchers has been wasted.

Please feel free to forward this message to the rest of our group .

At 08:26 16/05/2005, Valérie Masson-Delmotte wrote:

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by dsm-mail.saclay.cea.fr (8.12.11/jtpda-5.4) with ESMTP id j4G6I6mU023329

for <masson@dsm-mail.saclay.cea.fr>; Mon, 16 May 2005 08:18:06 +0200

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(Content Technologies SMTPRS 4.3.17) with ESMTP id

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Mon, 16 May 2005 08:18:07 +0200

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by agrione.extra.cea.fr (8.12.11/8.12.11) with ESMTP id j4G6FXcJ010248;

Mon, 16 May 2005 08:15:33 +0200

(envelope-from jouzel@dsm-mail.saclay.cea.fr)

Received: from shiva.jussieu.fr (shiva.jussieu.fr [134.157.0.129])

by cirse.extra.cea.fr (8.12.10/8.12.10/CEAnet-Internet.4.0) with ESMTP id j4G6I5AN028850;

Mon, 16 May 2005 08:18:05 +0200 (MEST)

Received: from [134.157.81.169] (169.ext.jussieu.fr [134.157.81.169])

by shiva.jussieu.fr (8.12.11/jtpda-5.4) with ESMTP id j4G6I069096644

; Mon, 16 May 2005 08:18:03 +0200 (CEST)

X-Ids: 165

Mime-Version: 1.0

X-Sender: jzipsl@mailhost.ipsl.jussieu.fr (Unverified)

Message-Id: <v04220801beae642fdb0b@[134.157.81.184]>

In-Reply-To:

<C10DEAFD7469D611878C00B0D0F37B8B012424B2@sophia.saclay.cea.fr>

References: <C10DEAFD7469D611878C00B0D0F37B8B012424B2@sophia.saclay.cea.fr>

Date: Mon, 16 May 2005 07:57:43 -0700

To: CAMINADE Jean Pierre <CAMINADE@dsmmail.cea.fr>

From: Jean Jouzel <Jean.jouzel@cea.fr>

Subject: Re: URGENT : IMPRINT en RTN ?

Cc: masson@dsm-mail.saclay.cea.fr

Content-Type: multipart/alternative;

boundary="=====-1095865763==\_ma====="

X-Greylist: Sender IP whitelisted, not delayed by milter-greylist-1.7.2

(shiva.jussieu.fr [134.157.0.165]); Mon, 16 May 2005 08:18:05 +0200 (CEST)

X-Antivirus: scanned by sophie at shiva.jussieu.fr

X-Miltered: at dsm-mail with ID 42883B1E.000 by Joe's j-chkmail

([1]http://j-chkmail.ensmp.fr)!

X-Miltered: at shiva.jussieu.fr with ID 42883B18.001 by Joe's j-chkmail

([2]http://j-chkmail.ensmp.fr)!

X-CEA-Source: externe

X-CEA-DebugSpam: 13%

X-CEA-Spam-Report: The following antispam rules were triggered by this message:

Rule	Score	Description
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DATE_IN_FUTURE_06_12	1.300	Date: is 6 to 12 hours after Received: date
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X-CEA-Spam-Hits: DATE\_IN\_FUTURE\_06\_12 1.3, \_\_CT 0, \_\_CTYPE\_HAS\_BOUNDARY 0, \_\_CTYPE\_MULTIPART 0, \_\_CTYPE\_MULTIPART\_ALT 0, \_\_HAS\_MSGID 0, \_\_MIME\_VERSION 0, \_\_SANE\_MSGID 0

X-Spam-Checker-Version: SpamAssassin 2.64 (2004-01-11) on dsm-mail.cea.fr

X-Spam-Level:

X-Spam-Status: No, hits=-2.9 required=4.0 tests=BAYES\_00,DATE\_IN\_FUTURE\_06\_12 autolearn=no version=2.64

Cher Jean - Pierre,

Excuse-moi de réagir un peu tardivement (je reviens de Chine).

Mais surtout merci pour ce courrier et l'aide proposée ; je pense vraiment que cela vaudrait le coup de le relancer sous la forme RTN et que l'obtention de post-docs correspond bien à l'idée d'imprint (exploitation des données, modélisation).

Pour faire avancer les choses je mets copie à Valérie Masson - Delmotte une des

chevilles ouvrières d'IMPRINT au LSCE. Je suggère à valérie de te contacter directement.

Bien amicalement Jean

Bonjour Jean,

J'ai appris ce matin au GTN environnement qu'IMPRINT n'avait pas été accepté.

Avez-vous pensé à le relancer sous la forme d'un (ou de plusieurs) RTN-Marie Curie (Research Training Network) pour l'appel du 8 septembre qui est richement doté (220 MEuros ! du jamais vu !); le montant demandé peut aller jusqu'à 6 MEuros, pas très loin d'IMPRINT.

Il s'agit de proposer une série de post-docs et de thèses articulés autour d'un véritable projet de recherche; environ 3 à 4 CDD pour chaque participant.

La DSM a une expérience dans ce domaine (Greencycles rien qu'au LSCE); on peut t'aider à te monter un projet taillé sur mesures.

Aujourd'hui je ne vois que cette solution car manifestement la ligne "modélisation climat" ne repassera pas au 4ème appel et je ne vois rien d'autre d'aussi bien "doté" dans le paysage du FP6 (qui est sur sa fin).

Cordialement

JPC

<[3]http://promos.hotbar.com/promos/promodll.dll?RunPromo&El=&SG=&RAND=25607&partner=hotbar>

Jean Jouzel

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<br>

<br>

</blockquote></x-html>

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[4]<http://www.cru.uea.ac.uk/cru/people/briffa/>

## References

1. <http://j-chkmail.ensmp.fr/>
2. <http://j-chkmail.ensmp.fr/>
3. <http://promos.hotbar.com/promos/promodll.dll?RunPromo&El=&SG=&RAND=25607&partner=hotbar>
4. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: Eystein.Jansen@geo.uib.no  
Subject: wishing to talk  
Date: Wed May 18 10:31:11 2005

so can you give me a number where I can reach you - after your meeting . I am in and out trying to do various things , but wish to discuss "next steps" . Did you get my email last evening?

Keith

--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

## References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
To: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Fwd: imprint  
Date: Wed, 18 May 2005 14:16:38 +0200

<x-flowed>

Hi Keith,  
for your information, I have enclosed the letter received on the outcome of phase 1, and the guidance for Stage 2. We will dig up more. I also talked with Christoph Heinze who said this definately has the flair of someone in the review panel having an agenda of revenge, and that this could be an element of a formal complaint.

More later,  
Eystein

--

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Fax: +47-55-584330  
</x-flowed>

Attachment Converted: "c:\eudora\attach\IMPRINT\_QI\_letter 1.pdf"

Attachment Converted: "c:\eudora\attach\IMPRINT\_ESR 1.pdf"

From: Phil Jones <p.jones@uea.ac.uk>  
To: "Michael E. Mann" <mann@virginia.edu>  
Subject: Empire Strikes Back - return of proper science !  
Date: Fri May 20 13:45:26 2005

Mike,

Just reviewed Caspar's paper with Wahl for Climatic Change. Looks pretty good. Almost reproduced your series and shows where MM have gone wrong. Should keep them quiet for a while. Also they release all the data and the R software. Presume you know all about this. Should make Keith's life in Ch 6 easy !

Also, confidentially for a few weeks, Christy and Spencer have admitted at the Chicago CCSP meeting that their 2LT record is wrong !! They used the wrong sign for the diurnal correction ! Series now warms - not quite as much as the surface but within error bands. Between you and me, we'll be going with RSS in Ch 3 and there will be no discrepancy with the surface and the models. Should make Ch 3 a doddle now ! Keep quiet about this until Bern at least. Can tell you more then. RSS (Carl Mears and Frank Wentz) found the mistake !

The skeptic pillars are tumbling !

Cheers

Phil

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University of East Anglia  
Norwich Email p.jones@uea.ac.uk  
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From: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
To: imprint-ssc@bjerknes.uib.no  
Subject: Urgent-next step  
Date: Fri, 20 May 2005 23:30:47 +0200  
Cc: stocker@climate.unibe.ch, André Berger <berger@astr.ucl.ac.be>

<x-flowed>

Dear friends of the Imprint - SSC,

After seeing the evaluation summary of our proposal, and not least the same for Millennium, it is clear to me that we have been very badly treated, first by the public advice from the Commission in Utrecht who advised the community to create a proposal which we did, but which is orthogonal to what they now have decided to negotiate, later by the random way we were reviewed and the many inconsistencies in the evaluation. Compared to this the Millennium review was full of subjective phrases and a number of negative aspects were glossed over. The review is an insult, and it appears likely that elements in the panel bear some grudges against our community. In order to get the 0.5 point difference between Imprint and Millennium they had to give a number of very imbalanced statements. They also had to raise the management score of Millennium to 4 by the xtended panel despite criticisms by the reviewers that the management was not well laid out.

I feel that the review was very biased and the result is that they will probably fund a project with only limited relevance to the call, and miss a major opportunity of integrating European paleoclimate research and climate modelling and create a new major step forward.

We have been advised to send a formal letter of complaint to the Commission, asking for a renewed evaluation, not because we think there is a good chance that it will lead to much, but we think it

is important that they know that they have upset a community consisting of top level European scientists, This may help us in the longer term.

The advice I have got is to send this to Pierre Valette, co-signed by the key partners, both their PIs and head of administration, with copies to our individual national members of the Global Change Panel of the EU.

So far there is no formal decision on which proposal to fund, this may happen in September after negotiations with the selected proposals. There is a seldom precedence in Europe that such an intervention has been successful, but very rarely.

In phrasing such a letter we have to be very careful and make sure our message is clear and fair, but I think it needs to be done.

I would therefore ask you to respond immediately to this mail as to whether you think we should go this route or not. We will then in a few days send out a draft for comments, if you agree that we shall send in a complaint. We have to move fast here, so I hope you will be quick.

Concerning the other proposals on what to do, there are many good ideas, and I think we should have a meeting in the autumn to discuss the strategy of securing paleo in the 7th Framwork program. The text is out for review now, and we all need to suggest changes through our national representatives. I will distribute a list of who this is for the various countries over the week-end.

I am also working on formulating text to help launch our ideas in teh European Parliament via Atte´s wife.

Best wishes,

Eystein

</x-flowed>



From: Jonathan Overpeck <jto@u.arizona.edu>  
To: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: IPCC - your section  
Date: Mon, 23 May 2005 22:46:11 -0700  
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith - thanks again for the help in Beijing. We hope you found a fabulous clay pot or at least some good views of China.

We know it's going to be extra hard on you to get everything done on time, but we're hoping you can more-or-less stick to the schedule we just sent around. Your section is going to be the big one, and we need to make sure we have as much review and polishing as possible. If we don't we (especially you) will pay heavily at FOD review time. Lots of work now saves even more work later. Or so the real veterans tell us.

Lastly, we wanted you to know that we can probably win another page or two (total, including figs and refs) if you end up needing it. Susan didn't promise this, but she gave us the feeling that we could get it if we ask - but probably only for your section, and maybe an extra page for general refs (although we're not going to mention this to the others, since we're not sure we can get it). Note that some of the methodological parts of your sections should go into supplemental material - this has to be written just as carefully, but it gives you another space buffer. All this means you can do a good job on figures, rather than the bare minimum. We're hoping you guys can generate something compelling enough for the TS and SPM - something that will replace the hockey-stick with something even more compelling.

Anyhow, thanks in advance for what is most likely not going to be your number 1 summer to remember. That said, what we produce should provide real satisfaction.

Best, Peck and Eystein

--

Jonathan T. Overpeck  
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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>  
To: Kevin Trenberth <trenbert@ucar.edu>  
Subject: Re: Ch 3  
Date: Thu May 26 11:15:11 2005

Kevin,

I'll broach it with the UK people. Need to consider timing in November, once we get the comments or maybe after the ChCh meeting. Been to Boulder in Jan and Feb before so know what to expect ! Early Feb would seem best. Not thought about going to the AMS so won't.

A few problems with Figures today. Hopefully they will get resolved in the not too distant future. Dave E has at least sent one email.

Seeing our granddaughter on Saturday, but should have some good time for the Chapter on Sunday and Monday (at home).

Cheers

Phil

At 17:11 25/05/2005, you wrote:

Hi Phil

I am attaching the updated Fig 3.4.? I have also in .ps that can be converted if need be.

Dennis has also plotted the Fu data and I'll send a version a bit later. But need to have consistent colors.

I am encouraged that the text is getting a lot better. The FOD is approaching close to what will be final, we should find. After that point the figs should only be updates and minor changes, and the text is modified to respond to comments, that we will have to address more systematically next time. The SOD does become close to final: still subject to all the reviews and late breaking material.

Key thing is for you and me to make sure we converge, and don't do a wholesale replacement of a section without careful checking.

I have decided not to attend AMS AGM next year in January so that I can work on the SOD. I would be glad to invite you to come for a visit for a week and I suspect we can also come up with some funds to help: at the price of a seminar. e.g. we could split it by you doing airfare and we do local accommodation or vice versa? This summer Tom Stocker is here and working with Jerry on chap 10. I think it could be worthwhile, main question is best timing. Perhaps late Jan or early Feb? That time of year can be cold here: usually not that much snow or if it does snow it does not last long in Boulder: great skiing nearby if you are interested in that. Mean T in Jan is about 0C but highs not uncommon about 10C, and have been over 20C with chinook. Cold at night. So good idea.

Cheers

Kevin

Phil Jones wrote:

Kevin,

Things seem to be coming in. Will work on 3.5-3.7 tomorrow. 3.2 and the Appendices now back with David. The Appendices read pretty good - lots of useful background material. It will be shame to lose it to a web site. Once David gets these back these should be almost good enough to go out to all on July 15 (or whenever we said).

A thought kept recurring - there must be a better way to do this ! Although the FOD reviews will be different from the ZOD (and many more), I'm prepared to come to Boulder for a week

in early 2006 if needed. I think I can get the money from the UK to do this. Question is

will be it be worthwhile. Better if we were both locked away somewhere other than one of our institutions, but then we wouldn't have the infrastructure, support (email, printers etc).

Anyway, give it some thought. You'll know more than I do about some much the FOD and SOD change. Q is whether a week or a fortnight is sufficient. If we knew that a few of the

key people in the chapter were at their desks, the text should show a marked improvement.

Assuming here the majority of the Figures set by then - just a few need updating.

Cheers

Phil

At 17:03 24/05/2005, you wrote:

Hi Phil

Thanks for update: monday is a holiday here: Memorial Day, seems weird that Brian is working?

My approach to the revisions at this stage is not to take the material sent and wholesale replace it, but cautiously compare and insert if it makes sense. i.e. you and I need to act as editors with a fairly strong hand. I suspect 3.7 may have some useful material but it could degrade the section by further adding material that is not especially relevant. I'll bet it does not shorten it, which is desired still.

I am clearly not on same page as Brian wrt clouds and radiation, and I am interested in his take on it all, given the new material and changes. I am not a fan of Norris' stuff. We have updated Fig 3.4.1 on water vapor thru 2004: the ocean trend drops to 1.2%/decade. So you can help a lot by putting your take on the 3.4 stuff: it may also require some careful wording to accommodate different views if we can't see eye to eye. For instance, on the dimming, the recent Pinker paper uses ISCCP and I simply don't believe the trends from ISCCP at all. Saying Wielicki and ISCCP agree actually damns them both. Or similarly saying Norris and ISCCP agree causes problems (this relates to upper cloud, which Norris gets from total minus lower, but those two sets of data are

not homogeneous: there is not a lower cloud ob for every total; using means, esp zonal means without differencing each ob potentially causes major problems).

Dennis is starting on the 3.6 figs today plus the Sahel one.

Cheers

Kevin

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## References

1. <mailto:p.jones@uea.ac.uk>
2. <mailto:trenbert@ucar.edu>
3. <http://www.cgd.ucar.edu/cas/>

From: Phil Jones <p.jones@uea.ac.uk>  
To: "David Easterling" <David.Easterling@noaa.gov>  
Subject: Re: Fig. 3.7.1  
Date: Thu May 26 15:12:40 2005  
Cc: david.parker@metoffice.gov.uk, pmzhai@cma.gov.cn, Kevin Trenberth <trenbert@cgd.ucar.edu>

Dave,

Thanks for the update on the maps. Can you calculate a CRU time series from what you have? Exactly which dataset do you have? Is it CRU TS 2.0? If this is it then OK. This is the infilled one, so variance may be a little low in early years. Hopefully your calculations will agree with Aiguo. I don't have anyone here to do this at the moment. There seem a lot of deadlines at the moment here, which is making it hard for me to find quality time for Ch3. Luckily there is a holiday weekend coming up and I hope to use that to get 3.5-3.7 looked over. 3.2 is now done and agreed with David. I'll tweak anything when I get your spatial maps. I came in with good intentions today, but have been answering emails and seeing students.

As for smoothing, we didn't agree. For temperature we are going with the HC 'approximate' 20-year binomial. I'll attach a figure David's produced to let you see that. I reckon if you did a 13-year binomial you'll get something like it. Remember to send David all the series for trend estimation when you have them.

I am assuming Bin Wang did 3.7.1. Can you clarify with Dave exactly what 3.7.1 is? Give him the method to calculate it. Also clarify the two Chen's.

I see that David has emailed his reading of the English. I was about to wright something like this. It is definitely the difference between two period averages and not extremes years in the periods. The caption obviously needs a lot of work - I'll have a go at that when I get to it.

If the 3 of us are having difficulties, what hope have we for the readers. If you can't get anything remotely like it I would suggest we drop it - but try David's English translation first !

Cheers  
Phil

At 14:11 26/05/2005, David Easterling wrote:

Phil,  
We will have the maps redone next week and I have started reworking the text for 3.3 Do you have a CRU global pcp time series for 1901-2003 you can send or should we calculate? I have the numbers for the figure Aiguo Dai sent. Also, we never decided on a standard smoothing routine. My preference is for a 13 or 9 point binomial with reflected ends, but we need to decide.

Last, it is still not clear who did figure 3.7.1, was it Bin Wang? The two Chen papers are by different authors, the 2004 EA monsoon paper is by T-C Chen of Iowa State U., and the 2002 paper and data set creator is Ming Chen at NOAA/CPC. I have requested the PREC/L data set from CPC. But I am not even sure exactly what 3.7.1 is, the title says change in mean annual range between the two periods, which I interpret to mean the difference between the highest and lowest years for the post 1976 period minus the difference between the highest and lowest from the pre-1976 period giving a measure of change in year to year consistency of monsoons. Also, there is a reference in the text that Chen et al. (2004) compiled PREC/L, but that is not the case, it should be Chen et al. (2002) as creator, but with an update to 2003.

Dave

Phil Jones wrote:

Dave,

I still don't understand why Bin Wang is involved in this ! Have you contacted Chen? Maybe it was Bin Wang. Have you looked into trying to reproduce it?

Panmao has sent me a revised 3.7.3 using HadSLP2. I'm going to contact Rob Allan about this one as he's been involved in developing HadSLP2.

Will you be in a position to send revised Figures soon? Any date also when you'll be working on the text of 3.3?

Cheers

Phil

At 19:44 25/05/2005, David Easterling wrote:

Phil,

I am trying to track down the source of Fig. 3.7.1 the epoch difference in monsoon rainfall map. It has a reference of Chen et al. 2004, which is the J. Climate paper on the east Asian monsoon, but this figure is not in the paper.

Someone must

of plotted it using their data, but not sure who. Do you know?

Dave

--

David R. Easterling, Ph.D.

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From: Georg Kaser <Georg.Kaser@uibk.ac.at>  
To: Olga Solomina <olgasolomina@yandex.ru>  
Subject: Re: glacier bullet, glossary, structure  
Date: Thu, 2 Jun 2005 20:19:37 +0200 (MEST)  
Cc: Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, Keith Briffa  
<k.briffa@uea.ac.uk>, Valérie Masson-Delmotte <Valerie.Masson@cea.fr>, Oyvind.Paasche@bjerknes.uib.no, Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>  
Dear Olga,

I deeply apologize for having not read your e-mail earlier. I was so overburdened with other obligations when coming back from Beijing that I gave myself the deadline of June 1 to start with IPCC work. As usual, circumstances have forced me to postpone this "deadline" to next Monday. For this, I had not realized that Chapter 6 has its first deadline tomorrow. I have now gone through the "Glaciers during the LIA" and "Glaciers during the MWP" paragraphs as well as through the "glacier bullet" you send today.

I think the LIA paragraph fits well into the Chapter 4 as a supplement to the "Observations" we concentrate on. The MWP is a bit out of focus (Observations!). As I mentioned earlier, I would be glad if chapter 6 could give glaciers appropriate space as being the only climate proxies which are exclusively governed by physical processes and are, thus, much safer to interpret than any other proxies. The fact that they give filtered information as a mean over longer time periods enables them to represent climate. Over the last years, glaciologists have started to investigate the impact of climate seasonality on glaciers and have also started to separate thermal and hygric variables driving glaciers. All this deserves much attention also beyond the "Observations" to be covered in Chapter 4.

A comment on the bullet: this is fine. The only point I would change is the one mentioning Africa. For Lewis Glacier, Mount Kenya, advances have been reconstructed from moraines around 1900 and (measured) thickening took place in the 1970s. Rwenzori glaciers have advanced in the late 1960s and early 1970s. A compilation of this is attached as well as a figure and a table from an ongoing compilation of the post-LIA retreat of tropical glaciers I am working on. Please keep them confidential. Note from this figure also the exception Kilimanjaro glaciers play. They have to be seen separately from anything else we observe in the tropics mainly because of the absolute lack of movement on the Plateau (there are also other reasons which would go beyond a readable e-mail). So, to make the long story short: (i) African glaciers are no exception to the global picture and (ii) Kilimanjaro glaciers are an exception in Africa, in the Tropics, and on the global picture. Thus, Kili glaciers should not be used as an example neither for Africa nor for the tropics. Although I am highly interested in Kilimanjaro myself running a research project there, I

strongly suggest to not overestimate its glaciers. According to a request from Susan Solomon I will address that briefly in Chapter 4.5. By the way, Kili glaciers only cover 2.6 km<sup>2</sup> out of 2,500 km<sup>2</sup> in the tropics (see table in attachment).

Hope this is of help and if you have any further question feel free to contact me. Best wishes, Georg

Georg Kaser ----- Institut fuer Geographie Innrain 52 A-6020 INNSBRUCK Tel: ++43 512 507 5407 Fax: ++43 512 507 2895 [http://meteo9.uibk.ac.at/IceClim/CRYO/cryo\\_a.html](http://meteo9.uibk.ac.at/IceClim/CRYO/cryo_a.html)

On Thu, 2 Jun 2005, Olga Solomina wrote:

> Dear colleagues,  
>  
> Please find attached my suggestions for the "Glacier bullet" (chapter 6). It  
> accumulates (and replaces) all "glacier cases" mentioned in different places  
> in our preliminary draft.  
>  
> I find that our first subdivision of the chapter to 2ka, 10ka etc. was more  
> natural rather than 6ka etc. - now we have a mixture of two systems.  
>  
> My suggestions for the glossary are:  
>  
> The Holocene (including Early, Mid, Late with approximate dates)  
> Little Ice Age  
> Neoglacial  
>  
> I also attach two paragraphs that I wrote for the Ch4 for the recent glacier  
> variations, though it is still unclear where it should be. I think both the  
> glacier recession from the LIA maximum positions and glacier advances  
> occurred during the MWP should be mentioned somewhere.  
>  
> Cheers,  
> olga  
>  
</x-flowed>

Attachment Converted: "c:\eudora\attach\KASER-1999GPCh.PDF"

Attachment Converted: "c:\eudora\attach\TropGlac.doc"

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
Subject: Re: Fwd: updated MWP figure  
Date: Wed Jun 15 16:13:36 2005

Eystein

tried phoning on your mobile - no luck - Don't like this Figure , but still having trouble working on ours. Have cut large bits out of my text and suggestions for cutting other bits , but will be a little late sending these bits. Can you ring to discuss (and IMPRINT) tomorrow ?

Keith

At 06:28 15/06/2005, you wrote:

Hi Keith,  
enclosed for your consideration.  
Eystein

Envelope-to: eystein.jansen@geo.uib.no  
Date: Tue, 14 Jun 2005 15:13:28 -0400  
From: Tom Crowley <tcrowley@duke.edu>  
X-Accept-Language: en-us, en  
To: J Overpeck <jto@u.arizona.edu>,  
"Jansen, Eystein " <eystein.jansen@geo.uib.no>,  
Tim Osborn <t.osborn@uea.ac.uk>

Subject: updated MWP figure  
X-checked-clean: by exiscan on alf  
X-UiB-SpamFlag: NO UIB: 0 hits, 8.0 required  
X-UiB-SpamReport: spamassassin found;

Hello,

I have been fiddling with the best way to illustrate the stable nature of the medieval warm period - the attached plot has eight sites that go from 946-1960 in decadal std. dev. units - although small in number there is a good geographic spread -- four are from the w. hemisphere, four from the east. I also plot the raw composite of the eight sites and scale it to the 30-90N decadal temp. record.

this record illustrates how the individual sites are related to the composite and also why the composite has no dramatically warm MWP -- there is no dramatically warm clustering of the individual sites.

use or lose as you wish, tom

--

---

Eystein Jansen

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--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

## References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Phil Jones <p.jones@uea.ac.uk>  
To: "Michael E. Mann" <mann@virginia.edu>  
Subject: An idea  
Date: Thu Jun 16 15:11:01 2005

Mike,

I will reply to Yasmine and say no tomorrow. Don't want to do it too soon. Keith and I and Tim have been having loads of discussions about Ch 6 for IPCC. Keith has to submit his latest draft tomorrow for better for worse. What I'm thinking is that sometime when the three of us here have some spare time - which may be some ways off, we'd like to do some experiments with different proxy combinations. Would you be happy sending us all the proxies you have (or Scott - the rookie) is putting together? If so can you arrange it. There is no rush. We won't pass any on or put on web sites etc. If we ever did get some time then we could do something - it will be slowly, not for this IPCC and unlikely to get written up or started until well into 2006.

Cheers  
Phil

Prof. Phil Jones  
Climatic Research Unit Telephone +44 (0) 1603 592090  
School of Environmental Sciences Fax +44 (0) 1603 507784  
University of East Anglia  
Norwich Email p.jones@uea.ac.uk  
NR4 7TJ  
UK

---

From: Phil Jones <p.jones@uea.ac.uk>  
To: Anders Moberg <anders@misu.su.se>  
Subject: Re: Reminder  
Date: Thu Jun 23 09:52:58 2005  
Cc: Isabelle Gouirand <isabelle.gouirand@natgeo.su.se>

Anders,

Sending again. Your server rejected this because of the extensions so changed them. Hoep you get them.

Phil

Anders,

Thanks for the files. I was aware that the EGU was starting a new paleo journal. I don't think there have been any issues yet.

I thought Keith had put those two series on our web site, but I can't find them either. However, I found them ages and put them with some of the other long tree-ring series. So here they are with others.

The ones you want should be in columns 1 and 2. The file starts in 1628BC, so it takes a while to get to them. They start in AD 500. I vaguely recall chopping off the 402-499 and 441-499 years because of sample size. Keith has more trw series now, so they could be improved. Keith should have a reconstruction from the Grudd et al. (2002) paper in The Holocene, but they must be on his machine.

I hope the papers for the two Fennoscandian series tell you what the base period is. Given the publication dates I would suspect it is 1951-80.

There are newer series for Jasper and Tasmania and I wouldn't bother doing anything with the two South American series.

Have a good summer break. Ruth and I have sat out every night this week so far. We watched birds the last two days denuding the cherry tree of cherries.

Cheers

Phil

At 07:52 23/06/2005, you wrote:

Phil,

Here are the data we used in our Nature paper, minus Indigirka and Lauritzen. All series are interpolated to annual resolution. Brief info in file headers. The details are found in the online supplementary info on nature.com

Lauritzen's email:

"S. E. Lauritzen" <stein.lauritzen@geo.uib.no>

The Finnish diatom series and all eastern tree ring series have been sent through personal contacts. The rest comes from the web, apart from GRIP which comes from you. Could you, in return, send me the data file for the Fennoscandian summer temperature reconstruction from either Briffa et al (Nature 1990) or Briffa et al (Clim Dyn 1992) - or both? I could not find any of these series on the CRU website.

I realize that Isabelle Gouirand will have to discuss these two papers. Starting from there and try to point out something new as regards the work done by Isabelle. By the way, do you know anything about this journal:

[1] <http://www.copernicus.org/EGU/cp/cp.html> ? I did not know it existed, before I was told about it yesterday.

Tomorrow starts my summer holidays, which last over the coming four weeks

Cheers,

Anders

At 10:07 2005-06-17 +0100, you wrote:

Anders,

When I got back the bus was still here and the driver had disappeared.

Hope the train came and you got to Stansted OK.

No rush for the paleo data - just when you have a few minutes.

Hopefully these colour plots are OK. I think I was going to pay something so forward any bills or tell Michelle to send to me.

Cheers

Phil

At 14:29 16/06/2005, you wrote:

Dear Michelle,

Thanks for your message. I expect your letter to arrive early next week, and I should be able to answer quickly.

Best regards,

Anders

MTheakst@wiley.co.uk wrote:

Dear Anders

We have just posted you colour proofs of your paper - when you receive these, please contact me to confirm whether we can proceed to publication.

We will be publishing your paper as part of Volume 25, Issue 9.

Best Wishes

Michelle

#####

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out of any bug or virus infection.

#####

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-----

## References

1. <http://www.copernicus.org/EGU/cp/cp.html>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: First draft of FOD

Date: Fri, 24 Jun 2005 11:52:25 -0600

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, t.osborn@uea.ac.uk, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>

Hi gang - I still have to weigh in on the great figs/text that Keith and Tim have created, but here's some feedback in the meantime.

I agree that a mean recon isn't the thing to do. Let me think more before I weigh in more on the fig. Working to get other LAs to get their stuff in.

As for the Southern Hem temperature change fig (and caption and a little text), I agree that you (Ricardo in the lead) should do it as you've proposed. We need a clear S. Hem statement, and although it should stress that the data are too few to create a reliable S Hem recon, we should show the data that are available. Thus, PLEASE proceed Ricardo on this tack. Also, can we include the borehole recon series from S. Africa and Australia (e.g., Pollack and Huang, 98)? I'm sure Henry Pollack would provide fast - cc Huang too, since he might be even faster. Keith and Tim, does that make sense?

Please note that I think we can find room for the above, regardless, if it is compelling enough.

As for ENSO, we will need to address for sure - based mainly on the more direct coral data rather than teleconnected (e.g., tree-ring) relationships. The latter don't seem to be definitive enough at this time - as I think we discussed in China. The same holds true for NAO/AO/PDO etc., and I think that we (Keith and Tim) will need to have this in their section - in an appropriately short manner. I'll provide more feedback on this soon, so don't sweat it for now.

Main thing is to go ahead on the S Hem temp fig/caption/short text., independent of ENSO etc discussions.

Thanks, Peck

>Eystein and Peck

>very quick initial response - as have not seen  
>Tim today. The Figure legends with very detailed  
>explanations is at the end of the text I sent  
>you already. The forcings ARE the ones that went  
>into the models , appropriately colour coded for  
>direct comparison - it was partly the difficulty  
>of getting all of these prescribed or diagnosed  
>forcings sorted out for each model that took Tim  
>so long. The uncertainty levels are a compromise  
>that chose came up with - see description in  
>caption , but we are considering other things .  
>Will get back to re the colours. Producing a  
>mean reconstruction is not in my opinion a  
>sensible thing to do so we will have to talk  
>about this. The question of space is crucial  
>regarding the Figure and reworking needed on  
>Regional stuff Ricardo and I need to know how  
>the space is panning out , and you opinions on  
>the reative importance of a SH regional Figure  
>versus an ENSO Figure.- and what about Monsoon  
>Peck? By the way, please clarify the space re  
>the Medieval Warm Period Box. Does this have to  
>come down , thought it was short enough?  
>Keith

>

> At 09:03 24/06/2005, Eystein Jansen wrote:  
>>Hi Keith and Tim,  
>>Lots of thanks for your hard work.  
>>I have gone through the FOD draft and the  
>>figures. Will send comments on text later today.  
>>Here some comments on the figures.  
>>I did not see the figure captions so it is not  
>>entirely transparent to me what went into the  
>>figures, hopefully all is material that is or  
>>will be published before the end of 2005. But  
>>anyhow, I think these figures are very good and  
>>in my view give the different reconstructions,  
>>the combined uncertainty as well as  
>>reconstructions and simulations brought  
>>together. I assume you have the Moberg et al  
>>reconstruction included, but not the Oerlemans,  
>>which will be treated in Ch. 4 (needs a x-ref).  
>>Concerning the way of displaying the  
>>uncertainties, it is not transparent to me how

>>the white and grey areas are produced. Would it  
>>be viable to make a single curve of the mean of  
>>the reconstructions to accompany the  
>>simulations? The white area underlying the  
>>simulations seem a bit weak, in the sence that  
>>a superficial reader might wonder if it  
>>displays something without content, perhaps a  
>>different shade or colour would be better.  
>>Conserning the simulations, it needs to be  
>>clarified that the simulations did not  
>>necessarily use the forcings displayed above,  
>>hence it may be misleading to place the  
>>forcings and simulations into the same figure.  
>>Concerning the forcings, I am a bit surprised  
>>that the amplitude of these are so close to  
>>each other. Although I haven't followed the  
>>litterature here in detail, my impression was  
>>that there is quite high discrepancies between  
>>the various solar reconstructions, but I may be  
>>wrong.

>>  
>>Ricardo asks about whether Peck and I have  
>>Ok-ed his suggested figure. To me it seems a  
>>good candidate for an ENSO illustration, with  
>>some polishing to make it less technical, but  
>>since Peck is more up to speed on this and  
>>working on the issue, I would leave it to him  
>>to weigh in on this matter.

>>  
>>Some first impressions for your consideration.

>>  
>>Cheers,  
>>Eystein

>>  
>>  
>>  
>>  
>>  
>>--

>>\_\_\_\_\_

>>Eystein Jansen  
>>Professor/Director  
>>Bjerknes Centre for Climate Research and  
>>Dep. of Earth Science, Univ. of Bergen  
>>Allégaten 55  
>>N-5007 Bergen  
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>>Fax: +47-55-584330

>

>--

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>

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--

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<http://www.ispe.arizona.edu/>

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From: Jonathan Overpeck <jto@u.arizona.edu>  
To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk  
Subject: First draft of FOD - figures  
Date: Mon, 27 Jun 2005 15:42:40 -0600  
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith and Tim - Eystein is going to chat with you tomorrow, and my goal is to get as much as I can to you guys today and tomorrow.

First, off the figures are great (!) - that was tough job, and I'm very impressed. Of course, I can already start to sense what the debates will be, but we can address that in the text. Here are some comments with respect to the figures - some are relevant to the text...

- 1) they really are great
- 2) is the instrumental series on the first fig (top and bottom) the same as featured in chapter 3? Need to say that.
- 3) rather than clogging up the caption with all the notes on each curve, how about a table for each of the two figures. Then you can include some more info on each recon - e.g., number of sites, types of proxies??) I'm thinking mainly that the captions are not pretty, but you may be able to include more summary info on each curve also
- 4) should we make all the series in their original and modified for the figure form available on a www site so that reviewers can play with the data and make sure they get their two cents in before this thing is all said and published? The WDC-A is ready to help w/ posting of data and figs (see below).
- 5) I like the expanding time axis, but I'd be prepared to have a second one with a linear axis. In fact, I'd put it up on the www page at the same time with the data. The more we do to help others understand, the better?
- 6) Also, it would be good to see both the data and the figure w/o the Gaussian-weighted filtering. What do these look like, can we make them available as suggested above. At the least, I'd like to see the fig w/o the filtering, even though I know it will be a mess. How

about a series of time series plots (same x and y axes as the big fig 1) - in each you show both the filtered and unfiltered series. I know this is a pain for Tim, but we really have to make sure we're not missing anything in the data. And also - that we anticipate what others will do, ask us to do, or squawk about.

7) On the forcing fig (fig 2) - why don't we see all the different experiment curves (e.g., dotted red) in the forcing plots a, b and c? Need to say why in the caption - and if they have the same forcing, so you can't see it on the plot, need to say it. This could be much easier in a table that indicates "same as X").

8) On fig 2 - does the recalculated envelop of reconstructed temps also include instrumental temps? Think so, but you should say it in the caption. Why doesn't the envelop go up to present? Can it? Might look better, and be more consistent w/ fig 1. If the envelop can't go to present, then maybe include the instrumental curve as in Fig 1.

9) reminders for the text (I'll think about these as I read a second time for editing) -

9a) need to explain why the recons don't continue going up w/ instrumental data at the end (post 1990?) - might what to mention something in caption, if you can shift all the other stuff to a table.

9b) there will be lots of discussion (during and post AR4 drafting) about what recon series (Fig 1) should or should not be believed. Thus, I think it is critical for us to same more about each recon - that is to INCLUDE what you wrote in blue, and perhaps to enhance. Need to really convince the reader that while not one recon is alone the truth (and hence Fig 1), they all have important strengths and weaknesses. But, the former outweigh the latter, so we've included them.

9c) I'm sure you saw the recent (to be infamous) Wall Stree Journal editorial - they showed what I think was a IPCC FAR curve - with the good old MWP and LIA etc (Lamb view? - I don't have the FAR w/ me). The way to handle the hocky stick might best be to put it in an historical perspective along with the older IPCC views. First, show your great figs, discuss them and what went into them, and then - after showing the state-of-the-art, discuss how much our understanding and view have changed. In this, simply compare each of the historical views (FAR, SAR, TAR) to the current view, and while

doing so, play down the controversy (s) - especially the hockey stick. The smart folks will realize that that the fluff in the news is just that, but those with a real stake in that debate will hopefully get the point that it doesn't matter...

10) lastly (almost), I'm sorry to ask again, but I still want to know what is wrong with Tom Crowley's latest plot with all the recons shown together back through the Med W Period? I need to send you my edits on the MWP box, but it seem to me that Tom's fig could go in that box - to help make the point that - sorry, guys - the MWP wasn't much compared to the recent GLOBAL warming...

11) lastly (promise) - don't foget that Eystein and I think we can get a page or two extra for your section in the end. This means you can do all the above, and I can help (next) with the modes and extremes sections, and we can get it all in.

Great job!

Thanks, Peck

--

Jonathan T. Overpeck  
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From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk, Eystein Jansen <eystein.jansen@geo.uib.no>

Subject: the Med Warm Period Box - Peck comments/edits

Date: Mon, 27 Jun 2005 22:14:09 -0600

<x-flowed>

Gentlemen - attached is the ZOD Med Warm Period Box with my edits/comments. I don't see anything sent since then, so hope I'm not editing the wrong thing. In any case, the Box was pretty nice as is, so I only made a few changes. Obviously, some updating w/ new studies is needed. The big issues are two:

1) the recent Wall Street Journal editorial that is creating all the crap in the US actually showed a time series from the IPCC FAR - if you don't have it, or Eystein can't send, I can scan it in (my Republican Dad sends me these things, although he's an increasingly rare breed of moderate Republican). My thought is that it might be worth adding a couple lines documenting how the view of the MWP changed with each assessment and new knowledge. In doing so, it could be made very clear that there is a reason that scientists don't show those old plots anymore. We need to move the debate beyond the FAR, SAR and TAR on this issue!

2) it would be cool to have another figure that made the point about no single synchronous period warmer than late 20th century. This is where I get soft with respect to Tom's plot. If it is published to the extent we need it, and if the composite or large-area average recon is the same as you are showing in your great new Fig 1, then it seems that it would be reasonable to show Tom's fig as part of the Box - just to show the same thing in a different way, and to hammer in one more nail. That said, I'm not sure if my two conditions above are met (I emailed Tom, no response yet - you might have insight), and I believe you just don't like Tom's fig for some - probably good - reason. But, I wanted us to think extra hard about whether there is SOME fig that might work?

That's it for tonight. Will finish editing your main text next work session tomorrow I hope.

Best, Peck

--

Jonathan T. Overpeck

Director, Institute for the Study of Planet Earth  
Professor, Department of Geosciences  
Professor, Department of Atmospheric Sciences

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Attachment Converted: "c:\eudora\attach\MWP\_box\_textjto.doc"

From: "Michael E. Mann" <mann@virginia.edu>  
To: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Re: Fwd: Re: NEED HELP!  
Date: Tue, 28 Jun 2005 07:21:55 -0400

Hi Keith,

Thanks--yes, we seem to back in the days of McCarthyism in the States. Fortunately, we have some good people who will represent us legally pro bono, and in the best case scenario, this backfires on these thugs...

The response of the wording is likely to change dramatically after consultation w/ lawyers, etc. but any feedback on the substance would nonetheless be very helpful...

thanks for both your help and your support,  
mike

At 05:48 AM 6/28/2005, you wrote:

Mike

just in and seeing this for time - will digest - but do not like look or implications of this at all

Keith

At 17:00 25/06/2005, you wrote:

Tim/Keith/Phil,

Please see attached letter from the U.S. House republicans. As Tom has mentioned below, it would be very helpful if I can get feedback from you all as I proceed w/ drafting a formal response.

Thanks in advance for any help,  
mike

Date: Sat, 25 Jun 2005 09:36:49 -0600

From: Tom Wigley <wigley@cgd.ucar.edu>

Organization: NCAR/CGD

User-Agent: Mozilla/5.0 (Windows; U; Windows NT 5.0; en-US; rv:1.4) Gecko/20030624 Netscape/7.1 (ax)

X-Accept-Language: en-us, en

To: Michael Oppenheimer <omichael@princeton.edu>

Cc: "Michael E. Mann" <mann@virginia.edu>, shs@stanford.edu, dlashof@nrdc.org, jhansen@giss.nasa.gov, mmaccrac@comcast.net, santer1@llnl.gov, wigley@ucar.edu, Caspar Ammann <ammann@ucar.edu>

Subject: Re: NEED HELP!

X-UVA-Virus-Scanned: by amavid-new at fork9.mail.virginia.edu

Mike,

There are broader implications of this, so it is important to respond well. It is

a pity you have to be the guinea pig after what you have gone through already, but you have many supporters.

I would not advise a legal route. I think you need to consider this as just another set of referees' comments and respond simply, clearly and directly. On the science side the key point is that the M&M criticisms are unfounded.

Although this may be difficult, remember that this is not really a criticism of you personally, but one aspect of a criticism of the foundations of global warming science by people both inside and outside of Congress who have ulterior motives.

There may, in fact, be an opportunity here. As you know, we suspect that there has been an abuse of the scientific review process at the journal editor level.

The method is to choose reviewers who are sympathetic to the anti-greenhouse view. Recent papers in GRL (including the M&M paper) have clearly not been reviewed by appropriate people. We have a strong suspicion that this is the case, but, of course, no proof because we do not know \*who\* the reviewers of these papers have been. Perhaps now is the time to make this a direct accusation and request (or demand) that this information be made available. In order to properly defend the good science it is essential that the reasons for bad science appearing in the literature be investigated.

The lever here is that the Subcommittee on Oversight and Investigations of the House Committee on Energy and Commerce is suggesting that your papers are bad science and asking (their point 8e) for the identity of people who reviewed your work. In response, it is completely fair and justifiable to point out that it is the papers that criticize your and related work that are bad science, and that, through the Subcommittee you can request the identities of the reviewers of all of these critical papers -- starting with M&M.

When you respond, there are a number of items that require a direct response from you alone. There are also a number of scientific points where you could give a multi-authored response. There are many people who have expertise in this area and familiarity with the scientific issues who I am sure would be willing to join you (I would be happy to do so).

At this stage, however, I would keep the group small. A few others could be added to the original email list nevertheless. I took the liberty of copying your plea and the Subcommittee's letter to Caspar Ammann, primarily because I think he can help with the scientific aspects better than most people. After all, he has been able to follow your method and reproduce your results, he has shown the flaws in M&M's work, he has investigated the bristlecone pine issue, and he has made all his software available on the web.

The others who could be added at this early stage are Ray Bradley and Malcolm Hughes, your 'co-conspirators' -- and perhaps Phil Jones, Keith Briffa and Tim Osborn. I do not know how 'powerful' these alien opinions may be in the present parochial context, but I note that the instigators of all this are Canadians and that the science has no national boundaries. Phil, Keith and Tim are useful because they have demonstrated the flaws in the von Storch work -- which is, I assume, the

Science paper that the Subcommittee's letter refers to.

A word of warning. I would be careful about using other, independent paleo reconstruction work as supporting the MBH reconstructions. I am attaching my version of a comparison of the bulk of these other reconstructions. Although these all show the hockey stick shape, the differences between them prior to 1850 make me very nervous. If I were on the greenhouse deniers' side, I would be inclined to focus on the wide range of paleo results and the differences between them as an argument for dismissing them all.

I attach also a run with MAGICC using central-estimate climate model parameters (DT2x = 2.6 degC, etc. -- see the TAR), and forcings used by Caspar in the runs with paleo-CSM. I have another Figure somewhere that compares MAGICC with paleo-CSM. The agreement is nearly perfect (given that CSM has internally generated noise while MAGICC is pure signal). The support for the hockey stick is not just the paleo reconstructions, but also the model results. If one takes the best estimates of past forcing off the shelf, then the model results show the hockey stick shape. No tuning or fudging here; this is a totally independent analysis, and critics of the paleo data, if they disbelieve these data, have to explain why models get the same result.

Of course, von Storch's model results do not show such good century timescale agreement, but this is because he uses silly forcing and has failed to account for the fact that his model was not in equilibrium at the start of the run (the subject of Tim Osborn et al.'s submitted paper).

This is a pain in the but, but it will all work out well in the end (unintentional pun

--

sorry). Good science will prevail.

Best wishes,

Tom.

-----  
Michael Oppenheimer wrote:

Michael:

This is outrageous. I'll contact some people who may be able to help right away.

-----

From: Michael E. Mann [[\[1\]mailto:mann@virginia.edu](mailto:mann@virginia.edu)][[2\]mailto:mann@virginia.edu](mailto:mann@virginia.edu)]

Sent: Friday, June 24, 2005 4:27 PM

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Subject: NEED HELP!

Importance: High

dear all,

this was predicted--they're of course trying to make things impossible for me. I need immediate help regarding recourse for free legal advice, etc.

mike

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References

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9. <mailto:wigley@ucar.edu>
10. <mailto:mann@virginia.edu>
11. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
12. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
13. <http://www.cru.uea.ac.uk/cru/people/briffa/>
14. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Jonathan Overpeck <jto@u.arizona.edu>  
To: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Re: Fwd: Re: updated MWP figure  
Date: Tue, 28 Jun 2005 10:11:05 -0600  
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith - might be worth talking on the phone - you, me and Eystein - after you get back. You could be right, but it is a powerful way to look at the issue. The question is whether the normalization could be preventing a warmer than late-20th century signal from appearing?

Should we instead update the Bradley Science graphic? That's not as effective in my opinion.

So, let's talk next week?

Going to a tree day meeting or a three day meeting - it has to be tough looking at tree data all day.

have fun, thx, peck

>Jonathan and Eystein

>I am leaving very early for a tree day meeting in Swansea , and will  
>be away til Monday. Presently buried in EC Reporting and other stuff  
>- but the reason I dislike the MWP Figure is that the simple  
>normalization of series as done , (regardless of regional selection  
>of specific proxies) gives a largely random amplitude to the various  
>records , depending on their spectral character, and of course,  
>equal weight to all regardless of the strength of their link with  
>local or NH temperatures). I will think about this - you are the  
>ultimate arbiter anyway .

>sorry to be so abruptly communicative

>Keith

>  
>At 16:10 28/06/2005, you wrote:

>>Hi Tom -- thanks for the extra effort. I'm pushing others on the  
>>author team to think hard about such a figure (space may end up  
>>being the hardest part), and I should have something to discuss w/  
>>you soon. Thanks for being willing to shift priorities if needed.

>>

>>FYI - I just got reviews back from an EOS piece that took over a

>>1.5 months to get. And of course, they want some edits. Not the  
>>speedy venue we once knew a loved, although I bet if you really  
>>keep it short and sweet it might go faster.

>>

>>Best, more soon, peck

>>

>>>X-Sieve: CMU Sieve 2.2

>>>Date: Tue, 28 Jun 2005 10:13:49 -0400

>>>From: Tom Crowley <tcrowley@duke.edu>

>>>X-Accept-Language: en-us, en

>>>To: Jonathan Overpeck <jto@u.arizona.edu>

>>>Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

>>>Subject: Re: updated MWP figure

>>>

>>>Hi Jonathan,

>>>

>>>let me answer the last question first - there are actually not  
>>>many records that go back that far and I have used, I think, every  
>>>one except Quelcaya, which being from the southern tropics makes  
>>>for a lonely but potential future inclusion (which makes no  
>>>difference on the conclusion).

>>>

>>>several of the sites include multiple time series - e.g., western  
>>>U.S. time series, w. Siberia time series, e. Asia, and w.  
>>>Greenland. I did not want to overweight any site though because  
>>>of the need for a geographic balance -- note that there are four  
>>>sites each in the w. hemisphere and e. hemisphere, and that the  
>>>distribution of sites in each hemisphere represents a good scatter.

>>>

>>>for almost all of these sites the references are easily imaginable  
>>>based on the location of the site, but they can be provided if you  
>>>are interested in including the figure.

>>>

>>>can you think of any long sites I have not included? right now I  
>>>cannot.....

>>>

>>>in the overlap interval of 1500-1850 our composite has highly  
>>>significant correlations with the Mann, Jones, and Briffa  
>>>reconstructions that contain much more data -- thereby suggesting  
>>>that use of only long time series provides a "reasonable" estimate  
>>>of the last 1100 years.

>>>

>>>I have not submitted this for publication but if you are

>>>interested in including this in ipcc I can knock off a tutorial

>>>note to eos on short notice.....

>>>

>>>I am attaching the figure in several different alternate formats -

>>>cannot easily do the two you suggest from my mac, but again I can

>>>get that done with more work if you are interested - let me know

>>>where to go next - note that I originally sent this along fyi,

>>>only to be used if you thought the figure was worthwhile -- if not

>>>I will just reorder the priority of writing it up as a note,

>>>tom

>>>

>>>Jonathan Overpeck wrote:

>>>

>>>>Hi Tom - thanks for sending this plot. I'm a bit late in

>>>>responding since we were moving to (and still into) our

>>>>sabbatical digs in SW CO.

>>>>

>>>>Would you be willing to provide more on this plot in order for me

>>>>to understand it better? I personally like the plot quite a bit,

>>>>but between the space restrictions and other's assessment,

>>>>whether we use it or not will take some real thinking.

>>>>

>>>>For example, it would help to have

>>>>

>>>>1) a higher resolution version - eps or ai?

>>>>2) a caption or text that would spell out which records are

>>>>included, and their origins (references)

>>>>3) a bibliography for those refs.

>>>>4) perhaps, you have a paper with this included? If so, can you

>>>>send a preprint?

>>>>5) some discussion of why you used the series (sites) you did,

>>>>and not others - more specifically, what's wrong with others?

>>>>

>>>>If you don't mind helping here, I'll promise to get it in the mix

>>>>for serious discussion. Of course, it's already in the mix since

>>>>Eystein forwarded to Keith, and you Tim, but I want to weigh in

>>>>as informed as possible. Trying to keep track of a lot, so your

>>>>help is much appreciated.

>>>>

>>>>Thanks! Peck

>>>>

>>>>>Hello,

>>>>>

>>>>>I have been fiddling with the best way to illustrate the stable  
>>>>>nature of the medieval warm period - the attached plot has eight  
>>>>>sites that go from 946-1960 in decadal std. dev. units -  
>>>>>although small in number there is a good geographic spread --  
>>>>>four are from the w. hemisphere, four from the east. I also  
>>>>>plot the raw composite of the eight sites and scale it to the  
>>>>>30-90N decadal temp. record.

>>>>>  
>>>>>this record illustrates how the individual sites are related to  
>>>>>the composite and also why the composite has no dramatically  
>>>>>warm MWP -- there is no dramatically warm clustering of the  
>>>>>individual sites.

>>>>>  
>>>>>use or lose as you wish, tom

>>>>>

>>>>

>>>>

>>>>

>>>>

>>>>

>>>>

>>

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>>--

>>Jonathan T. Overpeck  
>>Director, Institute for the Study of Planet Earth  
>>Professor, Department of Geosciences  
>>Professor, Department of Atmospheric Sciences

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From: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
To: wg1-ar4-ch06@joss.ucar.edu  
Subject: Re: [Wg1-ar4-ch06] abrupt and Important thoughts on References  
Date: Tue, 28 Jun 2005 23:13:56 +0200

<x-flowed>

Hi all,

Two things:

1. Concerning the 1470k pacing of DO-events. There are revisions underway in the layer-counting of the Greenland Ice Cores. A meeting in Copenhagen in August co-ordinated by Sigfus Johnsen will discuss the issue at length, but there may not be many papers out from the meeting that are citeable for IPCC. There is already the Shackleton paper which indicate that Greenland Ice Cores in MIS3 have an age model that are off by some millennia, and the preliminary data on the new age models indicate substantial revisions as far as I hear from talks given at various meetings. My thinking is that we neither can ignore the fact that current data indicate a 1470 pacing for some time interval of the ice cores if one apply the existing age scales. I think it would be foolish not to refer to it, I think the possibility that the system has the ability to enter into specific cycles is intriguing, and is a result that is well known and IPCC should not pretend we haven't heard about it. But we should make it less blunt than in the current version of the Abrupt Change subchapter, perhaps stating that the result is highly dependant on age models and we need time to absorb new research in order to verify the result.

2. Having the fortune of not being that close to the darker sides of US politics, I have the feeling that Peck's comment concerning referencing perhaps is a bit too "paranoic". I

think the advice is well taken not to overcite our own research, and make sure not to overlook other important contributions, but we should do our best to cite what we think are key results. In any case we will have the FOD review and have the opportunity to have all our good colleagues keeping us honest on this issue.

Cheers,  
Eystein

>Hi all - thanks Fortunat and Stefan for more  
>debate on the 1470. Sounds like the final  
>decision is up to Eystein, but I can guess the  
>way he's thinking.  
>  
>With regard to refs - remember that our goal is  
>to cut the number of references significantly.  
>Since this is an assessment and not a review, we  
>can delete all but the most recent and  
>comprehensive references. I don't like cutting  
>out the original refs any more than you, but we  
>just don't have room, and its more important to  
>have text than exhaustive references. Our  
>colleagues will hopefully understand, and if  
>they don't then they need to do an ego check.  
>It's more important that we make an impact with  
>policy makers rather than with citation indices.  
>  
>Does this make sense?  
>  
>In any case, please help make sure we trim the  
>total references DOWN in number by a significant  
>number. This is not happening the to degree it  
>should.  
>  
>Also, please not that in the US, the US Congress  
>is questioning whether it is ethical for IPCC  
>authors to be using the IPCC to champion their  
>own work/opinions. Obviously, this is wrong and

>scary, but if our goal is to get policy makers  
>(liberal and conservative alike) to take our  
>chapter seriously, it will only hurt our effort  
>if we cite too many of our own papers  
>(perception is often reality). PLEASE do not  
>cite anything that is not absolutely needed, and  
>please do not cite your papers unless they are  
>absolutely needed. Common sense, but it isn't  
>happening. Please be more critical with your  
>citations so we save needed space, and also so  
>we don't get perceived as self serving or worse.

>  
>Again, we can debate this if anyone thinks I've gone off the deep end.

>  
>Thanks, peck  
>PS - this is not to say anything critical of the  
>refs Fortunat is suggesting - we must cite the  
>most relevant papers, and we must be as up to  
>date as possible.

>  
>>Peck and all,

>>  
>>Fully agree. This '1470' yr periodicity is highly controversial and I  
>>was never convinced.  
>>We can use the space for better things that are relevant in the context  
>>of the anthropogenic GHG perturbation.

>>  
>>I miss the recent and relevant literature. Examples are Pahnke and Zahn,  
>>Science, 2005 and Stocker and Johnsen, Paleoclimatology 18, 2003, and  
>>Knutti et al., Nature, 2004  
>>Hemitt et al., Rev Geophysics, 2004 might be a good reference for  
>>Heinrich events.

>>  
>>  
>>  
>>Regards,

>>  
>>Fortunat

>>  
>>  
>>Jonathan Overpeck wrote:

>>>  
>>> Hi guys - I'm not aware of the age model changes that Eystein is

>>> talking about (however, I'm not in the Euro meeting circles, and  
>>> trust he's right), but I know of several studies (e.g., U/Th dated  
>>> (well dated) spelothem studies (plus C14 Cariaco) that indicate that  
>>> the GISP/GRIP age models are off by quite a bit pre 40kish. The other  
>>> studies agree, so it makes sense to me that the ice core gangs are  
>>> revising their age models. Regardless of the probabilities (note that  
>>> one finds evidence in quasi-periodic variance most all paleo  
>>> records), this significant age model change means that the "1470  
>>> beat" has to be off/wrong or something else other than we've been led  
>>> to believe. For the sake of playing it safe, we should play this beat  
>>> way down until there is new evidence that is more convincing that it  
>>> is for real. We can mention it, but we make it clear that the  
>>> evidence for it is not all that strong - at best.  
>>>  
>>> I'll cc this to Fortunat and Valerie too - we don't want to rush to  
>>> conclusions w/o good discussion.  
>>>  
>>> Thanks, Peck  
>>>  
>>> >Hi Eystein,  
>>> >  
>>> >concerning your comment on the 1470-year beat: I'm aware that in the  
>>> >new time scale, it is less regular (at least I heard this, have not  
>>> >tested myself yet).  
>>> >  
>>> >If you have two time scales, one showing a regularity and one not,  
>>> >then there are two possibilities.  
>>> >(1) The regular one is correct, in the other one the regularity got  
>>> >wiped out by random dating errors.  
>>> >(2) The one without regularity is correct, in the other one a  
>>> >regularity arose by chance due to random dating errors.  
>> >>  
>>> >The likelihood of the regularity found with the original GISP2 time  
>>> >scale occuring by chance is minute - I've done some more  
>>> >calculations, they are not complete yet but the likelihood is in the  
>> >>permil range. I think hypothesis (2) can be exluded at least at 99%  
>>> >confidence level.  
>>> >  
>>> >Stefan  
>>> >  
>>> >--  
>>> >To reach me directly please use: rahmstorf@ozean-klima.de  
>>> >(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)

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From: Valérie Masson-Delmotte <Valerie.Masson@cea.fr>  
To: Keith Briffa <k.briffa@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>  
Subject: Re: IPCC ch9 for information and check.  
Date: Tue, 28 Jun 2005 23:57:15 +0200  
Reply-to: Valerie.Masson@cea.fr

Content-Type: text/plain; charset=ISO-8859-1; format=flowed  
X-MIME-Autoconverted: from 8bit to quoted-printable by cirse.extra.cea.fr id j5SLvFxfj010843

<x-flowed>

Dear Keith,

I have read your text - despite of the heat wave here (40°C in my office in the afternoon...). I am a bit puzzled by the regional aspects. I think that you should make more clear in the beginning that there is very little new information / work conducted on the S Hemisphere / tropics and that most efforts have been focussed on the N Hemisphere, because you mention almost nothing for the S Hemisphere. Is ENSO considered as a regional mode of variability? I thought that it had almost global relevance at least in terms of impacts.

Valérie.

Keith Briffa a écrit :

> Pascale  
> I am sending what I sent Peck and Eystein  
> The regional stuff at the end is from Ricardo Villalba and will need  
> to be shortened /rewritten after advice from CLAs. Please note this is  
> only provisional and I have had no feedback from other LA and CLAs and  
> the text needs to be vetted/chopped or whatever. Please note also that  
> the blue text will likely disappear - no space. The Figure legends are  
> at the back of the text file. I will send Figures as a separate message  
> cheers  
> Keith  
> At 15:52 23/06/2005, Pascale Braconnot wrote:  
>  
>> Dear all,  
>>  
>> Here is what I send today to gaby and francis as a contribution for  
>> the first draft for chapt 9.3  
>>  
>> We know we have overlap between the two chapters (9 and 6). We need  
>> to make sure that the point of view is different (or slightly).  
>> in particular, chapter 6 days much more about the data (I nearly  
>> supress all ref to data in 9), and may be also on model evaluation  
>> (which i do not mention as such).  
>>  
>> It could be nice you send me your parts in chapter 6 when ready. I  
>> will have only a small time to adapt the chapt 9 contribution and

>> make changes in July.  
>>  
>> How things will work in chapter9 in the coming month.  
>>  
>> CLA recieved all the contributions, they work together next week  
>> (i still need to interact with gbi for the last millenium part and  
>> the update of the figure on detection: attribution, but gabi didn't  
>> had time to do it at the moment).  
>>  
>> Then Gabi and Francis will return comments to us (as well as internal  
>> comments withing LA of the chapter) and last changes will be provided  
>> for the end of July.  
>>  
>> On my side I am out of contact (mail etc) starting 22 July.  
>> I need thus to finish every thing for July 20.  
>>  
>> I hope the draft 1 writing is going on well on your side  
>>  
>> Cheers  
>>  
>> Pascale  
>>  
>>  
>>  
>  
> --  
> Professor Keith Briffa,  
> Climatic Research Unit  
> University of East Anglia  
> Norwich, NR4 7TJ, U.K.  
>  
> Phone: +44-1603-593909  
> Fax: +44-1603-507784  
>  
> <http://www.cru.uea.ac.uk/cru/people/briffa/>

</x-flowed>

Attachment Converted: "c:\eudora\attach\masson119.vcf"

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Caspar Ammann <ammann@ucar.edu>

Subject: Re: What's up with your paper with Eugene?

Date: Fri, 1 Jul 2005 12:46:59 -0600

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Stephen Schneider <shs@stanford.edu>, "Wahl, Eugene R" <wahl@alfred.edu>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi Caspar and Gene - Thanks. I look forward to hearing how things go  
- if the paper is in press by the first week of August, we'll cite it  
in the Chapter 6 of the FOD, but otherwise I guess it'll have to wait  
- that's ok too.

But... keep us posted (and send revised preprint when possible). Thanks! Peck

>Hi Peck,

>  
>you might have heard.. the thing is flying in everybody's face right  
>now... Mike-Ray-Malcolm, IPCC and NSF got these lovely letters from  
>the House of Representatives...

>  
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>of the WA paper, both strongly positive. Steve is probably waiting  
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>no other one is required that delays the process. I cc Steve, he  
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>to be at NCAR in early July and we will finish with revisions ASAP.

>  
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>Sun-Climate links,  
>Caspar

>  
>  
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>>could pls update us w/ the status of Wahl and Ammann? Most  
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>>  
>>Thanks! Peck

>  
>

>--

>Caspar M. Ammann  
>National Center for Atmospheric Research  
>Climate and Global Dynamics Division - Paleoclimatology  
>1850 Table Mesa Drive  
>Boulder, CO 80307-3000  
>email: ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

--

Jonathan T. Overpeck  
Director, Institute for the Study of Planet Earth  
Professor, Department of Geosciences  
Professor, Department of Atmospheric Sciences

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fax: +1 520 792-8795  
<http://www.geo.arizona.edu/>  
<http://www.ispe.arizona.edu/>  
</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: "Wahl, Eugene R" <wahle@alfred.edu>

Subject: RE: Wahl-Ammann paper

Date: Mon, 4 Jul 2005 21:53:23 -0600

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi Gene - good to hear from you. What you list below seems like it must be pretty good to me. Of course, we'd like to include all we can in the FOD, hence the interest in knowing if it's in press or not before the end of the month.

Just keep us updated, and if you feel comfortable sharing the ms. that'd be great, but only if you feel ok about sharing it. The key people are me, Eystein Jansen and Keith Briffa - we won't share it with others.

Thanks for keeping us up to date. Best, peck

>Hello Jonathan:

>

>Thanks for this info. Could you clue me in--I had heard through the >grapevine (ultimate source, Jerry Meehl) that the actual in-press >deadline for IPCC citations in the AR would be Jan 1 of 2006. On >the IPCC website I see mid-December for the Christchurch meeting.

>

>I assume this the same situation for Chapter 6, and thus the early >August deadline is for the FOD. Is this getting it correct?

>

>Let me know if viewing the submitted text would be of use to you, >and I'll ship at once.

>

>

>Hope you are well.

>

>Peace, Gene

>Dr. Eugene R. Wahl

>Asst. Professor of Environmental Studies

>Alfred University

>

>607-871-2604

>1 Saxon Drive

>Alfred, NY 14802

>

>\_\_\_\_\_

>

>From: Jonathan Overpeck [mailto:jto@u.arizona.edu]

>Sent: Fri 7/1/2005 2:46 PM

>To: Caspar Ammann

>Cc: Eystein Jansen; Stephen Schneider; Wahl, Eugene R; Keith Briffa

>Subject: Re: What's up with your paper with Eugene?

>

>

>

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>>>important - will it be in press by the end of the month?

>>>

>>>Thanks! Peck

>>

>>

>>--

>>Caspar M. Ammann

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>>Climate and Global Dynamics Division - Paleoclimatology

>>1850 Table Mesa Drive

>>Boulder, CO 80307-3000

>>email: ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

>

>

>--

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>Professor, Department of Geosciences

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--

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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>  
To: John Christy <john.christy@nsstc.uah.edu>  
Subject: This and that  
Date: Tue Jul 5 15:51:55 2005

John,

There has been some email traffic in the last few days to a week - quite a bit really, only a small part about MSU. The main part has been one of your House subcommittees wanting Mike Mann and others and IPCC to respond on how they produced their reconstructions and how IPCC produced their report.

In case you want to look at this see later in the email !

Also this load of rubbish !

This is from an Australian at BMRC (not Neville Nicholls). It began from the attached article. What an idiot. The scientific community would come down on me in no uncertain terms if I said the world had cooled from 1998. OK it has but it is only 7 years of data and it isn't statistically significant.

The Australian also alerted me to this blogging ! I think this is the term ! Luckily

I don't live in Australia.

[1]<http://mustelid.blogspot.com/2005/06/first-look-at-scs-msu-vn52.html>

Unlike the UK, the public in Australia is very very naïve about climate change, mostly because of our governments Kyoto stance, and because there is a proliferation of people with no climate knowledge at all that are prepared to do the gov bidding. Hence the general populace is at best confused, and at worst, antagonistic about climate change - for instance, at a recent rural meeting on drought, attended by politicians and around 2000 farmers, a Qld colleague - Dr Roger Stone - spoke about drought from a climatologist point of view, and suggested that climate change may be playing a role in Australias continuing drought+water problem. He was booed and heckled (and unfortunately some politicians applauded when this happened) - that's what we're dealing with due to columists such as the one I sent to you.

Now to your email. I have seen the latest Mears and Wentz paper (to Science), but am not reviewing it, thank goodness. I am reviewing a couple of papers on extremes, so that I can refer to them in the chapter for AR4. Somewhat circular, but I kept to my usual standards.

The Hadley Centre are working on the day/night issue with sondes, but there are

a lot of problems as there are very few sites in the tropics with both and where both can be distinguished. My own view is that the sondes are overdoing the cooling wrt MSU4 in the lower stratosphere, and some of this likely (IPCC definition) affects the upper troposphere as well. Sondes are a mess and the fact you get agreement with some of them is miraculous. Have you looked at individual sondes, rather than averages - particularly tropical ones? LKS is good, but the RATPAC update less so.

As for being on the latest VG analysis, Kostya wanted it to use the surface data. I thought the model comparisons were a useful aside, so agreed. Ben sent me a paper he's submitted with lots of model comparisons that I also thought a useful addition to the subject.

As for resolving all this (as opposed to the dogfight) I'm hoping that CCSP will come up with something - a compromise. I might be naive in this respect. I hope you are still emailing and talking to Carl and Frank. How is CCSP going? Are you still on schedule for end of August for your open review?

What will be interesting is to see how IPCC pans out, as we've been told we can't use any article that hasn't been submitted by May 31. This date isn't binding, but Aug 12 is a little more as this is when we must submit our next draft - the one everybody will be able to get access to and comment upon. The science isn't going to stop from now until AR4 comes out in early 2007, so we are going to have to add in relevant new and important papers. I hope it is up to us to decide what is important and new. So, unless you get something to me soon, it won't be in this version. It shouldn't matter though, as it will be ridiculous to keep later drafts without it. We will be open to criticism though with what we do add in subsequent drafts. Someone is going to check the final version and the Aug 12 draft. This is partly why I've sent you the rest of this email. IPCC, me and whoever will get accused of being political, whatever we do. As you know, I'm not political. If anything, I would like to see the climate change happen, so the science could be proved right, regardless of the consequences. This isn't being political, it is being selfish.

Cheers

Phil

IPCC stuff ---- just for interest !!!

**IPCC ASKED TO COME CLEAN OVER CONTROVERSIAL HOCKEY STICK STUDIES**

The Committee on Energy and Commerce, 23 June 2005

[2][http://energycommerce.house.gov/108/Letters/062305\\_Pachauri.pdf](http://energycommerce.house.gov/108/Letters/062305_Pachauri.pdf)

Joe Barton, Chairman

U.S. House of Representatives

June 23, 2005

To: Dr. Rajendra K. Pachauri  
Chairman  
Intergovernmental Panel on Climate Change  
C/O IPCC Secretariat  
World Meteorological Organization  
7 bis Avenue de La Paix  
C.P. 2300  
Ch- 1211 Geneva 2 Switzerland

Dear Chairman Pachauri:

Questions have been raised, according to a February 14, 2005 article in The Wall Street Journal, about the significance of methodological flaws and data errors in studies by Dr. Michael Mann and co-authors of the historical record of temperatures and climate change. We understand that these studies of temperature proxies (tree rings, ice cores, corals, etc.) formed the basis for a new finding in the 2001 United Nation's Intergovernmental Panel on Climate Change (IPCC) Third Assessment Report (TAR). This finding - that the increase in 20th century northern hemisphere temperatures is "likely to have been the largest of any century during the past 1,000 years" and that the "1990s was the warmest decade and 1998 the warmest year" - has since been referenced widely and has become a prominent feature of the public debate surrounding climate change policy.

However, in recent peer-reviewed articles in Science, Geophysical Research Letters, Energy & Environment, among others, researchers question the results of this work. As these researchers find, based on the available information, the conclusions concerning temperature

histories - and hence whether warming in the 20th century is actually unprecedented - cannot be

supported by the Mann et. al. studies. In addition, we understand from the February 14 Journal

and these other reports that researchers have failed to replicate the findings of these studies, in part because of problems with the underlying data and the calculations used to reach the conclusions. Questions have also been raised concerning the sharing and dissemination of the data and methods used to perform the studies. For example, according to the January 2005

Energy & Environment, the information necessary to replicate the analyses in the studies has not been made fully available to researchers upon request.

The concerns surrounding these studies reflect upon the quality and transparency of federally

funded research and of the IPCC review process - two matters of particular interest to the Committee. For example, one concern relates to whether IPCC review has been sufficiently robust

and independent. We understand that Dr. Michael Mann, the lead author of the studies in question, was also a lead author of the IPCC chapter that assessed and reported this very same work, and that two co-authors of the studies were also contributing authors to the same chapter. Given the prominence these studies were accorded in the IPCC TAR, we seek to

learn more about the facts and circumstances that led to acceptance and prominent use of this work in the IPCC TAR and to understand what this controversy indicates about the data quality of key IPCC studies.

In light of the Committee's jurisdiction over energy policy and certain environmental issues

in the U.S. House of Representatives, the Committee must have full and accurate information when considering matters relating to climate change policy. We open this review because the dispute surrounding these studies bears directly on important questions about the federally funded work upon which climate studies rely and the quality and transparency of analyses used

to support the IPCC assessment process. With the IPCC currently working to produce a fourth assessment report, addressing questions of quality and transparency in the underlying analyses

supporting that assessment, both scientific and economic, are of utmost importance if Congress

is eventually going to make policy decisions drawing from this work.

To assist us as we begin this review, and pursuant to Rules X and XI of the U.S. House of Representatives, please provide the following information requested below on or before July 11,

2005:

1. Explain the IPCC process for preparing and writing its assessment reports, including, but

not limited to: (a) how referenced studies are reviewed and assessed by the relevant Working Group; (b) the steps taken by lead authors, reviewers, and others to ensure the data underlying the studies forming the basis for key findings - particularly proxy and temperature data - are accurate and up to date; and (c) the IPCC requirements governing the quality of data used in reports.

2. What specifically did IPCC do to check the quality of the Mann et. al. studies and underlying data, cited in the TAR? Did IPCC seek to ensure the studies could be replicated?

3. What is your position with regard to: (a) the recent challenges to the quality of the Mann et. al. data, (b) related questions surrounding the sharing of methods and research for others to test the validity of these studies, and (c) what this controversy indicates about the data quality of key IPCC studies?

4. What did IPCC do to ensure the quality of data for other prominent historical temperature or proxy studies cited in the IPCC, including the Folland et. al. and Jones et. al. studies that were sources for the graphic accompanying the Mann et. al. graphic in the Summary for Policy Makers? Are the data and methodologies for such works complete and available for other researchers to test and replicate?

5. Explain (a) the facts and circumstances by which Dr. Michael Mann served as a lead author of the very chapter that prominently featured his work and (b) by which his work

became a finding and graphical feature of the TAR Summary for Policymakers.

6. Explain (a) how IPCC ensures objectivity and independence among section contributors and reviewers, (b) how they are chosen, and (c) how the chapters, summaries, and the full report are approved and what any such approval signifies about the quality and acceptance of particular research therein.

7. Identify the people who wrote and reviewed the historical temperature-record portions of the TAR, particularly Section 2.3, "Is the Recent Warming Unusual?" and explain all their roles in the preparation of the TAR, including, but not limited to, the specific roles in the writing and review process.

8. Given the questions about Mann et. al. data, has the Working Group I or the IPCC made any changes to specific procedures or policies, including policies for checking the quality of data, for the forthcoming Fourth Assessment Report? If so, explain in detail any such changes, and why they were made.

9. Does the IPCC or Working Group I have policies or procedures regarding the disclosure and dissemination of scientific data referenced in the reports? If so, explain in detail any such policies and what happens when they are violated.

Thank you for your assistance. If you have any questions, please contact Peter Spencer of the Majority Committee staff at (202) 226-2424.

Sincerely,

Joe Barton Chairman Chairman

Ed Whitfield

Subcommittee on Oversight and Investigations

cc: The Honorable John Dingell, Ranking Member

The Honorable Bart Stupak, Ranking Member,

Subcommittee on Oversight and Investigations

EDITOR'S NOTE: The House of Representatives has also written to National Science Foundation Director Arden Bement, Dr. Michael Mann, Dr. Malcolm K. Hughes, and Dr. Raymond S. Bradley, requesting information regarding their global warming studies; see "Letters Requesting Information Regarding Global Warming Studies" at

[3][http://energycommerce.house.gov/108/Letters/06232005\\_1570.htm](http://energycommerce.house.gov/108/Letters/06232005_1570.htm)

Prof. Phil Jones

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## References

1. <http://mustelid.blogspot.com/2005/06/first-look-at-scs-msu-vn52.html>
2. [http://energycommerce.house.gov/108/Letters/062305\\_Pachauri.pdf](http://energycommerce.house.gov/108/Letters/062305_Pachauri.pdf)
3. [http://energycommerce.house.gov/108/Letters/06232005\\_1570.htm](http://energycommerce.house.gov/108/Letters/06232005_1570.htm)

From: Phil Jones <p.jones@uea.ac.uk>  
To: "Neville Nicholls" <N.Nicholls@bom.gov.au>  
Subject: RE: Misc  
Date: Wed Jul 6 15:07:45 2005

Neville,

Mike's response could do with a little work, but as you say he's got the tone almost dead on. I hope I don't get a call from congress ! I'm hoping that no-one there realizes I have a US DoE grant and have had this (with Tom W.) for the last 25 years.

I'll send on one other email received for interest.

Cheers

Phil

At 14:21 06/07/2005, you wrote:

Thanks Phil.

I had seen the estimates of 0.12C for UAH 5.2, but wasn't sure if the version producing these trends had all the months corrected, and that John was happy with the corrections (I had heard that his initial estimate was that the change made a major difference to the trends, but that later calculations didn't support this). I think I have a pretty good idea now of the trends in the various data sets.

I have seen the Mears/Wentz paper, but will watch out for John's paper (I know I could have asked John about all of this, but I suspect he feels a bit over-burdened and harrassed at the moment, and I didn't want to add to the pressure on him, so thanks for passing this stuff on to me).

I thought Mike Mann's draft response was pretty good - I had expected something more vigorous, but I think he has got the "tone" pretty right. Do you expect to get a call from Congress?

Neville Nicholls

Bureau of Meteorology Research Centre

9th Floor, 700 Collins Street

Docklands, Melbourne, AUSTRALIA

PO Box 1289K, Melbourne, AUSTRALIA 3001

Phone: +61 (0)3 9669 4407

Fax: +61 (0)3 9669 4660

-----Original Message-----

From: Phil Jones [[1]mailto:p.jones@uea.ac.uk]

Sent: Wed 7/6/2005 5:57 PM

To: Neville Nicholls

Subject: Fwd: Misc

Neville,

Here's an email from John, with the trend from his latest version

in. Also

has trends for RATPAC and HadAT2. If you can stress in your talks that it is more likely the sondes are wrong - at least as a group. Some may be OK individually. The tropical ones are the key, but it is these that least

is know

about except for a few regions. The sondes clearly show too much cooling in the stratosphere (when compared to MSU4), and I reckon this must also affect their upper troposphere trends as well. So, John may be putting too much faith in them wrt agreement with UAH.

Happy for you to use the figure, if you don't pass on to anyone else.

Watch

out for Science though and the Mears/Wentz paper if it ever comes out.

Also, do point out that looking at surface trends from 1998 isn't very clever.

Cheers

Phil

>Date: Tue, 05 Jul 2005 07:59:51 -0500

>From: John Christy <john.christy@nsstc.uah.edu>

>User-Agent: Mozilla/5.0 (Macintosh; U; PPC Mac OS X Mach-O; en-US; rv:1.4)

>Gecko/20030624 Netscape/7.1

>X-Accept-Language: en-us, en

>To: Phil Jones <p.jones@uea.ac.uk>

>Subject: Misc

>X-NSSTC-MailScanner: Found to be clean

>X-NSSTC-MailScanner-SpamCheck: not spam (whitelisted),

> SpamAssassin (score=-5.8, required 5, BAYES\_01 -5.40,

> RCVD\_IN\_ORBS 0.11, SIGNATURE\_LONG\_SPARSE -0.49,

> USER\_AGENT\_MOZILLA\_UA 0.00)

>X-MailScanner-From: john.christy@nsstc.uah.edu

>X-Spam-Score: 0.0

>X-Spam-Level: /

>X-Spam-Flag: NO

>

>Hi Phil:

>

>I've been getting round-about versions of rumors concerning our newly

>adjusted version 5.2 LT dataset. I believe I had indicated earlier to you

>that the correction was within our published margin of error. In any case

>here are the numbers that describe various aspects of v5.2

>1979-2004

>

>Global Trend +0.115 UAH, +0.125 RATPAC and +0.137 HadAT (note, when

>subsamped for the same latitudes in which sonde observations are

>available, UAH and HadAT are almost exactly the same.)

>

>Update of site by site comparison of UAH LT 5.2 and SH radiosondes from  
>Christy and Norris 2004:

>

>All 87 SH stations, no adjustments Raobs + 0.028 UAH +0.040

>74 best sites with adjustments Raobs +0.030 UAH +0.054

>

>These SH changes from the original publication were very minor because  
>most stations were outside the tropics where the diurnal error had  
>essentially no impact.

>

>A paper by Sherwood claims that Day minus Night is a legitimate way to go  
>about looking at sonde problems. The real problem though is that Day  
>minus Night is only an indicator of a sonde change, it does not determine  
>the change itself. Most notorious is the Philipps Mark III to Vaisala  
>RS-80 where the night warmed by about 0.3 C and the day by a little bit  
>less, which means the Day minus Night reveals a negative shift when in  
>fact both ob times have a significant positive shift (these sondes form a  
>signficiant part of the LKS dataset). Similar results occur for US VIZ  
>mini-art 2 to Micro-art software in 1990.

>

>I have many other sonde comparisons, and all are more consistent with the  
>UAH trends more than RSS and certainly VG. Indeed, I was curious to see  
>that your name was on VG's latest paper. I wish I had time to fill you in  
>on why the addition of the non-linear terms is a red herring (both UAH and  
>RSS have performed the calculations with and without the non-linear terms  
>with no impact on the trends) and why the latitudinal difference for  
>calculating the coefficients leads one astray. I'm a little nervous now  
>that you may have a "dog in this fight" as we say in Alabama while writing  
>up the IPCC. I expect my sonde comparisons to be included in the IPCC and  
>I will have further results demonstrating the problems with the Day minus  
>Night technique within a few months.

>

>I've lots to do now. Thanks for listening.

>

>John C.

>

>--

>\*\*\*\*\*

>John R. Christy

>Director, Earth System Science Center voice: 256-961-7763

>Professor, Atmospheric Science fax: 256-961-7751

>Alabama State Climatologist  
>University of Alabama in Huntsville  
>[2]<http://www.nsstc.uah.edu/atmos/christy.html>  
>  
>Mail: ESSC-Cramer Hall/University of Alabama in Huntsville, Huntsville  
>AL 35899  
>Express: Cramer Hall/ESSC, 320 Sparkman Dr., Huntsville AL 35805  
>

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UK

---

## References

1. <mailto:p.jones@uea.ac.uk>
2. <http://www.nsstc.uah.edu/atmos/christy.html>

From: Phil Jones <p.jones@uea.ac.uk>  
To: Kevin Trenberth <trenbert@ucar.edu>  
Subject: One small thing  
Date: Mon Jul 11 13:36:14 2005

Kevin,

In the caption to Fig 3.6.2, can you change 1882-2004 to 1866-2004 and add a reference to Konnen (with umlaut over the o) et al. (1998). Reference is in the list. Dennis must have picked up the MSLP file from our web site, that has the early pre-1882 data in. These are fine as from 1869 they are Darwin, with the few missing months (and 1866-68) infilled by regression with Jakarta. This regression is very good ( $r > 0.8$ ). Much better than the infilling of Tahiti, which is said in the text to be less reliable before 1935, which I agree with.

Cheers  
Phil

Prof. Phil Jones  
Climatic Research Unit Telephone +44 (0) 1603 592090  
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University of East Anglia  
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From: Bette Otto-Bliesner <ottobli@ucar.edu>

To: hegerl@duke.edu

Subject: Re: Sensitivity, LGM and otherwise

Date: Wed, 13 Jul 2005 18:34:00 -0600 (MDT)

Cc: Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>, cddhr@giss.nasa.gov, Keith Briffa <k.briffa@uea.ac.uk>, francis.zwiers@ec.gc.ca

<x-flowed>

Hi Gabi,

Here is the section from the FOD draft that includes the new PMIP-2 results. The radiative forcings have been modified based on new calculations. Note the PMIP-2 LGM model results included in the FOD do not include vegetation or atmospheric aerosol changes so for these results the radiative forcing estimate is 5.7 +/- 1.3 W/m<sup>2</sup>.

Bette

---

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On Wed, 13 Jul 2005 hegerl@duke.edu wrote:

>  
>  
> Hi chapter 6,  
>  
> I am getting a bit nervous about the sensitivity stuff, since  
> chapter 10 wants our version from us (blush nowhere near there)  
> for their summary of all things sensitivity - so I am in the middle  
> of the pipeline....  
> All I'd need is the text from the ZOD, if you want to update anything  
> or make me aware of refs, thats fine, but not as urgent.  
> Did the ZOD have the ice age sensitivity?  
>  
> thank you and sorry...

>  
> **Gabi**  
>  
> -----  
> **Gabriele Hegerl**  
> Dept. of Earth and Ocean Sciences, Nicholas School of the Environment  
> Duke University, Durham NC 27708  
> phone 919-684-6167, fax 919-684-5833  
> email: hegerl@duke.edu <http://www.eos.duke.edu/Faculty/hegerl.html>  
> -----  
>  
>  
>  
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Attachment Converted: "c:\eudora\attach\What do ice ages tell us\_071105.doc"

From: Jonathan Overpeck <jto@u.arizona.edu>  
To: cddhr@giss.nasa.gov, rahmstorf@ozean-klima.de, Bette Otto-Bleisner  
<ottobli@ncar.ucar.edu>, Keith Briffa <k.briffa@uea.ac.uk>, joos  
<joos@climate.unibe.ch>, olgasolomina@yandex.ru, Eystein Jansen  
<eystein.jansen@geo.uib.no>, jto@u.arizona.edu  
Subject: IMPORTANT - The next steps for chapter 6 enroute to THE FOD  
Date: Thu, 14 Jul 2005 21:48:56 -0600

<x-flowed>

Hi all - in the last few emails, we have suggested that you serve as "head" lead authors for the various sections of our chapter. One main purpose of this email is to make sure you are comfortable with the responsibility and have time for it. The other main goal is to explain what is expected of each of you.

First, here's a list of who's heading what sections. We picked you guys since you have proven to be intellectual leaders on the team, but also because you have track records of getting the job done on time. The one person we worry about is Olga, since she is leaving soon for the field, but nonetheless, we'd like all her input on Box 6.3 before she leaves. We will take over after then.

Exec Summary and Section 6.1 - PECK and EYSTEIN  
Section 6.2 - DAVID  
Section 6.3 - STEFAN  
Section 6.4 - BETTE  
Section 6.5 - KEITH  
Section 6.6 - FORTUNAT  
Box 6.1 - DAVID  
Box 6.2 - FORTUNAT  
Box 6.3 - OLGA  
Box 6.4 - KEITH

Second, what is needed? Here is a list that has come to mind. We'd like you all to comment on this list (use the email list used for this email), so that we all agree about what we're doing in the next couple weeks.

1) Your primary job is to make sure your section (text, tables, figs and refs) is as perfect as possible. Each of us has to be careful about how we schedule things so that we have the job DONE by July 24.

2) Each of you should solicit feedback and edits from the ENTIRE LA team, plus relevant CAs. This is obviously to get the best ideas possible, but also to ensure that all on the LA team have had input. Please create a check list and make sure that you have some sort of feedback (at least an "OK") from each LA. We suggest you start asap, and don't expect LAs to just respond to the emails we just sent - many of the LAs just don't respond in a timely fashion (thankfully, you guys are not on that list!).

2.5) Monitor all chapter listserv traffic for your input, as some LAs prefer to communicate only in that way.

3) Please explicitly ask for feedback on the text, tables, figs and refs.

4) With respect to text, try hard to get it down to size (see below), and to ensure that it is FOCUSED on only that science which is policy relevant. ALL TEXT should support an Exec Summary Bullet. If it doesn't the text should be removed, or a bullet created for discussion with our team. Also, although it is ultimately our job to try to make the chapter flow as one document, please do what you can to make your section's text flow with the other sections. Look to make sure all information is compatible across sections, and that the same type of language/style is used (to the extent you can).

4.4) We hope that you will start your process by reading THE ENTIRE CHAPTER carefully, and sending your comments for each section to the "head" LA for that section. This will get things moving fast, and help with the compatibility issues mentioned in #4 above.

5) With respect to the figures (and table), make sure each one is as compelling as possible. To save space (see below) you might decide a figure has to go. You might decide a new figure has to be included (only if there is space!). Work to get the figure redrafted where needed to be perfect - a sign of ultimate success will be that our figs get into the TS/SPM docs. Peck will be on that team, and will push hard, but figures MUST BE POLICY RELEVANT AND COMPELLING.

6) With respect to refs, please make sure that only the most relevant ones are cited, and that all of the citations are complete and entered into your copy of the master chapter endnote file. Although we expect to cite our own work where it makes sense, please be double sure that we're not going overboard in this regard - it won't look good to the outside world (e.g., skeptics) if we appear self-serving at all.

7) If you run into any debates that can't be easily solved (i.e. with all LAs happy), please consult with us. It is our job to make the ultimate calls, since someone has to do it. Again, it is our goal to make sure that no one is left with a bad feeling about our product. On the other hand, we have to make sure we stick to only the best science.

8) We'll be asking to make sure we have all the CAs listed. Let us know if you need to consult with any new ones. AGAIN, we must do what it takes to get the science and message as perfect as possible. CA consultation at this point is encouraged where it will help. For example, we need to get out the Pre-Q box to some Pre-Q experts - we are discussing w/ David.

9) At any point you need input, ask. We are happy to talk on the phone, and can call you or a group if you want a conference call. We are doing this already, and it can save lots of time. Or email. Both of us will be mostly around save a day or two.

10) Size and need to cut some sections. Because of recent changes in the TSU, we haven't been able to get the latest word, but we suspect

that our comments in the FOD draft just sent are true - some sections have a real space issue (factor in figures), others less so. We'll provide more on this soon, and we expect that if you follow the above guidelines, you'll be getting things into more focus, and hopefully less space - especially section 6.3. When thinking about Figs, Tables and Refs, also be thinking "How can I save space?"

11) Feel free to bring in other LAs to help you coordinate. For example, for section 6.3, Bette and Dominique (to be back soon) can be a big help, Stefan. Keith is working with Tim and Ricardo, but also some others to do the job he has left. Etc.

12) We will start sending more info next week, and will help reach consensus on what we're doing, and by when if needed. Let us know what we've missed, and what might be wrong or unclear.

Ok, that's more than enough.

Thanks again for helping us lead the next big push!

Best, Peck and Eystein

--

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Professor, Department of Geosciences  
Professor, Department of Atmospheric Sciences

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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>  
To: Tom Wigley <wigley@cgd.ucar.edu>  
Subject: Re: paleoT  
Date: Fri Jul 15 11:06:31 2005

Tom,

This Briffa series is just a three site average (trees from Tornetrask, Polar Urals and Taimyr) - all in northern Eurasia. It is therefore for a limited region and is likely just the summer, whereas some of the others have regressed on annual T for the NH (or north of 20N).

Of these 3, the first two are in most of the other series (Esper, Crowley, Jones, Mann) and also for HF in Moberg. Not sure whether Taimyr is in any of the others. Esper uses a different standardization approach, but should have most of the same trees, but only TRW. The others use our reconstructions which have MXD is as well.

Have you tried these correlations after extracting the LF trends (say residuals from a 30 or 50 yr filter)? Would expect some of them to be much, much lower.

Keith's reconstruction that would be much better is the one that goes back to only about 1400. Do you have this? Go here [1]<http://www.ncdc.noaa.gov/paleo/paleo.html> then click on paleo data, then on obtaining and look for Keith's - it says 600 years in the title. You can get the data.

Cheers

Phil

At 21:57 14/07/2005, you wrote:

Phil,

I eventually refiltered all the paleo data and have compared these with likewise filtered MAGICC output. Very interesting results.

Can you comment, off the record, on Keith's paleo series.

Here are correlations of individual series against the 7 series average.

(Different series lengths, but essentially same results over common lengths.)

SERIES	1000-1610	1610-1995	1000-1995
Briffa	-.272	.262	.207
Esper	.583	.917	.687
Crowley	.879	.946	.902
Jones	.773	.917	.861
Mann	.760	.856	.822
M&J-NH	.929	.965	.936
Moberg	.904	.856	.871

Correlations with the climate model are not the same -- but Briffa is again the clear outlier.

Why?

Tom.

Prof. Phil Jones

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## References

1. <http://www.ncdc.noaa.gov/paleo/paleo.html>

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
To: Jonathan Overpeck <jto@u.arizona.edu>  
Subject: Re: Your spaghetti figure  
Date: Mon, 18 Jul 2005 07:39:13 +0200  
Cc: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

<x-flowed>

Hi,  
if what Tom writes is correct, then I would think it is not necessary to have a separate paper. But we need to be sure so as not to break any of the regulations since this will be one of the most scrutinized sections of the whole 4AR. I guess it is now up to how Keith and Tim takes the MWP box further and what ends up in the figure.

Cheers,  
Eystein

At 21:35 -0600 17-07-05, Jonathan Overpeck wrote:

>Hi Tom - thx for the quick response. It sounds  
>like you don't need to do the extra pub. Keith  
>and Eystein, do you agree? Tom can help make  
>sure everything is ok, and should probably be a  
>Contributing Author for the effort. Is that  
>appropriate, all? Tom has already given us lots  
>of useful review comments, and I suspect (am I  
>right, Tom) that would be willing to review some  
>more, in addition to helping make sure Keith and  
>Tim get the figure we're thinking about right?  
>Of course, if we run into a methodological or  
>space problem, the fig might still not make it,  
>but Keith, Eystein and I talked and have agreed  
>that it would be good to hammer home that  
>available data do not support the concept of a  
>single (or multiple) globally synchronous (e.g.,  
>to the degree that the late 20th century is)  
>warm events during anyone's definition of  
>Medieval times. We also agreed that this fig  
>would focus on that issue only, and not Medieval  
>warmth vs 20th century. This amplitude issue is

>dealt with in the main "temps of the last 2K"  
>figs that Tim and Keith produced. But, given all  
>the misunderstanding and misrepresenting that is  
>going on wrt to the Medieval Warm Period, we  
>concluded that it's worth the extra space to  
>address the issue in more than one way - hence  
>the decision to try to do something along the  
>lines of your figure.

>  
>It's in Keith and Tim's hands for the next step - they're working away.

>  
>Thanks again to all, best, peck

>  
>Thx, peck

>  
>>Quoting Jonathan Overpeck <jto@u.arizona.edu>:

>>  
>>  
>>Jonathan, can do, but I am wondering if we need to - seven of the curves have  
>>been processed in the way we describe in the  
>>Hegerl et al paper to nature that  
>>gabi sent you - s.d.s even listed in  
>>supplementary file. the only exception is  
>>the Alberta record, which Lockhart (sp?)  
>>extended recently to about 900 - that  
>>is published too - so each of the records has  
>>gone through some peer-processing  
>>- so should the figure itself, based on those data, still require an extra  
>>reference? if so I will still do it, but I  
>>wonder if it is needed. please get  
>>back to me soon on this, tom

>>  
>>> Hi Tom - Looks like we (Keith) is going to try to come up w/ a new  
>>> version of your figure for our MWP Box. We're banking on Susan giving  
>>> us the extra space for this and a couple other things, but I  
>>> recommend you do that quick EOS paper you mentioned. Still ok?

>>>  
>>> Many thanks.

>>>  
>>> best, peck

>>> --  
>>> Jonathan T. Overpeck  
>>> Director, Institute for the Study of Planet Earth

>>> Professor, Department of Geosciences  
>>> Professor, Department of Atmospheric Sciences

>>>

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>--

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From: Keith Briffa <k.briffa@uea.ac.uk>  
To: jto@u.arizona.edu,eystein.jansen@geo.uib.no,tcrowley@duke.edu  
Subject: thoughts and Figure for MWP box  
Date: Mon Jul 18 17:12:06 2005

Dear Peck, Eystein and Tom

At this point we thought it was important to review where we think we are with the MWP Figure.

First, we have no objection to a Figure . Our only concerns have been that we should  
1/... be clear what we wish this Figure to illustrate (in the specific context of the MWP box) - note that this is very different from trying to produce a Figure in such a way as to bias what it says (I am not suggesting that we are, but we have to guard against any later charge that we did this). We say this because there are intonations in some of Peck's previous messages that he wishes to "nail" the MWP - i.e. this could be interpreted as trying to say there was no such thing, and

2/ ...agree that we have done this in the best way.

The truth is that there IS a period of relative warmth around the end of the 1st and start of the 2nd millennium C.E. , but that there are much fewer data to base this conclusion on (and hence the uncertainty around even our multiple calibrated multi-proxy reconstructions are wide). The geographical spread of data also impart a northern (and land) bias in our early proxy data. My understanding of Tom's rationale with the Figure is that we should show how, because the timing of maximum pre-20th century warmth is different in different records, the magnitude of the warmest period (for the Hemisphere , or globe, as a whole) is less than the recently observed warmth.

The reconstructions we plot in Chapter 6 already express the mean Hemispheric warmth (after various selection and scaling of data), and so the additional information that the MWP box figure should show must relate to the scatter of the proxy data. There seems to be a consensus that this is best done by showing individual records , and we are happy to agree. What we worry very much about, however, is that we should not produce a Figure that then conflicts with the picture of proxy evidence for Hemispheric mean warmth as a whole,shown in the main Chapter Figure. By showing a composite (as Tom has done) and scaling against another (30-90degrees N) temperature record - this is just what is done.

As we promised, Tim has produced a similar Figure, using the same series plus a few extras, but omitting the composite mean and the scaling against instrumental temperatures. The idea was to include as many of the original input series (to the various reconstructions) as we could - though avoiding conflicting use of different versions of the same data. The precise selection of records will have to be agreed and, presumably, based on some clear, objective criteria that we would need to justify (this will not be straight forward). This, along with Tom's plot (forwarded by Peck) is in the attachment.

We would like to get your opinion now, and especially Tom's, on the points regarding the composite and scaling. We would be in favour of just showing the series - but do they make the point (and emphasise the message of the text in the box)? Or does the scatter of the

various series as plotted, dilute the message about the strength of 20th century mean warming (note the apparently greater scatter in the 20th century in our figure than in Tom's)? Can you all chip in here please.

best wishes

Keith and Tim

P.S. We agreed in Beijing that we should definitely ask Tom to be a CA .

--

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From: Keith Briffa <k.briffa@uea.ac.uk>  
To: Tim Osborn <t.osborn@uea.ac.uk>  
Subject: Fwd: Re: thoughts and Figure for MWP box  
Date: Wed Jul 20 10:18:03 2005

Date: Tue, 19 Jul 2005 15:38:31 +0100  
To: Tom Crowley <tcrowley@duke.edu>, Jonathan Overpeck <jto@u.arizona.edu>  
From: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Re: thoughts and Figure for MWP box  
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>  
Tom et al  
thanks for remarks - in response to Tom's questions  
At 18:23 18/07/2005, Tom Crowley wrote:

a few comments -

1) are you trying to choose between my way of presenting things and your way - ie, w/out composite?

Yes

2) with your data, do they all go through from beginning to end?

pretty much - and have been standardised over the maximum period for each (not necessarily the best way?)

3) why include chesapeake, which is likely a salinity record?

Because Moberg used it in their latest reconstruction - I agree that I would not use it because of the dubious temperature signal (salinity effect and no local replication) and poor dating control (and I do not like the way the Moberg method effectively over weights the low-frequency predictor series in their analysis).

4) some of your data are from virtually the same site - Mangazeja and yamal are both w. siberia - I composited data available from multiple sites to produce one time series, which is equally counted against the other regions, which might (greenland, w.U.S., e. Asia) or might not have multiple records in them

Just to reiterate - I understood after the group chat with Susan S. in Beijing , that we were being asked to try to produce a "cloud" diagram including as many of "original" predictor series ,from all the reconstructions, to see if it provided an "obvious" picture of the unprecedented warming over the last millennium or so. Tim and I are in no way trying to produce a different Figure for the sake of producing a different Figure . In practice this is hard to do (because some records are sensible "local" composites already, and how far do you go in showing all input data? The problem of what and how to composite is tricky - and no obviously "correct" way is apparent. Having said this , Tom's way is fine with me (provided the composites are robust) and we get general agreement. Am happy to go with Tom's Figure , or version that incorporates as many records as possible - but as we have said - without the composite or temperature scaled add ons.

5) I am not sure whether it is wise to add me to the CA list, just because the reviewer

is supposed to be impartial and a CA loses that appearance of impartiality if he has now been included as a CA - may want to check with Susan S. on this one to be sure - still happy to provide advice

My own position on this is that you are an "unofficial" referee, who has (and still is) making a significant contribution - I see no conflict

6) I am happy to go in either direction - include or not include my figure - all I need are specific directions as to what to do, as CLAs you people need to decide, and then just tell me what or what not to do

Agree - CLAs please rule on the individual record/composite question - I am very happy to go with Tom's Figure. We did ours because we were asked to.

7) I am a little unhappy with the emphasis on hemispheric warmth - lets face it, almost all of the long records are from 30-90N - the question is: how representative is 30-90N to the rest of the world? for the 20th c. one can do correlations with the instrumental record, but co2 has almost certainly increased the correlation scale beyond what it was preanthropogenic.

Absolutely agree , and hope this comes over in text (and bullets) - if not needs strengthening (note David R's comments).

you could correlate with Quelcaya - not sure how many other records there are that are annual resolution - in the tropics I have produced a tropical composite (corals + Quelc.) but it only goes back to ~1780 - corals just don't live v long - in that interval at least the agreement is satisfactory with the mid latitude reconstruction but there is only 100 years extra of independent information beyond the instrumental record..

We have gone round in circles over this , but understand consensus to be that Quelc. not a clean temperature record. Agree corals would be better longer (the new coral-based reconstruction by Rob Wilson et al goes back to 1700 and shows unprecedented tropical warming . Along with the text from Julie we can not go much further, but the importance of extending the tropical (and SH records needs to be very clear)

.THIS MAY NEED TO BE ADDRESSED AS A GENERAL ISSUE SOMEWHERE (SHORTLY) IN YOUR DOC

Really hope it is already - but advise if you think not

tom

Thanks for this - lets take lead from J and E now (also can you advise on state of play with the Hegerl et al manuscript?)

thanks

Keith

Jonathan Overpeck wrote:

Hi Keith, Eystein and Tom: See below (**BOLD**) for my comments. Thanks for moving this forward and making sure we do it right (i.e., without any bias, or perception of bias).

Dear Peck, Eystein and Tom

At this point we thought it was important to review where we think we are with the MWP Figure.

First, we have no objection to a Figure . Our only concerns have been that we should 1/... be clear what we wish this Figure to illustrate (in the specific context of the MWP box) - note that this is very different from trying to produce a Figure in such a way as to bias what it says (I am not suggesting that we are, but we have to guard against any later charge that we did this). We say this because there are intonations in some of Peck's previous messages that he wishes to "nail" the MWP - i.e. this could be interpreted as trying to say there was no such thing, and

**SORRY TO SCARE YOU. I \*\*ABSOLUTELY\*\* AGREE THAT WE MUST AVOID ANY BIAS OR PERCEPTION OF BIAS. MY COMMENT ON "NAILING" WAS MADE TO MEAN THAT ININFORMED PEOPLE KEEPING COMING BACK TO THE MWP, AND DESCRIBING IT FOR WHAT I BELIEVE IT WASN'T. OUR JOB IS TO MAKE IT CLEAR WHAT IT WAS WITHIN THE LIMITS OF THE DATA. IF THE DATA ARE NOT CLEAR, THEN WE HAVE TO BE NOT CLEAR. THAT SAID, I THINK TOM'S FIGURE CAPTURED WHAT I HAVE SENSED IS THE MWP FOR A LONG TIME, AND BASED ON OTHER SOURCES OF INFO - INCLUDING KEITH'S PROSE. THE IDEA OF A FIGURE, IS THAT FIGURES CAN BE MORE COMPELLING AND CONNECT BETTER THAN TEXT. ALSO, THERE ARE MANY WAYS TO LOOK AT THE MWP, AND AS LONG AS WE DON'T INTRODUCE BIAS OR ANYTHING ELSE THAT WILL DILUTE THE MESSAGE IN THE END, THE IDEA IS TO SHOW THE MWP IN MORE WAYS THAN TWO (THAT IS, THE EXISTING FIGS IN THE TEXT THAT KEITH AND TIM MADE).**

2/ ...agree that we have done this in the best way.

The truth is that there IS a period of relative warmth around the end of the 1st and start of the 2nd millennium C.E. , but that there are much fewer data to base this conclusion on (and hence the uncertainty around even our multiple calibrated multi-proxy reconstructions are wide). The geographical spread of data also impart a northern (and land) bias in our early proxy data.

**NEED TO BE CLEAR ABOUT THIS BIAS IN THE CAPTION AND BOX TEXT**

My understanding of Tom's rationale with the Figure is that we should show how, because the timing of maximum pre-20th century warmth is different in different records, the magnitude of the warmest period (for the Hemisphere , or globe, as a whole) is less than the recently observed warmth.

**YES, BUT IN A WAY THAT SAYS "LOOK, HERE ARE THE ACTUAL REGIONAL CURVES - CHECK IT OUT FOR YOURSELF" INSTEAD OF JUST SAYING (IN A SCIENTIFICALLY MORE STANDARD MANNER - HERE ARE THE VARIOUS, MOST ROBUST, LARGE AREA RECONSTRUCTIONS. IN MY MIND, THE LATTER (KEITH/TIM FIGS IN THE MAIN TEXT) WILL BE THE MOST APPEALING/CONVINCING TO PALEOCLIMATE SCIENTISTS, BUT TOM'S MIGHT HELP THERE, AND CERTAINLY WITH NON-PALEO SCIENTISTS AND POLICY FOLKS. MIGHT HELP... IF IT DOESN'T NOTHING LOST, BUT IF IT COULD HURT CONVEYING UNDERSTANDING, THEN ITS BAD TO USE THE NEW FIGURE.**

The reconstructions we plot in Chapter 6 already express the mean Hemispheric warmth (after various selection and scaling of data), and so the additional information that the MWP box figure should show must relate to the scatter of the proxy data. There seems to be a consensus that this is best done by showing individual records , and we are happy to agree.

What we worry very much about, however, is that we should not produce a Figure that then conflicts with the picture of proxy evidence for Hemispheric mean warmth as a

whole, shown in the main Chapter Figure. By showing a composite (as Tom has done) and scaling against another (30-90degrees N) temperature record - this is just what is done.

ABSOLUTELY RIGHT - CAN'T HAVE CONFLICT.

As we promised, Tim has produced a similar Figure, using the same series plus a few extras, but omitting the composite mean and the scaling against instrumental temperatures. The idea was to include as many of the original input series (to the various reconstructions) as we could - though avoiding conflicting use of different versions of the same data. The precise selection of records will have to be agreed and, presumably, based on some clear, objective criteria that we would need to justify (this will not be straight forward). This, along with Tom's plot (forwarded by Peck) is in the attachment.

We would like to get your opinion now, and especially Tom's, on the points regarding the composite and scaling. We would be in favour of just showing the series - but do they make the point (and emphasise the message of the text in the box)? Or does the scatter of the various series as plotted, dilute the message about the strength of 20th century mean warming (note the apparently greater scatter in the 20th century in our figure than in Tom's)? Can you all chip in here please.

best wishes

WHAT ABOUT THE IDEA THAT WE ONLY SHOW THE SERIES FOR THE MWP, SINCE THE COMPARISON TO THE 20TH CENTURY IS DONE WELL (AND BEST?) IN THE TEXT FIGS (WHICH I'M ATTACHING JUST IN CASE TOM DOESN'T HAVE, ALONG WITH THE TEXT - IF YOU HAVE TIME, TOM, PLEASE READ COMMENT ON ANYTHING YOU WISH, BUT CERTAINLY THE LAST 2000 YEARS BIT - ASSUME YOU'LL BE DOING THIS AT THE REVIEW STAGE ANYHOW...)

ANOTHER THING THAT IS A REAL ISSUE IS SHOWING SOME OF THE TREE-RING DATA FOR THE PERIOD AFTER 1950. BASED ON THE LITERATURE, WE KNOW THESE ARE BIASED - RIGHT? SO SHOULD WE SAY THAT'S THE REASON THEY ARE NOT SHOWN? OF COURSE, IF WE ONLY PLOT THE FIG FROM CA 800 TO 1400 AD, IT WOULD DO WHAT WE WANT, FOCUS ON THE MWP ONLY - THE TOPIC OF THE BOX - AND SHOW THAT THERE WERE NOT ANY PERIODS WHEN ALL THE RECORDS ALL SHOWED WARMTH - I.E., OF THE KIND WE'RE EXPERIENCING NOW.

TWO CENTS WORTH

Keith and Tim

P.S. We agreed in Beijing that we should definitely ask Tom to be a CA .

TRUE - BUT HAS ANYONE CONFIRMED W/ TOM. TOM, YOU OK W/ THIS?

THANKS - A GREAT DISCUSSION, AND LETS SAY THE JURY IS STILL OUT ON THIS FIGURE UNTIL WE ALL ARE COMFORTABLE WITH WHAT IT LOOKS LIKE IN THE END.

BEST, PECK

--

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Fax: +44-1603-507784

Attachment converted: Macintosh HD:mwpbox\_figures.pdf (PDF /«IC») (0008A8AE)

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

--

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[2]<http://www.cru.uea.ac.uk/cru/people/briffa/>

## References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: Tim Osborn <t.osborn@uea.ac.uk>  
Subject: Fwd: Re: the regional section and MWP Figure  
Date: Wed Jul 20 11:03:15 2005

From: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>  
To: "Jonathan Overpeck" <jto@u.arizona.edu>,  
"Edward R. Cook" <drdendro@ldeo.columbia.edu>  
Cc: "Keith R. Briffa" <k.briffa@uea.ac.uk>,  
<eystein.jansen@geo.uib.no>

Subject: Re: the regional section and MWP Figure  
Date: Tue, 19 Jul 2005 15:35:39 -0300  
X-Mailer: Microsoft Outlook Express 6.00.2800.1437

Dear Keith and Ed,

Please, find attached the new version of the SH figure for the IPCC. I have now included the New Zealand record. All the records have been scaled to 4 °C amplitude. Variability in the Tas record is reduced compared to New Zealand and Patagonian records. The reference lines is the mean used for the calibration period in each record, 15 C for New Zealand, 14.95 C for Tasmania and 0 C for the Patagonian records (they show departures). Please, let me know if you want to introduce some changes in the figure. The opposite phase in the Patagonia-New Zealand records is so clear before 1850, which is consistent with our previous TPI. For instance, in the instrumental record the 1971 and 1976 are the coolest summer in northern Patagonian during the past 70 years, but the warmest in New Zealand reconstruction!! This out of phase relationship between regions in the Southern Hemisphere points out to the difficulty of using few records to get a hemispheric average. Cheers,

Ricardo

----- Original Message -----

From: "Jonathan Overpeck" <jto@u.arizona.edu>  
To: "edwardcook" <drdendro@ldeo.columbia.edu>  
Cc: "Keith Briffa" <>; "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>;  
"Eystein Jansen" <>

Sent: Monday, July 18, 2005 1:09 PM

Subject: Re: the regional section and MWP Figure

Thanks Ed - Ricardo, can you get the data from Henry? What do you think, Keith?

Best, Peck

>Given the nature of the SH and what Ricardo put  
>together, I would keep the Australian and South  
>African borehole records separate. Henry

>Pollack can provide them, I am sure. He gave an  
>excellent talk at a meeting in Canberra that I  
>recently participated in.

>  
>Cheers,

>  
>Ed

>  
>P.S. Ricardo, here is the Oroko temperature reconstruction.

>  
>JANUARY-MARCH TEMPERATURES RECONSTRUCTED FROM  
>OROKO SWAMP, NEW ZEALAND SILVER PINE TREE RINGS  
>BE ADVISED THAT THE DATA AFTER 1958 ARE INSTRUMENTAL  
>TEMPERATURES

> YEAR TEMP °C

> 900 13.751  
> 901 14.461  
> 902 13.236  
> 903 13.331  
> 904 13.483  
> 905 13.632  
> 906 12.959  
> 907 13.628  
> 908 13.372  
> 909 12.868  
> 910 13.244  
> 911 13.793  
> 912 14.048  
> 913 14.444  
> 914 13.095  
> 915 14.036  
> 916 13.215  
> 917 13.698  
> 918 13.570  
> 919 13.665  
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> 922 14.762  
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> 924 14.077  
> 925 14.713  
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> 928 13.793  
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> 933 14.110  
> 934 14.968  
> 935 14.391  
> 936 15.484  
> 937 15.554  
> 938 14.977  
> 939 15.303  
> 940 15.179  
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> 978 14.977  
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> 993 14.754  
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> 995 13.623  
> 996 14.300  
> 997 13.937  
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> 999 14.011  
> 1000 12.976  
> 1001 13.904  
> 1002 13.500  
> 1003 13.586  
> 1004 14.090  
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>  
>On Jul 17, 2005, at 10:40 PM, Jonathan Overpeck wrote:

>

>>Thanks Ricardo and Ed! I personally am not a  
>>big fan of the Jones and Mann SH recon. It is  
>>based on so little. On the other hand, it is in  
>>the literature. So, I leave it up to you and  
>>Keith to decide - perhaps Eystein can weigh in  
>>too.

>>  
>>I do, however, think it would be really helpful  
>>to included the borehole data (see prev.  
>>emails) - either as a single SH curve, or  
>>(probably better) two regional curves  
>>(Australia and S. Africa). Is there a reason  
>>this is not a good idea? Can't complain about  
>>snow bias down there...

>>  
>>Thanks again - I look forward to seeing the  
>>next draft and figure - complete w/ borehole I  
>>hope.

>>  
>>thx, Peck

>>  
>>>Hi Keith,

>>>  
>>>Please, find attached my last version of the SH temp. As you know, Ed  
Cook

>>>returned my original version of the SH with minor comments. Overall, he  
>>>agreed with the text. Still I am waiting from him the Oroco Swamp data to  
>>>include in the Figure, which first draft I sent you more than a month  
ago.

>>>  
>>>In the last version I have included a first paragraph referring to the  
Jones

>>>and Mann (2003) temperature reconstruction for the SH. At that time we  
have

>>>to decide if we want to have the hemispheric (Jones and Mann) and the  
>>>regional views (Tasmania, New Zealand, Patagonia, maybe include  
Antarctica

>>>(Ommem et al. 2005)), or just one of them. If we decide to stay with the  
>>>hemispheric view, we should include Jones and Mann reconstruction at the  
>>>bottom of one of your figures. In cases that we decide to maintain both  
>>>hemispheric and regional views, we should include Jones and Mann at the  
>>>bottom of my figure. Please, could you check with Peck and Eystein to  
see

>>>the best way to proceed? Thanks,

>>>

>>>Ricardo

>>>

>>>

>>>

>>>----- Original Message -----

>>>From: "Keith Briffa" <k.briffa@uea.ac.uk>

>>>To: <jto@u.arizona.edu>; "Eystein Jansen" <Eystein.Jansen@geo.uib.no>

>>>Cc: <ricardo@lab.cricyt.edu.ar>; "Ed Cook" <drdendro@ldgo.columbia.edu>

>>>Sent: Friday, July 15, 2005 11:01 AM

>>>Subject: the regional section and MWP Figure

>>>

>>>> Guys

>>>> still need the SH temp bit from Ricardo/ED to edit and am exploring  
the

>>>>MWP

>>>> Figure - but the concept still is unclear to me - but we agreed to do  
a

>>>> plot like Tom's . The regional section is still a worry - I am happy  
to

>>>> very briefly edit the section on NAO (possibly incorporate the ENSO  
>>>>stuff )

>>>> but my understanding is that this section is best done (to incorporate  
>>>>also

>>>> the regional moisture work of Ed ) by Ricardo /Ed with input my Peck.  
This

>>>> is still my opinion. I also would appreciate feedback re the regional  
>>>> forcing section that I think we may have to drop - but perhaps not.

>>>> Therefore I ask that when i get the SH temp stuff I will incorporate  
it

>>>>but

>>>> that you guys (Peck, Ricardo, Ed and Eystein interacting over the  
North

>>>> Atlantic bit) first review and redo the regional section .

>>>> It is important to get feedback from Henry re the borehole stuff and  
>>>> involve Tom in the debate with all of us , of the value of the Figure

. In

>>>> meantime , will experiment with the Figure and review existing text  
and

>>>>bullets

>>>> Keith

>>>>

>>>> Keith

>>>>

>>>>

>>>>

>>>> --

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>>>>

>>>>

>>>

>>>Attachment converted: Macintosh HD:Southern

>>>hemisphere2.doc (WDBN/«IC») (0008A6E0)

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>>

>>--

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>>

>=====

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>=====

--

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[6]<http://www.cru.uea.ac.uk/cru/people/briffa/>

## References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.geo.arizona.edu/>
3. <http://www.ispe.arizona.edu/>
4. <http://www.geo.arizona.edu/>
5. <http://www.ispe.arizona.edu/>
6. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: David Rind <drind@giss.nasa.gov>  
To: Stefan Rahmstorf <rahmstorf@ozean-klima.de>  
Subject: Re: [Wg1-ar4-ch06] Comments on Section 6.3  
Date: Wed, 20 Jul 2005 12:11:20 -0400  
Cc: wg1-ar4-ch06@joss.ucar.edu

<x-flowed>

Dear Stefan,

The distinction here is that GCMs attempt to calculate from first principles the zeroth and first order processes that dominate the problem they are studying, whereas EMICs parameterize many of those processes. The fact that EMICs can reproduce GCM results suggest that their parameterizations have been tuned to do so - but this does not in any way imply that if one alters the forcing or boundary conditions outside of a small range, or apply them to completely different problems, that the two types of models will react similarly. In fact, there is a history of this - the first "EMICs" had a very large sensitivity to a 2% solar insolation change; then they had to be re-tuned to prevent that from happening. EMICs are used for paleo-problems because of their ability to take large time-steps, but there is no free lunch - in doing so, they sacrifice calculating the fundamental physical processes the way the real world does it. GCMs have storms, they have real water vapor transports, they have winds calculated from solving the conservation of momentum equation, etc. etc. There is a quantum difference between the fundamental approaches - it is not a continuum, in which there are no real differences, everything is simply a matter of opinion, there is no such thing as truth - that's the argument that greenhouse skeptics use to try to make science go away.

Because we can't use GCMs for long-time scale problems, we do the best we can - we use these heavily parameterized models. If we could use GCMs for those problems, EMICs could then be tuned to produce the GCM results on those time-scales as well. But in this case we have no way to validate the EMIC results - and since the first principles are not being used, we cannot know whether they represent a physically consistent solution or not. Therefore all they can do is suggest interactions among processes, a useful though not definitive addition to the field.

David

ps - concerning CLIMBER-2, I asked a number of leading climate scientists to read the model description paper. Peter Stone was the only person I asked who thought the model was at all useful for studying the types of problems we are discussing. And it was not only GCM scientists. If you want to hear further cogent arguments concerning its inapplicability, consider contacting Bill Rossow (the recent winner of a major honor as a leading climate scientist) but make sure your email program or telephone accepts unexpurgated text.

At 4:22 PM +0200 7/20/05, Stefan Rahmstorf wrote:

>Dear David,

>

>I take from your response that you consider all models that  
>parameterise an important first-order process "conceptual models". I  
>can live with that - but then there are only conceptual climate  
>models around. Any coupled climate GCM that I know of parameterises  
>oceanic convection (and in a very crude way), hence it is a  
>conceptual model in your terms, and there is no fundamental  
>distinction of category between your model and our model.

>

>To me the scientific question is not whether an important process is  
>parameterised (many are in GCMs) - it is how well this  
>parameterisation works, for the task at hand. We have tested the  
>feedbacks in great detail (e.g., the cloud, water vapour, lapse rate  
>and snow/ice albedo feedbacks for 2xCO<sub>2</sub>) in our model and they  
>perform quantitatively within the range simulated by various GCMs.  
>The same is true for many other diagnostics - the model has taken  
>part in model intercomparisons with GCMs and always falls within the  
>range of different GCMs, in a quantitative way. To repeat that  
>point, the quantitative differences between different GCMs are  
>larger than the typical difference between our model and a GCM. So I  
>see no basis for your claim that this model can only "suggest orders  
>of magnitude". That's just plain wrong from all the evidence that I  
>have seen (a lot). If you have concrete evidence to the contrary,  
>other than just knowing one person who happens to agree with you,  
>please come forward with it.

>

>Stefan

>

>--

>To reach me directly please use: rahmstorf@ozean-klima.de

>(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)

>

>Stefan Rahmstorf

>[www.ozean-klima.de](http://www.ozean-klima.de)

>[www.realclimate.org](http://www.realclimate.org)

---

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<http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>

</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: Jonathan Overpeck <jto@u.arizona.edu>, Tom Crowley <tcrowley@duke.edu>  
Subject: Re: CLA feedback on Tom and the MWP  
Date: Wed Jul 20 12:18:22 2005  
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, t.osborn@uea.ac.uk

Hi all

think this is resolved now (virtually) -

We use series that total to Tom/Gabi composite, and we can cite this as an example of the scatter of regional records "in a typical reconstruction". This avoids very difficult issue of what is the best way to aggregate certain data sets - we are simply illustrating the point with one published (by then) data set.

The issue of the composite is then not an issue either, because it is not a new (unpublished) composite that we were concerned about - though I still believe it is a distraction to put the composite in. It would be best to use data from 800 or 850 at least, and go to 1500 (?) and presumably normalise over the whole period of data shown. OK? Even though you guys all wish to go with the reduced period (ie not up the present), but my own instinct is that this might later come back to haunt us - but will take your lead.

I agree the look of the Figure should match the others.

So, if Tom will send the data sets (his regional curves), Tim will plot and send back asap for scrutiny. Thanks Tom and thanks for your help with this - further comments on latest version of 6.5 (last 2000 years) still welcome, though will be incorporating a few changes in response to David and Fortunat input, and SH bit (from Ricardo and Ed) still to go in and regional section to be revised (after input from Peck et al.)

cheers

Keith

.

At 21:42 19/07/2005, Jonathan Overpeck wrote:

Hi Keith and Tim: Just got off the phone with Eystein, and hopefully he will sleep ok knowing that we have a plan for the MWP fig and Tom...

Please ask questions if we don't cover all the key points, but here's what we think:

- 1) the MWP fig should span the MWP only, and should emphasize variation in regional amplitude (we agree that we must be clear that this fig is not a reconstruction) - that is, it is best to use time series representing regions, assuming that the regional series do represent a region ok with one or more input series. We want to avoid a regional bias if we can - this is what got us into all the MWP misunderstanding in the first place, perhaps (e.g., nice MWP in Europe/Atlantic region - must be global)
- 2) If you guys could agree on the series and the interval, that'd be great. We agree it would be good to start before 1000 and end before the Renaissance (15th century?). If you want more feedback on these issues, we're happy to provide, but it seems logical

that you pick series and intervals so that each series covers the entire interval selected.

3) Don't use the Chesapeak record - it is likely biased by salinity

4) We'd like Keith and Tim to draft the final figure so that it matches the look and style of the other two figs they have made. Hope this is doable. Tom, does Keith have all the data? Thanks for sending if not.

5) We agree that Tom should NOT be a CA given that he was officially one of the ZOD reviewers. Of course, this doesn't represent a real conflict, but we need to avoid even the appearance of conflict. We greatly appreciate all the feedback that Tom is providing! Is this plan ok w/ you Tom? We think you're cool with it, but just want to check one more time.

That... it is. Please let us know if there are any more questions. Keith - feel free to try and get Eystein on his cell doing your work hours if you want quick feedback. Or we can do this by email - he's not in a very email friendly place right now, but the fishing appears to be ok.

Again, thanks to you both for all the discussion and thought that has gone into this figure.

Best, peck

--

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## References

1. <http://www.geo.arizona.edu/>
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3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Stefan Rahmstorf <rahmstorf@ozean-klima.de>  
To: David Rind <drind@giss.nasa.gov>  
Subject: Re: [Wg1-ar4-ch06] Comments on Section 6.3  
Date: Wed, 20 Jul 2005 12:39:05 +0200  
Cc: Eystein Jansen <Eystein.Jansen@geo.uib.no>

Dear chapter 6 friends,

I have a request on procedure. In the interest of a good and constructive working atmosphere, I would suggest that all of us focus on sober scientific arguments and refrain from unnecessarily derogatory comments about the work of colleagues. I'm referring in this case to David's comment

- this reference is overused, especially for such a simplistic model

The reference concerned is our theory of DO events which appeared in Nature in 2001 and has since been cited 133 times according to the Web of Science (a sign of overuse?) The model concerned is the CLIMBER-2 model, featured in over 50 peer-reviewed publications since 1998, including 7 in Nature and Science.

This model is different from David's model, because it has been constructed for a different purpose, but it is not "simplistic". It would never occur to me to call David's model "simplistic" because it does not include an interactive continental ice sheet model, vegetation model, carbon cycle model, sediment model and isotope model.

I'm absolutely open to any rational scientific criticism and discussion, but I can see no purpose in derogatory statements like the above, which include not even a trace of scientific argument. This kind of thing only poisons the working atmosphere in our group, which I thought was very positive and a great pleasure in Beijing.

Regards, Stefan

--

To reach me directly please use: [1]rahmstorf@ozean-klima.de  
(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)

Stefan Rahmstorf  
[2]www.ozean-klima.de  
[3]www.realclimate.org

---

Wg1-ar4-ch06 mailing list  
Wg1-ar4-ch06@joss.ucar.edu <http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>

## References

1. <mailto:rahmstorf@ozean-klima.de>
2. <http://www.ozean-klima.de/>

3. <http://www.realclimate.org/>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Tom Crowley <tcrowley@duke.edu>

Subject: Re: CLA feedback on Tom and the MWP

Date: Wed, 20 Jul 2005 14:23:24 -0600

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Tom - thanks. Good points regarding regional labeling. Defn stick to Tibetan Plateau!

best, peck

>Keith, if you can find more I see no problem - it seems that a lot  
>of the data you used was via Cook and colleagues - I was unable to  
>locate a full length record from Quebec in that time series, but  
>maybe you are relying on something else - if so can I have it!?  
>  
>other suggestions: provide a more general label to sites - eg,  
>mangazeyek (sp)/yamal could be listed as polar urals - taimyr  
>central Siberia.  
>  
>China should be relabeled as east Asia as it does include some  
>information from Japan and the Tibetan Plateau (L. Thompson) and we  
>don't want to get into some political to-do by calling Tibet  
>"Chinese".  
>  
>that's all I can think of for present, good sailing, tom  
>  
>Keith Briffa wrote:  
>  
>>Hi all  
>>think this is resolved now (virtually) -  
>>  
>>We use series that total to Tom/Gabi composite , and we can cite  
>>this as an example of the scatter of regional records "in a typical  
>>reconstruction". This avoids very difficult issue of what is the  
>>best way to aggregate certain data sets - we are simply  
>>illustrating the point with one published (by then) data set.  
>>The issue of the composite is then not an issue either , because it  
>>is not a new (unpublished) composite that we were concerned about -  
>>though I still believe it is a distraction to put the composite in.  
>>It would be best to use data from 800 or 850 at least , and go to

>>1500 (?) and presumably normalise over the whole period of data  
>>shown. OK? Even though you guys all wish to go with the reduced  
>>period (ie not up the present) , but my own instinct is that this  
>>might later come back to haunt us - but will take your lead.

>>I agree the look of the Figure should match the others.

>>So, if Tom will send the data sets (his regional curves) , Tim will  
>>plot and send back asap for scrutiny. Thanks Tom and thanks for  
>>your help with this - further comments on latest version of 6.5  
>>(last 2000 years) still welcome , though will be incorporating a  
>>few changes in response to David and Fortunat input , and SH bit  
>>(from Ricardo and Ed) still to go in and regional section to be  
>>revised (after input from Peck et al.)

>>cheers

>>Keith

>>.

>>

>>

>> At 21:42 19/07/2005, Jonathan Overpeck wrote:

>>

>>>Hi Keith and Tim: Just got off the phone with Eystein, and  
>>>hopefully he will sleep ok knowing that we have a plan for the MWP  
>>>fig and Tom...

>>>

>>>Please ask questions if we don't cover all the key points, but  
>>>here's what we think:

>>>

>>>1) the MWP fig should span the MWP only, and should emphasize  
>>>variation in regional amplitude (we agree that we must be clear  
>>>that this fig is not a reconstruction) - that is, it is best to  
>>>use time series representing regions, assuming that the regional  
>>>series do represent a region ok with one or more input series. We  
>>>want to avoid a regional bias if we can - this is what got us into  
>>>all the MWP misunderstanding in the first place, perhaps (e.g.,  
>>>nice MWP in Europe/Atlantic region - must be global)

>>>

>>>2) If you guys could agree on the series and the interval, that'd  
>>>be great. We agree it would be good to start before 1000 and end  
>>>before the Renaissance (15th century?). If you want more feedback  
>>>on these issues, we're happy to provide, but it seems logical that  
>>>you pick series and intervals so that each series covers the  
>>>entire interval selected.

>>>

>>>3) Don't use the Chesapeak record - it is likely biased by salinity

>>>

>>>4) We'd like Keith and Tim to draft the final figure so that it  
>>>matches the look and style of the other two figs they have made.  
>>>Hope this is doable. Tom, does Keith have all the data? Thanks for  
>>>sending if not.

>>>

>>>5) We agree that Tom should NOT be a CA given that he was  
>>>officially one of the ZOD reviewers. Of course, this doesn't  
>>>represent a real conflict, but we need to avoid even the  
>>>appearance of conflict. We greatly appreciate all the feedback  
>>>that Tom is providing! Is this plan ok w/ you Tom? We think you're  
>>>cool with it, but just want to check one more time.

>>>

>>>That... it is. Please let us know if there are any more questions.  
>>>Keith - feel free to try and get Eystein on his cell doing your  
>>>work hours if you want quick feedback. Or we can do this by email  
>>>- he's not in a very email friendly place right now, but the  
>>>fishing appears to be ok.

>>>

>>>Again, thanks to you both for all the discussion and thought that  
>>>has gone into this figure.

>>>

>>>Best, peck

>>>--

>>>Jonathan T. Overpeck  
>>>Director, Institute for the Study of Planet Earth  
>>>Professor, Department of Geosciences  
>>>Professor, Department of Atmospheric Sciences

>>>

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>>>

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>>

>>--

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--

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</x-flowed>

From: Tim Osborn <t.osborn@uea.ac.uk>  
To: Keith Briffa <k.briffa@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>  
Subject: Re: crowley  
Date: Wed Jul 20 16:58:40 2005  
Cc: p.jones@uea.ac.uk

Hi Tom,

as a followup to Keith's email, it might be quite likely that one of the series you plot is replaced by the instrumental record after 1960, because the file from Crowley and Lowery that is available at the WDC-Paleoclimate contains such a record. The header states:

-----  
Crowley and Lowery 2000 (Ambio 29, 51)  
Northern Hemisphere Temperature Reconstruction  
Modified as published in Crowley 2000  
(Science v289 p.270, 14 July 2000)  
Data from Fig. 1, Crowley 2000:  
Decadally smoothed time series of Crowley-Lowery reconstruction  
spliced into smoothed Jones et al instrumental record after 1860  
(labeled CL2.Jns11), and a slight modification (labeled CL2)  
of the original Crowley and Lowery reconstruction to 1965.

-----  
The URL of this file is:

[1][ftp://ftp.ncdc.noaa.gov/pub/data/paleo/gcmoutput/crowley2000/crowley\\_lowery2000\\_nht.txt](ftp://ftp.ncdc.noaa.gov/pub/data/paleo/gcmoutput/crowley2000/crowley_lowery2000_nht.txt)  
and it is listed here:

[2]<http://www.ncdc.noaa.gov/paleo/recons.html>

Cheers

Tim

At 12:22 18/07/2005, Keith Briffa wrote:

as a first quick response - the Crowley numbers came from his paper with Lowery. I seem to remember that there were 2 versions of the composite that he produced - certainly we used the data that did not include Sargasso and Michigan site data. I presume the other (from the CRU web site) were the data used by Phil and Mike Mann that they got from him (where exactly did you pick them up from?) and could be the other data set (with those sites included). It seems odd that the values are so high in the recent period of this series and could conceivably be instrumental data, but would have to check. The scaling of the data we used to produce the Crowley curve that formed one of the lines in our spaghetti diagram (that we put on the web site under my name and made available to NGDC), was based on taking the unscaled composite he sent and re-calibrating against April - Sept. average for land North of 20 degrees Lat., and repeating his somewhat bazaar calibration procedure (which deliberately omitted the data between 1900-1920 that did not fit with the instrumental data (remember his data are also decadal smoothed

values). In fact , as we were using summer data we calibrated over 1881-1900 (avoiding the high early decades that I still believe are biased in summer) and 1920 - 1960 , whereas he used 1856-1880 and 1920-1965. Of the precise details might differ - but the crux of the matter is that I suspect one of the Figures you show may have instrumental data in the recent period - but not ours. If you say exactly where these series came from I can ask Tim (who will have done the calibrations) to check.

As for the second question , the QR data are averaged ring widths from relatively few site chronologies in the high north (mostly N.Eurasia - Scandinavia, Yamal, Taimyr), though with a few other site data added in as stated. The 2001 data are the MXD data from near 400 sites and provide the best interannual to multidecadal indication of summer temps for land areas north of 20 degrees than any of the true proxy (ie not including instrumental ) data. No idea what the correlation over the common 600 year period is - but I have never said that the ring width is anything other than summer temps for the area it covers .

Keith

At 20:38 15/07/2005, you wrote:

Keith,

Look at the attached. Can you explain to me why these plots differ -- particularly after 1880?

Could you also explain why the Briffa data in QR 2000 are so poorly correlated with the Briffa 2001 data?

I think I know the answers, but I want an independent and spontaneous answer from you.

Thanks,

Tom.

--

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## References

1. [ftp://ftp.ncdc.noaa.gov/pub/data/paleo/gcmoutput/crowley2000/crowley\\_lowery2000\\_nht.txt](ftp://ftp.ncdc.noaa.gov/pub/data/paleo/gcmoutput/crowley2000/crowley_lowery2000_nht.txt)
2. <http://www.ncdc.noaa.gov/paleo/recons.html>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>, "Jonathan Overpeck" <jto@u.arizona.edu>  
Subject: Re: the regional section and MWP Figure  
Date: Thu Jul 21 08:51:37 2005  
Cc: <drdendro@ldgo.columbia.edu>, <eystein.jansen@geo.uib.no>

Hi Ricardo and all

this all seems fine with me - the question of the temperature observations is a moot one - but some included seems a good idea - 1 South American and 1 New Zealand is fine - length not as important as proximity to the records shown (but need to see what they look like). will wait on other numbers - Henry is best qualified to cite most appropriate SH borehole data and could supply a line of text . Ricardo can you ask him for these?

best wishes

Keith

At 13:52 20/07/2005, Ricardo Villalba wrote:

Hi Keith, Ed, Peck, Eystein

Regarding Peck's suggestions,

- 1) should we include instrumental data? If not, it could lessen the impact. Rio de Janeiro, starting in 1851, is the longest, homogeneous temperature record from the Southern Hemisphere. In New Zealand and Australia, temperature records start at the same time. We do not have any long record for the 18th century, even the first half of the 19th century. The hemispheric record from the Southern Hemisphere will be discussed in Chapter 2 and we do not have any additional information to provide.
- 2) we need to include the two borehole (see previous email from me and Ed) Definitely!! I do not have the records here in Mendoza. Keith, do you have access to these data? As soon as I receive the borehole records I will incorporate them in the figures. I would appreciate receiving the key references to properly cite the records.
- 3) we would like to ask Keith and Tim (pretty please...) to draft the final figure so that it matches the other in the section and MWP box. Is this ok, and do you have the data to do the job. If not, we trust your kind colleagues can send upon request?

At the time the figure is ready, I will send all the data to Keith and Tim to draft the final figure, and the final text to incorporate in the FOD.

Cheers,

Ricardo

----- Original Message -----

From: "Jonathan Overpeck" <jto@u.arizona.edu>

To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

Cc: "Keith Briffa" <k.briffa@uea.ac.uk>; <drdendro@ldgo.columbia.edu>;

"Eystein Jansen" <eystein.jansen@geo.uib.no>

Sent: Tuesday, July 19, 2005 5:55 PM

Subject: Re: the regional section and MWP Figure

Hi SH gang - Thanks for keeping things moving

Ricardo. Eystein and I just discussed this fig on

the phone and would like to suggest the following:

1) should we include instrumental data? If not, it could lessen the impact.

2) we need to include the two borehole (see previous email from me and Ed)

3) we would like to ask Keith and Tim (pretty please...) to draft the final figure so that it matches the other in the section and MWP box. Is

this ok, and do you have the data to do the job.

If not, we trust your kind colleagues can send

upon request?

Many thanks, Peck and Eystein

>Dear Keith and Ed,

>

>Please, find attached the new version of the SH figure for the IPCC. I

have

>now included the New Zealand record. All the records have been scaled to 4

>°C amplitude. Variability in the Tas record is reduced compared to New

>Zealand and Patagonian records. The reference lines is the mean used for

the

>calibration period in each record, 15 C for New Zealand, 14.95 C for

>Tasmania and 0 C for the Patagonian records (they show departures). Please,

>let me know if you want to introduce some changes in the figure. The

>opposite phase in the Patagonia-New Zealand records is so clear before

1850,

>which is consistent with our previous TPI. For instance, in the

instrumental

>record the 1971 and 1976 are the coolest summer in northern Patagonian

>during the past 70 years, but the warmest in New Zealand reconstruction!!

>This out of phase relationship between regions in the Southern Hemisphere

>points out to the difficulty of using few records to get a hemispheric

>average. Cheers,

>

>Ricardo

>

>----- Original Message -----

>From: "Jonathan Overpeck" <jto@u.arizona.edu>

>To: "edwardcook" <drdendro@ldeo.columbia.edu>

>Cc: "Keith Briffa" <>; "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>;

>"Eystein Jansen" <>

>Sent: Monday, July 18, 2005 1:09 PM

>Subject: Re: the regional section and MWP Figure

>

>

>Thanks Ed - Ricardo, can you get the data from Henry? What do you think,

>Keith?

>Best, Peck

>

>>Given the nature of the SH and what Ricardo put

>>together, I would keep the Australian and South

>>African borehole records separate. Henry

>>Pollack can provide them, I am sure. He gave an

>>excellent talk at a meeting in Canberra that I

>>recently participated in.

>>

>>Cheers,

>>

>>Ed

>>

>>P.S. Ricardo, here is the Oroko temperature reconstruction.

>>

>>JANUARY-MARCH TEMPERATURES RECONSTRUCTED FROM

>>OROKO SWAMP, NEW ZEALAND SILVER PINE TREE RINGS

>>BE ADVISED THAT THE DATA AFTER 1958 ARE INSTRUMENTAL

>>TEMPERATURES

>> YEAR TEMP °C

>> 900 13.751

>> 901 14.461

>> 902 13.236

>> 903 13.331

>> 904 13.483

>> 905 13.632

>> 906 12.959

>> 907 13.628

>> 908 13.372

>> 909 12.868

>> 910 13.244

>> 911 13.793

>> 912 14.048

>> 913 14.444

>> 914 13.095

>> 915 14.036  
>> 916 13.215  
>> 917 13.698  
>> 918 13.570  
>> 919 13.665  
>> 920 13.871  
>> 921 13.966  
>> 922 14.762  
>> 923 14.325  
>> 924 14.077  
>> 925 14.713  
>> 926 14.081  
>> 927 14.218  
>> 928 13.793  
>> 929 14.151  
>> 930 14.985  
>> 931 13.599  
>> 932 14.663  
>> 933 14.110  
>> 934 14.968  
>> 935 14.391  
>> 936 15.484  
>> 937 15.554  
>> 938 14.977  
>> 939 15.303  
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>> 941 15.591  
>> 942 14.737  
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>> 945 14.449  
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>> 948 15.257  
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>> 950 15.303  
>> 951 15.513  
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>> 954 15.459  
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>> 957 13.850

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>> 966 14.486  
>> 967 14.444  
>> 968 14.436  
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>> 970 13.809  
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>> 996 14.300  
>> 997 13.937  
>> 998 14.040  
>> 999 14.011  
>> 1000 12.976

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>> 1002 13.500  
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>> 1004 14.090  
>> 1005 13.809  
>> 1006 13.413  
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>> 1008 13.892  
>> 1009 14.151  
>> 1010 14.391  
>> 1011 13.793  
>> 1012 14.626  
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>> 1015 13.017  
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>> 1018 13.471  
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>> 1021 13.203  
>> 1022 14.090  
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>> 1024 13.755  
>> 1025 13.826  
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>> 1079 13.331  
>> 1080 14.267  
>> 1081 13.644  
>> 1082 13.549  
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>> 1084 13.549  
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>> 1086 13.479

>> 1087 12.848  
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>> 1090 13.793  
>> 1091 14.387  
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>> 1093 14.114  
>> 1094 14.754  
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>> 1099 15.059  
>> 1100 15.055  
>> 1101 16.057  
>> 1102 15.208  
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>> 1117 14.391  
>> 1118 14.981  
>> 1119 15.125  
>> 1120 13.817  
>> 1121 12.897  
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>> 1123 14.271  
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>> 1138 14.779  
>> 1139 15.129  
>> 1140 15.117  
>> 1141 14.849  
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>> 1156 15.364  
>> 1157 15.855  
>> 1158 15.422  
>> 1159 14.515  
>> 1160 15.810  
>> 1161 15.628  
>> 1162 15.402  
>> 1163 15.092  
>> 1164 15.298  
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>> 1179 14.510  
>> 1180 14.865  
>> 1181 14.036  
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>> 1198 15.014  
>> 1199 14.539  
>> 1200 14.044  
>> 1201 14.733  
>> 1202 14.853  
>> 1203 15.298  
>> 1204 13.772  
>> 1205 13.991  
>> 1206 14.651  
>> 1207 14.836  
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>> 1998 15.417  
>> 1999 16.354

>>

>>On Jul 17, 2005, at 10:40 PM, Jonathan Overpeck wrote:

>>

>>>Thanks Ricardo and Ed! I personally am not a  
>>>big fan of the Jones and Mann SH recon. It is  
>>>based on so little. On the other hand, it is in  
>>>the literature. So, I leave it up to you and  
> >>Keith to decide - perhaps Eystein can weigh in  
>>>too.

>>>

>>>I do, however, think it would be really helpful  
>>>to included the borehole data (see prev.  
>>>emails) - either as a single SH curve, or  
>>>(probably better) two regional curves  
>>>(Australia and S. Africa). Is there a reason  
>>>this is not a good idea? Can't complain about  
>>>snow bias down there...

>>>

>>>Thanks again - I look forward to seeing the  
>>>next draft and figure - complete w/ borehole I  
> >>hope.

>>>

>>>thx, Peck

>>>

>>>>Hi Keith,

>>>>

>>>>Please, find attached my last version of the SH temp. As you know, Ed  
>Cook

>>>>returned my original version of the SH with minor comments. Overall, he  
>>>>agreed with the text. Still I am waiting from him the Oroco Swamp data  
to

>>>>include in the Figure, which first draft I sent you more than a month  
>ago.

>>>>

>>>>In the last version I have included a first paragraph referring to the

>Jones

>>>>and Mann (2003) temperature reconstruction for the SH. At that time we

>have

>>>>to decide if we want to have the hemispheric (Jones and Mann) and the

>>>>regional views (Tasmania, New Zealand, Patagonia, maybe include

>Antarctica

>>>>(Ommem et al. 2005)), or just one of them. If we decide to stay with the

>>>>hemispheric view, we should include Jones and Mann reconstruction at the

>>>>bottom of one of your figures. In cases that we decide to maintain both

>>>>hemispheric and regional views, we should include Jones and Mann at the

>>>>bottom of my figure. Please, could you check with Peck and Eystein to

>see

>>>>the best way to proceed? Thanks,

>>>>

>>>>Ricardo

>>>>

>>>>

>>>>

>>>>----- Original Message -----

>>>>From: "Keith Briffa" <k.briffa@uea.ac.uk>

>>>>To: <jto@u.arizona.edu>; "Eystein Jansen" <Eystein.Jansen@geo.uib.no>

>>>>Cc: <ricardo@lab.cricyt.edu.ar>; "Ed Cook" <drdendro@ldgo.columbia.edu>

>>>>Sent: Friday, July 15, 2005 11:01 AM

>>>>Subject: the regional section and MWP Figure

>>>>

>>>>> Guys

>>>>> still need the SH temp bit from Ricardo/ED to edit and am exploring

>the

>>>>>MWP

>>>>> Figure - but the concept still is unclear to me - but we agreed to

do

>a

>>>>> plot like Tom's . The regional section is still a worry - I am

happy

>to

>>>>> very briefly edit the section on NAO (possibly incorporate the ENSO

>>>>>stuff )

>>>>> but my understanding is that this section is best done (to

incorporate

>>>>>also

>>>>> the regional moisture work of Ed ) by Ricardo /Ed with input my

Peck.

>This

>>>> is still my opinion. I also would appreciate feedback re the regional

>>>> forcing section that I think we may have to drop - but perhaps not.

>>>> Therefore I ask that when i get the SH temp stuff I will incorporate >it

>>>>but

>>>> that you guys (Peck, Ricardo, Ed and Eystein interacting over the >North

>>>> Atlantic bit) first review and redo the regional section .

>>>> It is important to get feedback from Henry re the borehole stuff and

>>>> involve Tom in the debate with all of us , of the value of the

Figure

>. In

>>>> meantime , will experiment with the Figure and review existing text >and

>>>>bullets

>>>> Keith

>>>>

>>>> Keith

>>>>

>>>>

>>>>

>>>> --

>>>> Professor Keith Briffa,

>>>> Climatic Research Unit

>>>> University of East Anglia

>>>> Norwich, NR4 7TJ, U.K.

>>>>

>>>> Phone: +44-1603-593909

>>>> Fax: +44-1603-507784

>>>>

>>>> [1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

>>>>

>>>>

>>>>

>>>>Attachment converted: Macintosh HD:Southern

>>>>hemisphere2.doc (WDBN/«IC») (0008A6E0)

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>>>Jonathan T. Overpeck

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>>>Professor, Department of Geosciences  
>>>Professor, Department of Atmospheric Sciences  
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>>>[3]<http://www.ispe.arizona.edu/>  
>>>

>>=====

>>Dr. Edward R. Cook  
>>Doherty Senior Scholar and  
>>Director, Tree-Ring Laboratory  
>>Lamont-Doherty Earth Observatory  
>>Palisades, New York 10964 USA  
>>Email: [drdendro@ldeo.columbia.edu](mailto:drdendro@ldeo.columbia.edu)  
>>Phone: 845-365-8618  
>>Fax: 845-365-8152

>>=====

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>  
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>Attachment converted: Macintosh HD:SHregteml 1.JPG (JPEG/«IC») (0008ADC3)

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## References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.geo.arizona.edu/>
3. <http://www.ispe.arizona.edu/>
4. <http://www.geo.arizona.edu/>
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6. <http://www.geo.arizona.edu/>
7. <http://www.ispe.arizona.edu/>
8. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: Tim Osborn <t.osborn@uea.ac.uk>  
Subject: Fwd: Re: CLA feedback on Tom and the MWP  
Date: Thu Jul 21 08:53:21 2005

Date: Wed, 20 Jul 2005 09:53:34 -0400  
From: Tom Crowley <tcrowley@duke.edu>  
User-Agent: Mozilla/5.0 (Macintosh; U; PPC Mac OS X Mach-O; en-US; rv:1.4)  
Gecko/20030624 Netscape/7.1  
X-Accept-Language: en-us, en  
To: Keith Briffa <k.briffa@uea.ac.uk>  
CC: Jonathan Overpeck <jto@u.arizona.edu>,  
Eystein Jansen <eystein.jansen@geo.uib.no>, t.osborn@uea.ac.uk  
Subject: Re: CLA feedback on Tom and the MWP  
X-UEA-Spam-Score: 0.0  
X-UEA-Spam-Level: /  
X-UEA-Spam-Flag: NO

Keith, if you can find more I see no problem - it seems that a lot of the data you used was via Cook and colleagues - I was unable to locate a full length record from Quebec in that time series, but maybe you are relying on something else - if so can I have it!?

other suggestions: provide a more general label to sites - eg, mangazeyek (sp)/yamal could be listed as polar urals - taimyr central Siberia.

China should be relabeled as east Asia as it does include some information from Japan and the Tibetan Plateau (L. Thompson) and we don't want to get into some political to-do by calling Tibet "Chinese".

that's all I can think of for present, good sailing, tom

Keith Briffa wrote:

Hi all

think this is resolved now (virtually) -

We use series that total to Tom/Gabi composite, and we can cite this as an example of the scatter of regional records "in a typical reconstruction". This avoids very difficult issue of what is the best way to aggregate certain data sets - we are simply illustrating the point with one published (by then) data set.

The issue of the composite is then not an issue either, because it is not a new (unpublished) composite that we were concerned about - though I still believe it is a distraction to put the composite in. It would be best to use data from 800 or 850 at least, and go to 1500 (?) and presumably normalise over the whole period of data shown. OK? Even though you guys all wish to go with the reduced period (ie not up the present), but my own instinct is that this might later come back to haunt us - but will take your lead.

I agree the look of the Figure should match the others.

So, if Tom will send the data sets (his regional curves) , Tim will plot and send back asap for scrutiny. Thanks Tom and thanks for your help with this - further comments on latest version of 6.5 (last 2000 years) still welcome , though will be incorporating a few changes in response to David and Fortunat input , and SH bit (from Ricardo and Ed) still to go in and regional section to be revised (after input from Peck et al.)

cheers

Keith

.  
At 21:42 19/07/2005, Jonathan Overpeck wrote:

Hi Keith and Tim: Just got off the phone with Eystein, and hopefully he will sleep ok knowing that we have a plan for the MWP fig and Tom...

Please ask questions if we don't cover all the key points, but here's what we think:

1) the MWP fig should span the MWP only, and should emphasize variation in regional amplitude (we agree that we must be clear that this fig is not a reconstruction) - that is, it is best to use time series representing regions, assuming that the regional series do represent a region ok with one or more input series. We want to avoid a regional bias if we can - this is what got us into all the MWP misunderstanding in the first place, perhaps (e.g., nice MWP in Europe/Atlantic region - must be global)

2) If you guys could agree on the series and the interval, that'd be great. We agree it would be good to start before 1000 and end before the Renaissance (15th century?). If you want more feedback on these issues, we're happy to provide, but it seems logical that you pick series and intervals so that each series covers the entire interval selected.

3) Don't use the Chesapeake record - it is likely biased by salinity

4) We'd like Keith and Tim to draft the final figure so that it matches the look and style of the other two figs they have made. Hope this is doable. Tom, does Keith have all the data? Thanks for sending if not.

5) We agree that Tom should NOT be a CA given that he was officially one of the ZOD reviewers. Of course, this doesn't represent a real conflict, but we need to avoid even the appearance of conflict. We greatly appreciate all the feedback that Tom is providing! Is this plan ok w/ you Tom? We think you're cool with it, but just want to check one more time.

That... it is. Please let us know if there are any more questions. Keith - feel free to try and get Eystein on his cell doing your work hours if you want quick feedback. Or we can do this by email - he's not in a very email friendly place right now, but the fishing appears to be ok.

Again, thanks to you both for all the discussion and thought that has gone into this figure.

Best, peck

--

Jonathan T. Overpeck

Director, Institute for the Study of Planet Earth

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3. <http://www.cru.uea.ac.uk/cru/people/briffa/>
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From: Tom Crowley <tcrowley@duke.edu>  
To: Tim Osborn <t.osborn@uea.ac.uk>  
Subject: Re: MWP figure  
Date: Thu, 21 Jul 2005 12:54:14 -0400  
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Tim, we are getting close but there are a few items to discuss:

- 1) seven of the eight time series are from the Hegerl et al paper, now out for review in Nature
- 2) the eighth time series is from Brian Luckmans recent extension of the Alberta record to the 10th century - we used his original time series in the H et al paper because the comparisons between model and observations had been going on for a while, in fact before the new Luckman paper came out, and we did not want to switch horses in midstream by changing the composite - as you know the Luckman paper is either accepted or published in CD, so there is no problem changing that
- 3) although technically the time series are not the same they are very close, if you want me to do some comparisons I can, but I could not get to it until probably tuesday of next week - I don't particularly see any problem in making such an addition
- 4) we cannot extend the time series back to 800 without dropping out something - the reason we start at 945 is that is the first year when all the records are available - if we go back to 800 we do so at the cost of dropping 2 or possibly even 3 records. as our Dark Ages reconstruction starting at 560 indicates (att.), the biggest warming between 800-1900 is in the late 10th century (960-995), we did not think we missing out on anything by starting at 945 rather than 800.

I await your feedback on this increasingly intricate issue, tom

Tim Osborn wrote:

> Hi Tom,  
>  
> In Keith's email below, when he says "we use series that total to  
> Tom/Gabi composite", he doesn't mean that \*our\* mock up of the figure  
> uses these series, but that if the series shown in \*your\* draft figure  
> are the same as those used in the Hegerl/Crowley recon that is  
> currently submitted ("...a twice validated climate record...") then we  
> will go with \*your\* figure. It is fine then to include the "composite  
> series" and the instrumental data and a temperature scale. Our  
> previous concerns about these latter points were that it might be seen  
> as another new NH temperature reconstruction. But if in fact the  
> composite and its expression as a temperature are not a new NH T  
> recon, but are in fact identical to the published (submitted, at  
> least) Hegerl/Crowley NH T recon (which is already included in the  
> main intercomparison figure) then there's no problem.  
>  
> Does your figure equate to the new Hegerl/Crowley NH T recon? If so,

> we should go with your MWP figure, though the CLAs want me to draw it  
> in the same style as the others and also cut the time period down to a  
> few centuries spanning the MWP. Keith suggests beginning in 800 or 850.

>  
> Would it be possible therefore to send the data series you used for  
> your figure, but beginning in 800/850, so I can plot the figure in the  
> required form?

>  
> Cheers

>  
> Tim

>  
> At 14:53 20/07/2005, Tom Crowley wrote:

>  
>> Keith, if you can find more I see no problem - it seems that a lot of  
>> the data you used was via Cook and colleagues - I was unable to  
>> locate a full length record from Quebec in that time series, but  
>> maybe you are relying on something else - if so can I have it!?

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>> other suggestions: provide a more general label to sites - eg,  
>> mangazeyek (sp)/yamal could be listed as polar urals - taimyr  
>> central Siberia.

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>> China should be relabeled as east Asia as it does include some  
>> information from Japan and the Tibetan Plateau (L. Thompson) and we  
>> don't want to get into some political to-do by calling Tibet "Chinese".

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>> that's all I can think of for present, good sailing, tom

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>> Keith Briffa wrote:

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>>> changes in response to David and Fortunat input , and SH bit (from  
>>> Ricardo and Ed) still to go in and regional section to be revised  
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>>> Keith  
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>>>> Hi Keith and Tim: Just got off the phone with Eystein, and  
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>>>> Please ask questions if we don't cover all the key points, but  
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>>>> That... it is. Please let us know if there are any more questions.  
>>>> Keith - feel free to try and get Eystein on his cell doing your  
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>>>> Again, thanks to you both for all the discussion and thought that  
>>>> has gone into this figure.  
>>>>  
>>>> Best, peck  
>>>> --  
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> web: <http://www.cru.uea.ac.uk/~timo/>  
> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>  
>

</x-flowed>

Attachment Converted: "c:\eudora\attach\CH.DA.jpg"



From: Keith Briffa <k.briffa@uea.ac.uk>  
To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>  
Subject: Re: the regional section and MWP Figure  
Date: Thu Jul 21 15:43:01 2005  
Cc: "Jonathan Overpeck" <jto@u.arizona.edu>, <drdendro@ldgo.columbia.edu>, <eystein.jansen@geo.uib.no>, "Tim Osborn" <t.osborn@uea.ac.uk>

Ricardo

Tim is contacting Henry now - so forget Boreholes for time being  
cheers

Keith

At 15:23 21/07/2005, Ricardo Villalba wrote:

Hi Keith and all,

Following Peck's advise I will include for each reconstruction the observed record. Obviously, I have the Patagonian instrumental records, but I need from Ed the Tasmania and Hokitika (New Zealand) observed records. Sorry for my ignorance on borehole, but I am not familiar with Henry's work. If you send me his e-mail, I could ask him for the data and a line of text on borehole from the SH. Cheers,

Ricardo

----- Original Message -----

From: "Keith Briffa" <k.briffa@uea.ac.uk>  
To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>; "Jonathan Overpeck" <jto@u.arizona.edu>  
Cc: <drdendro@ldgo.columbia.edu>; <eystein.jansen@geo.uib.no>  
Sent: Thursday, July 21, 2005 4:51 AM  
Subject: Re: the regional section and MWP Figure

Hi Ricardo and all

this all seems fine with me - the question of the temperature observations is a moot one - but some included seems a good idea - 1 South American and 1 New Zealand is fine - length not as important as proximity to the records shown (but need to see what they look like).

will wait on other numbers - Henry is best qualified to cite most appropriate SH borehole data and could supply a line of text . Ricardo can you ask him for these?

best wishes

Keith

At 13:52 20/07/2005, Ricardo Villalba wrote:

>Hi Keith, Ed, Peck, Eystein

>

>

>

>Regarding Peck's suggestions,

>

>

>1) should we include instrumental data? If not, it could lessen the impact.

>

>

>

>Rio de Janeiro, starting in 1851, is the longest, homogeneous temperature record from the Southern Hemisphere. In New Zealand and Australia, temperature records start at the same time. We do not have any long record for the 18th century, even the first half of the 19th century. The hemispheric record from the Southern Hemisphere will be discussed in Chapter

>2 and we do not have any additional information to provide.

>

>

>2) we need to include the two borehole (see previous email from me and Ed)

>

>

>  
>Definitely!! I do not have the records here in Mendoza. Keith, do you have  
>access to these data? As soon as I receive the borehole records I will  
>incorporate them in the figures. I would appreciate receiving the key  
>references to properly cite the records.

>  
>  
>3) we would like to ask Keith and Tim (pretty  
>please...) to draft the final figure so that it  
>matches the other in the section and MWP box. Is  
>this ok, and do you have the data to do the job.  
>If not, we trust your kind colleagues can send  
>upon request?

>  
>  
>At the time the figure is ready, I will send all the data to Keith and Tim  
>to draft the final figure, and the final text to incorporate in the FOD.  
>Cheers,

>  
>Ricardo

>  
>  
>  
>  
>----- Original Message -----

>From: "Jonathan Overpeck" <jto@u.arizona.edu>  
>To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>  
>Cc: "Keith Briffa" <k.briffa@uea.ac.uk>; <drdendro@ldgo.columbia.edu>;  
>"Eystein Jansen" <eystein.jansen@geo.uib.no>  
>Sent: Tuesday, July 19, 2005 5:55 PM  
>Subject: Re: the regional section and MWP Figure

>  
>  
>Hi SH gang - Thanks for keeping things moving  
>Ricardo. Eystein and I just discussed this fig on  
>the phone and would like to suggest the following:

>  
>1) should we include instrumental data? If not, it could lessen the impact.  
>2) we need to include the two borehole (see previous email from me and Ed)  
>3) we would like to ask Keith and Tim (pretty  
>please...) to draft the final figure so that it  
>matches the other in the section and MWP box. Is  
>this ok, and do you have the data to do the job.  
>If not, we trust your kind colleagues can send  
>upon request?

>  
>Many thanks, Peck and Eystein

>  
>>Dear Keith and Ed,

>>  
>>Please, find attached the new version of the SH figure for the IPCC. I  
>have  
>>now included the New Zealand record. All the records have been scaled to  
>4  
>>°C amplitude. Variability in the Tas record is reduced compared to New  
>>Zealand and Patagonian records. The reference lines is the mean used for  
>the  
>>calibration period in each record, 15 C for New Zealand, 14.95 C for  
>>Tasmania and 0 C for the Patagonian records (they show departures).  
Please,  
>>let me know if you want to introduce some changes in the figure. The  
>>opposite phase in the Patagonia-New Zealand records is so clear before

>1850,  
>>which is consistent with our previous TPI. For instance, in the  
>instrumental  
>>record the 1971 and 1976 are the coolest summer in northern Patagonian  
>>during the past 70 years, but the warmest in New Zealand reconstruction!!  
>>This out of phase relationship between regions in the Southern Hemisphere  
>>points out to the difficulty of using few records to get a hemispheric  
>>average. Cheers,

>>  
>>Ricardo  
>>  
>>----- Original Message -----  
>>From: "Jonathan Overpeck" <jto@u.arizona.edu>  
>>To: "edwardcook" <drdendro@ldeo.columbia.edu>  
>>Cc: "Keith Briffa" <>; "Ricardo Villalba" <ricardo@lab.criicyt.edu.ar>;  
>>"Eystein Jansen" <>  
>>Sent: Monday, July 18, 2005 1:09 PM  
>>Subject: Re: the regional section and MWP Figure

>>  
>>  
>>Thanks Ed - Ricardo, can you get the data from Henry? What do you think,  
>>Keith?  
>>Best, Peck

>>  
>>>Given the nature of the SH and what Ricardo put  
>>>together, I would keep the Australian and South  
>>>African borehole records separate. Henry  
>>>Pollack can provide them, I am sure. He gave an  
>>>excellent talk at a meeting in Canberra that I  
>>>recently participated in.

>>>  
>>>Cheers,  
>>>  
>>>Ed  
>>>  
>>>P.S. Ricardo, here is the Oroko temperature reconstruction.

>>>  
>>>JANUARY-MARCH TEMPERATURES RECONSTRUCTED FROM  
>>>OROKO SWAMP, NEW ZEALAND SILVER PINE TREE RINGS  
>>>BE ADVISED THAT THE DATA AFTER 1958 ARE INSTRUMENTAL  
>>>TEMPERATURES

YEAR	TEMP °C
900	13.751
901	14.461
902	13.236
903	13.331
904	13.483
905	13.632
906	12.959
907	13.628
908	13.372
909	12.868
910	13.244
911	13.793
912	14.048
913	14.444
914	13.095
915	14.036
916	13.215
917	13.698
918	13.570
919	13.665

>>> 920 13.871  
>>> 921 13.966  
>>> 922 14.762  
>>> 923 14.325  
>>> 924 14.077  
>>> 925 14.713  
>>> 926 14.081  
>>> 927 14.218  
>>> 928 13.793  
>>> 929 14.151  
>>> 930 14.985  
>>> 931 13.599  
>>> 932 14.663  
>>> 933 14.110  
>>> 934 14.968  
>>> 935 14.391  
>>> 936 15.484  
>>> 937 15.554  
>>> 938 14.977  
>>> 939 15.303  
>>> 940 15.179  
>>> 941 15.591  
>>> 942 14.737  
>>> 943 14.007  
>>> 944 14.865  
>>> 945 14.449  
>>> 946 14.350  
>>> 947 15.096  
>>> 948 15.257  
>>> 949 15.789  
>>> 950 15.303  
>>> 951 15.513  
>>> 952 16.111  
>>> 953 15.723  
>>> 954 15.459  
>>> 955 14.015  
>>> 956 13.083  
>>> 957 13.850  
>>> 958 14.069  
>>> 959 13.772  
>>> 960 14.873  
>>> 961 14.692  
>>> 962 14.923  
>>> 963 14.527  
>>> 964 15.034  
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>>> 1870 15.022  
>>> 1871 15.468  
>>> 1872 14.993  
>>> 1873 14.890  
>>> 1874 14.638  
>>> 1875 14.898  
>>> 1876 14.993  
>>> 1877 14.366  
>>> 1878 14.333  
>>> 1879 13.454  
>>> 1880 15.369  
>>> 1881 15.109  
>>> 1882 15.187  
>>> 1883 15.278  
>>> 1884 14.308  
>>> 1885 15.026  
>>> 1886 15.385  
>>> 1887 15.183  
>>> 1888 14.127  
>>> 1889 14.985  
>>> 1890 15.480  
>>> 1891 14.717  
>>> 1892 15.773  
>>> 1893 14.807  
>>> 1894 15.451  
>>> 1895 15.179  
>>> 1896 13.780  
>>> 1897 14.531  
>>> 1898 13.912  
>>> 1899 14.354  
>>> 1900 15.290  
>>> 1901 15.752  
>>> 1902 14.886  
>>> 1903 15.216  
>>> 1904 15.938  
>>> 1905 15.208  
>>> 1906 14.279  
>>> 1907 14.923  
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>>> 1909 15.501  
>>> 1910 15.633  
>>> 1911 15.212

>>> 1912 13.648  
>>> 1913 14.329  
>>> 1914 15.389  
>>> 1915 14.704  
>>> 1916 15.983  
>>> 1917 14.779  
>>> 1918 14.432  
>>> 1919 14.024  
>>> 1920 14.040  
>>> 1921 14.622  
>>> 1922 15.315  
>>> 1923 14.560  
>>> 1924 15.835  
>>> 1925 14.927  
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>>> 1927 15.220  
>>> 1928 16.433  
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>>> 1939 13.842  
>>> 1940 13.879  
>>> 1941 14.725  
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>>> 1944 15.133  
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>>> 1946 14.048  
>>> 1947 14.098  
>>> 1948 14.923  
>>> 1949 14.733  
>>> 1950 14.581  
>>> 1951 15.121  
>>> 1952 14.073  
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>>> 1960 14.970  
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>>> 1962 15.289  
>>> 1963 14.991  
>>> 1964 14.395  
>>> 1965 14.991  
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>>> 1967 14.948  
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>>> 1970 15.779  
>>> 1971 16.354  
>>> 1972 15.247  
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>>> 1974 15.353  
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>>> 1982 14.991  
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>>> 1984 15.353  
>>> 1985 15.225  
>>> 1986 15.587  
>>> 1987 15.140  
>>> 1988 14.863  
>>> 1989 16.098  
>>> 1990 15.417  
>>> 1991 14.991  
>>> 1992 14.096  
>>> 1993 14.160  
>>> 1994 15.183  
>>> 1995 15.119  
>>> 1996 15.630  
>>> 1997 14.927  
>>> 1998 15.417  
>>> 1999 16.354

>>>

>>>On Jul 17, 2005, at 10:40 PM, Jonathan Overpeck wrote:

>>>

>>>>Thanks Ricardo and Ed! I personally am not a  
>>>>big fan of the Jones and Mann SH recon. It is  
>>>>based on so little. On the other hand, it is in  
>>>>the literature. So, I leave it up to you and  
>>>>Keith to decide - perhaps Eystein can weigh in  
>>>>too.

>>>>

>>>>I do, however, think it would be really helpful  
>>>>to included the borehole data (see prev.  
>>>>emails) - either as a single SH curve, or  
>>>>(probably better) two regional curves  
>>>>(Australia and S. Africa). Is there a reason  
>>>>this is not a good idea? Can't complain about  
>>>>snow bias down there...

>>>>

>>>>Thanks again - I look forward to seeing the  
>>>>next draft and figure - complete w/ borehole I  
>>>>hope.

>>>>

>>>>thx, Peck

>>>>

>>>>>Hi Keith,

>>>>>

>>>>>Please, find attached my last version of the SH temp. As you know, Ed  
>>>>>Cook

>>>>>returned my original version of the SH with minor comments. Overall,  
he

>>>>>agreed with the text. Still I am waiting from him the Oroco Swamp data  
>to

>>>>>include in the Figure, which first draft I sent you more than a month  
>>>>>ago.

>>>>>

>>>>>In the last version I have included a first paragraph referring to the

>>Jones  
>>>>and Mann (2003) temperature reconstruction for the SH. At that time  
we  
>>have  
>>>>to decide if we want to have the hemispheric (Jones and Mann) and the  
>>>>regional views (Tasmania, New Zealand, Patagonia, maybe include  
>>Antarctica  
>>>>(Ommem et al. 2005)), or just one of them. If we decide to stay with  
the  
>>>>hemispheric view, we should include Jones and Mann reconstruction at  
the  
>>>>bottom of one of your figures. In cases that we decide to maintain  
both  
>>>>hemispheric and regional views, we should include Jones and Mann at  
the  
>>>>bottom of my figure. Please, could you check with Peck and Eystein to  
>>see  
>>>>the best way to proceed? Thanks,  
>>>>  
>>>>Ricardo  
>>>>  
>>>>  
>>>>  
>>>>----- Original Message -----  
>>>>From: "Keith Briffa" <k.briffa@uea.ac.uk>  
>>>>To: <jto@u.arizona.edu>; "Eystein Jansen" <Eystein.Jansen@geo.uib.no>  
>>>>Cc: <ricardo@lab.cricyt.edu.ar>; "Ed Cook"  
<drdendro@ldgo.columbia.edu>  
>>>>Sent: Friday, July 15, 2005 11:01 AM  
>>>>Subject: the regional section and MWP Figure  
>>>>  
>>>>> Guys  
>>>>> still need the SH temp bit from Ricardo/ED to edit and am  
exploring  
>>the  
>>>>>MWP  
>>>>> Figure - but the concept still is unclear to me - but we agreed to  
>do  
>>a  
>>>>> plot like Tom's . The regional section is still a worry - I am  
>happy  
>>to  
>>>>> very briefly edit the section on NAO (possibly incorporate the  
ENSO  
>>>>>stuff )  
>>>>> but my understanding is that this section is best done (to  
>incorporate  
>>>>>also  
>>>>> the regional moisture work of Ed ) by Ricardo /Ed with input my  
>Peck.  
>>This  
>>>>> is still my opinion. I also would appreciate feedback re the  
>regional  
>>>>> forcing section that I think we may have to drop - but perhaps  
not.  
>>>>> Therefore I ask that when i get the SH temp stuff I will  
incorporate  
>>it  
>>>>>but  
>>>>> that you guys (Peck, Ricardo, Ed and Eystein interacting over the  
>>North  
>>>>> Atlantic bit) first review and redo the regional section .

> >>>>> It is important to get feedback from Henry re the borehole stuff  
> and  
> >>>>> involve Tom in the debate with all of us , of the value of the  
> >Figure  
> >. In  
> >>>>> meantime , will experiment with the Figure and review existing  
> text  
> >and  
> >>>>>bullets  
> >>>>> Keith  
> >>>>>  
> >>>>> Keith  
> >>>>>  
> >>>>>  
> >>>>>  
> >>>>> --  
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> >>>>>  
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> >>>>>hemisphere2.doc (WDBN/«IC») (0008A6E0)  
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> >>>>>--  
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> >>>>>Fax: 845-365-8152  
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>>[5]<http://www.ispe.arizona.edu/>  
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## References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.geo.arizona.edu/>
3. <http://www.ispe.arizona.edu/>
4. <http://www.geo.arizona.edu/>
5. <http://www.ispe.arizona.edu/>
6. <http://www.geo.arizona.edu/>

7. <http://www.ispe.arizona.edu/>
8. <http://www.cru.uea.ac.uk/cru/people/briffa/>
9. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Tim Osborn <t.osborn@uea.ac.uk>  
To: Tom Crowley <tcrowley@duke.edu>, Keith Briffa <k.briffa@uea.ac.uk>  
Subject: MWP figure  
Date: Thu, 21 Jul 2005 16:07:58 +0100  
Cc: Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>

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Hi Tom,

In Keith's email below, when he says "we use series that total to Tom/Gabi composite", he doesn't mean that *\*our\** mock up of the figure uses these series, but that if the series shown in *\*your\** draft figure are the same as those used in the Hegerl/Crowley recon that is currently submitted ("...a twice validated climate record...") then we will go with *\*your\** figure. It is fine then to include the "composite series" and the instrumental data and a temperature scale. Our previous concerns about these latter points were that it might be seen as another new NH temperature reconstruction. But if in fact the composite and its expression as a temperature are not a new NH T recon, but are in fact identical to the published (submitted, at least) Hegerl/Crowley NH T recon (which is already included in the main intercomparison figure) then there's no problem.

Does your figure equate to the new Hegerl/Crowley NH T recon? If so, we should go with your MWP figure, though the CLAs want me to draw it in the same style as the others and also cut the time period down to a few centuries spanning the MWP. Keith suggests beginning in 800 or 850.

Would it be possible therefore to send the data series you used for your figure, but beginning in 800/850, so I can plot the figure in the required form?

Cheers

Tim

At 14:53 20/07/2005, Tom Crowley wrote:

>Keith, if you can find more I see no problem - it seems that a lot of the  
>data you used was via Cook and colleagues - I was unable to locate a full  
>length record from Quebec in that time series, but maybe you are relying  
>on something else - if so can I have it!?

>

>other suggestions: provide a more general label to sites - eg, mangazeyek

>(sp)/yamal could be listed as polar urals - taimyr central Siberia.

>

>China should be relabeled as east Asia as it does include some information  
>from Japan and the Tibetan Plateau (L. Thompson) and we don't want to get  
>into some political to-do by calling Tibet "Chinese".

>

>that's all I can think of for present, good sailing, tom

>

>Keith Briffa wrote:

>

>>Hi all

>>think this is resolved now (virtually) -

>>

>>We use series that total to Tom/Gabi composite , and we can cite this as  
>>an example of the scatter of regional records "in a typical  
>>reconstruction". This avoids very difficult issue of what is the best way  
>>to aggregate certain data sets - we are simply illustrating the point  
>>with one published (by then) data set.

>>The issue of the composite is then not an issue either , because it is  
>>not a new (unpublished) composite that we were concerned about - though I  
>>still believe it is a distraction to put the composite in. It would be  
>>best to use data from 800 or 850 at least , and go to 1500 (?) and  
>>presumably normalise over the whole period of data shown. OK? Even though  
>>you guys all wish to go with the reduced period (ie not up the present) ,  
>>but my own instinct is that this might later come back to haunt us - but  
>>will take your lead.

>>I agree the look of the Figure should match the others.

>>So, if Tom will send the data sets (his regional curves) , Tim will plot  
>>and send back asap for scrutiny. Thanks Tom and thanks for your help  
>>with this - further comments on latest version of 6.5 (last 2000 years)  
>>still welcome , though will be incorporating a few changes in response to  
>>David and Fortunat input , and SH bit (from Ricardo and Ed) still to go  
>>in and regional section to be revised (after input from Peck et al.)

>>cheers

>>Keith

>>.

>>

>>

>> At 21:42 19/07/2005, Jonathan Overpeck wrote:

>>

>>>Hi Keith and Tim: Just got off the phone with Eystein, and hopefully he  
>>>will sleep ok knowing that we have a plan for the MWP fig and Tom...

>>>

>>>Please ask questions if we don't cover all the key points, but here's  
>>>what we think:  
>>>  
>>>1) the MWP fig should span the MWP only, and should emphasize variation  
>>>in regional amplitude (we agree that we must be clear that this fig is  
>>>not a reconstruction) - that is, it is best to use time series  
>>>representing regions, assuming that the regional series do represent a  
>>>region ok with one or more input series. We want to avoid a regional  
>>>bias if we can - this is what got us into all the MWP misunderstanding  
>>>in the first place, perhaps (e.g., nice MWP in Europe/Atlantic region -  
>>>must be global)  
>>>  
>>>2) If you guys could agree on the series and the interval, that'd be  
>>>great. We agree it would be good to start before 1000 and end before the  
>>>Renaissance (15th century?). If you want more feedback on these issues,  
>>>we're happy to provide, but it seems logical that you pick series and  
>>>intervals so that each series covers the entire interval selected.  
>>>  
>>>3) Don't use the Chesapeake record - it is likely biased by salinity  
>>>  
>>>4) We'd like Keith and Tim to draft the final figure so that it matches  
>>>the look and style of the other two figs they have made. Hope this is  
>>>doable. Tom, does Keith have all the data? Thanks for sending if not.  
>>>  
>>>5) We agree that Tom should NOT be a CA given that he was officially one  
>>>of the ZOD reviewers. Of course, this doesn't represent a real conflict,  
>>>but we need to avoid even the appearance of conflict. We greatly  
>>>appreciate all the feedback that Tom is providing! Is this plan ok w/  
>>>you Tom? We think you're cool with it, but just want to check one more time.  
>>>  
>>>That... it is. Please let us know if there are any more questions. Keith  
>>>- feel free to try and get Eystein on his cell during your work hours if  
>>>you want quick feedback. Or we can do this by email - he's not in a very  
>>>email friendly place right now, but the fishing appears to be ok.  
>>>  
>>>Again, thanks to you both for all the discussion and thought that has  
>>>gone into this figure.  
>>>  
>>>Best, peck  
>>>--  
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From: David Rind <drind@giss.nasa.gov>  
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
Subject: Re: [wg1-ar4-ch06] Updated 6.1 (inc. Bette's comments)  
Date: Fri, 22 Jul 2005 13:17:42 -0400  
Cc: wg1-ar4-ch06@joss.ucar.edu

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Hi Eystein,

Thanks for your comments. With respect to the suggested changes in paragraphs 1,2 and 4, they seem fine to me. However, I think we need to include in paragraph 5 potential reasons as to why the substantial (and not just significant) high latitude warming that appears in the mid-Pliocene record is not produced in GCMs in response to higher CO<sub>2</sub>, in general - otherwise we leave the reader with a big question and no possible solution. The tendency of GCM simulations for the future climate to produce an NADW decrease forces those simulations to have minimal high latitude warming in the North Atlantic, exactly opposite the inference from the Pliocene paleo-record (which is quite robust in this respect at least). If the Pliocene record is indicating the opposite of what current models are predicting, it may be offering us a valuable clue...

The suggested reasons also include the comment that the lack of land ice at high northern latitudes might be a strong contributing cause - which would make it a no-analog situation, and hence not fully a GCM problem.

I would favor leaving those two sentences as they were.

David

At 5:19 PM +0200 7/22/05, Eystein Jansen wrote:  
>Hi,

>see enclosed some comments to the last version  
>of the deep time box. I propose some deletions  
>and some toning down of language. What do you  
>think?

>  
>Eystein  
>--

>\_\_\_\_\_

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>Professor/Director  
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>Allégaten 55  
>N-5007 Bergen  
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>  
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Wg1-ar4-ch06 mailing list  
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<http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>  
</x-flowed>

From: Eystein Jansen <eystein.jansen@geo.uib.no>  
To: David Rind <drind@giss.nasa.gov>  
Subject: Ad: Re: [wg1-ar4-ch06] Updated 6.1 (inc. Bette's comments)  
Date: Sat, 23 Jul 2005 09:40:27 +0200  
Reply-to: Eystein Jansen <eystein.jansen@geo.uib.no>  
Cc: wg1-ar4-ch06@joss.ucar.edu

Hi David,  
thanks for the reply. I think your arguments to add some comments of explanation re Pliocene warmth are convincing and that there is potential relevance for IPCC concerning lat. heat transport in a world with less land and sea ice. My concern is that I don't think the text should be interpreted to imply that the Mid Pliocene was free of Arctic sea ice and Greenland was ice free. There is evidence from the recent IODP Central Arctic Drilling (have to check what ref. to use) of sea ice cover through the Pliocene. I have published on IRD evidence for a Greenland ice sheet of some sort. Concerning THC, N Atlantic data indicate strong presence of NADW akin to now, but we cannot constrain overturning rate. Both Nordic Seas and Arctic Ocean was poorly ventilated and deep water formation to feed overflows was shallower, perhaps due to higher temperature?

Instead of deleting the section I proposed, I suggest changing it as follows: After (Rind and Chadler 1991) add , "for which available proxy data are inconclusive", and

Instead of writing "absence of land ice", write " reduced extent of land and sea ice". I will find the best refs for this on Monday.

Cheers  
Eystein

---

Wg1-ar4-ch06 mailing list  
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From: Tom Crowley <tcrowley@duke.edu>

To: Jonathan Overpeck <jto@u.arizona.edu>

Subject: participation in IPCC

Date: Mon, 25 Jul 2005 10:16:30 -0400

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi all, there is another reason why I should not be formally listed as an LA - it is my understanding that IPCC contributors have to be a little careful about getting involved in political matters that could be used to impugn the integrity of the process - well I am starting to do just that, with the attached commen in Eos, plus some radio interviews where I have been somewhat pointed in my thoughts.

I suppose its still ok to be a reviewer, but even then you might keep these comments in mind, tom

>

</x-flowed>

Attachment Converted: "c:\eudora\attach\Crowley1.EOS.2005.pdf"

From: Stefan Rahmstorf <rahmstorf@ozean-klima.de>

To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Last Millennium section 6.5 - comments by SR

Date: Tue, 26 Jul 2005 12:09:33 +0200

Cc: wg1-ar4-ch06@joss.ucar.edu, Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <Eystein.Jansen@geo.uib.no>

Hi Keith and all, (please everyone have a look at point (4))

I think section 6.5 is in remarkably good shape (certainly compared to my own..).

There are some comments from me:

(1) About the new proxy reconstructions, the section says: "Most of these are shown..." in the Figure. This immediately raises the question: why not all? Which one is not shown? This section will be scrutinised with great suspicion by some people, so we need to be careful.

Can you clarify which one you left out, and why? Or can we just write: "These are shown..." That would be much nicer.

(2) Several times you say "simply scaled" - would "scaled" do as well? The "simply" in this context sounds a bit like we criticise that.

(3) Is "predictand" a word that everybody knows? I'd never seen it before.

(4) Now here is my biggest question, that I think we need to discuss in the whole group. Figure 6.5.2-1 shows simulations of the past millennium, relative to 1500-1899 means. Is this really the best reference period?

Contra: it differs from how we show the data reconstructions, i.e., relative to 1961-1990. Everyone knows what that climate actually was, since there are good instrumental data for 1961-1990, so that it makes sense to look at changes relative to that period. Nobody knows what the real 1500-1899 mean was, so this is a fictitious baseline.

Pro: it gets rid of "end effects", i.e. model initialisation problems at the beginning (as in Von Storch 04), and different anthropogenic forcings used at the end (e.g. some ignore aerosols); the simulations look closer together in this way (right?)

I have not formed a clear opinion on what is best.

(5) Also on the figures: I like the grey bands, but here's a suggestion for improvement: instead of leaving the core region between those two bands white, I think they should also be shaded - either the same grey, or a darker shade of grey. This makes it more clear that we are talking about one, wide uncertainty band here, not about two separate things. It had me confused at first when I saw it, even though I was there when we discussed this in Beijing.

Final point: we need to keep an eye on developments concerning the model tests of the proxy method, there seem to be several important things in the pipeline there.

Cheers, Stefan

--

To reach me directly please use: [1]rahmstorf@ozean-klima.de  
(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)

Stefan Rahmstorf

[2]www.ozean-klima.de

[3]www.realclimate.org

References

1. <mailto:rahmstorf@ozean-klima.de>
2. <http://www.ozean-klima.de/>

3. <http://www.realclimate.org/>

From: Tim Osborn <T.Osborn@uea.ac.uk>  
To: eystein.jansen@geo.uib.no, jto@u.arizona.edu, k.briffa@uea.ac.uk  
Subject: MWP figure  
Date: Tue, 26 Jul 2005 20:00:29 +0100  
Cc: t.osborn@uea.ac.uk

Dear Keith, Peck and Eystein,

as you'll have seen from Tom C's replies to my fairly direct requests for the data that went into his MWP figure, he seems somehow reluctant to send it to me and prefers me to find it myself (including spending a week re-assembling a Mongolian composite). I have no time to do this, so have instead reverted to using the very similar data that we already had. I'm sure it's so similar that it tells the same story.

So, the attached file is my latest attempt at the MWP figure. It shows 8 local/regional proxy series, normalised over a common period after filtering to the 20-year and longer time scale. It also shows a composite mean, and no temperature scale. The period covered is 850 to 1350.

What do you think? Hopefully it is what you want.

I've started on the SH figure, having received data from Ricardo and borehole series for SH, S. Africa and Australia from Jason/Henry. I need to sort out Tasmania / New Zealand instrumental data - Ed has this, though I could extract appropriate boxes from the Jone et al. gridded data set if necessary.

I'll include these series:

S American trees\*2 plus instrumental T overlaid

S African and Australian boreholes (must also overlay instrumental T to explain why values are all negative - due to early sampling prior to strongest warmng)

Tasmanian and New Zealand trees\*2 plus instrumental T)

It may be Friday by the time I get this one done.

Cheers

Tim

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\ipccar4\_mwpbox.pdf"

From: Phil Jones <p.jones@uea.ac.uk>  
To: Kevin Trenberth <trenbert@ucar.edu>  
Subject: Re: New versions  
Date: Thu Jul 28 09:37:18 2005  
Cc: Susan Solomon <ssolomon@al.noaa.gov>

Kevin/Susan,

I'll look over 3.9. A quick look at the back references to sections which contain the detail summarized here, suggests that you've got the right level of section. I guess we could add a sentence to say that this/these are the principal section(s), but the whole of the x.x section is likely also relevant.

I've added Susan in to show what we're doing. It might be appropriate for other chapters. Part of my reason was traceability, but also we are referring to subsequent sections in Chapters 4 and 5.

The figures seem to be coming along well. Pdfs are also. I'll send another reminder about these out later today, when I've had one last look for a few of them. I'll attach section numbers as there are so few now.

Cheers

Phil

The bulleted points and back references are below.

- Global-mean surface temperatures show overall warming of 0.75°C over the 1901-2004 period although rates of temperature rise are much greater after 1979. Both land surface air temperatures and SST show warming although land regions have warmed at a faster rate than the oceans for both hemispheres in the past few decades, consistent with the much greater mass and thermal inertia of the oceans. Some areas have not warmed in recent decades, and a few have cooled although not significantly. [3.2.2]
- The warming of the climate is consistent with a widespread reduction in the number of frost days in mid-latitude regions. The latter is due to an earlier last day of frost in spring rather than a later start to the frost season in autumn. The increase in the number of daily warm extremes and reduction in cold extremes across over 70% of land regions studied have been most marked at night over the 1951-2003 period. The greater increase in nighttime as opposed to daytime temperatures has continued. [3.8.2.1]
- Widespread (but not ubiquitous) decreases in continental DTR since the 1950s occur with increases in cloud amounts, as expected from the impact of cloud cover on solar heating of the surface. [3.2.2; 3.4.3]
- The temperature increases are consistent with the observed nearly worldwide reduction in mountain glacier mass and extent. A few regions of the world where mountain glacier termini are determined by winter precipitation totals, as opposed to summer temperatures, do show some advances, but these are consistent with changes in circulation and associated increases in winter precipitation (e.g., southwestern Norway, parts of coastal Alaska, southern Chile and Fjordland of the South Island of New Zealand). Tropical ice caps in South America, Africa and Tibet have all shown remarkable declines in recent

decades. If continued, some may disappear within the next 30 years. Reduction in mass of such glaciers depends on local heat budgets, which is not necessarily reflected in local temperature changes. The temperature records all show a slight warming, but nowhere near the magnitude required to explain the rapid demise of the many of the ice caps. [4.5]

- Snow cover has decreased in many NH regions, particularly in the spring season, consistent with greater increases in spring as opposed to autumn temperatures in mid-latitude regions. The decrease is accompanied by increased active layer thickness above permafrost and decreased seasonally frozen ground depths. [3.3.2.3; 4.2.4, 4.8]

- Sea-ice extents have decreased in the Arctic, particularly in the spring and summer seasons, and patterns of the changes are consistent with regions showing a temperature increase, although changes in winds are also a major factor. Decreases are found in the length of the freeze season of river and lake ice. [3.2.2.3; 4.3, 4.4, 5.3.3]

- Surface temperature variability and trends since 1979 are consistent with those estimated by most analyses of satellite retrievals of lower-tropospheric temperatures, provided the latter are adequately adjusted for all issues of satellite drift, orbit decay, different satellites and stratospheric influence on the T2 records, and also with ERA-40 estimates of lower-tropospheric temperatures. The range from different datasets of global surface warming since 1979 is 0.15 to 0.18 compared to 0.12 to 0.19 K decade<sup>-1</sup> for MSU estimates of lower tropospheric temperatures. [3.4.1]

- Stratospheric temperature estimates from radiosondes, satellites (T4) and reanalyses are in qualitative agreement recording a cooling of between 0.3 and 0.8°C decade<sup>-1</sup> since 1979. Increasing evidence suggests increasing warming with altitude from 1979 to 2004 from the surface through much of the troposphere in the tropics, cooling in the stratosphere, and a higher tropopause, consistent with expectations from observed increased greenhouse gases and changes in stratospheric ozone. Over extratropical land, the larger warming at night is associated with larger surface temperature changes. [3.4.1]

- Radiation changes at the top-of the atmosphere from the 1980s to 1990s, possibly ENSO related in part, appear to be associated with reductions in tropical cloud cover, and are linked to changes in the energy budget at the surface and in observed ocean heat content in a consistent way. [3.4.3; 3.4.4]

- Surface specific humidity has also generally increased after 1976 in close association with higher temperatures over both land and ocean. Consistent with a warmer climate, total column water vapour has increased over the global oceans by  $1.2 \pm 0.3\%$  from 1988 to 2004, consistent in patterns and amount with changes in SST and a fairly constant relative humidity. Upper tropospheric water vapour has also increased in ways such that relative humidity remains about constant, providing a major positive feedback to radiative forcing. [3.4.2]

- Over land a strong negative correlation is observed between precipitation and surface temperature in summer and in low latitudes throughout the year, and areas that have become wetter, such as the eastern United States, have not warmed as much as other land areas. Increased precipitation is associated with increases in cloud and surface wetness, and thus increased evaporation. Although records are sparse, continental-scale estimates of pan evaporation show decreases, due to decreases in surface radiation associated with

increases in clouds, changes in cloud properties, and increases in air pollution in different regions from 1970 to 1990. There is tentative evidence to suggest that this has reversed in recent years. The inferred enhanced evaporation and reduced temperature increase is physically consistent with enhanced latent versus sensible heat fluxes from the surface in wetter conditions. [3.3.5; 3.4.4.2]

- Surface observations of cloud cover changes over land exhibit coherent variations on interannual to decadal time scales which are positively correlated with gauge-based precipitation measurements. [3.4.3]

- Consistent with rising amounts of water vapour in the atmosphere, increases in the numbers of heavy precipitation events (e.g., 90/95<sup>th</sup> percentile) have been reported from many land regions, even those where there has been a reduction in total precipitation. Increases have also been reported for rarer precipitation events (1 in 50 year return period), but only a few regions have sufficient data to assess such trends reliably. [3.4.2; 3.8.2.2]

- Patterns of precipitation change are much more spatially- and seasonally-variable than temperature change, but where significant changes do occur they are consistent with measured changes in streamflow. [3.3.4]

- Droughts have increased in various parts of the world. The regions where they have occurred seem to be determined largely by changes in SSTs, especially in the tropics, through changes in the atmospheric circulation and precipitation. Inferred enhanced evaporation and drying associated with warming and decreased precipitation are important factors in increases in drought. In the western United States, diminishing snow pack and subsequent summer soil moisture reductions have also been a factor. In Australia and Europe, direct links to warming have been inferred through the extreme nature of high temperatures and heat waves accompanying drought. [3.3.4, QACCS 3.3, 3.8.3, 4.x.x]

- Changes in the freshwater balance of the Atlantic Ocean over the past four decades have been pronounced as freshening has occurred in the North Atlantic and also south of 25°S, while salinity has increased in the tropics and subtropics, especially in the upper 500 m. The implication is that there have been increases in moisture transport by the atmosphere from the subtropics to higher latitudes, in association with changes in atmospheric circulation, including the NAO, thereby increasing precipitation over the northern ocean and in adjacent land areas (as observed). [3.3.2, 3.3.3, 5.3.2, 5.5.3]

- Changes in the large-scale atmospheric circulation are apparent. Increasing westerlies have been present in both hemispheres as enhanced annular modes. In the NH, the NAM and NAO change the flow from oceans to continents and are a major part of the wintertime observed change in storm tracks, precipitation and temperature patterns, especially over Europe and North Africa. In the SH, SAM changes, in association with the ozone hole, have been identified with recent contrasting trends of large warming in the Antarctic Peninsula, and cooling over interior Antarctica. [3.5, 3.6, 3.8.3]

- The 1976/1977 climate shift toward more El Niños has affected Pacific rim countries and monsoons throughout the tropics. Over North America, ENSO and PNA-related changes appear to have led to contrasting changes across the continent, as the west has warmed more than the east, while the latter has become cloudier and wetter. [3.6, 3.7]

- Variations in extratropical storminess are strongly associated mostly with changes in mean atmospheric circulation, such as changes and variations in ENSO, NAO, PDO, and SAM. Wind and significant wave height analysis support the reanalysis-based evidence for an increase in extratropical storm activity in the NH in recent decades. After the late 1990s, however, some of these variations seemed to change sign. [3.5, 3.6, 3.8.3.2]
- Changes are observed to occur in the number, distribution and tracks of tropical storms that are clearly related to ENSO phases and to a slightly lesser extent to the AMO and QBO modulations. Increases in intensity and lifetimes of tropical storms since the 1970s are consistent with increases in SSTs and atmospheric water vapour. [3.8.3.1]
- Sea level likely rose about  $18 \pm 3$  cm during the 20<sup>th</sup> century, but increased  $3.0 \pm 0.4$  mm/year after 1992, when confidence increases from global altimetry measurements. During this period, glacier melt has increased ocean mass by order 1.0 mm/year, increases in ocean heat content and associated ocean expansion are estimated to contribute 1.6 mm/year, while changes in land water storage are uncertain but may have taken water out of the ocean. Isostatic rebound contributes about 0.3 mm/year. This near balance gives increased confidence that the observed sea level rise is a strong indicator of warming, and an integrator of the cumulative energy imbalance at the top of atmosphere. [4.5, 4.7, 4.9.8, 5.2, 5.5]

At 23:47 27/07/2005, Kevin Trenberth wrote:

Phil

I placed new versions of the figure and text files on my ftp site. I implemented your suggestion of adding section numbers to the 3.9. I used the ones from the ZOD wrt other chapters. So they may change. I also added a small piece on freezing seasons on lakes and rivers that was mentioned in the last para but not in any bullets. You may like to comment on this as some are x.x, some are y.y.y and some are z.z.z.z.

In the first case the whole section is really applicable and so mentioning each subsection does not seem worthwhile. Should we go to the z.z.z.z level, as that is not in the TOC?

In doing this I found that two sections in 3.8 had very similar titles and so I changed that of 3.8.3 to explicitly say tropical and extratropical storms and extreme events, which are the 3 subsections. The Table of contents (TOC) is all up to date, and now corrected for one subsection that was mislabeled as level 2 instead of 3.

Several figures have been revised.

I am out tomorrow all day but Lisa tells me she is up to w in the references. So should have a complete new version on Friday. Hopefully several of the figures will be by upgraded then too. I have a new Fig 3.3.1 but can't work with it: something wrong with it, so I've asked Dave E for a different one. Main outstanding stuff is all waiting on Dave Easterling. I have requests in to Tom Karl on the 2 CCSP figures.

Following my earlier email I have responses on Figs 3.2.3: now good, 3.4.6 I did, 3.5.2, and one from Groisman. So only 7 figures not in final form.

I believe we have 74 figures in the sense that they are separate files.

That includes counts of 1 for several multipanel files (like some T ones or the



From: Fortunat Joos <joos@climate.unibe.ch>  
To: Fortunat Joos <joos@climate.unibe.ch>  
Subject: Re: [Wg1-ar4-ch06] introduction 6.2.1 - 6.4.1 holocene solar.  
Date: Thu, 28 Jul 2005 21:49:44 +0200  
Cc: Eystein Jansen <Eystein.Jansen@geo.uib.no>

Content-Type: text/plain; charset=iso-8859-1  
Content-Transfer-Encoding: 7bit  
X-Mime-Autoconverted: from 8bit to 7bit by courier 0.47

Hi Peck and Eystein,

Here a reduced version of Box 6.2, taking into account suggestions from David and Bette. The text is now 1.5 pages, i.e. just slightly above target. The entire Box should now fit on less than 1 IPCC page (Assigned 0.75 page).

I am willing to take the next effort to shorten when the review comments of the FOD are in.

With best regards,

Fortunat

Quoting Fortunat Joos <joos@climate.unibe.ch>:

> Hi Peck and Eystein,  
>  
> here my general comment on the introduction and specific comments on section  
> 6.2.1 and 6.4.1.  
>  
> 6.1 and 6.2.1:  
>  
> Well done!  
>  
> (1) Perhaps, words such as 'significant' and other value judgment terms could  
> be  
> used somewhat less. e.g. 'With proper care, current methodologies alloww more  
> accurate age models' more accurate than what? We always hope that things are  
> done with proper care.  
>  
> (2) The following sentence must in my opinion be deleted: 'but also note that

> new work reveals that  
> cosmogenic-isotope-derived estimates of solar forcing for the Holocene are  
> not  
> likely as well-constrained as commonly thought.'  
>  
> This is a very sweeping statement that is not backed up by the chapter text.  
> It  
> is also a very policy sensitive statement. We are either able to firmly  
> support  
> that or to drop it. I suspect that the paleo community would be divided about  
> this.  
>  
> Scott Lehmann has just shown me a plot with a really nice correlation between  
>  $\delta^{18}O$  in N-pachy in the North-Atlantic and sunspots over the past 400 years.  
> Yes, there appears to be a link.  
>  
> I also doubt that some of the existing work, e.g. Fleitman etc can be  
> dismissed  
> so easily.  
>  
> section 6.4.1:  
>  
> (3) I also think that the Holocene text on solar needs some readjustments.  
> Linking the studies suggesting solar changes and those with NADW variations  
> seems a somewhat improper comparison.  
>  
> The present text reads:  
>  
> 'Based on the correlation between changes in atmospheric concentrations of  
> cosmogenic isotopes ( $^{10}Be$  or  $^{14}C$ ) and climate proxy records, some authors  
> argue  
> that solar activity may be the driver for an organised centennial to  
> millennial  
> scale variability (e.g., (Bond et al., 2001; Fleitmann et al., 2003) (Karlen,  
> 1996) (Wang et al., 2005b), whereas others point to modes of variability  
> driven  
> by processes within the climate system, for instance related to the deep  
> ocean  
> circulation (Bianchi and McCave, 1999) (Duplessy et al., 2001) (Oppo et al.,  
> 2003) (Marchal et al., 2002).'

> I suggest to change it to something along the following line.  
>

> "Based on the correlation between changes in atmospheric concentrations of  
> cosmogenic isotopes (10Be or 14C) and climate proxy records, many studies  
> suggest that solar activity may be a driver for centennial to millennial  
> scale  
> variability (e.g., (Bond et al., 2001; Fleitmann et al., 2003) (Karlen, 1996)  
> (Wang et al., 2005b). The importance of (forced or unforced) modes of  
> variability within the climate system, for instance related to the deep ocean  
> circulation has been pointed out (Bianchi and McCave, 1999) (Duplessy et al.,  
> 2001) (Oppo et al., 2003) (Marchal et al., 2002)."

> With best regards,

> Fortunat

> Quoting Fortunat Joos <joos@climate.unibe.ch>:

>> Hi Stefan, Peck and all,

>> Here an update on the abrupt event figure and the figure caption. There  
> were

>> some lost lines in the one send yesterday - please delete. I have now also  
>> numbered some of the D/O events and the A1 to A4 events.

>> The purpose of the figure is to demonstrate the asynchronous evolution of  
> NH  
>> and SH temperatuere and the magnitude of the GHG changes during abrupt  
>> events.

>> Clearly, it would be great if the figure could be amended by other  
>> information,  
>> e.g. from the land or sediment records. We may also think about indicating  
>> the  
>> local Greenland temperatre change for the bigger events.

>> Any ideas, suggestions, comments are welcomed.

>> Peck: please include ERIC MONNIN as a Contributing author.

>> Eric has synchronized the Taylor Dome and Dome C data on the GRIP time  
> scale

>> and

>> helped me greatly to put together the records for the abrupt event and for  
>> the  
>> LGM-box figures.  
>>  
>> With best regards,  
>>  
>> Fortunat  
>>  
>> Quoting Fortunat Joos <joos@climate.unibe.ch>:  
>>  
>>> Hi,  
>>>  
>>> Here finally the abrupt event figure plus an update of the LGM-box  
> figure.  
>>> Will provide figure caption, section 6.6. text and shortened LGM-box  
>>> tomorrow.  
>>>  
>>> With best regards,  
>>>  
>>> Fortunat  
>>>  
>>> --  
>>> e-mail: joos@climate.unibe.ch;  
>>>  
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> Wg1-ar4-ch06 mailing list

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Attachment Converted: "c:\eudora\attach\joos\_Ch06\_FOD\_LGMBBox\_28jul05.doc"

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Wg1-ar4-ch06 mailing list

Wg1-ar4-ch06@joss.ucar.edu

<http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>

From: Phil Jones <p.jones@uea.ac.uk>  
To: Tim Osborn <t.osborn@uea.ac.uk>, "Tett, Simon" <simon.tett@metoffice.gov.uk>  
Subject: Re: Bristlecones!  
Date: Fri Jul 29 16:30:35 2005  
Cc: Keith Briffa <k.briffa@uea.ac.uk>

Simon,

If you go to this web page

[1]<http://www.ucar.edu/news/releases/2005/ammann.shtml>

You can click on a re-evaluation of MBH, which leads to a paper submitted to Climatic Change. This shows that MBH can be reproduced. The R-code to do this can be accessed and eventually the data - once the paper has been accepted.

IPCC will likely conclude that all MM arguments are wrong and have been answered in papers that have either come out or will soon. MBH is just one curve of many - more now than there were in 2001. MBH is still in the spaghetti of curves, and is not an outlier. If there are outliers it will be Esper et al. and another one.

Bristlecones are only crucial to the issue if you are MM. They misused them, by their PCA application. This is all well-known to those in the know.

I have reviewed the CC paper by Wahl and Ammann. It reproduces all the mistakes MM have made, so they know how and why their results have been achieved. I can send you the paper if you want, subject to the usual rules.

MBH have all responded to the same requests as IPCC got from the US Senate. Their responses are all posted at [2]<http://www.realclimate.org/>

The skeptics have shot themselves in the foot over this one.

Cheers

Phil

At 15:17 29/07/2005, Tim Osborn wrote:

At 14:27 28/07/2005, Tett, Simon wrote:

John Houghton is being quized by bits of the US senate. One question is "Whats the status of the review of the Mann hockey stick temperature curve? I understand that studies by Stephen McIntyre and Ross McKittrick suggest that it relied on the statistically insignificant bristlecone pine. Is the IPCC taking another look at that work, which forms the basis for much of todays climate change debate?"

My current thoughts on an answer is to say that other reconstructions show a similar pattern (though not magnitude). However how many of the other reconstructions use the bristlecone data? [I suspect yours does

not]

Hi Simon - I was away yesterday, so couldn't answer then. Hopefully it isn't too late to answer today.

(1) I don't understand what they mean by describing the bristlecone pine as "statistically insignificant".

(2) The Mann, Bradley and Hughes (MBH1999) reconstruction is only one small piece of information in today's climate change debate.

(3) As far as I understand, then yes the MBH1999 reconstruction does give quite a lot of weight to a few western US tree-ring series, which are mostly bristlecone pines for the longest records.

(4) Other reconstructions show similar shape (though not magnitude) and support similar conclusions (regarding the unprecedented nature of recent warmth/warming trend). This is the main argument to make, as you thought. Some of these other reconstructions do not include these bristlecones (e.g. Briffa, 2000; Crowley et al., 2003; Moberg et al., 2005; Briffa et al., 2001). Crowley and Moberg use different Bristlecone records I think. Other reconstructions do use the same Bristlecone pines (e.g., Mann and Jones, 2004). BUT the critical thing is that the studies either do not use these Bristlecone pines, or if they do use them, then they give them much more similar weighting to the other records used. I think MBH1999 is the only one that might give them a dominant weighting.

(5) IPCC is assessing all published work that relates to these issues in preparation for the AR4 in 2007. This includes the McIntyre and McKittrick papers as well as papers that report results contrary to McIntyre/McKittrick, such as the paper in press by Wahl and Amman that shows the Mann et al. results are reproducible.

cc'd for additional comments to Phil and Keith (when he's back).

Cheers

Tim

Dr Timothy J Osborn

Climatic Research Unit

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---

## References

1. <http://www.ucar.edu/news/releases/2005/ammann.shtml>
2. <http://www.realclimate.org/>
3. <http://www.cru.uea.ac.uk/~timo/>
4. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

From: Phil Jones <p.jones@uea.ac.uk>  
To: mann@psu.edu  
Subject: Re: Out in latest J. Climate  
Date: Thu Aug 4 09:49:54 2005

Mike,

Gabi was supposed to be there but wasn't either. I think Gabi isn't being objective as she might because of Tom C. I recall Keith telling me that her recent paper has been rejected, not sure if outright or not.

Gabi sees the issue from a D&A perspective, not whether any curve is nearer the truth, but just what the envelope of the range might be.

There is an issue coming up in IPCC. Every curve needs error bars, and having them is all that matters. It seems irrelevant whether they are right or how they are used. Changing timescales make this simple use impractical.

We have a new version of HadCRUT just submitted, so soon the'll be HadCRUT3v and CRUTEM3v. The land doesn't change much. This has errors associated with each point, but the paper doesn't yet discuss how to use them.

I'll attach this paper. Only just been submitted to JGR - not in this format though. This format lays it out better.

Thanks for reminding Scott.

Cheers

Phil

At 08:48 04/08/2005, you wrote:

Hi Phil,

Thanks for the heads up. Will be prepared for this then. I thought that Gabi Hegerl was involved with this guy? Doesn't she know better? It is disturbing that she hasn't set them straight on this.

By the way, as you may or may not have heard, its been discovered that there is a major error in Von Storch et al '04 that they now appear to be trying to hide (they have some obscure article in an Italian journal where they attempt to justify the error). There are several comments that have been or are soon to be submitted to Science about this. As it turns out, they introduces a spurious step in their supposed implementation of the MBH98 procedure in which they detrended the series first, gives completely wrong results.. Caspar Ammann and Gene Wahl and David Ritson of Stanford have both independently discovered this, because they noticed that amplitude of the calibrated signal in VS04 scales with the signal-to-noise ratio--this was the first clue that there was a major problem. There may be calls upon Science for them to retract their paper. The results are completely wrong, aside from the problems w/ the GKSS simulation. You

can expect to hear more about this soon...

I'll remind Scott about the proxies. He and Zhang are in the process of screening the proxy series for temperature signals, etc. Once they've done that, should be more useful. I expect we'll be able to get you some stuff by late August.

I did hear about the 3 papers coming out in Science. Apparently Donald Kennedy is doing an editorial that will discuss this in the context of the whole Barton business. That should be interesting...There will be articles by both Gavin and Steve Sherwood on "RealClimate" in coordination with the publication of the papers in Science Express. This should help turn the debate around.

talk to you later,

mike

Phil Jones wrote:

Mike,

He's been working with Myles Allen. Tim went to the first meeting of this Dutch funded project near Oxford last week.

Tim said they were doing some odd things, like correlating all the proxy series they had with CET (yes CET)! Even the few SH proxies they have. The others who went to the meeting were Zorita and Moberg. Zorita was still showing the GKSS run with Moberg series, even though its forcing is too large, it doesn't have aerosols in the 20th century and has spin up problems for the first 200 years.

Meeting wasn't that productive according to Tim. There was a belief amongst those there that all trees you used have lost low-freq, but this isn't true as you know.

Also, it was a good job Keith wasn't there (he didn't go as his father died the weekend before and he's not been in CRU since) as Martin assumed that RCS was developed by Esper (who also wasn't there). Tim put them right on this one, but RCS isn't applicable for normal tree sites, nor useful for bristlecones. Tim said Esper was wrong in his use of RCS, but they wouldn't accept that as Esper wasn't there to defend himself!

Basically only Tim knew anything about proxy data especially trees. Tim got the impression that they wanted to find that MBH is wrong. Given the previous comment, as you weren't there they are using double standards.

So, in conclusion, act carefully. Don't jump in, but some carefully thought through comments should be productive. Suggest they read the RevG article.

Martin isn't associated with the contrarians, but he's not in possession of the all the facts. He isn't aware of Casper's work, nor your latest study which you sent the other day, nor Rutherford et al.

There still seems to be a belief in these lower responding proxies. This is something we want to work on more here, as the only way it seems to show that these lower-freq proxies aren't that great is to use higher-freq proxies.

When you're back or sometime, can you remind Scott to send your

latest set of proxies. I'll have some time in the autumn to work on them as the AR4 should be in by Aug 12.

Science should be publishing 3 papers on the MSU issue by the end of Aug or early Sept. This is Mears/Wentz, Santer et al. and Sherwood et al. Latter shows that sondes are only truly reliable when flown at night. Daytime ones have all manner of problems with heating, just like air temps on board ships - hence the NMAT series.

I'll forward another email for interest.

Cheers

Phil

At 03:40 04/08/2005, you wrote:

Hi Phil,

Thanks, yes I'm in China now. As you might imagine, things have been very busy, but calming down a bit. Looks like Barton may be backing down...

Martin Jukes has an invited talk in my session. I invited him, because he was working w/ Stott et al, and so I assume he was legit, and not associated with the contrarians.

But if he's associated w/ the Dutch group, he may actually be a problem. Do you have additional information about him and what he has been up to?

Thanks,

mike

Phil Jones wrote:

Mike,

Good to hear it is out !

Hope the changeover is going OK and life is getting back to normal.

If you're not gone to China yet - you'll meet someone called Martin Dukes (?). He's giving a talk at your session. He knows about maths etc but not much about paleo ! Might need some education, but is probably OK. Not met him, but Tim has. Doing some worked funded by the Dutch govt on the hockey stick.

Cheers

Phil

At 04:05 03/08/2005, you wrote:

Dear Colleagues,

FYI, two papers attached:

First (reprint), Rutherford et al, is now out in latest issue of Journal of Climate.

This paper, aside from addressing other more scientifically-worthwhile issues, also happens to discredit most of the McIntyre and McKitrick claims.

Second (preprint), Mann et al, is formally in press (i.e., has gone off to the AMS production staff) in Journal of Climate. This paper strongly challenges the conclusions of von Storch et al (2004), and raises some methodological issues w/ the approach used

by Moberg et al (2005).

Feel free to pass along to others. Thanks

Mike

--

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[1]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

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## References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Jonathan Overpeck <jto@u.arizona.edu>  
To: Tim Osborn <t.osborn@uea.ac.uk>  
Subject: Re: MWP figure  
Date: Fri, 5 Aug 2005 14:57:36 -0600  
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, <oyvind.paasche@bjerknes.uib.no>

<x-flowed>

Hi Tim and Keith - Hope you're not going to kill me, but I was talking with Susan Solomon today, and she impressed me with the need to make several points if we can.

One issue (other to come in a subsequent email) is whether we can extend the MWP box figure to include the 15th century. I don't read the blogs that regularly, but I guess the skeptics are making hay of their being a global warm event around 1450AD. I agree w/ Susan that it is our obligation to weigh in on issues like this, so.... can we extend the fig to extend up to 1500AD?

Sorry about this, Tim. Of course we need it yesterday.

Thanks x10\*\*6

best, peck

>Dear Eystein, Peck and Keith,

>

>I spotted a minor error in the MWP figure  
>(reference period was 1001-2000 but should have  
>been 1001-1980 because some series stop in 1980)  
>and a typo in the legend, so here is a revised  
>MWP figure with these things corrected and a  
>slight adjustment to line thicknesses and font  
>sizes.

>

>As before I've included .ps, .pdf and .gif  
>versions because I'm not sure what you prefer.

>

>I've also drafted a caption - see attached .doc  
>file. Feel free to modify as necessary. I  
>think it covers the necessary details including  
>normalisation period, but perhaps it is a bit  
>"wordy" and unnecessarily repeats things already  
>in the MWB box text?

>

>I'm still working on SH figure/caption.

>  
>Cheers  
>  
>Tim  
>  
>  
>  
>Attachment converted: Macintosh HD:ipccar4\_mwpbox 1.pdf (PDF /«IC») (0008D1B9)  
>Attachment converted: Macintosh HD:ipccar4\_mwpbox.ps ( / ) (0008D1BA)  
>Attachment converted: Macintosh HD:Caption for  
>MWP box figure.doc (WDBN/«IC») (0008D1BB)  
>Attachment converted: Macintosh HD:ipccar4\_mwpbox.gif (GIFf/«IC») (0008D1BC)  
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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>  
To: Tim Osborn <t.osborn@uea.ac.uk>  
Subject: Re: MWP figure  
Date: Mon, 8 Aug 2005 11:12:37 -0600  
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, <oyvind.paasche@bjerknos.uib.no>

<x-flowed>

Hi Tim - Decisions, decisions... thanks so much for taking the initiative. I think - for the reason you state, we should go for the one that includes the 20th century. We make clear that these are not reconstructed temp, but normalized anomalies - this keeps us out of some trouble. But, I think the main message is that we're looking at this issue from every angle. And, we're letting others see the issue from every angle. It adds punch.

this means that the MWP box needs to talk about the period around 1400 - can you make sure that's on Keith's radar screen. I believe that historians talk about the Medieval Period going to at least 1450, so what the heck...

I you can adjust the caption to work, and then send both it and the final fig to Øyvind, me and Eystein that would be good - make sure Keith is ok with it all first, too.

Thanks Tim! Best, Peck

>Hi Peck,

>

>there is a period around 1400 AD when the proxy  
>records we've used in this MWP figure do  
>indicate a warm period - and all records show  
>positive anomalies at the same time. Thus it  
>couldn't/shouldn't be dismissed in the same way  
>as the MWP, as a period of disparate regional  
>behaviour, albeit with more records showing  
>warming than cooling. For 1400, all indicate  
>warming but with smaller magnitude than the 20th  
>century. If the figure were extended to cover  
>the 15th century, then it would also seem  
>necessary to extend it to the present so that  
>the 1400 period could be compared with the 20th  
>century.

>

>I've attached 3 versions of the figure.

>  
>850-1350 as originally sent.  
>  
>850-1500 showing warm anomaly in 1400, but  
>cannot tell how warm relative to present-day.  
>  
>850-2000 showing 1400 was not as anomalous as present-day.  
>  
>Take your pick, Peck!  
>  
>Cheers  
>  
>Tim and Keith  
>  
>At 21:57 05/08/2005, Jonathan Overpeck wrote:  
>>Hi Tim and Keith - Hope you're not going to  
>>kill me, but I was talking with Susan Solomon  
>>today, and she impressed me with the need to  
>>make several points if we can.  
>>  
>>One issue (other to come in a subsequent email)  
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>>blogs that regularly, but I guess the skeptics  
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>>to 1500AD?  
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>>Sorry about this, Tim. Of course we need it yesterday.  
>>  
>>Thanks x10\*\*6  
>>  
>>best, peck  
>>  
>>>Dear Eystein, Peck and Keith,  
>>>  
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>>>in 1980) and a typo in the legend, so here is  
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>>>As before I've included .ps, .pdf and .gif  
>>>versions because I'm not sure what you prefer.  
>>>  
>>>I've also drafted a caption - see attached

>>>.doc file. Feel free to modify as necessary.

>>>I think it covers the necessary details

>>>including normalisation period, but perhaps it

>>>is a bit "wordy" and unnecessarily repeats

>>>things already in the MWB box text?

>>>

>>>I'm still working on SH figure/caption.

>>>

>>>Cheers

>>>

>>>Tim

>

>

>Attachment converted: Macintosh HD:mwpbox8502000.pdf (PDF /«IC») (00091133)

>Attachment converted: Macintosh HD:mwpbox8501500.pdf (PDF /«IC») (00091134)

>Attachment converted: Macintosh HD:ipccar4\_mwpbox 2.pdf (PDF /«IC») (00091135)

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: David Rind <drind@giss.nasa.gov>

Subject: RE: solar MM

Date: Mon, 8 Aug 2005 11:24:37 -0600

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

David - sounds promising. So, the bottom line is that a little disagreement is ok - that's a reflection of the real uncertainty? But, the discrepancy is not all that big in the end? No need to take this to a higher level?

Keith Briffa is back on line and finishing off Section 6.5, so you might want to send him an email w/ suggestions that help keep chap 6 compatible w/ 2 and 9 - for example, with respect to solar, we acknowledge the forcing could be less than  $0.5 \text{ W/m}^2$ , and the uncertainty wrt to trop aerosols and land albedo is significant - we could easily be closer to chap 9's estimate. Would you say the key is that our analysis acknowledge the uncertainty so as to overlap well with the other chapters?

Keith - please make sure you send your new 6.5 to David too - while you were out, he was working hard w/ chap 2 and 9 to make sure we (the IPCC) avoid saying things that confuse. The comparison of radiative forcings from 3 different angles is what assessment is all about, and it's great David has had the patience to help figure it all out.

Thx, Peck

Hi Gabi,

The key to your proposed solution is the updated numbers from Chapter 2. If indeed the radiative forcing change to 1750 is -1.53, then presumably you have made this consistent with the earlier part of Chapter 9. The numbers previously looked like this (I haven't seen the latest version of 6.5, but I've included the previous estimates we had in the ZOD):

$\text{W/m}^2$

	Chapter 6	Chapter 9
--	-----------	-----------

MM	1750	
----	------	--

Greenhouse gases:	-2.4	-2.6
-------------------	------	------

TROP aerosols:	0.5	0.2
----------------	-----	-----

Solar	-0.5	-0.1
Volcanic:	?	?
Land albedo:	+0.4	0.03
Trop O3:	-0.35	-0.4
Strat O3:	+0.15	0.10
1'st indirect aerosol forcing		1.2
STRAT H2O		-0.13
AVIATION		-0.02
TOTAL	-2.2	-1.7

There is essentially no change in greenhouse gas forcing from 1750 to 1700 (see for example Crowley et al., GRL, 2003), so the difference in the estimated numbers is probably due to inclusion of more things or different choices in Chapter 2. A similar statement holds for trop aerosols. One can also use these two to presume that the same also holds true for land albedo. [The value listed for that in Chapter 9 is quite small compared to some other studies; e.g., Govindasamy et al., GRL, 28, 291-294, 2001.] So, to the extent these numbers are still discussed in Chapter 6, they should be made consistent with those in chapters 2 and 9.

With respect to your proposed paragraph below: I would drop the comments about trace gas differences but saying land albedo changes may have been greater, along with the additional solar change, could give us the  $-1.8 \text{ W/m}^2$  forcing.

Concerning the temperature response: the Moberg et al paper itself claims  $1^\circ\text{C}$  difference between 1500 and 2000, but the figure seems to show a larger number, perhaps  $1.3^\circ\text{C}$  (again, just eye-balling it). However, the coldest time period is not in the MM but before it. I think therefore a better estimate from that paper for the MM would be  $1^\circ\text{C}$ .

So, with respect to the sensitivity: if  $0.85 \text{ W/m}^2$  is unresolved, then we have a total forcing of  $\sim 0.95 \text{ W/m}^2$ , and a climate response varying between  $0.45^\circ\text{C}$  and  $1^\circ\text{C}$  - or a climate sensitivity for  $2\times\text{CO}_2$  of  $1.9^\circ\text{C}$  to  $4.2^\circ\text{C}$ , or pretty similar to standard IPCC estimates.

I think this will work!

David

At 1:02 PM -0400 8/6/05, hegerl@duke.edu wrote:

On Sat, 6 Aug 2005 hegerl@duke.edu wrote:

p.s. I modified the text for MM forcing according to below theory (please yell if its off!) which would say (and has questions for you): During the cool period of the Late Maunder Minimum (approximately 1675-1715), sunspots were generally missing, and solar irradiance is believed to have been smaller. This period will be used in Section 9.6 to discuss climate sensitivity; therefore we discuss its radiative forcing estimates . The estimated difference between present day solar irradiance and the late 17th century Maunder Minimum is presently -1.1 W/m<sup>2</sup> (best estimate, range -0.5 to -2 W/m<sup>2</sup> , Chapter 2), but with large

uncertainties. This leads to a best estimate radiative forcing of -0.2 W/m<sup>2</sup> (-0.1 to -0.35 W/m<sup>2</sup> 67% confidence interval; note that solar forcing from 1750 to the present is estimated having increased by 0.1 W/m<sup>2</sup> , chapter 2). Many radiative forcing changes, particularly those associated with industrialization, are very similar from the present to the Maunder Minimum as they are from the present to preindustrial (total forcing estimated of -1.53 W/m<sup>2</sup>, see 9.2.1.2). CO<sub>2</sub> may have been slightly lower (by???) , and land cover changes may also have been slightly greater between the Maunder Minimum and 1750. This yields an approximate net radiative forcing of -1.8 W m<sup>-2</sup> (between the late Maunder Minimum and the present, with large uncertainties.

>  
>> Hi David et al, >> I spent some more time pondering the MM forcing.  
> I think the best place to start is the updated chapter 2 forcing  
> from preindustrial, which is (according to what Joyce pulled out of  
  
> ch 2, so hope its correct):  
>  
> -1.53 from present to the 1750 period (all included that they deem  
> relevant, so no volcanoes because episodic, but all else in there  
> including contrails and other weird small stuff, I THINK it also  
> includes land cover changes)  
>  
> We would have to add -0.1 for the more reduced solar (given +0.1 1750 to  
> now from ch2, and 0.2 from MM on), and maybe some number for the  
> somewhat lower CO<sub>2</sub> between 1700 and 1750 (what would that be)? and  
> maybe another number for additional changes in land cover?  
>

> Overall, the number you had before of -1.8 (after adjusting solar down  
> to recent wisdom) seems now pretty good to me.  
> Should we keep it, or do you ahve another suggestion?  
> I am glad we didn't loose the forcing from MM to present :)))  
>  
> greetings, let me know what would be good for us to write (and then I'll  
> do the arithmetic for the best guess sensitivity once you guys also  
> check my numbers for high/low estimates of annual temp changes at that  
> period, right now its -0.45 Mann to -1.5 Moberg-readoffplotinahurry by me)  
>  
> Thanks in advance, I think we are very close to resolve this!  
>  
> Gabi  
>  
> On Fri, 5 Aug 2005, David Rind wrote:  
>  
>> As this continuing exchange has clarified, what's in Chapter 6 is  
>> inconsistent with what is in Chapter 2 (and Chapter 9 is caught in  
>> the middle!). Worse yet, we've managed to make global warming go  
>> away! (Maybe it really is that easy...:)  
>>  
>> David  
>>  
>> At 9:49 AM -0600 8/5/05, Bette Otto-Bliesner wrote:  
>>>Gabi,  
>>>  
>>>In Chap 6, we use 2.2 with a range of 1.9 to 2.6 W/m<sup>2</sup>. The  
>>>uncertainty range includes both uncertainties in the ice core  
>>>measurements and uncertainties in the radiative transfer  
>>>calculations.  
>>>  
>>>Bette  
>>>  
>>>\_  
>>>  
>>>  
>>>At 2:27 PM -0400 8/4/05, Gabi Hegerl wrote:  
>>>  
>>>David, so with the Judith correction only (solar down by 0.4), we  
>>>get a total forcing of  
>>>0.95 to MM, (after subtracting the 0.85 not realized yet according to Jim)  
>>>  
>>>Then, if the indirect effect and black carbon is added, wouldn't

>>>this reduce the forcing to nearly nothing?  
>>>(or what am I doing wrong, 2.2 changes to 1.8 with new solar, black  
>>>carbon and ind aerosol takes away  
>>>0.9. yielding 0.9 W/m\*\*2, then Jim says 0.85 of that is unrealized???)

>>  
>

> -----

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> Duke University, Durham NC 27708  
> phone 919-684-6167, fax 919-684-5833  
> email: hegerl@duke.edu <http://www.eos.duke.edu/Faculty/hegerl.html>

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From: Phil Jones <p.jones@uea.ac.uk>  
To: Kevin Trenberth <trenbert@ucar.edu>  
Subject: Re: [Fwd: wow]  
Date: Mon Aug 8 15:30:13 2005

OK. I agree with her on most. I was looking at the file over the weekend. The new 3.8.4 has helped as will the new ones on DTR when we get them. In the longer run I would like to get 3.7.1 and 3.7.2 redone - at least plotted better.

Also, in time, we will need to get the Sahel plot updated to have 2004 and 2005 in. Neil Ward was here for a few hours last week. He's now back at IRI, but he was surprised by the UK media and their reporting of the famine in Niger - saying it was all down to lack of rainfall. June in the region was above normal. Problems last year and locusts are the reason. The real reason may not matter on the ground, but the problems will recur as very little is planted this year.

Cheers

Phil

At 15:10 08/08/2005, you wrote:

I had an email exchange with Susan the preceded this.

She is making an early start on reading the chapter and started with ours, using the version I posted on thursday: so she is referring to the figure file for Ch 3.

Kevin

Phil Jones wrote:

Which ones ? Which version is she looking at?

Susan's been suggesting figures for the paleo chapter. At least we haven't had to cope with that.

Phil

At 15:01 08/08/2005, you wrote:

FYI

----- Original Message -----

Subject: wow

Date: Fri, 5 Aug 2005 18:08:21 -0600

From: Susan Solomon [1]<ssolomon@al.noaa.gov>

To: [2]trenbert@ucar.edu

References: [3]<p06020416bf194a5ef9bc@[140.172.240.163]>

[4]<4001.128.117.68.3.1123283585.squirrel@webmail.cgd.ucar.edu>

[5]<p0602040bbf19a6388172@[140.172.240.163]>



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2. <mailto:trenbert@ucar.edu>
3. <mailto:p06020416bf194a5ef9bc@%5B140.172.240.163%5D>
4. <mailto:4001.128.117.68.3.1123283585.squirrel@webmail.cgd.ucar.edu>
5. <mailto:p0602040bbf19a6388172@%5B140.172.240.163%5D>
6. <mailto:4148.24.8.173.64.1123285320.squirrel@webmail.cgd.ucar.edu>
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From: Tim Osborn <t.osborn@uea.ac.uk>  
To: Jason E Smerdon <jsmerdon@umich.edu>  
Subject: Re: SH figure for IPCC AR4  
Date: Tue Aug 9 14:14:43 2005  
Cc: Henry Pollack <hpollack@umich.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>

Thanks for the comments Jason/Henry. Just wanted to let you know that I've dropped the uncertainty ranges to be consistent with the other records and also cut the borehole series at the median sampling dates.

Cheers

Tim

At 16:45 04/08/2005, Jason E Smerdon wrote:

Hi Tim,

Henry and I apologize for not being available the last few days. Henry has been out of town and I have been in the midst of moving to New York. Nevertheless, we had the chance to cross paths today and discuss the figure and caption. We hope it is not too late to add our two cents.

We agree that the uncertainties on the borehole curves should be removed to make the display more consistent. We have also decided that it would be best to truncate the borehole curves at their median logging dates. For Australia and Africa those years are 1972 and 1986, respectively. If you wish to discuss the sampling densities, the total number of boreholes in Australia and Africa are 57 and 92, respectively. The SH has a total of 165 holes, compared to 695 in the NH.

Let us know if you need anything else. I hope this has not arrived too late and good luck with everything.

Best Regards,

Jason

From: Tim Osborn <t.osborn@uea.ac.uk>  
To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa  
<k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, Øyvind  
Paasche <oyvind.paasche@bjerknes.uib.no>  
Subject: New figure for box 6.4 - the Medieval Warm Period  
Date: Tue Aug 9 14:34:59 2005

Dear all again,

here is the MWP figure and caption. Note that I don't know what number it should have, because it is for a box not a standard section. So I've just called it "mwpbox" for now. Please can you give it the correct number and put it in the right place in the figures file? Also, when you have numbered it, please let us know so that we can refer to it in the MWP box text.

The figure now goes right up to the present, as requested!

I've attached a word document with caption and .gif figure embedded, but also separate .gif, .pdf and .ps files for the figure. If you don't want all these different formats, then please tell me which one(s) you want and I'll only send those in future!

Cheers

Tim

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: jto@u.arizona.edu,eystein.jansen@geo.uib.no  
Subject: Section on last 2000-years  
Date: Tue Aug 9 17:21:11 2005

Peck and Eystein

in case you tried (!), my phone has been broken for the last few days (yes - honestly). I am sorry I had to rush off - and stay longer than I had anticipated . The funeral was delayed

while a post-mortem examination had to be held to establish the precise cause of death.

Ironic

that dad had struggled on having had at least 3 heart attacks, 2 strokes, chronic diabetes and partial liver and kidney failure for some years (besides being virtually immobile and completely blind for 18 months). All in all , though it was a release, the actual demise was sudden and unexpected and I managed to arrive too late to be with him at the end. Given the time constraint , this "final" revision is not as considered as it might have been , but we have tried to take into account all comments available , and have given considerable attention to the IPCC terminology and emphasis on the bullet points . At this stage , however, there are some clear areas where future work will be required to keep abreast of recent developments and , perhaps, to re-balance the emphasis and structure. I apologise for not having responded directly to Fortunat, Stefan, Ricardo.Olga, David and Tom, but please be aware that I have considered all of their comments and done what I could to address them .Thanks Fortunat and Ricardo (and Ed - who should be added to the list of CAs) for the text and Figures and Henry and Jason for the help and data . David's suggestions about re-ordering the paragraphs was particularly difficult to resolve in my own mind , because I do see the logic , but equally , did not want to interfere with the time line approach to describing post- TAR work that underlies the current structure. as you can see I decided to leave the order as it was. It would be great if David and Fortunat could check cross Chapter referencing (eg in relation to forcings and detection chapters). We can revisit this , and the issue of McIntyre and McKitrick (centering of PCs in Mann et al reconstruction - which is clearly unfounded) until such time as the numerous responses are published.

The new SH section is in , and the MWP box slightly amended to take account of the new Figure.

Peck, I have considered your text on the regional section - and you will see that I have edited out some relating to future (and association between drought and SSTs) . I feel strongly that you are venturing into "observational" territory and speculation beyond what we should say. I have also amended the bullet points to reflect this. **YOU ARE THE ULTIMATE ARBITERS** and it is up to you if you wish to re-insert , but I will give you a continuing argument later about our overstepping the "paleo" boundary. Note also that the bullet on European summer 2004 has been altered to reflect what was a last minute , one-sentence , insertion in the first paragraph regarding Jurg Lutterbacher's Science paper - as there was

no mention of it otherwise. We had to remove the reference to "700 years in France" as I am not sure what this is , and it is not in the text anyway. The use of "likely" , "very likely" and my additional fudge word "unusual" are all carefully chosen where used. Tim has been a rock in the last minute rush here - not only doing the Figures , but also helping with the text. I am really grateful to him. He has sent the text , with some comments, and highlighted references, that need attention. If Oyvind can identify references and handle these problems with Endnote , we are also really grateful. The final references , if missing , are probably in the current text, the previous Endnote library , or in sections of text sent by Ricardo, Fortunat, Peck and Eystein. I trust when you guys have stiched the new text back in and the Figures etc. we will perhaps get a last chance to correct and check references etc. Thanks

Keith

--

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## References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Phil Jones <p.jones@uea.ac.uk>

To: Kevin Trenberth <trenbert@ucar.edu>, Peter Lemke <plemke@awi-bremerhaven.de>

Subject: Re: 3.9

Date: Wed Aug 10 10:49:18 2005

Peter, Kevin

Not having seen Ch 4, I agree that the term 'local heat budget' can be ambiguous. Are you also discussing the issue of 'dirty' glaciers? For the Alps, the Swiss (well Wilfried Haeberli) reckon that temperature alone cannot explain all the retreat in some recent summers (especially 2003). Would local heat budgets include the effects of local anthropogenic pollutants making the snow less white?

Lonnie Thompson has been on Quelccaya in the last couple of months and reports that it is in an awful state. Like Kilimanjaro, the recent annual layers aren't distinguishable. Lonnie reckons a lot of retreat is caused by sublimation. On Quelccaya Lonnie and Ray Bradley have put up an AWS (on Sajama too). They've not got as much data as they hoped as both have fallen over due to melting and also the guide who helped them put one on Quelccaya later went back and brought it back down to try and sell !

I'm happy with Kevin's draft, if local heat budgets is explained in your chapter.

Cheers

Phil

At 17:29 09/08/2005, Kevin Trenberth wrote:

Peter, Thanks (sorry I can't get rid of the blue).

I am cc'ing Phil on this: Georg has suggested instead the following.

The temperature increases are consistent with the observed nearly worldwide reduction in glacier and ice cap mass and extent with strongest recession rates in the 1930s and 1940s and after 1990 and little changes around 1970. Tropical glacier changes are synchronous with global ones, Kilimanjaro being an exception with radiatively forced constant retreat of the plateau ice. 20<sup>th</sup> Century glacier retreats are consistent with temperature variations. Before 1900, glacier fluctuations are probably not only reflecting temperature variations but mainly precipitation anomalies. In the Tropics, glacier changes are related to atmospheric moisture variations which, in turn, correlate with sea surface temperatures in the respective source regions and varying atmospheric circulation modes. In some regions (Alaska, Patagonia, Karakoram) moderately increased accumulation is observed indicating an amplified hydrological cycle.

I am not altogether happy with this wording. In this bullet it reflects findings from your chapter and ours (wrt precip, temp, circulation etc). I would propose the following as a compromise between the old text and the proposed:

The temperature increases are consistent with the observed nearly worldwide reduction in glacier and ice cap mass and extent in the 20th century. Tropical glacier changes in South America, Africa and Tibet are synchronous with global ones, and all have shown

declines in recent decades. If continued, some may disappear within the next 30 years. Local temperature records all show a slight warming, but not of the magnitude required to explain the rapid reduction in mass of such glaciers (e.g., on Kilimanjaro), which instead depends on local heat budgets. Glaciers and ice caps respond not only to temperatures but also changes in precipitation, and before 1900, glacier fluctuations are probably not only reflecting temperature variations but mainly precipitation anomalies. In some regions moderately increased accumulation observed in recent decades is consistent with changes in atmospheric circulation and associated increases in winter precipitation (e.g., southwestern Norway, parts of coastal Alaska, Patagonia, Karakoram, and Fjordland of the South Island of New Zealand).

Note I have retained a bit more detail on the regions affected, and tried to stay away from "radiatively forced" (whatever that means) and vague terms like "amplified hydrological cycle". I also want to retain more specific reference to the precip and circulation changes going together. Whether "local heat budgets" is adequate is my main question? I gather this is related to changes in cloud and sunshine, increased heating that goes into melting and ablation rather than temp increases. Should we spell that out? Do you deal with that? I also did not add the detail on the dates in first sentence as those should be in your chapter and they don't relate directly to the other variables.

Are my terms "20th century" and "recent decades" correct?

Thanks

Kevin

Peter Lemke wrote:

Dear Kevin,

after his return from the Kilimanjaro Georg has supplied a modification to the text in 3.9 concerning the glaciers.

I have made a tiny change further down in the text replacing "order" by "approximately" meaning 1mm/year and not implying, say, 3mm/year.

Best regards,

Peter

--

\*\*\*\*\*

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## References

1. <mailto:trenbert@ucar.edu>
2. <http://www.cgd.ucar.edu/cas/>

From: Phil Jones <p.jones@uea.ac.uk>  
To: Kevin Trenberth <trenbert@ucar.edu>  
Subject: Re:  
Date: Wed Aug 10 17:13:37 2005

Fine with me. Let's hope they agree by tomorrow.

Phil

At 17:11 10/08/2005, you wrote:

Ok so here is how it now reads:

The temperature increases are consistent with the observed nearly worldwide reduction in glacier and ice cap mass and extent in the 20<sup>th</sup> century. Tropical glacier changes in South America and Africa, and those in Tibet are synchronous with higher latitude ones, and all have shown declines in recent decades. Local temperature records all show a slight warming, but not of the magnitude required to explain the rapid reduction in mass of such glaciers (e.g., on Kilimanjaro). Glaciers and ice caps respond not only to temperatures but also changes in precipitation, and both global mean winter accumulation and summer melting have increased over the last half century in association with temperature increases. Other factors in recent ablation include changes in cloudiness and water vapour and associated radiation, and surface sensible heat exchange. Precipitation anomalies are also important before 1900 in glacier fluctuations. In some regions moderately increased accumulation observed in recent decades is consistent with changes in atmospheric circulation and associated increases in winter precipitation (e.g., southwestern Norway, parts of coastal Alaska, Patagonia, Karakoram, and Fjordland of the South Island of New Zealand) even as enhanced ablation has led to marked declines in mass balances in Alaska and Patagonia.

Kevin

Phil Jones wrote:

Sort of arguing that way. It is also the before 1900 part. Precip and temp anomalies are important at all times for glaciers. Their influence didn't change around 1900.

So what about Precipitation anomalies are also important before 1900.

I'd not got the implication. Adding also makes it clearer.

Phil

At 16:56 10/08/2005, Kevin Trenberth wrote:

Phil is arguing for changes to 4.5. Maybe the statement is too strong although it is consistent with the last para of 4.5.2.? An alternative might be: Precipitation anomalies are important before 1900. In the context this implies in addition to temperature.

Kevin

Phil Jones wrote:

Georg,

I've now also looked at the figures you sent from Ch 4. Kevin has the sentence, which Peter may have added? I reckon this is too strong. Can we omit it?

Sentence is

Before 1900, glacier fluctuations probably mainly reflect precipitation anomalies.

Reasoning

Is this a general statement. I wonder if we need it. Oerlemans uses estimated glacier termini positions (and related ELA changes) to infer past temperatures and you have his figure. I know he assumes precip to have remained essentially the same but he backs out temperature. Also glaciers in Europe advanced in the 17th and 18th centuries. It was cooler then (more so in winter than summer). I also have a paper resubmitted to JGR where Alpine precip shows no long-term changes since 1800. This uses loads of stations and is from the ALP-IMP project that ZAMG co-ordinate (Reinhard Boehm).

So the advances are caused by more precip, but the retreats by higher summer T and maybe less winter precip.

Cheers

Phil

At 16:23 10/08/2005, Kevin Trenberth wrote:

Hi Georg

Many thanks for the attachments. I had looked at the ZOD but this is much more informative. Based on your comments and the 4.5 section I have come up with the following bullet. Note that here we are writing for a general audience. I have now tried to include more clearly the factors involved. I think these are consistent with your chapter but the language in your chapter might be improved in a couple of places. For instance an important forcing is radiation (solar and IR) which are greatly impacted by clouds, water vapor, and albedo (the dirty cover on top of snow Phil referred to), and I thought these could be brought out better in your chapter. These are perhaps more basic than temperature lapse rates and precipitation gradients which are consequences. In 4.5.2 you use the term "radiatively forced" but it is not clear what that means. I suggest using some of these terms. Also it is not clear what "amplified hydrological cycle" means. [FYI, the expectation is for more intense precipitation, not necessarily for more total (owing to pollution effects). The former is determined by increased water vapor]. I took some of your words in the following. We need to emphasize that glaciers are not just high latitudes. I retained Kilimanjaro as that has received a lot of publicity. Some of this is necessarily abrupt, but there will be a reference to 4.5 immediately following this bullet. So the recent reversals in NZ and Norway can not be dealt with here.

Let me know if you have further suggestions. Again, many thanks

Regards

Kevin

o The temperature increases are consistent with the observed nearly worldwide reduction in glacier and ice cap mass and extent in the 20<sup>th</sup> century. Tropical glacier changes in South America and Africa, and those in Tibet are synchronous with higher latitude ones, and all have shown declines in recent decades. Local temperature records all show a slight warming, but not of the magnitude required to explain the rapid reduction in mass of such glaciers (e.g., on Kilimanjaro). Glaciers and ice caps respond not only to temperatures but also changes in precipitation, and both global mean winter accumulation and summer melting have increased over the last half century in association with temperature increases. Other factors in recent ablation include changes in cloudiness and water vapour and associated radiation, and surface sensible heat exchange. Before 1900, glacier fluctuations probably mainly reflect precipitation anomalies. In some regions moderately increased accumulation observed in recent decades is consistent with changes in atmospheric circulation and associated increases in winter precipitation (e.g., southwestern Norway, parts of coastal Alaska, Patagonia, Karakoram, and Fjordland of the South Island of New Zealand) even as enhanced ablation has led to marked declines in mass balances in Alaska and Patagonia.

Georg Kaser wrote:

Kevin,

Have many thanks for compiling and editing 3.9. I agree that the "radiatively forced" and the "amplified hydrological cycle" should be removed and I also agree with Phil's comment on the "local heat budget". In glaciology, the sum of each energy flux toward and from the respective snow/ice surface is considered to make up the "local heat budget". This also includes the sensible heat flux.

There are some other points in the text which I would like to comment:

1. Tropical glaciers are considered those in the South American Andes between Venezuela and Northern Bolivia, those in East Africa and those in Irian Jaya (New Guinea). In Chapter 4, Tibetan glaciers are taken as part of the Asian High Mountains (find the present state Chapter 4.5. "Glaciers and Ice Caps attached).

2. Alaska, Patagonia, Karakoram, Norway and NZ cannot be merged in the respective statement. In Alaska and Patagonia, moderately increased accumulation is accompanied by strongly enhanced ablation making the mass balances markedly negative. From glaciological sites, no studies concerning atmospheric circulation patterns are provided in the respective studies.

In the Karakoram mountains, enhanced accumulation has led to considerable glacier advances, increased winter accumulation from the Westerlies is only suggested but not subject of detailed studies. Heavy debris loads on the tongues probably prevent from enhanced ablation.

In Southwest Norway and NZ South Island, glacier advances have ceased around 2000. I don't know whether their advances shall still be mentioned in extension; I would not do so beyond the respective statement in Ch. 4.5.

3. "If continued, some may disappear within the next 30 years." This sentence can stand

for every mountain region in the world and should not be used for tropical mountains only. Everywhere, many small glaciers have disappeared since the 19th Century maxima and many will disappear soon in the Alps, the Caucasus, in the Asian High mountains etc. as well as in the Tropics. From the today's perspective Mount Kenya, all Mountains in the Rwenzori Range except Mt. Stanley, Irain Jaya will be without glaciers soon, probably sooner than Kilimanjaro; well known and studied glaciers in the Andes like Chacaltaya, Charquini and Pastoruri will also disappear soon. This is not because of a particular regional climate feature but just because they were already small when retreats started. As you will see from Figure 4.5.5. Kilimanjaro's plateau ice is particular, slope glaciers are less. The plateau glaciers retreat from their vertical walls where no accumulation is possible and since they do so, there is no way to find an equilibrium besides disappearance. The vertical walls are a result of cold temperatures high sublimation and strong solar radiance. There is no way to replace the retreat by ice dynamics on the flat summit plateau. Slope glaciers are only partially subject of this kind of ablation and their retreat rate seems to have slowed markedly (See insert of Fig 4.5.5). If Kilimanjaro is mentioned in 3.9. it must also be added that it is a particular case with complex relation to climate change.

4. All studies which investigate tropical glacier retreat and climate show the dominance of changes in energy and mass balance terms which are related to the atmospheric moisture content rather than locally measured air temperatures. Both increased and reduced moisture can lead to negative mass balances and it has done so in most cases studied (Cordillera Blanca, Peru, Cordillera Real, Bolivia, Antisana, Ecuador, Rwenzori, Mt. Kenia, Kilimanjaro). Yet, wherever respective analyses were made, correlations were found to anomalies in ENSO or Indian Oceans Indian Ocean Dipole Mode respectively strongly indicating global warming as the principle reason of the retreat.

I give you this lengthy explanation in order to make sure that the very compressed and condensed bullet in 3.9. gets the right content. I have started to change your paragraph suggestion accordingly but have to admit that, not being a native speaker myself, it either becomes very long or very awkward.

I also appreciate Phil's statement about Quelccaya and Sajama. Doug Hardy and Ray Bradley run AWS' there since a couple of years as well as on Kilimanjaro with all the problems of recording data at such high elevation sites. Doug is preparing a paper on the climate records there but it has still not reached it's final state.

Information on sublimation on Quelccaya is not published such as the positive mass balances and advances on several Andean glaciers between 1998 and 2002 are not published. Kilimanjaro has experienced both ablation as well as accumulation layers on the horizontal surfaces over the last years. I have just come back from fieldwork there last week and the last half year was a mass loss year. Being very much involved into tropical glaciers myself, I have to accept that such detailed information would be available for several hundreds of glaciers in the world each one providing 10 or more publications. Going into such details cannot be the aim of the report, I am afraid.

Best wishes,  
Georg

Georg Kaser

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[1][http://meteo9.uibk.ac.at/IceClim/CRYO/cryo\\_a.html](http://meteo9.uibk.ac.at/IceClim/CRYO/cryo_a.html)

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From: "Michael E. Mann" <mann@meteo.psu.edu>  
To: Caspar Ammann <ammann@ucar.edu>  
Subject: Re: [Fwd: Storch drift]  
Date: Fri, 12 Aug 2005 11:21:20 -0400  
Reply-to: mann@psu.edu  
Cc: Stefan Rahmstorf <rahmstorf@ozean-klima.de>, mann@psu.edu, Keith  
Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Phil Jones  
<p.jones@uea.ac.uk>

<x-flowed>  
Hi Caspar,

Thanks for the comments. Frankly, Von storch is being duplicitous here. He may tell certain audiences (like the NCAR group last month) that he is not suggesting that the GKSS simulation is realistic, because he knows he'll get skewered if he claims otherwise. But then he turns around to the press, and talks about how the Moberg et al reconstruction matches their model, etc. I frankly consider this dishonest, at best!

If what Stefan says is true (that the entire long-term trend, including the cold LIA in the model, is all due to the spinup problem), then it completely invalidates the use of that model for testing statistical reconstruction methodologies which require physically-consistent patterns of variance in the calibration period to reconstruct the past. But that's a separate issue.

As we now know, the far more damning fact is that Von Storch et al knowingly applied a procedure which is not the MBH98 procedure, and they think they can get away w/ admitting this now in some obscure Italian journal which isn't even in the ISI database. Tim/Phil/Keith: you may not know about the latter, but Caspar should be able to fill you in on this shortly...

Meanwhile, lets enjoy the media fiesta on MSU...

Mike

Caspar Ammann wrote:

> Stefan,  
>  
> this is very important news indeed. The runs will get a huge hit from  
> this. The only way a coupled model can get a continued trend (without  
> invoking an energy leak somewhere) is when there is a terrible  
> deep-ocean spin up available even for their present day  
> initialization, not to speak about the subsequent shock to  
> pre-industrial conditions. Did you really say 1.5 degrees? Wow, that  
> is quite a bit. Seems to me they must have used Levitus ocean data  
> with an atmospheric restart file, then hit it with the solar/GHG  
> changes. It seems rather large of a drop to come from a fully coupled  
> stage. 1.5 degrees is about 30% too large to be exclusively from the  
> atmospheric composition and solar irradiance, thus my suspicion  
> regarding levitus. Now it would be important to know what happend  
> because some people are using the run as a possible real-world

> scenario (although Hans in talks does not claim so).  
>  
> Caspar  
>  
> PS Now, bare in mind that the Science paper applies to the  
> reconstruction, and for the general discussion the influence of spinup  
> should not make that big of a difference (other than inflating the  
> difference of the coldest period to the calibration period, which  
> creates some issues discussed by Mike previously).  
>  
>  
>  
> Michael E. Mann wrote:  
>  
>>  
>>  
>> -----  
>>  
>> Subject:  
>> Storch drift  
>> From:  
>> Stefan Rahmstorf <rahmstorf@ozean-klima.de>  
>> Date:  
>> Thu, 11 Aug 2005 15:37:27 +0200  
>> To:  
>> mann@psu.edu  
>>  
>> To:  
>> mann@psu.edu  
>> CC:  
>> Gavin Schmidt <gschmidt@giss.nasa.gov>, Keith Briffa  
>> <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk  
>>  
>>  
>> Hi Mike,  
>>  
>> here is some interesting new info on the drift problem in the VS04  
>> runs. Irina Fast and Gerd B"rger submitted a comment about this to  
>> Science some months ago; it was rejected and they did not pursue it.  
>> I'm trying to encourage them to resubmit this elsewhere. I do not  
>> have the ms. but have seen several graphs. There are two key points.  
>>  
>> 1. The ECHO-G run started at year 900, the VS04 paper of course shows  
>> only results starting from year 1000. I've seen the full run now.  
>> Between 900 and 1000, the NH temperature drops by about 1.5 ||C!  
>> That's how severe their initialisation problem is. From my experience  
>> of how the THC responds after such step-function changes in forcing,  
>> the strong warming from 1050-1150 in VS04 could well be a rebound  
>> effect from the 1.5 ||C cooling that precedes it, since the THC tends  
>> to oscillate on such a time scale when forced rapidly.  
>>  
>> 2. Irina has run ECHO-G initialised with modern climate and then  
>> switching to pre-industrial conditions similar to the run shown by

>> VS04, but without any further variability in the forcing. Thus, this  
>> shows the pure drift from initialising this run - this is what Tim  
>> has been estimating in MAGICC. The actual drift in ECHO-G is even  
>> larger and more persistent than what Tim found: there is a cooling  
>> between the years 1000 and 2000 of over 0.6 ||C, and this is an almost  
>> linear trend over the whole time. I.e., not just drifting during the  
>> first few centuries, but over the entire 1000-year period.  
>>  
>> Cheers, Stefan  
>>  
>

--

Michael E. Mann  
Associate Professor  
Director, Earth System Science Center (ESSC)

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<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>  
To: mann@psu.edu  
Subject: Re: [Fwd: Storch drift]  
Date: Fri Aug 12 17:18:22 2005

Mike,

Yes it was him !

Phil

At 17:17 12/08/2005, you wrote:

Hi Phil,

Yeah--I've been told that one of the co-authors of the chapter (w/ the initials D.R.) has behaved poorly. Fortunately, w/ Peck, Stefan R., and Keith all authors on the chapter, it sounds as if the voices of reason are prevailing...

mike

Phil Jones wrote:

OK. Keith is also away next week. He's already gone.

He'll need to look more at all this before the next IPCC meeting in December.

You should have seen some of the crap comments he got. Not yours, but some of the other authors on the paleo chapter.

People who you think ought to know better. Most relating to MM. All mostly ignored. You'll be able to register to get the draft by early Sept.

Cheers

Phil

At 16:49 12/08/2005, you wrote:

Thanks Phil,

Can you tell Keith (confidentially) that Ammann and Wahl are submitting a comment to Science pointing out that von Storch knowingly did not apply the MBH98 procedure, and that all of the conclusions in that paper are wrong! There may be calls on Science to retract VS04, because the mistake undermines every single conclusion!!

mike

Phil Jones wrote:

Mike,

We have the Italian paper Well Keith does for his AR4 work.

Submission day for AR4 is today by the way.

I think the Italian journal is the one from a conf I went to 3 weeks after the Berne meeting. I didn't bother sending anything to the Italian meeting either, just like Berne. The journal the Italians were planning did look obscure when I was there, but I didn't write anything down, as I had no intention of sending anything.

Yes the MSU stuff is out. There will be something in Nature next week on it.

Off next week as a break from IPCC.

Cheers

Phil

At 16:21 12/08/2005, you wrote:

Hi Caspar,

Thanks for the comments. Frankly, Von storch is being duplicitous here. He may tell certain audiences (like the NCAR group last month) that he is not suggesting that the GKSS simulation is realistic, because he knows he'll get skewered if he claims otherwise. But then he turns around to the press, and talks about how the Moberg et al reconstruction matches their model, etc. I

frankly consider this dishonest, at best!

If what Stefan says is true (that the entire long-term trend, including the cold LIA in the model, is all due to the spinup problem), then it completely invalidates the use of that model for testing statistical reconstruction methodologies which require physically-consistent patterns of variance in the calibration period to reconstruct the past. But that's a separate issue.

As we now know, the far more damning fact is that Von Storch et al knowingly applied a procedure which is not the MBH98 procedure, and they think they can get away w/ admitting this now in some obscure Italian journal which isn't even in the ISI database.

Tim/Phil/Keith: you may not know about the latter, but Caspar should be able to fill you in on this shortly...

Meanwhile, lets enjoy the media fiesta on MSU...

Mike

Caspar Ammann wrote:

Stefan,

this is very important news indeed. The runs will get a huge hit from this. The only way a coupled model can get a continued trend (without invoking an energy leak somewhere) is when there is a terrible deep-ocean spin up available even for their present day initialization, not to speak about the subsequent shock to pre-industrial conditions.

Did you really say 1.5 degrees? Wow, that is quite a bit. Seems to me they must have used Levitus ocean data with an atmospheric restart file, then hit it with the solar/GHG changes. It seems rather large of a drop to come from a fully coupled stage. 1.5 degrees

is about 30% too large to be exclusively from the atmospheric composition and solar irradiance, thus my suspicion regarding levitus. Now it would be important to know what happens because some people are using the run as a possible real-world scenario (although Hans in talks does not claim so).

Caspar

PS Now, bare in mind that the Science paper applies to the reconstruction, and for the general discussion the influence of spinup should not make that big of a difference (other than inflating the difference of the coldest period to the calibration period, which creates some issues discussed by Mike previously).

Michael E. Mann wrote:

-----  
Subject:

Storch drift

From:

Stefan Rahmstorf <rahmstorf@ozean-klima.de>

Date:

Thu, 11 Aug 2005 15:37:27 +0200

To:

mann@psu.edu

To:

mann@psu.edu

CC:

Gavin Schmidt <gschmidt@giss.nasa.gov>, Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

Hi Mike,

here is some interesting new info on the drift problem in the VS04 runs. Irina Fast and Gerd Bürger submitted a comment about this to Science some months ago; it was rejected and they did not pursue it. I'm trying to encourage them to resubmit this elsewhere. I do not have the ms. but have seen several graphs. There are two key points.

1. The ECHO-G run started at year 900, the VS04 paper of course shows only results starting from year 1000. I've seen the full run now. Between 900 and 1000, the NH temperature drops by about 1.5 °C! That's how severe their initialisation problem is. From my experience of how the THC responds after such step-function changes in forcing, the strong warming from 1050-1150 in VS04 could well be a rebound effect from the 1.5 °C cooling that precedes it, since the THC tends to oscillate on such a time scale when forced rapidly.

2. Irina has run ECHO-G initialised with modern climate and then switching to pre-industrial conditions similar to the run shown by VS04, but without any further variability in the forcing. Thus, this shows the pure drift from initialising this run - this is what Tim has been estimating in MAGICC. The actual drift in ECHO-G is even larger and more persistent than what Tim found: there is a cooling between the years 1000 and 2000 of over 0.6 °C, and this is an almost linear trend over the whole time.

I.e., not just drifting during the first few centuries, but over the entire 1000-year period.

Cheers, Stefan

--

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--

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## References

1. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
2. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>
3. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: Phil Jones <p.jones@uea.ac.uk>  
To: Ben Santer <santer1@llnl.gov>, wigley@ucar.edu  
Subject: Last week's events  
Date: Mon Aug 22 16:22:28 2005

Ben and Tom,

Congratulations on the paper coming out on Aug 12.  
I did talk to Nature about the three papers.

Last week seems to have been a good one to have had off.  
I did this because of the IPCC submission deadline of Aug 12.  
As you said Tom, there were some stupid messages going  
around. If only these people would try and write peer-review  
papers, provided they get proper reviews. The one from  
Sonia should be kept as it proves that E&E is not a  
proper journal.

I almost missed the one with Pielke's resignation in. Is this  
going to make your CCSP task easier or harder? Presumably  
now you'll get all his comments to officially deal with. Maybe  
you'll be able to ignore them?

Cheers

Phil

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University of East Anglia  
Norwich Email p.jones@uea.ac.uk  
NR4 7TJ  
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---

From: Phil Jones <p.jones@uea.ac.uk>

To: mann@psu.edu, Christoph Kull <christoph.kull@pages.unibe.ch>

Subject: Re: PAGES/CLIVAR workshop

Date: Thu, 25 Aug 2005 14:28:41 +0100

Cc: Keith Briffa <k.briffa@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, Heinz Wanner <wanner@giub.unibe.ch>, Thorsten Kiefer <thorsten.kiefer@pages.unibe.ch>

<x-flowed>

Christoph,

It also looks OK to me. The bit highlighted in blue, should probably say something like ...identify the key issues.

I agree with Mike that the last two names on the list should be removed.

I have sent an email about the 4th meeting of IPCC, which I think is June 26-30, 2006. Just checking it is still that week, so there won't be a clash.

Cheers

Phil

At 13:40 25/08/2005, Michael E. Mann wrote:

>Dear Christoph,

>

>Looks pretty good to me. Only one issue. In our discussion of possible  
>participants in Bern, I think (someone correct me if I'm wrong) we  
>concluded that the last two on the list (w/ question marks) would be  
>unwise choices because they are likely to cause conflict than to  
>contribute to concensus and progress. A preferred alternative who was  
>mentioned was Simon Tett (though, it was pointed out, he may not be able  
>to participate for other reasons). We also noted that both Keith B. and  
>Tim. O are in the same European project as the two individuals in  
>question, and could adequately (better, in my opinion) represent any  
>contributions to the discussion from that project.

>

>mike

>

>Christoph Kull wrote:

>

>>Dear Phil, Keith, Mike and Heinz,

>>After dealing with the PAGES OSM the past weeks I made an attempt to  
>>finalize our "Past Millennia Workshop Concept" in order to contact CLIVAR as  
>>soon as possible for requesting support.

>>I incorporated your comments and suggestions in a balanced way and hope that

>>finally all of you may agree to the presented attached draft.

>>

>>Please get back to me with final remarks by Monday next week. I will

>>afterwards contact the CLIVAR office.

>>

>>All the best, thanks a lot for your cooperation and help!

>>Looking forward setting up a hopefully successful project.

>>Christoph

>

>

>--

>Michael E. Mann

>Associate Professor

>Director, Earth System Science Center (ESSC)

>

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>503 Walker Building FAX: (814) 865-3663

>The Pennsylvania State University email: mann@psu.edu

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>

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Norwich Email p.jones@uea.ac.uk

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</x-flowed>

From: "Michael E. Mann" <mann@meteo.psu.edu>  
To: Phil Jones <p.jones@uea.ac.uk>  
Subject: Re: PAGES/CLIVAR workshop  
Date: Fri, 26 Aug 2005 10:52:32 -0400  
Reply-to: mann@psu.edu  
Cc: Heinz Wanner <wanner@giub.unibe.ch>, Christoph Kull  
<christoph.kull@pages.unibe.ch>, Keith Briffa <k.briffa@uea.ac.uk>,  
"Michael E. Mann" <mann@virginia.edu>, Thorsten Kiefer  
<thorsten.kiefer@pages.unibe.ch>

Dear Phil et al,  
I agree on Mike Evans. I'm afraid I don't agree on Zorita. He has engaged in some very nasty, and in my opinion unprofessional email exchanges with some close colleagues of mine who have established some fundamental undisclosed errors in work he co-published with von Storch. Given this, I don't believe he can be involved in constructive dialogue of the sort we're looking for at this workshop. There are some similarly problematic issues w/ Cubasch, who like von Storch, who has engaged in inflammatory and ad hominem public commentary. There is no room for that on any side of the debate. If the Germans need to be represented here, I would suggest instead someone from the Potsdam group, such as Eva Bauer, who has been doing some very interesting work on modelling the climate of the past 2K,  
mike  
Phil Jones wrote:

Christoph,  
I have checked with IPCC and their 4th meeting is in the June 26-30 week in Bergen..  
As for Heinz's suggestions  
- Mike Evans would be OK  
- I'm nor sure that Mikami would contribute much  
See Keith's comment on Zorita  
Cheers  
Phil  
At 14:39 26/08/2005, Heinz Wanner wrote:

Dear Christoph,  
  
I have only a few additional comments concerning the planned workshop.  
  
First of all, I support this concept. Related to the topics, I heavily support to organize a discussion about how we can reconstruct different parameters independently.

It is important to try to reconstruct air pressure as a basic circulation parameter - if possible.

Concerning the participants:

- Write GooSSe;
  - Mikami from Japan (Tokyo Metropolitan University) could be an interesting Asian participant;
  - You mentioned Kevin Trenberth or Mark Cane. Both are absolutely okay, but why not invite a younger colleague like Mike Evans from Tucson?
  - If Phil and Mike do not support von Storch it does not make sense to invite him (and Eduardo Zorita?);
  - For me Ulrich Cubasch is an interesting modeler with good ideas about paleomodeling.
- Maybe Gavin can comment this when he is back from his China trip?

Cheers, Heinz

-----  
 -----  
 Dr. Heinz Wanner  
 Prof., Director NCCR Climate  
 -----

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Climate:

Office NCCR

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38

[1]www.giub.unibe.ch/klimet/  
climate.unibe.ch

[2]www.nccr-

[3]wanner@giub.unibe.ch  
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[6]<http://www.evsc.virginia.edu/faculty/people/mann.shtml>

#### References

1. <http://www.giub.unibe.ch/klimet/>
2. <http://www.nccr-climate.unibe.ch/>
3. <mailto:wanner@giub.unibe.ch>
4. <mailto:p.jones@uea.ac.uk>
5. <mailto:mann@psu.edu>
6. <http://www.evsc.virginia.edu/faculty/people/mann.shtml>

From: "Heinz Wanner" <wanner@giub.unibe.ch>

To: "Christoph Kull" <christoph.kull@pages.unibe.ch>

Subject: PAGES/CLIVAR workshop

Date: Fri, 26 Aug 2005 15:39:22 +0200

Cc: "Phil Jones" <p.jones@uea.ac.uk>, "Keith Briffa" <k.briffa@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, "Thorsten Kiefer" <thorsten.kiefer@pages.unibe.ch>

Dear Christoph,

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First of all, I support this concept. Related to the topics, I heavily support to organize a discussion about how we can reconstruct different parameters independently. It is important to try to reconstruct air pressure as a basic circulation parameter - if possible.

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- If Phil and Mike do not support von Storch it does not make sense to invite him (and Eduardo Zorita?);
- For me Ulrich Cubasch is an interesting modeler with good ideas about paleomodeling. Maybe Gavin can comment this when he is back from his China trip?

Cheers, Heinz

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-----  
Dr. Heinz Wanner

Prof., Director NCCR Climate

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[1][www.giub.unibe.ch/klimet/](http://www.giub.unibe.ch/klimet/)

[2][www.nccr-climate.unibe.ch](http://www.nccr-climate.unibe.ch)

[3][wanner@giub.unibe.ch](mailto:wanner@giub.unibe.ch)

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References

1. <http://www.giub.unibe.ch/klimet/>
2. <http://www.nccr-climate.unibe.ch/>
3. <mailto:wanner@giub.unibe.ch>

From: Keith Briffa <k.briffa@uea.ac.uk>

To: t.m.melvin@uea.ac.uk

Subject: Polar Urals

Date: Fri Sep 23 12:01:27 2005

Tom,

Can you crossdate these two series (trw and mxd) for the Polar Urals?  
Particularly check the 1032 value when only 3 samples.

Found this on the blogg site that Tim sent round. Whatever you do,  
don't respond on the blogg.

Cheers

Phil and Keith

--

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Climatic Research Unit  
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Fax: +44-1603-507784

[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

## References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Jonathan Overpeck <jto@u.arizona.edu>  
To: Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>  
Subject: Re: Fwd: Re: [Fwd: Re: Chapter 6 - Submitted Papers]  
Date: Sat, 24 Sep 2005 22:10:05 -0600  
Cc: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk, Eystein Jansen <eystein.jansen@geo.uib.no>

Hi all - let's see what Keith/Tim say about both papers. Eystein - can you call them on Monday if we haven't heard from them. If they don't have one or both of the papers, then we should ask Martin to delete from the chapter - Eystein, feel free to do this as soon as you get feedback from Keith/Tim. Mysterious...

Thanks, Peck

Date: Fri, 23 Sep 2005 13:14:19 +0200  
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
From: Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>  
Subject: Re: [Fwd: Re: Chapter 6 - Submitted Papers]  
Cc:  
Bcc:  
X-Attachments:

eystein-peck,

I think we agreed that the Wilson paper should be deleted, but i don't know why its still in there. The Briffa paper is new to me (i think).

Cheers,

Øyvind

Hi Keith, see correspondance below. Just to make sure. is the Briffa et al. paper submitted, or should it be deleted from the FOD? The ref to the Wilson et al. paper I assume comes from Peck/Julie, who can handle the issue. Right, Peck?

Eystein

Envelope-to: eystein.jansen@geo.uib.no  
Date: Thu, 22 Sep 2005 18:05:33 -0600  
To: eystein.jansen@geo.uib.no, jto@u.arizona.edu  
From: Martin Manning <mmanning@al.noaa.gov>  
Subject: [Fwd: Re: Chapter 6 - Submitted Papers]  
Cc: ssolomon@al.noaa.gov, ipcc-wg1@al.noaa.gov

X-checked-clean: by exiscan on alf  
X-UiB-SpamFlag: NO UIB: 1.8 hits, 8.0 required  
X-UiB-SpamReport: spamassassin found;  
0.8 BODY: Contains 'Dear (something)'  
1.0 BODY: Claims you can be removed from the list  
0.1 BODY: Message is 30% to 40% HTML  
0.0 BODY: HTML included in message

Dear Eystein and Peck

Following the release of the first draft of the WG1-AR4 we have had a response from Steve McIntyre (a name that should ring a bell) regarding unpublished literature in Chapter 6. He also asks about access to data sets but that is not an IPCC function so is easily dealt with.

The unpublished papers that he has picked up as not being available are:

Briffa, K.R., T.M. Melvin, V.V. Shishov, and et. al, 2005: Warm season temperatures across northern Eurasia: a 2000-year tree-ring based study. Quaternary Science Reviews(In preparation).

and

Wilson and al. 2005 (mentioned on page 6-31)

The first of these was I think meant to be deleted from the text here and we may have made an error in missing that. The second is cited but does not appear in the reference list so we did not pick it up as an unpublished paper that needed to be collected.

Could you please let me know:

- 1) are drafts for either of these papers available yet and if so can you send copies to the TSU?
- 2) how do you expect to use these references in the second draft - remembering that we can only use papers that are in press at that time and that the Briffa et al paper is used quite a bit - e.g. on page 6-29.

I am attaching the correspondence with McIntyre below for your information but the only issues you need to consider are those above, and we will handle any further interactions with McIntyre from here.

Thanks

Martin

Date: Tue, 20 Sep 2005 16:42:00 -0600

From: IPCC-WG1 <ipcc-wg1@al.noaa.gov>

User-Agent: Mozilla/5.0 (Macintosh; U; PPC Mac OS X Mach-O; en-US; rv:1.4)

Gecko/20030624 Netscape/7.1

X-Accept-Language: en-us, en

To: martin Manning <mmanning@al.noaa.gov>

Subject: [Fwd: Re: Chapter 6 - Submitted Papers]

X-Rcpt-To: <mmanning@aztec.al.noaa.gov>

X-DPOP: Version number suppressed

----- Original Message -----

Subject: Re: Chapter 6 - Submitted Papers

Date: Tue, 20 Sep 2005 13:30:52 -0400

From: Steve McIntyre [1]<stephen.mcintyre@utoronto.ca>

To: IPCC-WG1 [2]<ipcc-wg1@al.noaa.gov>

References: <026101c5bd56\$fbafb280\$6402a8c0@herbert> [3]<432F2687.3030101@al.noaa.gov>

<029101c5bd95\$4d2ae240\$6402a8c0@herbert> [4]<43303CC7.7080401@al.noaa.gov>

It's possible that the references were inadvertently left in, in which case your suggestion that a comment be pointed on the review form would obviously suffice. However, it's equally possible that the authors intend to use these references and they inadvertently failed to post them up on the website. If the latter, then they should ask the authors to post up the references. Could you verify which applies with the authors and, if the latter, take appropriate steps.

Additionally, I have attempted to locate van Ommen, Annals of Glaciology, 39, mentioned in the same section. Can you confirm that this volume has either been printed or made available electronically (as I am presently unable to locate wither). If not, then this should be made available in a pdf form at the website.

I have been unable to locate supplementary information or data archives for several of the articles posted at the pdf location for Chapter 6 and would appreciate assistance in this regard.

1) Hegerl et al, submitted. Can you provide me with an ftp location for the proxy data used in this study (which does not even list the proxies used) or post it at your website.

2) D'Arrigo et al, submitted. Again, this data has not been archived at WDCP. Can you provide me with an ftp location for the proxy data used in this study or post it at your website.

Similarly, the SI to Rutherford et al, 2005 does not contain the Briffa et al. data set. Again can you provide an ftp location for this dataset or otherwise provide it.

Thank you for your attention, Steve McIntyre

----- Original Message -----

From: [5]IPCC-WG1

To: [6]Steve McIntyre

Sent: Tuesday, September 20, 2005 12:45 PM

Subject: Re: Chapter 6 - Submitted Papers

Dear Dr McIntyre,

It would seem that the authors may have inadvertently left in the Wilson et al. and Briffa et al. citations, as I do not have copies of the preprints for either. I apologize for the discrepancy and have made note of this error for the authors for correction of the next draft, but if you would also like to comment on this in your review, please do so.

Best regards,

Melinda Tignor

Steve McIntyre wrote:

Thanks for the directions. I found 5 of them there. I was still unable to locate Briffa et al, 2005 (QSR in prep) or Wilson and al. 2005 (mentioned on page 6-31). Could you take a look for them. Thanks.

----- Original Message -----

From: [7]IPCC-WG1

To: [8]Steve McIntyre

Sent: Monday, September 19, 2005 4:58 PM

Subject: Re: Chapter 6 - Submitted Papers

Dear Mr McIntyre,

As mentioned on the Reviewer website, copies of unpublished literature may be downloaded for your review. Papers for each chapter are found at the same website:

URL: [9]<http://ipcc-wg1.ucar.edu/restricted/review/FOR/>

UserName: WG1-FOR

Password: 2005Nov04

Once inside the site, click on "Download Draft Chapters" on the left side of the page.

From here, scroll down the page where you downloaded the chapter to the last sentence,

"If you wish to see copies of unpublished papers cited in the draft chapters click here"

- click "here" and you will be taken to another page listing all the chapters with

unpublished literature and from clicking on the individual chapter you will be taken to

the list of unpublished literature.

Please let me know if I can be of further assistance.

Best regards,

Melinda Tignor

WGI TSU

Steve McIntyre wrote:

Dear Sirs,

The covering literature indicated that the website would provide access to submitted, in-press, or otherwise unpublished papers and reports that are cited in the draft WG I report. In connection with Chapter 6, I was unable to locate the following:

Briffa et al, 2005. (presumably this is the paper denoted as in prep. in the bibliography)  
D'Arrigo et al, submitted  
Hegerl et al, submitted  
Smerdon et al 2005. JGR (in review)  
Tett et al, submitted. Clim. Dyn. submitted.  
Wahl and Ammann 2004. (in review)  
Wilson and al, 2005 (referred to on page 6-31, but not in bibliography).

Could you please post these on the website or email me pdf's. For these unpublished articles, could you also provide locations of FTP sites where the underlying data may be reviewed.

Thank you for your attention,  
Stephen McIntyre

--  
~~~~~

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References

1. <mailto:stephen.mcintyre@utoronto.ca>
2. <mailto:ipcc-wg1@al.noaa.gov>
3. <mailto:432F2687.3030101@al.noaa.gov>
4. <mailto:43303CC7.7080401@al.noaa.gov>
5. <mailto:ipcc-wg1@al.noaa.gov>
6. <mailto:stephen.mcintyre@utoronto.ca>
7. <mailto:ipcc-wg1@al.noaa.gov>
8. <mailto:stephen.mcintyre@utoronto.ca>
9. <http://ipcc-wg1.ucar.edu/restricted/review/FOR/>
10. <mailto:ipcc-wg1@al.noaa.gov>
11. <mailto:ipcc-wg1@al.noaa.gov>
12. <mailto:ipcc-wg1@al.noaa.gov>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Phil Jones <p.jones@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>
Subject: McIntyre and D'Arrigo et al (submitted)
Date: Thu, 29 Sep 2005 09:20:00 +0100
Cc: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Dear Phil, Eystein and Peck,

I've already talked about this to Phil and Keith, but for Eystein's and Peck's benefit the emails copied below relate to McIntyre downloading a PDF of a manuscript cited by the IPCC paleo chapter and then apparently trying to interfere with the editorial process that the paper is currently going through at JGR.

I think this is an abuse of McIntyre's position as an IPCC reviewer.

Rosanne replied to my email below, to say that they **do** want this taken further. So...

Phil has agreed to forward these messages to Susan Solomon and Michael Manning.

Eystein and Peck: do you want to add anything too?

Cheers

Tim

>Date: Wed, 28 Sep 2005 09:08:22 +0100
>To: "Rob Wilson" <rob.wilson@ed.ac.uk>, "Rosanne D'Arrigo"
><druidd@ldeo.columbia.edu>
>From: Tim Osborn <t.osborn@uea.ac.uk>
>Subject: Re: Fw: D'Arrigo et al, submitted
>Cc: <K.briffa@uea.ac.uk>
>
>Dear Rob and Rosanne,
>
>I strongly agree that this is an abuse of his position as IPCC
>reviewer! The data archiving issues are a separate issue because I
>think there's no need for the data you used to be publicly available
>until the paper is actually published, and I would hope that the
>editor would respond appropriately. But the other comments could
>clearly influence the editorial/review process and this is very
>unfair when your paper has already been reviewed by
>others. McIntyre could of course submit a comment after your paper
>was published if he wished to criticize certain aspects, and that is
>the route he should have followed. He tried to stop publication of
>a paper that I was a co-author on, Rutherford et al. (2005), by
>contacting the editor of J. Climate with various criticisms -
>fortunately the editor told him firmly that the route to take was to

>submit a comment after publication. However, in our case the paper
>was already in press. In your case, with the editor's decision
>still to be made, there is clearly more scope for McIntyre to
>influence the decision in your case - and this certainly should not happen.

>
>The conditions which McIntyre (and all other IPCC reviewers) agreed
>to before downloading your manuscript were:

>
>"This site also provides access to copies of some submitted,
>in-press, or otherwise unpublished papers and reports that are cited
>in the draft WG I report. All such material is made available only
>to support the review of the IPCC drafts. These works are not
>themselves subject to the IPCC review process and are not to be
>distributed, quoted or cited without prior permission from their
>original authors in each instance."

>
>I don't think that contacting the journal editor with criticisms is
>"only to support the review of the IPCC drafts".

>
>I will take this issue up with the chapter lead authors and the WG1
>technical support unit - unless you prefer that I didn't. Please let me know.

>
>Cheers

>
>Tim

>
>At 08:33 28/09/2005, Rob Wilson wrote:

>>Hi Tim and Keith,
>>please see the e-mail (below) from Steve Macintyre to the Editor of JGR.

>>
>>This seems a major abuse of his position as reviewer for IPCC?

>>
>>In some respects, I don't mind having to address his comments (many
>>of which are already adequately explained I think, although a
>>detailed list of all data used could certainly go in an
>>appendix), but this just seems a bit off. After all, we have
>>addressed the reviewers comments and are currently awaiting a
>>decision. This e-mail may effect the decision greatly.

>>
>>Is he going to do this for all papers he does not quite agree with.

>>
>>comments?

>>
>>Rob

>>
>>-----

>>
>>
>>>From: "Steve McIntyre"

>>><<mailto:stephen.mcintyre@utoronto.ca>stephen.mcintyre@utoronto.ca>

>>>To: "Colin O'Dowd" <<mailto:jgr@nuigalway.ie>jgr@nuigalway.ie>

>>>Cc: "Rob Wilson"

>>><<mailto:rjwilson_dendro@blueyonder.co.uk>rjwilson_dendro@blueyonder.co.uk>,>

>>> "Rosanne D'Arrigo"

>>><<mailto:druidrd@ldeo.columbia.edu>druidrd@ldeo.columbia.edu>

>>>Subject: D'Arrigo et al, submitted

>>>Date: Tue, 27 Sep 2005 10:37:06 -0400

>>>Dear Dr O'Dowd,

>>>I am a reviewer for the IPCC Fourth Assessment Report (IPCC 4AR)

>>>and am writing in respect to a submission to your journal by

>>>D'Arrigo et al., entitled "On the Long-Term Context for Late 20th

>>>Century Warming." This article was referenced in chapter 6 of the

>>>Draft IPCC 4AR and made available to IPCC reviewers. In the course

>>>of my review, I contacted the senior author, Dr. D'Arrigo, for the

>>>FTP location of the data used in this article or for alternative

>>>access to the data. Dr D'Arrigo categorically refused and I was

>>>referred to the journal editor if I desired recourse.

>>>

>>>

>>>Data Citation and Archiving

>>>I point out that AGU policies for data citation and data archiving

>>>(<http://www.agu.org/pubs/data_policy.html>http://www.agu.org/pubs/data_policy.html

>>>) specifically require that authors provide data citation

>>>according to AGU standards and require that contributors archive

>>>data in permanent archives, such as the World Data Center for

>>>Paleoclimatology. For example, the policy states:

>>>

>>>

>>>1. Data sets cited in AGU publications must meet the same type of

>>>standards for public access and long-term availability as are

>>>applied to citations to the scientific literature. Thus data cited

>>>in AGU publications must be permanently archived in a data center ...

>>>2. Data sets that are available only from the author, through

>>>miscellaneous public network services, or academic, government or

>>>commercial institutions not chartered specifically for archiving

>>>data, may not be cited in AGU publications.

>>>

>>>

>>>On page 21 of D'Arrigo et al., there is a listing of "regional

>>>groupings" of data. In some cases, part of the data is archived at

>>>WDCP; in other cases, the data has been collected by the authors,

>>>but has not been archived.

>>>

>>>

>>>In cases, where the data has been archived, it has not been cited

>>>according to AGU policies. For example, the Torntraesk site is

>>>presumably swed019w, but this is not stated. The Polar Urals site

>>>appears to be a combination of russ021w, russ176w and russ022w,

>>>but this is not stated. The Quebec site appears to be a version of

>>>cana036, but a version that differs from the one archived, as it

>>>includes more series. The "Mongolia" site appears to be the

>>>authors' mong003 site, but a different version than the one

>>>archived (which commences at a different date). The "Yukon" series
>>>is a combination of two sites, which are not stated. At least one
>>>of the sites is a different version from the one archived. The
>>>Icefields site is again a different version than the one archived.
>>>Other data sets e.g. Seward, NW North America, Central Alaska,
>>>Wrangells, Coast Alaska, Central NWT, Southern Alaska, have been
>>>collected by the authors and are either not archived at all or
>>>archived in obsolete versions.

>>>
>>>

>>>In order that this submission comply with AGU policies on data
>>>archiving, I request that you require D'Arrigo et al. do (1)
>>>provide accurate data citations complying with AGU policies for
>>>all data sets presently archived at WDCP; (2) archive all "grey"
>>>data used in the article.

>>>
>>>

>>>Methodology

>>>The results of this article depend on methodological details,
>>>especially as to standardization procedures. However, these
>>>procedures are not described in objective or operational terms. I
>>>will illustrate some examples below:

>>> Page 21 – "In select cases, a power transform (PT) was applied
>>> to correct for data biases. This bias was assessed by correlation
>>> and residual analysis against both local and large scale
>>> temperature series." In which cases was PT applied and what were
>>> the objective criteria in the correlation and residual analysis,
>>> which were used to determine whether this should be applied.

>>>
>>>
>>>

>>>Page 21 – "Due to differing populations in the TR data, the
>>>data-sets were often grouped into 'common' populations. No one
>>>strategy is appropriate for all data-sets and careful evaluation
>>>of each composite data-set was made." That's nice, but what were
>>>the operational criteria which were used to allocate each case to
>>>the 5 different alternative procedures.

>>>
>>>

>>>Page 7 – "The standard error of the regression estimate (standard
>>>deviation of the regression residuals) from the full period
>>>calibration was used to generate the 2 sigma error bars and this
>>>was also adjusted (inflated) to account for the change (decrease)
>>>in explained variance in each nest." – The last adjustment is not
>>>described in operational terms. Shouldn't the standard error be
>>>realistically measured by the standard deviation from the
>>>verification period residuals?

>>>
>>>

>>>Page 20. "Successful modeling of paleoclimate data with the high
>>>temperatures of the late 1990s is essential if we are to make

>>>robust, definitive conclusions about past temperature amplitudes
>>>and variability." Abstract – "presently-available paleoclimatic
>>>reconstructions are inadequate for making specific inferences, at
>>>hemispheric scales, about MWP warmth relative to the anthropogenic
>>>period and that such comparisons can only still be made at the
>>>local/regional scale." Page 13. "After this period [mid-1980s],
>>>the divergence between the tree-ring and instrumental data results
>>>in weakening of calibration results and failed verification
>>>statistics". The authors contradict these caveats by proceeding to
>>>make a variety of inferences and claims "at hemispheric scales"
>>>about MWP warmth or lack thereof relative to the modern period. A
>>>comparison of their reconstruction to instrumental temperatures is
>>>prominently made in the Abstract, on page 10 and page 14. If the
>>>reconstructions are inadequate for making these inferences, then
>>>don't make them.

>>>

>>>

>>>Thank you for your consideration,

>>>

>>>

>>>Yours truly,

>>>Stephen McIntyre

>>>

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: mann@psu.edu, Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: heads up...
Date: Tue Nov 15 17:47:53 2005
Cc: Phil Jones <p.jones@uea.ac.uk>

Mike
thanks for this. When time allows we will do a response to this poster and simply post it on our web page. As others have said, the dating of the chronology in the Urals is not wrong - but the magnitude of the extreme years in the early Urals reconstruction were not adjusted to account for inflated variance related to low chronology replication - so they are sort of right that the emphasis on 1032 is probably overdone.

Anyway thanks again

Keith

At 15:29 15/11/2005, Michael E. Mann wrote:

Thanks Tim, Phil

yes, I never had any doubt he's wrong. In fact he's been wrong about just about every claim he's ever made. He almost had a point w/ the PCA centering, but as we all know, that doesn't matter at all in the end. The issue isn't whether or not he's right, as we all well know by now, but whether his false assertions have enough superficial plausability to get traction. In this case, they might, so probably good to at least be prepared.

I was told by a journalist Paul Thacker that his poster got prominent placement, probably not an accident (see forwarded email). I believe that Mike Schlesinger and David Karoly were there in the same session, so might be worth checking w/ them. I think Connie Woodhouse and Tom Wigley were also at the meeting, but not sure...

I suspect that this is the first in a line of attacks (I'm sure Tom C is next in line) that will ultimately get "published" one way or another. The GRL leak may have been plugged up now w/ new editorial leadership there, but these guys always have "Climate Research" and "Energy and Environment", and will go there if necessary.

They are telegraphing quite clearly where they are going w/ all of this...

Mike

Tim Osborn wrote:

Thanks for this Mike. We'd spotted an earlier draft of his poster and were a bit concerned about this receiving prominence at the meeting.

Did it arouse much discussion, do you know? Keith and Tom Melvin looked into the dating a while back when McIntyre first raised it and were quite satisfied with the published dating I think. Not sure what should be done - unless he submits something for peer-review. Cheers, Tim

At 14:53 15/11/2005, Michael E. Mann wrote:

not sure if you guys are aware, McIntyre presented this poster at the CCSP meeting. Apparently, they gave him a very prominent location, so that everyone entering the meeting would have seen the poster...

mike
can find at:
<[1]http://www.climatescience.gov/workshop2005/abstracts/p-gc-1.htm>http://www.climatescience.gov/workshop2005/abstracts/p-gc-1.htm

P-GC1.4

More on Hockey Sticks: The Case of Jones et al. [1998]

Stephen McIntyre, <[2]mailto:stephen.mcintyre@utoronto.ca>stephen.mcintyre@utoronto.ca

Multiproxy studies purporting to show 20th century uniqueness have been applied by policymakers, but they have received remarkably little independent critical analysis.

Jones et al. [1998] is a prominent multi-proxy study used by IPCC [2001] and others to affirm the hockey stick shaped temperature reconstruction of Mann et al. [1998].

However, the reconstruction of Jones et al. [1998] is based on only 3-4 proxies in the

controversial Medieval Warm Period, including non-arms-length studies by Briffa et al. [1992] and Briffa et al [1995]. We show that the Polar Urals data set in Briffa et al [1992] fails to meet a variety of quality control standards, both in replication and crossdating. The conclusion of Briffa et al. [1995] that 1032 was the "coldest year" of the millennium proves to be based on inadequate replication of only 3 tree ring cores, of which at least 2 are almost certainly incorrectly crossdated. We show that an ad hoc adjustment to the Tornetrask data set in Briffa et al [1992] cannot be justified. The individual and combined impact of defects in the Polar Urals data set and Tornetrask adjustments on the reconstruction of Jones et al [1998] is substantial and can be seen to have the effect of modifying what would otherwise indicate a pronounced Medieval Warm Period in the proxy reconstruction. Inhomogeneity problems in the Polar Urals and Tornetrask data sets, pertaining to altitude, minimum girth bias and pith centering bias will also be discussed.

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References

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2. <mailto:stephen.mcintyre@utoronto.ca>

3. <mailto:mann@psu.edu> % 3E mann@psu.edu
4. <http://www.met.psu.edu/dept/faculty/mann.htm>
5. <http://www.met.psu.edu/dept/faculty/mann.htm>
6. <http://www.cru.uea.ac.uk/~timo/>
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8. <http://www.met.psu.edu/dept/faculty/mann.htm>
9. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Caspar Ammann <ammann@ucar.edu>

Subject: Re: IPCC ref. regarding McIntyre and McKittrick

Date: Wed, 30 Nov 2005 09:21:37 -0700

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

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Thanks Caspar. This is good news. Please keep us posted. Best, Peck

>Hi everybody,

>

>just a quick update that I got word from the Chief Editor of GRL
>(Jay Famiglietti) that our comment in GRL about the MM paper earlier
>this year has finally been accepted. They are now soliciting a
>response from McIntyre and McKittrick, but that should now move
>rather quickly. No official word on the Climatic Change paper just
>yet.

>

>Cheers,

>Caspar

>

>PS Here the full references:

>

>Ammann C.M., and E.R. Wahl, accepted: Comment on "Hockey sticks,
>principle components, and spurious significance" by S. McIntyre and
>R. McKittrick, Geophys. Res. Lett., accepted.

>

>Wahl, E.R and C.M. Ammann, revised: Robustness of the Mann, Bradley,
>Hughes reconstruction of surface temperatures: Examination of
>criticisms based on the nature and processing of proxy climate
>evidence. Climatic Change, revised and in review.

>

>

>--

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From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: [Fwd: u seen?]
Date: Wed, 30 Nov 2005 11:04:40 -0500
Reply-to: mann@psu.edu
Cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

fair enough, I'll go w/ flimsy. The real problem is the fairly inflammatory wording of this, and the really flawed interpretations w.r.t. implicatinos for natural vs. anthropogenic variaibility.

normally I'd ignore, but the fact that Andy Revkin received this suggests they are trying to publicize this review paper, which I find a bit odd...

mike

Tim Osborn wrote:

> Hi Mike,
>
> I've seen this before (and probably Keith has too) because our EU
> "SOAP" project supported Rob Wilson, the second author. I'd say that
> it is "flimsy" rather than "shoddy"! Still, it's only supposed to be
> a "viewpoint" rather than new science.
>
> Tim
>
> At 15:31 30/11/2005, Michael E. Mann wrote:
>
>> thought you guys would be interested. pretty shoddy stuff in my view...
>>
>> mike
>>
>> --
>> Michael E. Mann
>> Associate Professor
>> Director, Earth System Science Center (ESSC)
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>> Department of Meteorology Phone: (814) 863-4075
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>> The Pennsylvania State University email: mann@psu.edu
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>> Return-Path: <anrevk@nytimes.com>
>> X-Original-To: mann@meteo.psu.edu
>> Delivered-To: mann@meteo.psu.edu
>> Received: from tr12n04.aset.psu.edu (tr12g04.aset.psu.edu
>> [128.118.146.130])
>> by mail.meteo.psu.edu (Postfix) with ESMTP id 2027520401A
>> for <mann@meteo.psu.edu>; Wed, 30 Nov 2005 10:15:10 -0500 (EST)
>> Received: from nytimes.com (nat-hq-gate-02.nytimes.com
>> [199.181.175.222])
>> by tr12n04.aset.psu.edu (8.13.2/8.13.2) with ESMTP id
>> jAUFF8P22437280
>> for <mann@psu.edu>; Wed, 30 Nov 2005 10:15:08 -0500
>> Message-Id: <6.1.2.0.2.20051130101420.02d14460@smtp-store.nytimes.com>
>> X-Sender: anrevk@smtp-store.nytimes.com
>> X-Mailer: QUALCOMM Windows Eudora Version 6.1.2.0
>> Date: Wed, 30 Nov 2005 10:14:45 -0500
>> To: mann@psu.edu
>> From: Andy Revkin <anrevk@nytimes.com>
>> Subject: u seen?
>> Mime-Version: 1.0
>> Content-Type: multipart/alternative;
>> boundary="====_79165303===.ALT"
>> X-NYTOriinatingHost: , 10.149.64.222
>> X-Virus-Scanned: amavisd-sophos
>> X-PSU-Spam-Flag: NO
>> X-PSU-Spam-Hits: 0.695
>> X-PSU-Spam-Level: *
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>> X-Spam-Level:
>> X-Spam-Status: No, score=-1.6 required=5.0
>> tests=AWL,BAYES_00,HTML_00_10,
>> HTML_MESSAGE,MIME_QP_LONG_LINE autolearn=no version=3.0.2
>>
>> purely fyi.. u seen?

>>

>>

>>> Quaternary Science Reviews, Volume 24, Issues 20-21 , November 2005,

>>> Pages 2164-2166

>>> <http://tinyurl.com/b95ee>

>>>

>>> Climate: past ranges and future changes

>>>

>>> Jan Esper a), Robert J.S. Wilson b), David C. Frank a), Anders

>>> Moberg c), Heinz Wanner d) and Jürg Luterbacher d)

>>>

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>>> b) School of GeoSciences, Grant Institute, Edinburgh University,

>>> Edinburgh, UK

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>>> Sweden

>>> d) NCCR Climate and Institute of Geography, University of Bern, 3012

>>> Bern, Switzerland

>>>

>>> Abstract

>>>

>>> Comparison of large-scale temperature reconstructions over the past

>>> millennium reveals agreement on major climatic episodes, but

>>> substantial divergence in reconstructed (absolute) temperature

>>> amplitude. We here detail several research priorities to overcome

>>> this 'amplitude desideratum', and discuss the relevance of this

>>> effort for the prediction of future temperature changes and the

>>> meaning of the Kyoto protocol.

>>>

>>> Persisting controversy (Regalado, 2005) surrounding a pioneering

>>> northern hemisphere temperature reconstruction (Mann et al., 1999)

>>> indicates the importance of such records to understand our changing

>>> climate. Such reconstructions, combining data from tree rings,

>>> documentary evidence and other proxy sources are key to evaluate

>>> natural forcing mechanisms, such as the sun's irradiance or volcanic

>>> eruptions, along with those from the widespread release of

>>> anthropogenic greenhouse gases since about 1850 during the

>>> industrial (and instrumental) period. We here demonstrate that our

>>> understanding of the shape of long-term climate fluctuations is

>>> better than commonly perceived, but that the absolute amplitude of

>>> temperature variations is poorly understood. We argue that the

>>> knowledge of this amplitude is critical for predicting future

>>> trends, and detail four research priorities to solve this

>>> incertitude: (i) reduce calibration uncertainty, (ii) preserve
>>> 'colour' in proxy data, (iii) utilize accurate instrumental data,
>>> and (iv) update old and develop new proxy data.

>>>
>>> When matching existing temperature reconstructions (Jones et al.,
>>> 1999; Mann et al., 1999; Briffa, 2000; Esper et al., 2002; Moberg,
>>> et al., 2005) over the past 1000 years, although substantial
>>> divergences exist during certain periods, the timeseries display a
>>> reasonably coherent picture of major climatic episodes: 'Medieval
>>> Warm Period', 'Little Ice Age' and 'Recent Warming' (Fig. 1).
>>> However, when calibrated against instrumental temperature records,
>>> these same reconstructions splay outwards with temperature
>>> amplitudes ranging from 0.4 to 1.0 °C for decadal means (Moberg et
>>> al., 2005). Further, a comparison of commonly used regression and
>>> scaling approaches shows that the reconstructed absolute amplitudes
>>> easily vary by over 0.5 °C, depending on the method and instrumental
>>> target chosen (Esper et al., 2005). Overall, amplitude discrepancies
>>> are in the order of the total variability estimated over the past
>>> millennium, and undoubtedly confuse future modelled temperature
>>> trends via parameterisation uncertainties related to inadequately
>>> simulated behaviour of past variability.

>>>
>>> Fig. 1. Course of temperature variations. Large-scale temperature
>>> reconstructions scaled to the same mean and variance over the common
>>> period 1000-1979 AD, and their arithmetic mean. The normalisation
>>> highlights the similarity between the records, but broadly ignores
>>> the differing calibration statistics with instrumental data, and
>>> their particular 'shapes' and distribution of variance, e.g. during
>>> the instrumental and pre-instrumental periods. The average
>>> correlation between the original reconstructions is 0.47, and 0.64
>>> after smoothing (as done in the figure using a 40-year low-pass
>>> filter). Lag-1 autocorrelations range from 0.52 (Jones98) to 0.93
>>> (Moberg05; with no variability <4 years represented).

>>>
>>>
>>> Solutions to reduce calibration uncertainty include the use of
>>> pseudo-proxy experiments (Osborn and Briffa, 2004; von Storch et
>>> al., 2004) derived from ensemble simulations of different models
>>> (Knutti et al., 2002; Stainforth et al., 2005) to test statistical
>>> calibration methods, e.g. principal component (Cook et al., 1994)
>>> and timescale-dependent (Osborn and Briffa, 2000) regression. Such
>>> analyses, however, should mimic the character of empirical proxy
>>> data, e.g. the decline of replication (numbers of sites, quality per

>>> site) back in time, and the addition of noise typical to empirical
>>> proxy data (i.e., not just white; Mann and Rutherford, 2002).
>>> Further, reconstructions from areas such as Europe (Luterbacher et
>>> al., 2004; Xoplaki et al., 2005), where long instrumental series and
>>> high densities of proxy records exist, allow extended calibration
>>> periods and increased degrees of freedom enabling the assessment of
>>> robust relationships at all timescales (i.e., low and high
>>> frequency), both critical to reduce calibration uncertainty.
>>> Subsequent comparison of such regional records with hemispheric
>>> reconstructions that can be downscaled should provide greater
>>> understanding of reconstructed amplitudes at larger spatial scales.
>>>

>>> Accurate preservation and assessment of low-to-high frequency
>>> variation ('colour') in proxy data, and a selected use of certain
>>> frequency bands that best fit those of instrumental data (Moberg et
>>> al., 2005), are further desirable when compiling large-scale
>>> reconstructions that seek to yield the true absolute temperature
>>> amplitude. This approach, however, requires a comprehensive
>>> examination of regional proxy data including the seasonality of
>>> temperature signals, and a selection of only those records that
>>> effectively capture low-frequency climate variation. Inclusion of
>>> regional tree ring records in which long-term trends are not
>>> preserved, should be avoided in efforts to reconstruct low frequency
>>> temperature variations (Esper et al., 2004; Melvin, 2004). In these
>>> data, such limitations primarily occur when age-related biases from
>>> tree-ring series are individually estimated and removed ('the
>>> segment length curse' Cook et al., 1995). Similar considerations
>>> apply to documentary evidence, long isotope records and other proxy
>>> sources that should, on a site-by-site basis, be examined for
>>> potential low-frequency limitations.
>>>

>>> The instrumental target data chosen (Esper et al., 2005), and
>>> adjustments made to these data are also vital to the reconstructed
>>> amplitude. A recent analysis of a carefully homogenised instrumental
>>> network from the Alps and surrounding areas (Böhm et al., 2001), for
>>> example, shows the annual temperature trend over the last ca 110
>>> years to be 1.1 °C-twice that observed over the same alpine
>>> gridboxes in the global dataset provided by the Climatic Research
>>> Unit (Jones et al., 1999). Such changes in the character of
>>> observational data, resulting from homogeneity adjustments and
>>> methodology differences (Moberg et al., 2003), directly affect the
>>> temperature amplitude in proxy-based reconstructions, since
>>> instrumental calibration sets the pulse in these paleorecords

>>> (Büntgen et al., 2005). Accurate instrumental data are therefore
>>> crucial to the reconstructed amplitude, and this again argues for
>>> regional studies where mutual verification between proxy and
>>> instrumental records is viable (Frank and Esper, 2005; Wilson et
>>> al., 2005).

>>>
>>> Finally, more proxy data covering the full millennium and
>>> representing the same spatial domain as the instrumental target data
>>> (e.g., hemisphere) are required to solve the amplitude puzzle. The
>>> current pool of 1000-year long annually resolved temperature proxies
>>> is limited to a handful of timeseries, with some of them also
>>> portraying differing seasonal (e.g., summer or annual) responses.
>>> Furthermore, the strength of many of these local records and
>>> literally all tree ring chronologies varies and almost always
>>> declines back in time (Cook et al., 2004). The reasons are manifold
>>> and include dating uncertainty, loss of signal fidelity in the
>>> recent period, assumptions about signal stationarity, reduction of
>>> sample replication, etc., and are generally not considered in the
>>> uncertainty estimates of combined large-scale reconstructions. Also,
>>> data from the most recent decades, absent in many regional proxy
>>> records, limits the calibration period length and hinders tests of
>>> the behaviour of the proxies under the present 'extreme' temperature
>>> conditions. Calibration including the exceptional conditions since
>>> the 1990s would, however, be necessary to estimate the robustness of
>>> a reconstruction during earlier warm episodes, such as the Medieval
>>> Warm Period, and would avoid the need to splice proxy and
>>> instrumental records together to derive conclusions about recent
>>> warmth.

>>>
>>> So, what would it mean, if the reconstructions indicate a larger
>>> (Esper et al., 2002; Pollack and Smerdon, 2004; Moberg et al., 2005)
>>> or smaller (Jones et al., 1998; Mann et al., 1999) temperature
>>> amplitude? We suggest that the former situation, i.e. enhanced
>>> variability during pre-industrial times, would result in a
>>> redistribution of weight towards the role of natural factors in
>>> forcing temperature changes, thereby relatively devaluing the impact
>>> of anthropogenic emissions and affecting future predicted scenarios.
>>> If that turns out to be the case, agreements such as the Kyoto
>>> protocol that intend to reduce emissions of anthropogenic greenhouse
>>> gases, would be less effective than thought. This scenario, however,
>>> does not question the general mechanism established within the
>>> protocol, which we believe is a breakthrough.

>>>

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>>
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>> Recent Arctic coverage: www.nytimes.com/pages/science/sciencereport
>> Book on the Amazon: The Burning Season (www.islandpress.org/burning)
>> Acoustic-Roots Band: www.sonicbids.com/unclewade
>
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> phone: +44 1603 592089
> fax: +44 1603 507784
> web: <http://www.cru.uea.ac.uk/~timo/>
> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
>

--

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Associate Professor
Director, Earth System Science Center (ESSC)

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The Pennsylvania State University email: mann@psu.edu
University Park, PA 16802-5013

<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: "Raymond S. Bradley" <rbradley@geo.umass.edu>, Malcolm Hughes
<mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa
<k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt
<gschmidt@giss.nasa.gov>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>,
Caspar Ammann <ammann@ucar.edu>
Subject: Esper et al...
Date: Fri, 02 Dec 2005 09:15:09 -0500
Reply-to: mann@psu.edu

<x-flowed>

thought you all would be interested in this. Esper et al have played
right into the hands of the contrarians:

<http://www.foxnews.com/story/0,2933,177380,00.html>

The wording o their abstract is frankly just irresponsible...

Mike

--

Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)

| | |
|-----------------------------------|-----------------------|
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| University Park, PA 16802-5013 | |

<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: Tom Wigley <wigley@cgd.ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: HadCRUT2v
Date: Mon, 12 Dec 2005 15:16:28 -0700
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Ben Santer <santer1@llnl.gov>

<x-flowed>

Phil,

Why is there so much missing data for the South Pole? The period Jan 75 thru Dec 90 is all missing except Dec 81, July & Dec 85, Apr 87, Apr & Sept 88, Apr 89. Also, from and including Aug 2003 is missing.

Also -- more seriously but correctable. The S Pole is just represented by a single box at 87.5S (N Pole ditto I suspect). This screws up area averaging. It would be better to put the S Pole value in ALL boxes at 87.5S.

I have had to do this in my code -- but you really should fix the 'raw' gridded data.

For area averages, the difference is between having the S Pole represent the whole region south of 85S, and having (as now) it represent one 72nd of this region. It is pretty obvious to me what is better.

This affects the impression of missing data too of course.

Tom.

</x-flowed>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: P.Jones@uea.ac.uk, "Tom Wigley" <wigley@cgd.ucar.edu>
Subject: Re: HadCRUT2v
Date: Tue Dec 13 13:07:32 2005
Cc: "Ben Santer" <santer1@llnl.gov>

Dear all,

attached is a plot of the monthly anomalies from the only box with non-missing data in the bottom row of Phil's grid (centred at 87.5 S). This is from HadCRUT2v that I picked up from the CRU data store in June this year.

Clearly the dates Tom listed are missing in my version too. Furthermore, the values from 1971-1975 are abnormal. They are not all identical, but are all near zero. Perhaps multiplied by 0.1?

Similar problems are apparent in HadCRUT and CRUTEM2v too.

But CRUTEM2 has no gaps and no abnormal periods at the South Pole, so perhaps CRUTEM2 is fine? Tom - if it's urgent, you could extract the South Pole time series from CRUTEM2 and use it to overwrite the other 3 data sets until Phil corrects them.

Regarding the weighting issue...

Given that the grid doesn't have equal-area boxes, there are always going to be compromises with weighting. Even if you do something to sort out the problem at the S. Pole, how about the isolated boxes around the coast of Antarctica, which will be given much less weight than an isolated box in the tropics which might also have only 1 station in. This is partly reasonable because of differences in spatial correlation of temperatures between tropics and high latitudes, but I'm sure that they don't compensate exactly.

Specifically for the poles...

Putting the temperature data into a single box will clearly underweight its contribution in area averages (is it significant from a practical point of view once you get to hemispheric or global scales though?).

Replicating it into all boxes in the bottom row will, on the other hand, gives it too much weight. If the area weighting is calculated simply as $\cos(\text{latitude})$ then the South Pole data will be given this weighting:

$$72 * \cos(87.5) = 3.14$$

whereas one box on the equator (or just off) will be given this weighting:

$$1 * \cos(2.5) = 1.00$$

so, if replicated around all boxes at 87.5 S, the South Pole would have three times the weight of a single tropical box (compared with 23 times less weight if South Pole data appears in only one box).

Perhaps put it in every fourth box, giving a weighting of 0.79 (bit less than tropical, which is reasonable for spatial correlation reasons)?

Cheers

Tim

At 04:11 13/12/2005, P.Jones@uea.ac.uk wrote:

Tom,

In NZ at the IPCC meeting. Will be here until Dec 17. When I get back I'm off to Switzerland for Christmas on Dec 21.

The South Pole shouldn't be missing. I have all the data for Amundsen-Scott from 1957. I put the data in at one 5 degree grid box, so it doesn't get overweighted.

The South Pole should be at the last grid box (2592) in the 72 by 36 array. Putting the data in all 87.5-90S boxes would overweight the S.Pole stations.

There isn't any data at the N. Pole.

Maybe Tim could check on the missing S.Pole data. I reckon it should be there in all the datasets CRUTEM2 and HadCRUT2 and the v versions.

Cheers

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> Tom.

>

From: P.Jones@uea.ac.uk
To: jen.hardwick@metoffice.gov.uk
Subject: [Fwd: Re: HadCRUT2v]
Date: Tue, 13 Dec 2005 21:14:30 -0000 (GMT)
Cc: t.osborn@uea.ac.uk, philip.brohan@metoffice.gov.uk

Dear Jen,

There seems to be a problem with the South Pole box (#2592). The data are in CRUTEM2(v) but not in HadCRUT2(v). See the plot and email from Tim Osborn.

Email Tim if you can find what is up. The boxes in the two datasets should be the same.

I'm in NZ at IPCC.

Cheers
Phil

----- Original Message -----

Subject: Re: HadCRUT2v
From: "Tim Osborn" <t.osborn@uea.ac.uk>
Date: Tue, December 13, 2005 1:07 pm
To: P.Jones@uea.ac.uk
"Tom Wigley" <wigley@cgd.ucar.edu>
Cc: "Ben Santer" <santer1@llnl.gov>

Dear all,

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>
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>> Tom.

>>

Dr Timothy J Osborn

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web: <http://www.cru.uea.ac.uk/~timo/>

sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\southpole.gif"

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>, P.Jones@uea.ac.uk
Subject: Re: HadCRUT2v
Date: Wed Dec 14 09:57:27 2005
Cc: Ben Santer <santer1@llnl.gov>

At 21:58 13/12/2005, Tom Wigley wrote:

Phil,

Before you finalize anything, please let me get back to you with some additional thoughts. There are some wrinkles that you and Tim don't seem to have thought of.

Tom.

Tom

One further thing (possibly one of the extra wrinkles?) is that while you could put the S Pole data from CRUTEM2 (where it seems correct) into HadCRUT2, it isn't quite correct to put it (as I wrongly suggested) into CRUTEM2v and HadCRUT2v because those should have their high frequency deviations scaled to remove sample-size-related biases. Only a minor difference.

Tim

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>

Subject: more on TS feedback

Date: Sun, 18 Dec 2005 13:53:11 -0700

<x-flowed>

Dear Keith, Bette and Eystein:

This email should be read after the one to the entire team - it provides post LA3/TS feedback on figures. Since Bette is going on a short vacation, she and I emailed about her new LIG fig before I left, so she's ready to go when she gets home.

Keith (and Tim), on the other hand, have lots to consider, and I just wanted to reiterate to you (and Bette) that it's a priority for me and Eystein to help you brainstorm all these figures. Here are a few more comments I got on Keith/Tim Figs:

For 6.8:

1) removing the oldest portion of the records from the plot is only ok IF:
-we can justify on an obvious and objective basis - for example that sample depth hits goes down significantly at ca. 700AD or wherever we want to chop it.
-We don't remove part of the series that will give rise to accusations of bias
Thus, it might be better to leave as was in the FOD, just to be safe, or to try multiple versions.

2) had a long talk with Martin Manning about the idea of multiple plots, vs just the existing one (by the way, the TS team WANTS the instrumental part of the fig as we agreed to modify in Chap 6 sessions). I think the best idea is to keep the bottom panel as is, with modifications
- keep the error bars as is
- try a version with some sort of annually-resolved volc forcing placed at the top of the panel, with eruption (sufate) lines sticking down farther for big eruptions
- try inserting some representation of average (median? or?) sample depth along the bottom (time) side of the panel. This will thus show, lots of sample depth back to ca. 1700, then less and less (in steps?). Martin suggests we go one step farther and color the sample depth part of the plot with different colors, based on our expert judgement of confidence. We could have two or three colors - one color for the interval overwhich we have "very likely" confidence (e.g., in the exec summary) and another for just "very." perhaps we want a third for some term reflecting "don't trust inferences regarding hemispheric temp that much over this interval" - this will obviously take some thinking/creativity, but this fig will go all the way to the TSM, so it's worth the effort.

3) linear axis for sure

4) if would still be good to try a density shaded version of this

plot (instead of all the recon lines) for the TS and SPM. When in doubt, make an extra version. We can then share with our team and with Susan.

Thanks for doing this!

Also, FYI, Gabe indicated that her regional plots were not scaled separately. Surprising, but maybe the models are actually better than we thought.

Best, Peck

--

Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences

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<http://www.geo.arizona.edu/>
<http://www.ispe.arizona.edu/>
</x-flowed>

From: "David Willans" <david@futerra.co.uk>
To: <training@futerra.co.uk>
Subject: Training Dates
Date: Mon, 19 Dec 2005 18:10:53 -0000

Hello,

Some dates for your new year diary...

Futerra are launching a series of masterclasses on communicating sustainable development in early 2006.

Communicating Climate Change on a Local and Regional Level

12.30 - 5.30pm

Thursday 26 January 2006

Communicating Sustainable Development

12.30 - 5.30pm

Thursday 23 February 2006

Communicating Climate Change

12.30 - 5.30pm

Thursday 30 March 2006

Using international case studies and proven communication tools, each session is designed to build your confidence to plan and implement campaigns.

"Enthusiastic and friendly trainers with a tremendous amount of knowledge" - Past participant

For more information or to book then please see the attached flyer or visit our [1]website.
The groups will be kept to only 15 people, so please sign up early to avoid disappointment.

The Futerra team wish you a very merry Christmas!

David

David Willans

Consultant

Futerra Sustainability Communications Ltd

[2]www.futerra.co.uk

We've moved! Please note new contact details

Direct Dial: +44 (0)20 7378 4003

Switchboard: +44 (0)20 7378 4000

84 Long Lane

London SE1 4AU

Attachment Converted: "c:\eudora\attach\Futerra_Masterclass.pdf"

References

1. http://www.futerracom.org/auto.php?inc=case&site_cat=1&site_sub=17&case=0
2. [outbind://41-00000000C60442BB81504F4199CB74C59420FE1E049E2A00/www.futerra.co.uk](mailto:41-00000000C60442BB81504F4199CB74C59420FE1E049E2A00@www.futerra.co.uk)

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Stefan Rahmstorf <rahmstorf@ozean-klima.de>
Subject: Re: [Wg1-ar4-ch06] Follow-up from Christchurch
Date: Mon, 19 Dec 2005 21:32:37 -0700
Cc: wg1-ar4-ch06@joss.ucar.edu

<x-flowed>

Hi Stefan and team - great. David Rind is getting the solar forcing series de jour (latest Lean). I expect Keith back on line soon, and then he can help us figure out what type of simulation(s) we'd like, and what other forcings we ought to use. My take is that it would be good to use the same forcing used in the runs currently in Fig 6.10 (or at least the "best" of those runs - subjective, I'm sure, and all with the old larger amplitude Lean solar), but with the new reduced amplitude forcing.

Fig 6.10 currently has the Bauer et al, 2003 run w/ CLIMBER - is it CLIMBER2? Could/should we just re-run with the new solar in place of the old solar (I don't have the paper here - was the solar used scaled to Lean?).

I'll cc this to the entire team, as there might be other ideas on how to do this - I think we would want two simulations over the last 400 years. One w/ the old Lean solar, one with the new. If we could use one of the existing plotted runs as the "old Lean" run, then we only need one new run. The idea is to show what difference TAR solar (old Lean) vs. AR4 solar (new Lean) means.

So, lets see what Keith and others say, and then line things up to get the run done. If we can do it w/ CLIMBER, great. If we need to involve another EMIC (assuming we're not going to get a AOGCM run done in less than a month), then we need to line that up. Whatever model we use, it should be one already in use by the AR4, so we don't have to worry about the results being published - just the model. Make sense?

Thanks again for the quick reply. Best, Peck

>Dear Jonathan,

>

>concerning item 8: we can deliver a millennium simulation with any
>given forcing provided to us within days. (Actually takes just about

>1 hour to run on the computer with CLIMBER-2.)

>

>Cheers,

>Stefan

--

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Wg1-ar4-ch06 mailing list
Wg1-ar4-ch06@joss.ucar.edu
<http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>
</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Fwd: [Wg1-ar4-ch06] Follow-up from Christchurch
Date: Wed, 21 Dec 2005 15:43:11 +0000

<x-flowed>

>Date: Sun, 18 Dec 2005 13:53:03 -0700
>To: wg1-ar4-ch06@joss.ucar.edu
>From: Jonathan Overpeck <jto@u.arizona.edu>
>X-Virus-Scanned: amavisd-new at ucar.edu
>Subject: [Wg1-ar4-ch06] Follow-up from Christchurch
>X-BeenThere: wg1-ar4-ch06@joss.ucar.edu
>X-Mailman-Version: 2.1.1
>List-Id: <wg1-ar4-ch06.joss.ucar.edu>
>List-Help: <mailto:wg1-ar4-ch06-request@joss.ucar.edu?subject=help>
>List-Post: <mailto:wg1-ar4-ch06@joss.ucar.edu>
>List-Subscribe: <http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>,
> <mailto:wg1-ar4-ch06-request@joss.ucar.edu?subject=subscribe>
>List-Archive: <http://www.joss.ucar.edu/mailman/private/wg1-ar4-ch06>
>List-Unsubscribe: <http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>,
> <mailto:wg1-ar4-ch06-request@joss.ucar.edu?subject=unsubscribe>
>Sender: wg1-ar4-ch06-bounces@joss.ucar.edu
>X-UEA-Spam-Score: 0.0
>X-UEA-Spam-Level: /
>X-UEA-Spam-Flag: NO

>
>Hi Chapter 6 Friends - Just wanted to thank you all for a great IPCC
>meeting and solid progress toward the SOD of Chapter 6, as well as
>give you a report on the TS meeting that took place on Friday. I'm
>in transit, so haven't been able to see any emails, but I suspect
>Eystein is also sending some updates on what we need to be doing.
>We'll have to work fast and hard to make all the deadlines, but I
>think its safe to say that our chapter will have real impact. I want
>to personally thank you for your dedication to our team effort!

>
>PLEASE READ THE FOLLOWING CAREFULLY

>
>The TS/SPM meeting on Friday was exhausting, as appears to be
>traditional for all things IPCC. But, it was quite impressive in
>terms of how paleo was viewed by the broader WG1 team of authors.
>This is reflected in the decision to consider (without any pushing

>from me, believe it or not) several new figures from our chapter.
>Below I list these along with the others that will need refinement
>for use by the TS. Please note where I insert "ACTION ITEM" - these
>are very time sensitive assignments that should be carried out ASAP
>(i.e., before the new year where possible). Note that everything
>(i.e., figures) in the TS will also have to be in our chapter.

>
>1) the orbital box. Eystein and I have the draft completed by
>Valerie et al in New Zealand. We will read/edit (ACTION ITEM) and
>send around to the group for further editing. The TS version might
>have to be altered to reflect the broader audience, and I'm not yet
>sure what figure would best go with the TS version. I believe
>Valerie (ACTION ITEM) is exploring (with Stefan?) a nice figure that
>illustrates the mechanisms of orbital forcing.

>
>2) there will also be an model evaluation box in the TS that will
>have paleo. Once I get more feedback on this (Chap 8 is leading on
>this box), I'll connect the rest of our team with this effort, with
>Bette in the role of lead chap 6 person.

>
>3) there will a sea level box led by Chap 5. I'm not sure what the
>fig will look like in this box, but if Dick (ACTION ITEM) can
>produce his new Chap 6 sea level figure FAST, we can float it as a
>possible contributor to the TS Box figure. It would be great to get
>paleo sea level perspectives in this box!

>
>4) there will be expanded discussion of abrupt change with focus on
>paleo - Richard Alley is leading this, and I think that will be a
>real plus in making sure the discussion isn't just model based

>
>4) Keith's sites through time figure is also still a TS item. There
>will hopefully also be a fig showing the distribution of
>instrumental sites. Keith has the ACTION ITEM on his figure. Peck
>and Eystein can help get the data released to Keith and Tim if
>needed - just let us know.

>
>5) Keith's 6.8 figure will have to be worked on to find the best
>mode of presentation, and I have a separate email on this one for
>him and Tim. The TS team would like to see inserted on the fig
>(e.g., along the lower edge of the figure, perhaps) some depiction
>of how the site number used changes back in time, and some color
>coding to denote how our expert judgement suggests the implied
>confidence in the recons change back in time. I'm guessing this will

>require some phone conversations to think through with Keith (ACTION
>ITEM for Eystein, Peck and Keith).

>

>6) A NEW FIGURE - depicting inferred solar forcing over the last X
>centuries. The request is that we show Judith Lean et al's latest
>for 1600 to present. This could include the volcanic forcing too,
>but it seems more appropriate that we stick with our plan to add
>this to the expanded 6.8. We'll have to try both figs (this new one,
>and the expanded 6.8) figure w/ and w/o the volcanic series (i.e.,
>detrended multi-core average excess sulfate from each of two polar
>regions) on each fig. I think Keith/Tim gets the ACTION ITEM on all
>this figure stuff - Perhaps David (ACTION ITEM) can send Judith's
>latest solar recon to Keith?

>

>7) Expanded/modified recent forcing figureS by Fortunat (ACTION
>ITEM). One will be for Chap 6, the other will combine Chap 2 and 6
>perspectives into a single figure for the TS. I'll send a separate
>fig to Fortunat with the details, but everyone likes his new rate of
>change depiction, and the TS team also wants a ice core tropospheric
>aerosol record too (e.g., for the last couple centuries - Jean
>Jouzel thought we could do this using Greenland ice core data, and
>we'd add this to the TS fig (and either a chap 2 or 6 figure, since
>everything in the TS has to also be in a chapter.

>

>8) A NEW FIGURE for the TS (and maybe not chapt 6, since we already
>have 6.8 and 6.10 with most of the info) should be the one of
>Keith's that we showed in our plenary talk on Thursday - the
>multi-model range of simulated change over the last 1000 (red
>shading) superimposed on our chap 6 observed record (represented by
>grey shading as in the fig we showed). Requested modifications for
>Keith/Tim (ACTION ITEM) include: a) using a 20th century ref period
>as in the current Fig 6.8, b) adding (where possible) simulations
>that include natural forcing only (and thus not enough warming in
>20th century) and c) adding one or more EMIC simulations using the
>new Lean solar recon (at least over the last 400 years, with all the
>other forcing). This last one is tricky, since no one at the TS mtg
>thought such a simulation exists, BUT it seems it is ok for us to
>get/use a new long simulation by one of the EMIC models used in Chap
>10. Peck (ACTION ITEM) needs to figure out how to get this, but
>Thomas Stocker indicated he'd help. Stefan - what about you guys
>doing this? Who else could we ask for fast turnaround?

>

>9) Another NEW FIGURE (that I actually fought including since we

>don't want to be seen showing off our own stuff) of Last
>Interglacial (LIG) Change. The TS team (and Susan) really liked this
>paleo message, so we came up with a proposed scheme (which I already
>discussed with Bette - who has the ACTION ITEM) that will involve
>the inclusion of more than one LIG climate simulation, plotted with
>observations superimposed, and perhaps more than one LIG ice sheet
>reconstruction as well. Should Tarasov and Peltier be considered for
>this fig (forced by ice-core inferred LIG climate)? Are there
>others? For this figure to work, it has to be a synthesis of
>multiple studies, not just the recent Otto-Bleisner et al effort.

>
>So, that is the news - all good from the view point of chap 6
>exposure/impact, but of course, not so good in terms of the
>additional fast-turn-around work that is needed. The other tough
>issue is that - after several negotiating sessions with Susan (the
>last one with Jean Jouzel helping) - the best we could do is get our
>page limit increased from 30 to 35 pages. That doesn't sound too
>bad, except that we have to a) get all our existing material into
>less space than now (we're currently at an estimated 36 pages) AND
>b) get the new figures mentioned above in (two I think - solar, plus
>the LIG fig). We can do it, but everyone has to be thinking NOW
>about how to reduce our text.

>
>Again, many thanks for all the travel and hard work over the last
>two weeks. Also (in advance) for all the hard work coming up this
>month and the next two.

>
>Best, Peck

>--
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</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Fwd: new climate model runs
Date: Tue Jan 3 09:35:18 2006

Date: Sun, 1 Jan 2006 21:28:08 -0700
To: joos <joos@climate.unibe.ch>, rahmstorf@ozean-klima.de
From: Jonathan Overpeck <jto@u.arizona.edu>
Subject: new climate model runs
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>,
Keith Briffa <k.briffa@uea.ac.uk>
X-UEA-Spam-Score: 0.0
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO

Happy New Year Stefan and Fortunat - just wanted to check in to see where things stand with the EMIC runs you were going to do for the revised Fig 6.10 - that is, with the new Lean solar forcing, and (where the published runs don't already exist) with the old Lean forcing. Again, the purpose of all this is to assess what difference the new solar forcing makes.

Eystein and I are hoping that you've figured out the best experimental framework - e.g., what other forcing series to use. It would be great if you used the same volcanic and trace gas series, if that is possible. I'm cc'ing this to Keith in the hope that he can help us make sure we're making the right decisions.

Also, since Keith is going to be making the new figure comparing the range of obs climate over the last 1000 years to the range of simulated climate over the last 1000 years (i.e., like the fig we showed in our second/Thursday plenary talk), it would be worth thinking if there is any way to scale the solar forcing over the entire last 1000 years to Judith's new reduced-amplitude solar forcing. I'm not sure this is straightforward or not, but if it was possible, we'd have your new runs for inclusion in the new obs vs. simulated climate fig too - this would be helpful.

In any case, the purpose of this email is just to see where we stand, and help keep things moving.

Thanks, Peck

--

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References

1. <http://www.geo.arizona.edu/>
2. <http://www.ispe.arizona.edu/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Fortunat Joos <joos@climate.unibe.ch>

Subject: Re: new climate model runs

Date: Tue, 3 Jan 2006 12:08:15 -0700

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, StefanRahmstorf

Keith Briffa <k.briffa@uea.ac.uk>, Anders Levermann <Anders.Levermann@pik-potsdam.de>

<x-flowed>

Hi Fortunat et al - glad you have the forcing and can get it out to Anders/Stefan et al. Please do so with recommendations (perhaps building on mine, but suggest what you think is best) for experimental setup - what complete set of forcings should be used, etc.

Please note that we'd like (can we get from both of your groups??) simulated climate to present in two forms: 1) with natural (Lean solar plus volc) plus anthropogenic forcing and 2) with natural only also. It would be good if the results from your runs (Swiss and German) were directly comparable with each other.

Also, please note that I'm waiting for everyone to return to the TSU and let us know the official schedule for the next couple months. There is a finite chance that we'll need your runs, and the figures (which Keith and Tim Osborn will be drafting) well BEFORE the end of January. The reason for this is that this material will be used in the next draft of the TS/SPM (and will need iteration), and we are also likely to be under pressure to have all our figures out for broader WG1 review in January. So, we hope you can speed things up to be run sooner in Jan. OK?

I tried to attach the Christchurch Chap 6 plenary talk, but my phone line is not allowing it today. Will send soon. The figure that is being considered (wanted, might be the better word) for the TS is the one on the upper right of page 7 of the pdf I will send.

Please keep me, Eystein, and Keith in the loop as things develop. It would be great to know what your planned completion date is once you have things running (hopefully soon, pretty please... - we can't afford to be late with things anymore)

Many thanks! Peck

>Hi,

>
>ALL the best for 2006!
>
>I got the forcing from Judith and will send it tomorrow as I am on a slow
>connection right now.
>
>We plan to have the calculation by end of Januar as we are pretty busy with
>various tasks.
>
>Fortunat
>
>
>
>Quoting Stefan Rahmstorf <rahmstorf@ozean-klima.de>:
>
>> Jonathan,
>> as I said earlier: we're ready to roll as soon as we get that forcing.
>> Who can provide it?
>> Stefan
>>
>> Jonathan Overpeck wrote:
>>
>> > Happy New Year Stefan and Fortunat - just wanted to check in to see
>> > where things stand with the EMIC runs you were going to do for the
>> > revised Fig 6.10 - that is, with the new Lean solar forcing, and
>> > (where the published runs don't already exist) with the old Lean
>> > forcing. Again, the purpose of all this is to assess what difference
>> > the new solar forcing makes.
>> >
>> > Eystein and I are hoping that you've figured out the best experimental
>> > framework - e.g., what other forcing series to use. It would be great
>> > if you used the same volcanic and trace gas series, if that is
>> > possible. I'm cc'ing this to Keith in the hope that he can help us
>> > make sure we're making the right decisions.
>> >
>> > Also, since Keith is going to be making the new figure comparing the
>> > range of obs climate over the last 1000 years to the range of
>> > simulated climate over the last 1000 years (i.e., like the fig we
>> > showed in our second/Thursday plenary talk), it would be worth
>> > thinking if there is any way to scale the solar forcing over the
>> > entire last 1000 years to Judith's new reduced-amplitude solar
>> > forcing. I'm not sure this is straightforward or not, but if it was
>> > possible, we'd have your new runs for inclusion in the new obs vs.

>> > simulated climate fig too - this would be helpful.
>> >
>> > In any case, the purpose of this email is just to see where we stand,
>> > and help keep things moving.
>> >
>> > Thanks, Peck

>>
>>
>>
>>
>>
>
>
>--

>e-mail: joos@climate.unibe.ch;

>
>Until November 23
> National Centre for Atmospheric Research, Terrestrial Sciences, CGD
> 1850 Table Mesa Drive, Boulder, CO, 80305
> ++1-303 497 13 44 (office)
>
> home address:
> 3655 Emerson Avenue, Boulder, CO, 80305
> ++1-303 494 69 52 (home)

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>After November 24
> Climate and Environmental Physics
> Sidlerstr. 5, CH-3012 Bern
> Phone: ++41(0)31 631 44 61 Fax: ++41(0)31 631 87 42
> Internet: <http://www.climate.unibe.ch/~joos/>

--

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Fortunat Joos <joos@climate.unibe.ch>
Subject: Re: [Fwd: Re: [Wg1-ar4-ch06] Follow-up from Christchurch]
Date: Wed, 4 Jan 2006 17:32:22 -0700
Cc: Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Eystein Jansen <Eystein.Jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, Anders.Levermann@pik-potsdam.de, Gian-Kasper Plattner <plattner@climate.unibe.ch>, Thomas Stocker <stocker@climate.unibe.ch>

<x-flowed>

Hi Fortunat and friends - I suggest that we (Fortunat, can you do this?) ask Thomas Stocker since he has lots of experience w/ IPCC and knows what we're trying to do too. Is this ok?

If it's ok (and I'm guessing that it might not be ok to use an unpublished extended solar series, as Fortunat suggest - but it would be more comparable to other results in the same figure (our old 6.10)), I think scaling to Bard would be better since this is what has been done more in the other simulations published and in the old Fig. 6.10 - am I correct?

If we can't scale Judith's new recon back to 1000, then we'll just have some simulated series back to 1610.

Again, thanks Fortunat for figuring it all out.

best, peck

>Hi Peck,

>

>Thanks for your thoughts. We will try to have a complete forcing series next

>week.

>

>Stefan and Anders are you happy with time series of radiative forcings in W/m²

>for a) solar - b) volcanic - c) CO₂ -d) sum of non-CO₂? Is it correct that you

>do not need concentrations and burdens for individual gases and anthropogenic

>and natural (volcanic and others) aerosols?

>

>For extrapolation of the Lean series it might be possible to use the Bard et

>al., Tellus, Be-10 record as it has been used widely. Another option would be

>to use 14C-derived solar modulation (Muscheler et al). This is more sophisticated, but solar modulation has up-to-date not been used in climate

>models. In any case, extrapolation of the Lean

>serie might be challenged in the
>IPCC context as we are leaving the area of published results.

>
>Regards,

>
>Fortunat

>
>
>Quoting Jonathan Overpeck <jto@u.arizona.edu>:

>
>> Hi Fortunat, Stefan and gang - Have you given any
>> thought to scaling the new solar forcing
>> estimates from Lean (sent w/ this email - thanks)
>> in some way (e.g., to 14C/10Be) so that the new
>> simulations could cover the last 1000 years,
>> rather than the last 400? This would be nice
>> given that we'll plot the new runs in a fig with
>> the existing/published runs (old fig 6.10). Might
>> take a little more work for someone, but could
>> you, for example, take an old solar series used
>> in a recent simulation shown in the old Fig 6.10,
>> and calculate the amplitude reduction implied by
>> the new Lean data over the last 400 years, and
>> then apply that same reduction (assuming it's
>> relatively constant - I'm being lazy here and not
>> ready up) to the old solar forcing back to 1000
>> AD?

>>
>> Might be a stupid idea, so it's ok to say so.
>> Please let me know what you think - again, it
>> would be good if both groups could use the same
>> forcing.

>>
>> Thanks again, peck

>>
>> >Dear all,
>> >
>> >Here the data I got from Judith Lean. Please
>> >note that Judith Lean provided the data for the
>> >IPCC context. We should inform Judit of the
>> >results as requested by her and as a matter of
>> >courtesy.

>> >
>> >Considering the other forcings, we will use
>> >updated historical forcing as used for chapter
>> >10 scenario calculation based on the
>> >formulations and the assessment provided in
>> >chapter 2. We are currently in the process of
>> >compiling these series.

>> >
>> >With best regards,

>> >
>> >Fortunat

>> >--

```
>> >
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>> > Internet: http://www.climate.unibe.ch/~joos/
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>> >
>> >Delivered-To: joos@climate.unibe.ch
>> >Return-Path: <jlean@ssd5.nrl.navy.mil>
>> >Received: from mailhub03.unibe.ch (mailhub03.unibe.ch
[::ffff:130.92.9.70])
>> > (TLS: TLSv1/SSLv3,256bits,AES256-SHA)
>> > by phkup10 with esmtp; Fri, 23 Dec 2005 22:17:45 +0100
>> > id 0003FA0D.43AC697A.000077F8
> >>Received: from localhost (scanhub02-eth0.unibe.ch [130.92.254.66])
>> > by mailhub03.unibe.ch (Postfix) with ESMTMP id 304BD249D8
>> > for <joos@climate.unibe.ch>; Fri, 23 Dec 2005 22:21:27 +0100 (CET)
>> >Received: from mailhub03.unibe.ch ([130.92.9.70])
>> > by localhost (scanhub02.unibe.ch [130.92.254.66]) (amavisd-new,
port
>> 10024)
>> > with LMTP id 10205-12-31 for <joos@climate.unibe.ch>;
>> > Fri, 23 Dec 2005 22:21:26 +0100 (CET)
>> >Received: from mail2.nrl.navy.mil (smaill2.nrl.navy.mil
[132.250.1.147])
>> > by mailhub03.unibe.ch (Postfix) with ESMTMP id 27C4F24CC8
>> > for <joos@climate.unibe.ch>; Fri, 23 Dec 2005 22:21:07 +0100 (CET)
> >>Received: from ccssun1.nrl.navy.mil
>(ccssun1.nrl.navy.mil [132.250.113.66])
>> > by mail2.nrl.navy.mil (8.13.4/8.13.4) with ESMTMP id jBNLL2mG029848
>> > for <joos@climate.unibe.ch>; Fri, 23 Dec 2005 16:21:02 -0500 (EST)
>> >Received: from [132.250.166.98] (sdpc28.nrl.navy.mil
[132.250.166.98])
>> > by ccssun1.nrl.navy.mil (8.13.1/8.13.1) with ESMTMP id
jBNLKulM003512
>> > for <joos@climate.unibe.ch>; Fri, 23 Dec 2005 16:20:56 -0500 (EST)
>> >Message-ID: <43AC6A37.5040905@ssd5.nrl.navy.mil>
>> >Date: Fri, 23 Dec 2005 16:20:55 -0500
>> >From: Judith Lean <jlean@ssd5.nrl.navy.mil>
>> >User-Agent: Mozilla Thunderbird 1.0.7 (Windows/20050923)
>> >X-Accept-Language: en-us, en
>> >Mime-Version: 1.0
>> >Content-Type: multipart/mixed; boundary="=_phkup10-25635-1136296413-
0001-3"
>> >To: Fortunat Joos <joos@climate.unibe.ch>
>> >Subject: Re: [Wgl-ar4-ch06] Follow-up from Christchurch
>> >References:
>> ><a06210219bfcalbb02c99@[10.100.1.158]>
>> ><43A7680A.9090404@ozean-klima.de>
>> ><a06210208bfcd374f0e53@[192.168.1.5]>
>> ><43A89A68.6060702@ozean-klima.de>
>> ><a06210211bfcf46657cfb@[192.168.1.5]>
>> ><43AA0D0D.3080809@ozean-klima.de>
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>> <43AA58B3.4010206@climate.unibe.ch>
>> >In-Reply-To: <43AA58B3.4010206@climate.unibe.ch>
>> >X-Scanned-By: MIMEDefang 2.52
>> >X-Virus-checked: by University of Berne
>> >
>> >Dear Fortunat,
>> >
>> >Attached is a file of the new lower estimates of
>> >annual TSI since 1610, as well as references
>> >that describe how the irradiance was
>> >reconstructed. For comparison, I've also
>> >attached the earlier (GRL, 2000) reconstruction
>> >which has larger long-term variability.
>> >
>> >I can also send you monthly mean values since
>> >1880 if you would prefer those. As well, instead
>> >of the total irradiance, I can send you files of
>> >actual spectra - depending on what you want to
>> >use as input to your model I can make the
>> >spectra on a specified wavelength grid, if this
>> >>would help.
>> >
>> >Let me know if you need more than just the
>> >annual TSI. As well, I'd be interested to hear
>> >about your results! (which I guess I'll be able
>> >to read in IPCC).
>> >
>> >Best wishes,
>> >Judith
>> >.
>> >Fortunat Joos wrote:
>> >
>> >>Dear Judith,
>> >>
>> >>Please allow me to contact you with regard to
>> >>your solar forcing reconstructions.
>> >>
>> >>IPCC WGI chapter 6 is planning to run a couple
>> >>of intermediate complexity models (Climber and
>> >>BernCC) with your new low solar forcing records
>> >>for comparing the impact of low and high solar
>> >>on NH temperature. Would you mind to provide us
>> >>with your most recent, published forcing
>> >>estimates as shown in chapter 2. An ascii (or
>> >>excel table) would be fine. Could you provide a
>> >>central value as well as uncertainty estimates.
>> >>The material should be fully consistent with
>> >>chapter 2 for cross-reference.
>> >>
>> >>Thank you for all your help,
>> >>
>> >>Fortunat Joos
>> >>
>> >>Stefan Rahmstorf wrote:


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>> >Fri Jul 29 17:56:43 2005
>> >Total Solar Irradiance consistent with Wang et al (ApJ, 2005)
>> >Background component used in Lean (GRL, 2000) is reduced by 0.27
>> >   Year      11yr Cycle  11yr+background
>> >   1610.5    1365.8477    1365.5469
>> >   1611.5    1365.8342    1365.5300
>> >   1612.5    1366.2461    1365.9279
>> >   1613.5    1366.3650    1366.0399
>> >   1614.5    1366.4451    1366.1143
>> >   1615.5    1366.1591    1365.8314
>> >   1616.5    1365.7358    1365.4148
>> >   1617.5    1365.6107    1365.2889
>> >   1618.5    1365.6038    1365.2783
>> >   1619.5    1365.7001    1365.3684
>> >   1620.5    1365.7001    1365.3645
>> >   1621.5    1365.7001    1365.3607
>> >   1622.5    1365.7001    1365.3568
>> >   1623.5    1365.7001    1365.3530
>> >   1624.5    1365.6621    1365.3121
>> >   1625.5    1365.8926    1365.5303
>> >   1626.5    1365.7816    1365.4191
>> >   1627.5    1365.7106    1365.3418
>> >   1628.5    1365.7577    1365.3518
>> >   1629.5    1365.7261    1365.2922
>> >   1630.5    1365.5946    1365.1428
>> >   1631.5    1365.6255    1365.1515
>> >   1632.5    1365.5946    1365.1183
>> >   1633.5    1365.6951    1365.2158
>> >   1634.5    1365.6157    1365.1362
>> >   1635.5    1365.6249    1365.1411
>> >   1636.5    1365.5946    1365.1080
>> >   1637.5    1365.5946    1365.1046
>> >   1638.5    1366.0768    1365.5710
>> >   1639.5    1366.1344    1365.6241
>> >   1640.5    1365.7001    1365.1936
>> >   1641.5    1365.5946    1365.0815
>> >   1642.5    1365.9277    1365.4006
>> >   1643.5    1365.7183    1365.1824
>> >   1644.5    1365.6761    1365.1272
>> >   1645.5    1365.5946    1365.0454
>> >   1646.5    1365.5946    1365.0449
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>> >   1648.5    1365.5946    1365.0424
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>> >   1651.5    1365.5946    1365.0383
>> >   1652.5    1365.6227    1365.0657
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>> >   1654.5    1365.5995    1365.0358
>> >   1655.5    1365.5981    1365.0260
>> >   1656.5    1365.5989    1365.0249
>> >   1657.5    1365.5961    1365.0199
>> >   1658.5    1365.5946    1365.0145
>> >   1659.5    1365.5946    1365.0125

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| >> | > | 1687.5 | 1365.5953 | 1365.0126 | |
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| >> | > | 1694.5 | 1365.5946 | 1365.0179 | |
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| >> | > | 1793.5 | 1365.8828 | 1365.5756 | |
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| >> | > | 1898.5 | 1365.6982 | 1365.3899 | |
| >> | > | 1899.5 | 1365.6534 | 1365.3381 | |
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| > | > | > | 1901.5 | 1365.5294 | 1365.2292 |
| >> | > | 1902.5 | 1365.5165 | 1365.2378 | |
| >> | > | 1903.5 | 1365.7083 | 1365.4479 | |
| >> | > | 1904.5 | 1365.9651 | 1365.7180 | |
| >> | > | 1905.5 | 1365.7684 | 1365.5291 | |
| >> | > | 1906.5 | 1365.9651 | 1365.7255 | |
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| > | > | 1849.5 | 1366.1829 | 1365.3910 |
| >> | > | 1850.5 | 1365.9812 | 1365.1982 |
| >> | > | 1851.5 | 1366.0029 | 1365.1567 |
| >> | > | 1852.5 | 1365.9446 | 1365.0192 |
| >> | > | 1853.5 | 1365.8448 | 1364.8820 |
| >> | > | 1854.5 | 1365.7162 | 1364.7728 |
| >> | > | 1855.5 | 1365.6262 | 1364.7249 |
| >> | > | 1856.5 | 1365.6163 | 1364.7605 |
| >> | > | 1857.5 | 1365.7169 | 1364.8938 |
| >> | > | 1858.5 | 1365.9066 | 1365.0951 |
| >> | > | 1859.5 | 1366.1260 | 1365.3092 |
| >> | > | 1860.5 | 1366.1963 | 1365.3649 |
| >> | > | 1861.5 | 1366.0916 | 1365.2426 |
| >> | > | 1862.5 | 1365.9496 | 1365.0697 |
| >> | > | 1863.5 | 1365.8821 | 1364.9423 |
| > | > | 1864.5 | 1365.8370 | 1364.8333 |
| >> | > | 1865.5 | 1365.7534 | 1364.7239 |
| >> | > | 1866.5 | 1365.6909 | 1364.6573 |
| >> | > | 1867.5 | 1365.6382 | 1364.6069 |
| >> | > | 1868.5 | 1365.7977 | 1364.7668 |
| >> | > | 1869.5 | 1366.0325 | 1365.0073 |
| >> | > | 1870.5 | 1366.2708 | 1365.2471 |
| >> | > | 1871.5 | 1366.2054 | 1365.2012 |
| >> | > | 1872.5 | 1366.1576 | 1365.1577 |
| >> | > | 1873.5 | 1365.9580 | 1364.9529 |
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| >> | > | 1875.5 | 1365.7035 | 1364.6147 |
| >> | > | 1876.5 | 1365.6586 | 1364.5499 |
| >> | > | 1877.5 | 1365.6543 | 1364.5630 |
| >> | > | 1878.5 | 1365.6135 | 1364.5669 |
| >> | > | 1879.5 | 1365.6255 | 1364.6174 |
| >> | > | 1880.5 | 1365.7689 | 1364.7728 |
| >> | > | 1881.5 | 1365.9124 | 1364.9196 |
| >> | > | 1882.5 | 1365.9313 | 1364.9546 |
| >> | > | 1883.5 | 1365.9791 | 1364.9996 |
| >> | > | 1884.5 | 1365.8812 | 1364.9010 |
| >> | > | 1885.5 | 1365.7909 | 1364.7394 |
| >> | > | 1886.5 | 1365.6487 | 1364.5116 |
| >> | > | 1887.5 | 1365.6234 | 1364.4189 |
| >> | > | 1888.5 | 1365.5962 | 1364.3615 |
| >> | > | 1889.5 | 1365.5652 | 1364.3398 |
| >> | > | 1890.5 | 1365.5912 | 1364.3982 |
| >> | > | 1891.5 | 1365.8303 | 1364.6823 |
| >> | > | 1892.5 | 1365.9163 | 1364.8152 |
| >> | > | 1893.5 | 1366.0458 | 1364.9875 |
| >> | > | 1894.5 | 1366.1332 | 1365.1044 |
| >> | > | 1895.5 | 1366.0166 | 1364.9883 |
| >> | > | 1896.5 | 1365.8434 | 1364.7870 |
| >> | > | 1897.5 | 1365.7094 | 1364.6105 |
| >> | > | 1898.5 | 1365.6982 | 1364.5564 |
| >> | > | 1899.5 | 1365.6534 | 1364.4856 |
| >> | > | 1900.5 | 1365.6216 | 1364.4579 |

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| >> | > | 1901.5 | 1365.5294 | 1364.4176 | |
| >> | > | 1902.5 | 1365.5165 | 1364.4843 | |
| >> | > | 1903.5 | 1365.7083 | 1364.7440 | |
| >> | > | 1904.5 | 1365.9651 | 1365.0502 | |
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| >> | > | 1907.5 | 1365.8604 | 1364.9320 | |
| >> | > | 1908.5 | 1365.9426 | 1364.9508 | |
| >> | > | 1909.5 | 1365.8459 | 1364.8024 | |
| >> | > | 1910.5 | 1365.7173 | 1364.6566 | |
| >> | > | 1911.5 | 1365.6285 | 1364.5870 | |
| >> | > | 1912.5 | 1365.5706 | 1364.5724 | |
| >> | > | 1913.5 | 1365.5739 | 1364.6243 | |
| >> | > | 1914.5 | 1365.6302 | 1364.7195 | |
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| >> | > | 1917.5 | 1366.2821 | 1365.4149 | |
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| >> | > | 1921.5 | 1365.7351 | 1364.9114 | |
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| >> | > | 1928.5 | 1365.9885 | 1365.3811 | |
| >> | > | 1929.5 | 1365.9429 | 1365.3519 | |
| >> | > | 1930.5 | 1365.9159 | 1365.3588 | |
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| >> | > | 1932.5 | 1365.6583 | 1365.2067 | |
| >> | > | 1933.5 | 1365.5300 | 1365.1062 | |
| >> | > | 1934.5 | 1365.6361 | 1365.2339 | |
| >> | > | 1935.5 | 1365.8500 | 1365.4572 | |
| >> | > | 1936.5 | 1366.2373 | 1365.8521 | |
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| >> | > | 1946.5 | 1365.9818 | 1365.9720 | |
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| >> | > | 1949.5 | 1366.2528 | 1366.2626 | |
| >> | > | 1950.5 | 1366.0098 | 1366.0220 | |
| >> | > | 1951.5 | 1365.7721 | 1365.7882 | |
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| >> | > | 1953.5 | 1365.6313 | 1365.6204 | |
| >> | > | 1954.5 | 1365.6599 | 1365.6467 | |

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|----|---|--------|-----------|-----------|
| >> | > | 1955.5 | 1365.7793 | 1365.7719 |
| >> | > | 1956.5 | 1366.3097 | 1366.3141 |
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| >> | > | 1963.5 | 1365.7152 | 1365.6451 |
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| >> | > | 1976.5 | 1365.6458 | 1365.6442 |
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| >> | > | 1978.5 | 1366.2616 | 1366.3101 |
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| >> | > | 1983.5 | 1366.1989 | 1366.2001 |
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| >> | > | 1987.5 | 1365.7865 | 1365.7990 |
| >> | > | 1988.5 | 1366.0792 | 1366.0918 |
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| >> | > | 1990.5 | 1366.5499 | 1366.5624 |
| >> | > | 1991.5 | 1366.4423 | 1366.4547 |
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| >> | > | 1993.5 | 1366.0251 | 1366.0377 |
| >> | > | 1994.5 | 1365.7937 | 1365.8063 |
| >> | > | 1995.5 | 1365.6962 | 1365.7087 |
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| >> | > | 1997.5 | 1365.7365 | 1365.7489 |
| >> | > | 1998.5 | 1366.0986 | 1366.1111 |
| >> | > | 1999.5 | 1366.3817 | 1366.3942 |
| >> | > | 2000.5 | 1366.6620 | 1366.6744 |

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>> --

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</x-flowed>

From: Tom Wigley <wigley@ucar.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Nature: Review of manuscript 2005-12-14395
Date: Tue, 10 Jan 2006 13:45:26 -0700

<x-flowed>

Keith,

Thanx for this. Interesting. However, I do not think your response is very good. Further, there are grammatical and text errors, and (shocking!!) you have spelled McKitrick wrong. This is a sure way to piss them off.

They claim that three cores do not cross-date for TRW. They also say (without results) that the same applies to MXD (these results may be in their Supp. Mat. -- I presume you checked this).

So, all you need say is ...

- (1) TRW was not the only data used for cross-dating.
- (2) When MXD is used there are clear t-value peaks, contrary to their claim. You can show your Fig. 4 to prove this.
- (3) The 3-core-composite cross-dates with other (well-dated) chronologies (Yamal and Polurula), confirming the MXD-based dating. You can show your Fig. 5 to prove this.

You could say all this in very few words -- not many more than I have used above. As it is, your verbosity will leave any reader lost.

There are some problems still. I note that 1032 is not cold in Yamal. Seems odd. Is it cold in **all** of the three chronologies at issue? Or did a reindeer crap next to one of the trees?

Also, there seems to be a one-year offset in the 1020s in your Fig. 6.

I hope this is useful. I really think you have to do (and can do) a better job in combatting the two Ms. If this stuff gets into Nature, you still have a chance to improve it. Personally, I think it would

be good for it to appear since, with an improved response, you can make MM look like ignorant idiots.

Tom.

=====

Keith Briffa wrote:

> Dear Emma
> I am very sorry for the delay in returning this response to the
> submitted Brief Communication By McIntyre and McKitric . I have been
> extremely busy and to substantiate my written remarks it was necessary
> to dig out the original data and produce a number of Figures
> illustrating the true nature of the cross-dating of the data . I have
> (or at least my Research Associate Tom) has now done this and I am
> finally in a position to write the response. This is contained in the
> WORD file attached to this message . The Figures are attached in a
> separate file. I am happy for you to send the attached written
> response to McIntyre and McKitric , but I would prefer if you would
> NOT send the Figures , at least until these are posted on the Climatic
> (hopefully sometime tomorrow). I am accepting your offer of sending
> this response directly to you rather than sending it through the
> Nature system . Sorry that it is a little long.
> If you decide to publish their communication (which I consider very
> unlikely , given its entirely fallacious content) I would expect
> Nature to publish this response and find room to publish my Figures
> (even if only as Supplementary material). Thank you again for your
> patience.
> yours sincerely
> Keith

>
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>
>
>

>> At 10:30 06/01/2006, you wrote:
>> >Content-Type: multipart/alternative;
>> boundary="_-----=_113654340816203"
>> >MIME-Version: 1.0
>> >X-Mailer: MIME::Lite 3.01 (F2.6; B2.12; Q2.03)
>> >Date: Fri, 6 Jan 2006 10:30:08 UT
>> >Message-Id: <113654340854@www11>

>> >Content-Transfer-Encoding: 7bit

>> >

>> >Dear Professor Briffa

>> >

>> >I am writing to you on behalf of Rosalind Cotter, with regard to
>> >your Reply to the Communications Arising manuscript by Dr Irwing and
>> >co-authors entitled "A gender difference in intelligence?". Should
>> >you now have had the chance to consider the paper, we would be
>> >grateful if you could send us your comments as soon as possible.

>> >

>> >We would respectfully remind you that if we do not hear from you
>> >within the next few days, we shall proceed with the reviewing
>> >process without a Reply from you (in accordance with our guide to
>> >authors).

>>

>> >

>> >Alternatively, if it would be more convenient, please send your
>> >reply directly to me by return email. However, please highlight
>> >those comments that are confidential and which should be passed on
>> >to the authors.

>> >

>> >Thank you in advance for your assistance in this matter.

>> >

>> >Yours sincerely

>> >

>> >

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>> >

>> >For Dr Rosalind Cotter

>> >

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> --

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From: Keith Briffa <k.briffa@uea.ac.uk>

To: Valerie.Masson@cea.fr,Eystein.Jansen@geo.uib.no, "Eric W Wolff" <EWWO@bas.ac.uk>

Subject: Urgent request for reference letter

Date: Fri Jan 13 15:38:01 2006

Dear Valerie, Eystein and Eric,

We (that is Phil and myself - and of course also Bo) are hoping that you can help us greatly with an application Bo Vinther is submitting to the EU for a Marie Curie Intra-European Fellowship (EIF) , specifically to spend time with Phil and I at CRU working on the dating and interpretation of seasonally-resolved ice core data and tree-ring data.

We are allowed to submit up to 3 reviews or testimonials (though these must be submitted directly through the We would be really grateful if each of you would agree to provide one of these. Unfortunately, if you can make the time to help, these must in submitted by next Thursday.

Please accept our apologies for the lateness of this request - but you can probably understand that , as usual things have had to be cut fine.

The first stage of evaluation is based only on the quality of the applicant (70%) and the quality of the proposed research plan (30%). If the proposal gets through to the second evaluation stage , then other factors such as the quality of the hosts and host institution become relevant .

At this stage we would ask that you read the attached Science Plan and details of the Quality of the Candidate, and write an assessment based primarily on these. We will send precise details of how to submit them early next week. You probably also know just how strong the competition is these days for such awards , so reviews have to be particularly glowing, but it is only because Phil and I are so keen to work with Bo that we are taking the liberty of asking for your support. I am sure you know , and certainly Valerie has indicated to me, how impressive Bo's work is. I am sure he is the sort of person for whom these awards are meant, as he is someone who will be doing important work to advance the field one day.

I am attaching (virtually final) drafts of the relevant sections , which are all that you need to be able to write these testimonials. We will send the full applications when they are complete. It would also help if you stressed your own distinguished qualifications , that make you so well qualified to offer this review. Please let me know whether you are able to do this for us.

Thank you lots
Keith and Phil

--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Fortunat Joos <joos@climate.unibe.ch>

Subject: Re: Millennium simulations

Date: Thu, 19 Jan 2006 11:04:17 -0700

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>

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Thanks Fortunat. I got the sense from Susan that she'd love to see good old raw ice core data, but I think it makes more sense for Tim and Keith to use what you've sent. It is based on multiple ice cores, and it provides some consistence with our modeling figs.

Tim and Keith - how are you doing? Let me know if you want to discuss figs you're working on beyond what I suggested in my December emails. I appreciate your dealing with the heavy load!

best, peck

>Hi all,

>

>Here the Crowley data from 1001 to 1998. The data were multiplied by
>0.7 to factor in an albedo of 30% (see header of file for more
>clarification). The data in the forcing file send yesterday have
>been extended artificially to year 850 (mirroring the data from 1000
>to 1150) and shift in time by 0.5 to bring all forcing data to
>mid-year.

>

>With best wishes,

>

>Fortunat

>

>Jonathan Overpeck wrote:

>>Hi Fortunat - thanks for pulling all the new EMIC simulation
>>forcing together, and fast. Keith and Tim want (have been asked,
>>might be the best way to say it...) to put together a figure that
>>depicts volcanic forcing. Since you're using Cowley's recon, that
>>might be the best for them too. Can you send Tim (cc me and Keith
>>too) the data series for 1000 to present?

>>

>>Thanks, Peck

>

>--

>
> Climate and Environmental Physics,
> Physics Institute, University of Bern
> Sidlerstr. 5, CH-3012 Bern
> Phone: ++41(0)31 631 44 61 Fax: ++41(0)31 631 87 42
> Internet: <http://www.climate.unibe.ch/~joos/>
>
>
>#
># Hi All,
>#
># some of you have requested the forcing time series used in last years
># Science paper. I referred you to a NOAA website. But I now realize there
># may be incomplete information in the explanations, in the sense that the
># solar and tropospheric aerosol forcing was listed as net radiative forcing
># after accounting for the 30% albedo of the earths atmosphere. some 1D ebm
># do not explicitly consider the albedo term, but virtually all other models
># so.
>#
># In order to ensure that everyone is on the same page with respect to
># evaluating the forcing terms I use I am sending each of you an ftp address
># where you can download estimates of volcano, solar, greenhouse gas, and
># tropospheric (1000-1998) using total forcing prior to accounting for the
># planetary albedo.
>#
># The ftp address is:
>#
># anonymous FTP to stommel.tamu.edu
># cd incoming/FORCING
># get forc-total-4.12.01.txt
>#
># a few other comments -
>#
># all units are in W/m^2
>#
># hl in volc time series refers to the fact that eruptions of unknown origin
># have been assigned a high latitude (hl) origin. There are "tails" to most
># of the large eruptions that were determined based on the estimated
># e-folding time of the aerosols as being about 1 year
>#
># Sol.Be10 refers to the Beryllium 10 measurements of Bard et al but scaled
># by me to the Lean et al changes over the last 400 years. After further
># reflection I think the Be10 may be the most reliable of the solar indices.

>#
># GHG refers to greenhouse gases
>#
># Aer refers to tropospheric aerosols
>#
># sorry about any confusion the prior data may have caused, regards, Tom
>#
># Thomas J. Crowley
># Dept. of Oceanography
># Texas A&M University
># College Station, TX 77843-3146
># 979-845-0795
># 979-847-8879 (fax)
># 979-845-6331 (alternate fax)

>#####
># HERE converted from original file: forc-total-4.12.01.txt
># WITH Planetary Albedo factored in: volc and sol-be10 multiplied by 0.7

>#####

>#Year Vol.hl.cct Sol.Be10/Lean.splice

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| > 1002.00 | 0.0000 | 0.0420 |
| > 1003.00 | 0.0000 | 0.0420 |
| > 1004.00 | -0.5600 | 0.0420 |
| > 1005.00 | -0.2100 | 0.0350 |
| > 1006.00 | 0.0000 | 0.0280 |
| > 1007.00 | 0.0000 | 0.0210 |
| > 1008.00 | 0.0000 | 0.0140 |
| > 1009.00 | 0.0000 | 0.0070 |
| > 1010.00 | 0.0000 | 0.0000 |
| > 1011.00 | 0.0000 | -0.0070 |
| > 1012.00 | 0.0000 | -0.0140 |
| > 1013.00 | 0.0000 | -0.0140 |
| > 1014.00 | 0.0000 | -0.0140 |
| > 1015.00 | -1.9530 | -0.0210 |
| > 1016.00 | -0.7000 | -0.0210 |
| > 1017.00 | 0.0000 | -0.0280 |
| > 1018.00 | 0.0000 | -0.0280 |
| > 1019.00 | 0.0000 | -0.0350 |
| > 1020.00 | 0.0000 | -0.0350 |
| > 1021.00 | 0.0000 | -0.0350 |
| > 1022.00 | 0.0000 | -0.0420 |
| > 1023.00 | 0.0000 | -0.0420 |

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| > 1024.00 | 0.0000 | -0.0490 |
| > 1025.00 | 0.0000 | -0.0490 |
| > 1026.00 | -2.2890 | -0.0560 |
| > 1027.00 | -0.8400 | -0.0560 |
| > 1028.00 | 0.0000 | -0.0630 |
| > 1029.00 | 0.0000 | -0.0630 |
| > 1030.00 | 0.0000 | -0.0630 |
| > 1031.00 | 0.0000 | -0.0700 |
| > 1032.00 | 0.0000 | -0.0700 |
| > 1033.00 | 0.0000 | -0.0770 |
| > 1034.00 | 0.0000 | -0.0770 |
| > 1035.00 | 0.0000 | -0.0840 |
| > 1036.00 | 0.0000 | -0.0840 |
| > 1037.00 | 0.0000 | -0.0840 |
| > 1038.00 | 0.0000 | -0.0840 |
| > 1039.00 | 0.0000 | -0.0910 |
| > 1040.00 | 0.0000 | -0.0910 |
| > 1041.00 | 0.0000 | -0.0910 |
| > 1042.00 | 0.0000 | -0.0910 |
| > 1043.00 | 0.0000 | -0.0980 |
| > 1044.00 | 0.0000 | -0.0980 |
| > 1045.00 | 0.0000 | -0.0980 |
| > 1046.00 | 0.0000 | -0.0980 |
| > 1047.00 | 0.0000 | -0.0980 |
| > 1048.00 | 0.0000 | -0.1050 |
| > 1049.00 | 0.0000 | -0.1050 |
| > 1050.00 | 0.0000 | -0.1050 |
| > 1051.00 | 0.0000 | -0.1050 |
| > 1052.00 | 0.0000 | -0.1050 |
| > 1053.00 | 0.0000 | -0.1050 |
| > 1054.00 | 0.0000 | -0.1120 |
| > 1055.00 | 0.0000 | -0.1050 |
| > 1056.00 | 0.0000 | -0.1050 |
| > 1057.00 | 0.0000 | -0.1050 |
| > 1058.00 | -3.1570 | -0.1050 |
| > 1059.00 | -1.1900 | -0.1050 |
| > 1060.00 | 0.0000 | -0.1050 |
| > 1061.00 | 0.0000 | -0.1050 |
| > 1062.00 | -0.8400 | -0.1050 |
| > 1063.00 | -0.2800 | -0.0980 |
| > 1064.00 | 0.0000 | -0.0910 |
| > 1065.00 | 0.0000 | -0.0840 |
| > 1066.00 | 0.0000 | -0.0700 |

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| > 1067.00 | 0.0000 | -0.0630 |
| > 1068.00 | 0.0000 | -0.0560 |
| > 1069.00 | 0.0000 | -0.0490 |
| > 1070.00 | 0.0000 | -0.0420 |
| > 1071.00 | 0.0000 | -0.0350 |
| > 1072.00 | 0.0000 | -0.0210 |
| > 1073.00 | 0.0000 | -0.0140 |
| > 1074.00 | 0.0000 | -0.0070 |
| > 1075.00 | 0.0000 | 0.0000 |
| > 1076.00 | 0.0000 | 0.0070 |
| > 1077.00 | 0.0000 | 0.0140 |
| > 1078.00 | 0.0000 | 0.0210 |
| > 1079.00 | 0.0000 | 0.0210 |
| > 1080.00 | -0.5600 | 0.0280 |
| > 1081.00 | -0.2100 | 0.0350 |
| > 1082.00 | 0.0000 | 0.0420 |
| > 1083.00 | 0.0000 | 0.0490 |
| > 1084.00 | 0.0000 | 0.0490 |
| > 1085.00 | 0.0000 | 0.0490 |
| > 1086.00 | 0.0000 | 0.0560 |
| > 1087.00 | 0.0000 | 0.0560 |
| > 1088.00 | 0.0000 | 0.0560 |
| > 1089.00 | 0.0000 | 0.0560 |
| > 1090.00 | 0.0000 | 0.0630 |
| > 1091.00 | 0.0000 | 0.0630 |
| > 1092.00 | 0.0000 | 0.0630 |
| > 1093.00 | 0.0000 | 0.0700 |
| > 1094.00 | 0.0000 | 0.0700 |
| > 1095.00 | 0.0000 | 0.0770 |
| > 1096.00 | 0.0000 | 0.0770 |
| > 1097.00 | -1.2950 | 0.0770 |
| > 1098.00 | -0.4900 | 0.0840 |
| > 1099.00 | 0.0000 | 0.0840 |
| > 1100.00 | 0.0000 | 0.0910 |
| > 1101.00 | 0.0000 | 0.0910 |
| > 1102.00 | 0.0000 | 0.0910 |
| > 1103.00 | 0.0000 | 0.0980 |
| > 1104.00 | 0.0000 | 0.0980 |
| > 1105.00 | 0.0000 | 0.1050 |
| > 1106.00 | 0.0000 | 0.1050 |
| > 1107.00 | 0.0000 | 0.1050 |
| > 1108.00 | 0.0000 | 0.1120 |
| > 1109.00 | 0.0000 | 0.1190 |

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| > 1110.00 | 0.0000 | 0.1260 |
| > 1111.00 | 0.0000 | 0.1330 |
| > 1112.00 | 0.0000 | 0.1400 |
| > 1113.00 | 0.0000 | 0.1470 |
| > 1114.00 | 0.0000 | 0.1540 |
| > 1115.00 | 0.0000 | 0.1540 |
| > 1116.00 | 0.0000 | 0.1540 |
| > 1117.00 | 0.0000 | 0.1540 |
| > 1118.00 | 0.0000 | 0.1540 |
| > 1119.00 | 0.0000 | 0.1610 |
| > 1120.00 | 0.0000 | 0.1610 |
| > 1121.00 | 0.0000 | 0.1610 |
| > 1122.00 | 0.0000 | 0.1610 |
| > 1123.00 | 0.0000 | 0.1610 |
| > 1124.00 | 0.0000 | 0.1610 |
| > 1125.00 | 0.0000 | 0.1610 |
| > 1126.00 | 0.0000 | 0.1610 |
| > 1127.00 | 0.0000 | 0.1610 |
| > 1128.00 | 0.0000 | 0.1610 |
| > 1129.00 | 0.0000 | 0.1680 |
| > 1130.00 | 0.0000 | 0.1680 |
| > 1131.00 | 0.0000 | 0.1680 |
| > 1132.00 | 0.0000 | 0.1680 |
| > 1133.00 | 0.0000 | 0.1680 |
| > 1134.00 | 0.0000 | 0.1750 |
| > 1135.00 | 0.0000 | 0.1750 |
| > 1136.00 | 0.0000 | 0.1820 |
| > 1137.00 | 0.0000 | 0.1820 |
| > 1138.00 | 0.0000 | 0.1820 |
| > 1139.00 | 0.0000 | 0.1820 |
| > 1140.00 | 0.0000 | 0.1820 |
| > 1141.00 | 0.0000 | 0.1820 |
| > 1142.00 | 0.0000 | 0.1820 |
| > 1143.00 | 0.0000 | 0.1820 |
| > 1144.00 | 0.0000 | 0.1820 |
| > 1145.00 | 0.0000 | 0.1890 |
| > 1146.00 | 0.0000 | 0.1890 |
| > 1147.00 | 0.0000 | 0.1820 |
| > 1148.00 | 0.0000 | 0.1820 |
| > 1149.00 | 0.0000 | 0.1820 |
| > 1150.00 | 0.0000 | 0.1820 |
| > 1151.00 | 0.0000 | 0.1750 |
| > 1152.00 | 0.0000 | 0.1750 |

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| > 1153.00 | 0.0000 | 0.1750 |
| > 1154.00 | 0.0000 | 0.1750 |
| > 1155.00 | 0.0000 | 0.1680 |
| > 1156.00 | 0.0000 | 0.1680 |
| > 1157.00 | 0.0000 | 0.1610 |
| > 1158.00 | 0.0000 | 0.1540 |
| > 1159.00 | 0.0000 | 0.1470 |
| > 1160.00 | 0.0000 | 0.1470 |
| > 1161.00 | 0.0000 | 0.1400 |
| > 1162.00 | 0.0000 | 0.1400 |
| > 1163.00 | 0.0000 | 0.1470 |
| > 1164.00 | 0.0000 | 0.1470 |
| > 1165.00 | 0.0000 | 0.1470 |
| > 1166.00 | -0.7700 | 0.1540 |
| > 1167.00 | -0.2800 | 0.1470 |
| > 1168.00 | 0.0000 | 0.1470 |
| > 1169.00 | 0.0000 | 0.1470 |
| > 1170.00 | 0.0000 | 0.1400 |
| > 1171.00 | 0.0000 | 0.1400 |
| > 1172.00 | 0.0000 | 0.1330 |
| > 1173.00 | 0.0000 | 0.1330 |
| > 1174.00 | 0.0000 | 0.1260 |
| > 1175.00 | -4.8580 | 0.1330 |
| > 1176.00 | -1.8200 | 0.1330 |
| > 1177.00 | -0.7000 | 0.1330 |
| > 1178.00 | 0.0000 | 0.1330 |
| > 1179.00 | 0.0000 | 0.1330 |
| > 1180.00 | 0.0000 | 0.1400 |
| > 1181.00 | 0.0000 | 0.1400 |
| > 1182.00 | 0.0000 | 0.1400 |
| > 1183.00 | 0.0000 | 0.1400 |
| > 1184.00 | 0.0000 | 0.1400 |
| > 1185.00 | 0.0000 | 0.1400 |
| > 1186.00 | 0.0000 | 0.1400 |
| > 1187.00 | 0.0000 | 0.1400 |
| > 1188.00 | 0.0000 | 0.1400 |
| > 1189.00 | 0.0000 | 0.1400 |
| > 1190.00 | 0.0000 | 0.1400 |
| > 1191.00 | 0.0000 | 0.1400 |
| > 1192.00 | 0.0000 | 0.1400 |
| > 1193.00 | 0.0000 | 0.1400 |
| > 1194.00 | -1.6940 | 0.1470 |
| > 1195.00 | -0.6300 | 0.1470 |

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| > 1196.00 | 0.0000 | 0.1540 |
| > 1197.00 | 0.0000 | 0.1540 |
| > 1198.00 | 0.0000 | 0.1610 |
| > 1199.00 | 0.0000 | 0.1610 |
| > 1200.00 | 0.0000 | 0.1610 |
| > 1201.00 | 0.0000 | 0.1610 |
| > 1202.00 | 0.0000 | 0.1680 |
| > 1203.00 | 0.0000 | 0.1680 |
| > 1204.00 | 0.0000 | 0.1680 |
| > 1205.00 | -0.9100 | 0.1680 |
| > 1206.00 | -0.3500 | 0.1610 |
| > 1207.00 | 0.0000 | 0.1610 |
| > 1208.00 | 0.0000 | 0.1610 |
| > 1209.00 | 0.0000 | 0.1610 |
| > 1210.00 | 0.0000 | 0.1610 |
| > 1211.00 | 0.0000 | 0.1610 |
| > 1212.00 | 0.0000 | 0.1540 |
| > 1213.00 | 0.0000 | 0.1540 |
| > 1214.00 | 0.0000 | 0.1540 |
| > 1215.00 | 0.0000 | 0.1540 |
| > 1216.00 | 0.0000 | 0.1540 |
| > 1217.00 | 0.0000 | 0.1540 |
| > 1218.00 | 0.0000 | 0.1540 |
| > 1219.00 | 0.0000 | 0.1540 |
| > 1220.00 | 0.0000 | 0.1540 |
| > 1221.00 | 0.0000 | 0.1540 |
| > 1222.00 | 0.0000 | 0.1540 |
| > 1223.00 | 0.0000 | 0.1540 |
| > 1224.00 | 0.0000 | 0.1540 |
| > 1225.00 | 0.0000 | 0.1540 |
| > 1226.00 | 0.0000 | 0.1540 |
| > 1227.00 | -0.5600 | 0.1540 |
| > 1228.00 | -0.2100 | 0.1540 |
| > 1229.00 | -3.5630 | 0.1540 |
| > 1230.00 | -1.3300 | 0.1540 |
| > 1231.00 | -0.4900 | 0.1540 |
| > 1232.00 | 0.0000 | 0.1540 |
| > 1233.00 | 0.0000 | 0.1610 |
| > 1234.00 | 0.0000 | 0.1540 |
| > 1235.00 | 0.0000 | 0.1470 |
| > 1236.00 | 0.0000 | 0.1400 |
| > 1237.00 | 0.0000 | 0.1330 |
| > 1238.00 | 0.0000 | 0.1330 |

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| > 1239.00 | 0.0000 | 0.1260 |
| > 1240.00 | 0.0000 | 0.1260 |
| > 1241.00 | 0.0000 | 0.1260 |
| > 1242.00 | 0.0000 | 0.1190 |
| > 1243.00 | 0.0000 | 0.1190 |
| > 1244.00 | 0.0000 | 0.1120 |
| > 1245.00 | 0.0000 | 0.1120 |
| > 1246.00 | 0.0000 | 0.1120 |
| > 1247.00 | 0.0000 | 0.1050 |
| > 1248.00 | 0.0000 | 0.1050 |
| > 1249.00 | 0.0000 | 0.1050 |
| > 1250.00 | 0.0000 | 0.0980 |
| > 1251.00 | 0.0000 | 0.0980 |
| > 1252.00 | 0.0000 | 0.0910 |
| > 1253.00 | 0.0000 | 0.0910 |
| > 1254.00 | 0.0000 | 0.0840 |
| > 1255.00 | 0.0000 | 0.0700 |
| > 1256.00 | 0.0000 | 0.0630 |
| > 1257.00 | 0.0000 | 0.0490 |
| > 1258.00 | 0.0000 | 0.0490 |
| > 1259.00 | -8.2740 | 0.0490 |
| > 1260.00 | -3.0800 | 0.0420 |
| > 1261.00 | -1.1200 | 0.0420 |
| > 1262.00 | 0.0000 | 0.0420 |
| > 1263.00 | 0.0000 | 0.0420 |
| > 1264.00 | 0.0000 | 0.0420 |
| > 1265.00 | 0.0000 | 0.0420 |
| > 1266.00 | 0.0000 | 0.0420 |
| > 1267.00 | 0.0000 | 0.0420 |
| > 1268.00 | 0.0000 | 0.0420 |
| > 1269.00 | 0.0000 | 0.0420 |
| > 1270.00 | 0.0000 | 0.0420 |
| > 1271.00 | 0.0000 | 0.0490 |
| > 1272.00 | 0.0000 | 0.0490 |
| > 1273.00 | 0.0000 | 0.0490 |
| > 1274.00 | 0.0000 | 0.0420 |
| > 1275.00 | -2.0650 | 0.0420 |
| > 1276.00 | -0.7700 | 0.0420 |
| > 1277.00 | 0.0000 | 0.0350 |
| > 1278.00 | 0.0000 | 0.0350 |
| > 1279.00 | 0.0000 | 0.0350 |
| > 1280.00 | 0.0000 | 0.0280 |
| > 1281.00 | 0.0000 | 0.0280 |

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| > 1282.00 | 0.0000 | 0.0280 |
| > 1283.00 | 0.0000 | 0.0280 |
| > 1284.00 | 0.0000 | 0.0280 |
| > 1285.00 | -2.6250 | 0.0280 |
| > 1286.00 | -0.9800 | 0.0280 |
| > 1287.00 | 0.0000 | 0.0210 |
| > 1288.00 | 0.0000 | 0.0210 |
| > 1289.00 | 0.0000 | 0.0210 |
| > 1290.00 | 0.0000 | 0.0210 |
| > 1291.00 | 0.0000 | 0.0140 |
| > 1292.00 | 0.0000 | 0.0140 |
| > 1293.00 | 0.0000 | 0.0140 |
| > 1294.00 | 0.0000 | 0.0140 |
| > 1295.00 | -2.5410 | 0.0070 |
| > 1296.00 | -0.9100 | 0.0070 |
| > 1297.00 | 0.0000 | 0.0070 |
| > 1298.00 | 0.0000 | 0.0070 |
| > 1299.00 | 0.0000 | 0.0000 |
| > 1300.00 | 0.0000 | 0.0000 |
| > 1301.00 | 0.0000 | -0.0000 |
| > 1302.00 | 0.0000 | -0.0000 |
| > 1303.00 | 0.0000 | -0.0070 |
| > 1304.00 | 0.0000 | -0.0070 |
| > 1305.00 | 0.0000 | -0.0070 |
| > 1306.00 | 0.0000 | -0.0070 |
| > 1307.00 | 0.0000 | -0.0070 |
| > 1308.00 | 0.0000 | -0.0140 |
| > 1309.00 | 0.0000 | -0.0140 |
| > 1310.00 | 0.0000 | -0.0140 |
| > 1311.00 | 0.0000 | -0.0140 |
| > 1312.00 | 0.0000 | -0.0140 |
| > 1313.00 | 0.0000 | -0.0210 |
| > 1314.00 | 0.0000 | -0.0210 |
| > 1315.00 | 0.0000 | -0.0210 |
| > 1316.00 | 0.0000 | -0.0210 |
| > 1317.00 | 0.0000 | -0.0280 |
| > 1318.00 | 0.0000 | -0.0280 |
| > 1319.00 | 0.0000 | -0.0280 |
| > 1320.00 | 0.0000 | -0.0280 |
| > 1321.00 | 0.0000 | -0.0350 |
| > 1322.00 | 0.0000 | -0.0350 |
| > 1323.00 | 0.0000 | -0.0350 |
| > 1324.00 | 0.0000 | -0.0350 |

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| > 1325.00 | 0.0000 | -0.0350 |
| > 1326.00 | 0.0000 | -0.0350 |
| > 1327.00 | 0.0000 | -0.0350 |
| > 1328.00 | 0.0000 | -0.0280 |
| > 1329.00 | -2.1910 | -0.0280 |
| > 1330.00 | -0.7700 | -0.0280 |
| > 1331.00 | 0.0000 | -0.0280 |
| > 1332.00 | 0.0000 | -0.0280 |
| > 1333.00 | 0.0000 | -0.0280 |
| > 1334.00 | 0.0000 | -0.0280 |
| > 1335.00 | 0.0000 | -0.0210 |
| > 1336.00 | 0.0000 | -0.0140 |
| > 1337.00 | 0.0000 | -0.0070 |
| > 1338.00 | 0.0000 | 0.0070 |
| > 1339.00 | 0.0000 | 0.0140 |
| > 1340.00 | 0.0000 | 0.0140 |
| > 1341.00 | 0.0000 | 0.0140 |
| > 1342.00 | 0.0000 | 0.0140 |
| > 1343.00 | 0.0000 | 0.0140 |
| > 1344.00 | 0.0000 | 0.0140 |
| > 1345.00 | -1.8550 | 0.0140 |
| > 1346.00 | -0.7000 | 0.0140 |
| > 1347.00 | 0.0000 | 0.0140 |
| > 1348.00 | 0.0000 | 0.0210 |
| > 1349.00 | 0.0000 | 0.0210 |
| > 1350.00 | 0.0000 | 0.0210 |
| > 1351.00 | 0.0000 | 0.0210 |
| > 1352.00 | 0.0000 | 0.0280 |
| > 1353.00 | 0.0000 | 0.0280 |
| > 1354.00 | 0.0000 | 0.0280 |
| > 1355.00 | 0.0000 | 0.0280 |
| > 1356.00 | 0.0000 | 0.0280 |
| > 1357.00 | 0.0000 | 0.0350 |
| > 1358.00 | 0.0000 | 0.0350 |
| > 1359.00 | 0.0000 | 0.0350 |
| > 1360.00 | 0.0000 | 0.0420 |
| > 1361.00 | 0.0000 | 0.0420 |
| > 1362.00 | 0.0000 | 0.0420 |
| > 1363.00 | 0.0000 | 0.0490 |
| > 1364.00 | 0.0000 | 0.0490 |
| > 1365.00 | 0.0000 | 0.0490 |
| > 1366.00 | 0.0000 | 0.0560 |
| > 1367.00 | 0.0000 | 0.0560 |

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| > 1368.00 | 0.0000 | 0.0560 |
| > 1369.00 | 0.0000 | 0.0630 |
| > 1370.00 | 0.0000 | 0.0630 |
| > 1371.00 | 0.0000 | 0.0630 |
| > 1372.00 | 0.0000 | 0.0700 |
| > 1373.00 | 0.0000 | 0.0700 |
| > 1374.00 | 0.0000 | 0.0700 |
| > 1375.00 | -0.7700 | 0.0700 |
| > 1376.00 | -0.2800 | 0.0700 |
| > 1377.00 | 0.0000 | 0.0700 |
| > 1378.00 | 0.0000 | 0.0700 |
| > 1379.00 | 0.0000 | 0.0700 |
| > 1380.00 | 0.0000 | 0.0630 |
| > 1381.00 | 0.0000 | 0.0630 |
| > 1382.00 | 0.0000 | 0.0630 |
| > 1383.00 | 0.0000 | 0.0630 |
| > 1384.00 | 0.0000 | 0.0630 |
| > 1385.00 | 0.0000 | 0.0630 |
| > 1386.00 | 0.0000 | 0.0630 |
| > 1387.00 | -0.7700 | 0.0560 |
| > 1388.00 | -0.2800 | 0.0560 |
| > 1389.00 | 0.0000 | 0.0560 |
| > 1390.00 | 0.0000 | 0.0560 |
| > 1391.00 | 0.0000 | 0.0560 |
| > 1392.00 | 0.0000 | 0.0490 |
| > 1393.00 | 0.0000 | 0.0420 |
| > 1394.00 | 0.0000 | 0.0420 |
| > 1395.00 | 0.0000 | 0.0350 |
| > 1396.00 | 0.0000 | 0.0280 |
| > 1397.00 | 0.0000 | 0.0210 |
| > 1398.00 | 0.0000 | 0.0070 |
| > 1399.00 | 0.0000 | -0.0000 |
| > 1400.00 | 0.0000 | -0.0070 |
| > 1401.00 | 0.0000 | -0.0070 |
| > 1402.00 | 0.0000 | -0.0140 |
| > 1403.00 | 0.0000 | -0.0140 |
| > 1404.00 | 0.0000 | -0.0140 |
| > 1405.00 | 0.0000 | -0.0210 |
| > 1406.00 | 0.0000 | -0.0210 |
| > 1407.00 | 0.0000 | -0.0210 |
| > 1408.00 | -0.7000 | -0.0210 |
| > 1409.00 | -0.2800 | -0.0280 |
| > 1410.00 | 0.0000 | -0.0280 |

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| > 1411.00 | 0.0000 | -0.0280 |
| > 1412.00 | 0.0000 | -0.0280 |
| > 1413.00 | 0.0000 | -0.0350 |
| > 1414.00 | 0.0000 | -0.0350 |
| > 1415.00 | 0.0000 | -0.0420 |
| > 1416.00 | 0.0000 | -0.0490 |
| > 1417.00 | 0.0000 | -0.0560 |
| > 1418.00 | 0.0000 | -0.0630 |
| > 1419.00 | 0.0000 | -0.0700 |
| > 1420.00 | 0.0000 | -0.0770 |
| > 1421.00 | 0.0000 | -0.0840 |
| > 1422.00 | 0.0000 | -0.0840 |
| > 1423.00 | 0.0000 | -0.0840 |
| > 1424.00 | 0.0000 | -0.0840 |
| > 1425.00 | 0.0000 | -0.0910 |
| > 1426.00 | 0.0000 | -0.0910 |
| > 1427.00 | 0.0000 | -0.0910 |
| > 1428.00 | 0.0000 | -0.1050 |
| > 1429.00 | 0.0000 | -0.1120 |
| > 1430.00 | 0.0000 | -0.1260 |
| > 1431.00 | 0.0000 | -0.1330 |
| > 1432.00 | 0.0000 | -0.1470 |
| > 1433.00 | 0.0000 | -0.1540 |
| > 1434.00 | -0.4900 | -0.1610 |
| > 1435.00 | -0.1400 | -0.1680 |
| > 1436.00 | 0.0000 | -0.1750 |
| > 1437.00 | 0.0000 | -0.1890 |
| > 1438.00 | 0.0000 | -0.1960 |
| > 1439.00 | 0.0000 | -0.2030 |
| > 1440.00 | 0.0000 | -0.2100 |
| > 1441.00 | 0.0000 | -0.2170 |
| > 1442.00 | 0.0000 | -0.2170 |
| > 1443.00 | 0.0000 | -0.2240 |
| > 1444.00 | 0.0000 | -0.2240 |
| > 1445.00 | 0.0000 | -0.2240 |
| > 1446.00 | 0.0000 | -0.2240 |
| > 1447.00 | 0.0000 | -0.2240 |
| > 1448.00 | 0.0000 | -0.2240 |
| > 1449.00 | 0.0000 | -0.2240 |
| > 1450.00 | 0.0000 | -0.2240 |
| > 1451.00 | 0.0000 | -0.2240 |
| > 1452.00 | 0.0000 | -0.2240 |
| > 1453.00 | -3.0800 | -0.2240 |

> 1454.00 -1.1200 -0.2310
> 1455.00 0.0000 -0.2310
> 1456.00 0.0000 -0.2310
> 1457.00 0.0000 -0.2380
> 1458.00 0.0000 -0.2380
> 1459.00 -2.9400 -0.2310
> 1460.00 -3.1710 -0.2310
> 1461.00 -1.1900 -0.2240
> 1462.00 0.0000 -0.2240
> 1463.00 0.0000 -0.2170
> 1464.00 0.0000 -0.2170
> 1465.00 0.0000 -0.2170
> 1466.00 -0.7700 -0.2100
> 1467.00 -0.2800 -0.2100
> 1468.00 0.0000 -0.2030
> 1469.00 0.0000 -0.2030
> 1470.00 0.0000 -0.1960
> 1471.00 0.0000 -0.1890
> 1472.00 0.0000 -0.1820
> 1473.00 0.0000 -0.1680
> 1474.00 0.0000 -0.1610
> 1475.00 0.0000 -0.1540
> 1476.00 0.0000 -0.1470
> 1477.00 0.0000 -0.1330
> 1478.00 0.0000 -0.1260
> 1479.00 0.0000 -0.1120
> 1480.00 0.0000 -0.0980
> 1481.00 -0.7000 -0.0910
> 1482.00 0.0000 -0.0910
> 1483.00 -0.9800 -0.0910
> 1484.00 0.0000 -0.0910
> 1485.00 0.0000 -0.0910
> 1486.00 0.0000 -0.0980
> 1487.00 0.0000 -0.0980
> 1488.00 0.0000 -0.0980
> 1489.00 0.0000 -0.0980
> 1490.00 0.0000 -0.0980
> 1491.00 0.0000 -0.0980
> 1492.00 0.0000 -0.0980
> 1493.00 0.0000 -0.1050
> 1494.00 0.0000 -0.1050
> 1495.00 -0.8400 -0.1050
> 1496.00 -0.2800 -0.1050

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| > 1497.00 | 0.0000 | -0.1050 |
| > 1498.00 | 0.0000 | -0.1050 |
| > 1499.00 | 0.0000 | -0.1050 |
| > 1500.00 | 0.0000 | -0.1050 |
| > 1501.00 | 0.0000 | -0.1050 |
| > 1502.00 | 0.0000 | -0.1050 |
| > 1503.00 | 0.0000 | -0.1050 |
| > 1504.00 | -0.7000 | -0.1120 |
| > 1505.00 | -0.2800 | -0.1050 |
| > 1506.00 | 0.0000 | -0.1050 |
| > 1507.00 | 0.0000 | -0.1050 |
| > 1508.00 | 0.0000 | -0.1050 |
| > 1509.00 | 0.0000 | -0.1050 |
| > 1510.00 | 0.0000 | -0.1050 |
| > 1511.00 | 0.0000 | -0.1050 |
| > 1512.00 | 0.0000 | -0.1050 |
| > 1513.00 | 0.0000 | -0.1120 |
| > 1514.00 | 0.0000 | -0.1120 |
| > 1515.00 | 0.0000 | -0.1190 |
| > 1516.00 | 0.0000 | -0.1190 |
| > 1517.00 | 0.0000 | -0.1260 |
| > 1518.00 | 0.0000 | -0.1260 |
| > 1519.00 | 0.0000 | -0.1260 |
| > 1520.00 | 0.0000 | -0.1330 |
| > 1521.00 | 0.0000 | -0.1330 |
| > 1522.00 | 0.0000 | -0.1330 |
| > 1523.00 | 0.0000 | -0.1330 |
| > 1524.00 | 0.0000 | -0.1330 |
| > 1525.00 | 0.0000 | -0.1400 |
| > 1526.00 | 0.0000 | -0.1400 |
| > 1527.00 | -1.9530 | -0.1400 |
| > 1528.00 | -0.7000 | -0.1470 |
| > 1529.00 | 0.0000 | -0.1470 |
| > 1530.00 | 0.0000 | -0.1540 |
| > 1531.00 | 0.0000 | -0.1540 |
| > 1532.00 | 0.0000 | -0.1540 |
| > 1533.00 | 0.0000 | -0.1610 |
| > 1534.00 | 0.0000 | -0.1610 |
| > 1535.00 | 0.0000 | -0.1610 |
| > 1536.00 | 0.0000 | -0.1680 |
| > 1537.00 | 0.0000 | -0.1680 |
| > 1538.00 | 0.0000 | -0.1680 |
| > 1539.00 | 0.0000 | -0.1680 |

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| > 1540.00 | 0.0000 | -0.1750 |
| > 1541.00 | 0.0000 | -0.1750 |
| > 1542.00 | 0.0000 | -0.1680 |
| > 1543.00 | 0.0000 | -0.1680 |
| > 1544.00 | 0.0000 | -0.1680 |
| > 1545.00 | 0.0000 | -0.1680 |
| > 1546.00 | 0.0000 | -0.1680 |
| > 1547.00 | 0.0000 | -0.1610 |
| > 1548.00 | 0.0000 | -0.1610 |
| > 1549.00 | 0.0000 | -0.1610 |
| > 1550.00 | 0.0000 | -0.1610 |
| > 1551.00 | 0.0000 | -0.1610 |
| > 1552.00 | 0.0000 | -0.1610 |
| > 1553.00 | 0.0000 | -0.1610 |
| > 1554.00 | 0.0000 | -0.1610 |
| > 1555.00 | 0.0000 | -0.1610 |
| > 1556.00 | 0.0000 | -0.1610 |
| > 1557.00 | 0.0000 | -0.1610 |
| > 1558.00 | 0.0000 | -0.1610 |
| > 1559.00 | 0.0000 | -0.1610 |
| > 1560.00 | 0.0000 | -0.1610 |
| > 1561.00 | 0.0000 | -0.1540 |
| > 1562.00 | 0.0000 | -0.1540 |
| > 1563.00 | 0.0000 | -0.1540 |
| > 1564.00 | -1.8340 | -0.1540 |
| > 1565.00 | -0.7000 | -0.1470 |
| > 1566.00 | 0.0000 | -0.1470 |
| > 1567.00 | 0.0000 | -0.1470 |
| > 1568.00 | 0.0000 | -0.1400 |
| > 1569.00 | 0.0000 | -0.1400 |
| > 1570.00 | -0.4900 | -0.1400 |
| > 1571.00 | -0.1400 | -0.1330 |
| > 1572.00 | 0.0000 | -0.1330 |
| > 1573.00 | 0.0000 | -0.1330 |
| > 1574.00 | 0.0000 | -0.1260 |
| > 1575.00 | 0.0000 | -0.1260 |
| > 1576.00 | 0.0000 | -0.1260 |
| > 1577.00 | 0.0000 | -0.1190 |
| > 1578.00 | 0.0000 | -0.1120 |
| > 1579.00 | 0.0000 | -0.1050 |
| > 1580.00 | 0.0000 | -0.0980 |
| > 1581.00 | 0.0000 | -0.0910 |
| > 1582.00 | 0.0000 | -0.0840 |

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| > 1583.00 | 0.0000 | -0.0840 |
| > 1584.00 | 0.0000 | -0.0770 |
| > 1585.00 | 0.0000 | -0.0700 |
| > 1586.00 | 0.0000 | -0.0630 |
| > 1587.00 | -3.1290 | -0.0630 |
| > 1588.00 | -2.2120 | -0.0560 |
| > 1589.00 | -0.8400 | -0.0560 |
| > 1590.00 | 0.0000 | -0.0490 |
| > 1591.00 | 0.0000 | -0.0420 |
| > 1592.00 | 0.0000 | -0.0420 |
| > 1593.00 | 0.0000 | -0.0350 |
| > 1594.00 | 0.0000 | -0.0280 |
| > 1595.00 | 0.0000 | -0.0210 |
| > 1596.00 | 0.0000 | -0.0210 |
| > 1597.00 | 0.0000 | -0.0140 |
| > 1598.00 | 0.0000 | -0.0070 |
| > 1599.00 | 0.0000 | 0.0000 |
| > 1600.00 | 0.0000 | 0.0070 |
| > 1601.00 | -3.8010 | 0.0070 |
| > 1602.00 | -1.4000 | 0.0140 |
| > 1603.00 | -0.4900 | 0.0140 |
| > 1604.00 | 0.0000 | 0.0140 |
| > 1605.00 | 0.0000 | 0.0140 |
| > 1606.00 | 0.0000 | 0.0140 |
| > 1607.00 | 0.0000 | 0.0210 |
| > 1608.00 | 0.0000 | 0.0210 |
| > 1609.00 | 0.0000 | 0.0210 |
| > 1610.00 | 0.0000 | 0.0210 |
| > 1611.00 | 0.0000 | 0.0280 |
| > 1612.00 | 0.0000 | 0.0280 |
| > 1613.00 | -1.9530 | 0.0280 |
| > 1614.00 | -0.7000 | 0.0350 |
| > 1615.00 | 0.0000 | 0.0350 |
| > 1616.00 | 0.0000 | 0.0350 |
| > 1617.00 | 0.0000 | 0.0420 |
| > 1618.00 | 0.0000 | 0.0420 |
| > 1619.00 | 0.0000 | 0.0420 |
| > 1620.00 | 0.0000 | 0.0420 |
| > 1621.00 | 0.0000 | 0.0490 |
| > 1622.00 | -2.1700 | 0.0490 |
| > 1623.00 | -0.7700 | 0.0490 |
| > 1624.00 | 0.0000 | 0.0560 |
| > 1625.00 | 0.0000 | 0.0560 |

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| > 1626.00 | 0.0000 | 0.0560 |
| > 1627.00 | 0.0000 | 0.0560 |
| > 1628.00 | 0.0000 | 0.0560 |
| > 1629.00 | 0.0000 | 0.0560 |
| > 1630.00 | 0.0000 | 0.0630 |
| > 1631.00 | 0.0000 | 0.0630 |
| > 1632.00 | 0.0000 | 0.0560 |
| > 1633.00 | 0.0000 | 0.0490 |
| > 1634.00 | 0.0000 | 0.0420 |
| > 1635.00 | 0.0000 | 0.0350 |
| > 1636.00 | 0.0000 | 0.0280 |
| > 1637.00 | 0.0000 | 0.0210 |
| > 1638.00 | 0.0000 | 0.0140 |
| > 1639.00 | 0.0000 | 0.0140 |
| > 1640.00 | 0.0000 | 0.0070 |
| > 1641.00 | -3.8500 | 0.0070 |
| > 1642.00 | -1.4000 | 0.0000 |
| > 1643.00 | -0.5600 | -0.0000 |
| > 1644.00 | 0.0000 | -0.0070 |
| > 1645.00 | -0.2800 | -0.0070 |
| > 1646.00 | -0.0700 | -0.0140 |
| > 1647.00 | 0.0000 | -0.0140 |
| > 1648.00 | 0.0000 | -0.0210 |
| > 1649.00 | 0.0000 | -0.0280 |
| > 1650.00 | 0.0000 | -0.0280 |
| > 1651.00 | 0.0000 | -0.0350 |
| > 1652.00 | 0.0000 | -0.0420 |
| > 1653.00 | 0.0000 | -0.0490 |
| > 1654.00 | 0.0000 | -0.0560 |
| > 1655.00 | 0.0000 | -0.0630 |
| > 1656.00 | 0.0000 | -0.0700 |
| > 1657.00 | 0.0000 | -0.0770 |
| > 1658.00 | 0.0000 | -0.0840 |
| > 1659.00 | 0.0000 | -0.0910 |
| > 1660.00 | 0.0000 | -0.0910 |
| > 1661.00 | 0.0000 | -0.0980 |
| > 1662.00 | 0.0000 | -0.0980 |
| > 1663.00 | 0.0000 | -0.0980 |
| > 1664.00 | 0.0000 | -0.0980 |
| > 1665.00 | 0.0000 | -0.0980 |
| > 1666.00 | 0.0000 | -0.0980 |
| > 1667.00 | -1.6940 | -0.0980 |
| > 1668.00 | -0.6300 | -0.0980 |

> 1669.00 0.0000 -0.1050
> 1670.00 0.0000 -0.1050
> 1671.00 0.0000 -0.1050
> 1672.00 0.0000 -0.1120
> 1673.00 0.0000 -0.1120
> 1674.00 -2.3590 -0.1260
> 1675.00 -0.8400 -0.1330
> 1676.00 0.0000 -0.1400
> 1677.00 0.0000 -0.1400
> 1678.00 0.0000 -0.1400
> 1679.00 0.0000 -0.1400
> 1680.00 0.0000 -0.1400
> 1681.00 -1.9530 -0.1400
> 1682.00 -0.7000 -0.1470
> 1683.00 0.0000 -0.1470
> 1684.00 0.0000 -0.1470
> 1685.00 0.0000 -0.1540
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> 1698.00 0.0000 -0.1330
> 1699.00 0.0000 -0.1260
> 1700.00 0.0000 -0.1260
> 1701.00 0.0000 -0.1260
> 1702.00 0.0000 -0.1260
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> 1705.00 0.0000 -0.1190
> 1706.00 0.0000 -0.1120
> 1707.00 0.0000 -0.1050
> 1708.00 0.0000 -0.1050
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> 1710.00 0.0000 -0.0980
> 1711.00 0.0000 -0.0910

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| > 1712.00 | 0.0000 | -0.0840 |
| > 1713.00 | 0.0000 | -0.0840 |
| > 1714.00 | 0.0000 | -0.0770 |
| > 1715.00 | 0.0000 | -0.0700 |
| > 1716.00 | 0.0000 | -0.0630 |
| > 1717.00 | 0.0000 | -0.0630 |
| > 1718.00 | 0.0000 | -0.0560 |
| > 1719.00 | 0.0000 | -0.0490 |
| > 1720.00 | 0.0000 | -0.0420 |
| > 1721.00 | 0.0000 | -0.0490 |
| > 1722.00 | 0.0000 | -0.0490 |
| > 1723.00 | 0.0000 | -0.0490 |
| > 1724.00 | 0.0000 | -0.0280 |
| > 1725.00 | 0.0000 | -0.0280 |
| > 1726.00 | 0.0000 | 0.0070 |
| > 1727.00 | 0.0000 | 0.0070 |
| > 1728.00 | 0.0000 | 0.0210 |
| > 1729.00 | -2.3240 | -0.0000 |
| > 1730.00 | 0.0000 | 0.0560 |
| > 1731.00 | 0.0000 | 0.0350 |
| > 1732.00 | 0.0000 | -0.0000 |
| > 1733.00 | 0.0000 | -0.0140 |
| > 1734.00 | 0.0000 | -0.0140 |
| > 1735.00 | 0.0000 | 0.0070 |
| > 1736.00 | 0.0000 | 0.0490 |
| > 1737.00 | 0.0000 | 0.0070 |
| > 1738.00 | 0.0000 | 0.0000 |
| > 1739.00 | -0.9100 | 0.0560 |
| > 1740.00 | 0.0000 | 0.0630 |
| > 1741.00 | 0.0000 | 0.0700 |
| > 1742.00 | 0.0000 | 0.0420 |
| > 1743.00 | 0.0000 | 0.0420 |
| > 1744.00 | 0.0000 | 0.0490 |
| > 1745.00 | 0.0000 | 0.0630 |
| > 1746.00 | 0.0000 | 0.0560 |
| > 1747.00 | 0.0000 | 0.0630 |
| > 1748.00 | 0.0000 | 0.1890 |
| > 1749.00 | 0.0000 | 0.1540 |
| > 1750.00 | 0.0000 | 0.1540 |
| > 1751.00 | 0.0000 | 0.1190 |
| > 1752.00 | 0.0000 | 0.1120 |
| > 1753.00 | 0.0000 | 0.0910 |
| > 1754.00 | 0.0000 | 0.0700 |

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| > 1755.00 | 0.0000 | 0.0630 |
| > 1756.00 | 0.0000 | 0.0560 |
| > 1757.00 | 0.0000 | 0.0700 |
| > 1758.00 | 0.0000 | 0.0840 |
| > 1759.00 | 0.0000 | 0.0910 |
| > 1760.00 | 0.0000 | 0.0840 |
| > 1761.00 | 0.0000 | 0.0980 |
| > 1762.00 | 0.0000 | 0.0840 |
| > 1763.00 | 0.0000 | 0.0770 |
| > 1764.00 | 0.0000 | 0.0700 |
| > 1765.00 | 0.0000 | 0.0560 |
| > 1766.00 | 0.0000 | 0.0560 |
| > 1767.00 | 0.0000 | 0.0980 |
| > 1768.00 | 0.0000 | 0.1330 |
| > 1769.00 | 0.0000 | 0.1680 |
| > 1770.00 | 0.0000 | 0.1680 |
| > 1771.00 | 0.0000 | 0.1610 |
| > 1772.00 | 0.0000 | 0.1470 |
| > 1773.00 | 0.0000 | 0.1190 |
| > 1774.00 | 0.0000 | 0.1120 |
| > 1775.00 | 0.0000 | 0.0910 |
| > 1776.00 | 0.0000 | 0.1050 |
| > 1777.00 | 0.0000 | 0.1260 |
| > 1778.00 | 0.0000 | 0.1820 |
| > 1779.00 | 0.0000 | 0.1820 |
| > 1780.00 | 0.0000 | 0.1470 |
| > 1781.00 | 0.0000 | 0.1610 |
| > 1782.00 | 0.0000 | 0.1260 |
| > 1783.00 | -2.3590 | 0.1120 |
| > 1784.00 | 0.0000 | 0.0980 |
| > 1785.00 | 0.0000 | 0.1120 |
| > 1786.00 | 0.0000 | 0.1750 |
| > 1787.00 | 0.0000 | 0.2100 |
| > 1788.00 | 0.0000 | 0.2030 |
| > 1789.00 | -0.8400 | 0.2030 |
| > 1790.00 | -0.2800 | 0.1820 |
| > 1791.00 | 0.0000 | 0.1540 |
| > 1792.00 | 0.0000 | 0.1470 |
| > 1793.00 | 0.0000 | 0.1190 |
| > 1794.00 | 0.0000 | 0.1120 |
| > 1795.00 | 0.0000 | 0.0840 |
| > 1796.00 | 0.0000 | 0.0700 |
| > 1797.00 | 0.0000 | 0.0490 |

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| > 1798.00 | 0.0000 | 0.0350 |
| > 1799.00 | 0.0000 | 0.0210 |
| > 1800.00 | 0.0000 | 0.0210 |
| > 1801.00 | 0.0000 | 0.0490 |
| > 1802.00 | 0.0000 | 0.0280 |
| > 1803.00 | 0.0000 | -0.0140 |
| > 1804.00 | 0.0000 | -0.0210 |
| > 1805.00 | 0.0000 | -0.0210 |
| > 1806.00 | 0.0000 | -0.0490 |
| > 1807.00 | 0.0000 | -0.0630 |
| > 1808.00 | 0.0000 | -0.0700 |
| > 1809.00 | -3.8500 | -0.0770 |
| > 1810.00 | -1.4000 | -0.0770 |
| > 1811.00 | -0.5600 | -0.0770 |
| > 1812.00 | 0.0000 | -0.0770 |
| > 1813.00 | 0.0000 | -0.0700 |
| > 1814.00 | 0.0000 | -0.0700 |
| > 1815.00 | -4.1860 | -0.0630 |
| > 1816.00 | -1.5400 | -0.0420 |
| > 1817.00 | -0.5600 | -0.0420 |
| > 1818.00 | 0.0000 | -0.0420 |
| > 1819.00 | 0.0000 | -0.0420 |
| > 1820.00 | 0.0000 | -0.0490 |
| > 1821.00 | 0.0000 | -0.0490 |
| > 1822.00 | 0.0000 | -0.0420 |
| > 1823.00 | 0.0000 | -0.0420 |
| > 1824.00 | 0.0000 | -0.0280 |
| > 1825.00 | 0.0000 | -0.0070 |
| > 1826.00 | 0.0000 | 0.0140 |
| > 1827.00 | 0.0000 | 0.0420 |
| > 1828.00 | 0.0000 | 0.0630 |
| > 1829.00 | 0.0000 | 0.0770 |
| > 1830.00 | -0.8400 | 0.0980 |
| > 1831.00 | -3.4020 | 0.0840 |
| > 1832.00 | -1.2600 | 0.0770 |
| > 1833.00 | 0.0000 | 0.0770 |
| > 1834.00 | 0.0000 | 0.0910 |
| > 1835.00 | -2.0650 | 0.1400 |
| > 1836.00 | -0.7700 | 0.2030 |
| > 1837.00 | 0.0000 | 0.2170 |
| > 1838.00 | 0.0000 | 0.1960 |
| > 1839.00 | 0.0000 | 0.1820 |
| > 1840.00 | -0.7000 | 0.1680 |

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| > 1841.00 | -0.2800 | 0.1470 |
| > 1842.00 | 0.0000 | 0.1330 |
| > 1843.00 | -1.0500 | 0.1190 |
| > 1844.00 | -0.4200 | 0.1190 |
| > 1845.00 | 0.0000 | 0.1260 |
| > 1846.00 | 0.0000 | 0.1330 |
| > 1847.00 | 0.0000 | 0.1470 |
| > 1848.00 | 0.0000 | 0.1750 |
| > 1849.00 | 0.0000 | 0.1680 |
| > 1850.00 | 0.0000 | 0.1330 |
| > 1851.00 | 0.0000 | 0.1330 |
| > 1852.00 | 0.0000 | 0.1260 |
| > 1853.00 | 0.0000 | 0.1050 |
| > 1854.00 | -0.5600 | 0.0840 |
| > 1855.00 | 0.0000 | 0.0700 |
| > 1856.00 | 0.0000 | 0.0700 |
| > 1857.00 | 0.0000 | 0.0910 |
| > 1858.00 | 0.0000 | 0.1190 |
| > 1859.00 | 0.0000 | 0.1610 |
| > 1860.00 | 0.0000 | 0.1680 |
| > 1861.00 | 0.0000 | 0.1540 |
| > 1862.00 | 0.0000 | 0.1330 |
| > 1863.00 | 0.0000 | 0.1260 |
| > 1864.00 | 0.0000 | 0.1190 |
| > 1865.00 | 0.0000 | 0.0980 |
| > 1866.00 | 0.0000 | 0.0910 |
| > 1867.00 | 0.0000 | 0.0840 |
| > 1868.00 | 0.0000 | 0.1050 |
| > 1869.00 | 0.0000 | 0.1400 |
| > 1870.00 | 0.0000 | 0.1820 |
| > 1871.00 | 0.0000 | 0.1680 |
| > 1872.00 | 0.0000 | 0.1610 |
| > 1873.00 | 0.0000 | 0.1260 |
| > 1874.00 | 0.0000 | 0.0980 |
| > 1875.00 | 0.0000 | 0.0700 |
| > 1876.00 | 0.0000 | 0.0630 |
| > 1877.00 | 0.0000 | 0.0560 |
| > 1878.00 | 0.0000 | 0.0420 |
| > 1879.00 | 0.0000 | 0.0420 |
| > 1880.00 | 0.0000 | 0.0560 |
| > 1881.00 | 0.0000 | 0.0840 |
| > 1882.00 | 0.0000 | 0.0700 |
| > 1883.00 | -2.5900 | 0.0490 |

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| > 1884.00 | -0.9800 | 0.0630 |
| > 1885.00 | 0.0000 | 0.0560 |
| > 1886.00 | 0.0000 | 0.0420 |
| > 1887.00 | 0.0000 | 0.0350 |
| > 1888.00 | 0.0000 | 0.0350 |
| > 1889.00 | 0.0000 | 0.0350 |
| > 1890.00 | 0.0000 | 0.0420 |
| > 1891.00 | 0.0000 | 0.0770 |
| > 1892.00 | 0.0000 | 0.1050 |
| > 1893.00 | 0.0000 | 0.1050 |
| > 1894.00 | 0.0000 | 0.1120 |
| > 1895.00 | 0.0000 | 0.1120 |
| > 1896.00 | 0.0000 | 0.0980 |
| > 1897.00 | 0.0000 | 0.0700 |
| > 1898.00 | 0.0000 | 0.0770 |
| > 1899.00 | 0.0000 | 0.0630 |
| > 1900.00 | 0.0000 | 0.0560 |
| > 1901.00 | 0.0000 | 0.0420 |
| > 1902.00 | -2.5200 | 0.0350 |
| > 1903.00 | -0.9100 | 0.0560 |
| > 1904.00 | 0.0000 | 0.0770 |
| > 1905.00 | 0.0000 | 0.0490 |
| > 1906.00 | 0.0000 | 0.0700 |
| > 1907.00 | -0.2590 | 0.0560 |
| > 1908.00 | 0.0000 | 0.0630 |
| > 1909.00 | 0.0000 | 0.0630 |
| > 1910.00 | 0.0000 | 0.0490 |
| > 1911.00 | 0.0000 | 0.0490 |
| > 1912.00 | -1.4560 | 0.0560 |
| > 1913.00 | -0.5600 | 0.0630 |
| > 1914.00 | -0.5600 | 0.0770 |
| > 1915.00 | 0.0000 | 0.1260 |
| > 1916.00 | 0.0000 | 0.1470 |
| > 1917.00 | 0.0000 | 0.1820 |
| > 1918.00 | 0.0000 | 0.1750 |
| > 1919.00 | 0.0000 | 0.1470 |
| > 1920.00 | 0.0000 | 0.1330 |
| > 1921.00 | 0.0000 | 0.1190 |
| > 1922.00 | 0.0000 | 0.1050 |
| > 1923.00 | 0.0000 | 0.1050 |
| > 1924.00 | -0.9170 | 0.1050 |
| > 1925.00 | -0.3500 | 0.1120 |
| > 1926.00 | 0.0000 | 0.1190 |

| | | |
|-----------|---------|--------|
| > 1927.00 | 0.0000 | 0.1400 |
| > 1928.00 | 0.0000 | 0.1260 |
| > 1929.00 | 0.0000 | 0.1120 |
| > 1930.00 | 0.0000 | 0.1190 |
| > 1931.00 | 0.0000 | 0.1190 |
| > 1932.00 | 0.0000 | 0.1120 |
| > 1933.00 | 0.0000 | 0.1120 |
| > 1934.00 | 0.0000 | 0.1330 |
| > 1935.00 | 0.0000 | 0.1610 |
| > 1936.00 | 0.0000 | 0.2170 |
| > 1937.00 | 0.0000 | 0.2170 |
| > 1938.00 | 0.0000 | 0.2170 |
| > 1939.00 | 0.0000 | 0.2170 |
| > 1940.00 | 0.0000 | 0.2310 |
| > 1941.00 | 0.0000 | 0.2240 |
| > 1942.00 | 0.0000 | 0.2100 |
| > 1943.00 | 0.0000 | 0.1960 |
| > 1944.00 | 0.0000 | 0.2030 |
| > 1945.00 | 0.0000 | 0.2310 |
| > 1946.00 | 0.0000 | 0.2380 |
| > 1947.00 | 0.0000 | 0.2940 |
| > 1948.00 | 0.0000 | 0.3220 |
| > 1949.00 | 0.0000 | 0.3080 |
| > 1950.00 | 0.0000 | 0.2870 |
| > 1951.00 | 0.0000 | 0.2660 |
| > 1952.00 | 0.0000 | 0.2520 |
| > 1953.00 | 0.0000 | 0.2450 |
| > 1954.00 | 0.0000 | 0.2380 |
| > 1955.00 | 0.0000 | 0.2730 |
| > 1956.00 | -0.2870 | 0.3500 |
| > 1957.00 | 0.0000 | 0.3920 |
| > 1958.00 | 0.0000 | 0.3780 |
| > 1959.00 | 0.0000 | 0.3360 |
| > 1960.00 | 0.0000 | 0.3430 |
| > 1961.00 | -0.2800 | 0.3010 |
| > 1962.00 | -0.1960 | 0.2730 |
| > 1963.00 | -0.4480 | 0.2590 |
| > 1964.00 | -0.8330 | 0.2310 |
| > 1965.00 | -0.7070 | 0.2240 |
| > 1966.00 | -0.4620 | 0.2450 |
| > 1967.00 | -0.3080 | 0.2590 |
| > 1968.00 | -0.5950 | 0.2660 |
| > 1969.00 | -0.7420 | 0.2660 |

> 1970.00 -0.3570 0.2450
> 1971.00 -0.1400 0.2170
> 1972.00 -0.0770 0.2310
> 1973.00 -0.1610 0.2100
> 1974.00 -0.2660 0.2100
> 1975.00 -0.6300 0.1960
> 1976.00 -0.2870 0.1960
> 1977.00 -0.1050 0.2310
> 1978.00 -0.1540 0.3220
> 1979.00 -0.1890 0.4060
> 1980.00 -0.1120 0.4130
> 1981.00 -0.1120 0.3850
> 1982.00 -1.6870 0.3360
> 1983.00 -2.1420 0.3220
> 1984.00 -0.8260 0.2800
> 1985.00 -0.2730 0.2590
> 1986.00 -0.3080 0.2590
> 1987.00 -0.2310 0.2870
> 1988.00 -0.1400 0.3710
> 1989.00 -0.1120 0.4200
> 1990.00 -0.1120 0.4200
> 1991.00 -1.1200 0.3920
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> 1994.00 -0.3920 0.2730
> 1995.00 -0.1820 0.2730
> 1996.00 -0.1260 0.2590
> 1997.00 -0.0980 0.2730
> 1998.00 -0.0490 0.3220

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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>
Subject: Re: Fwd: RE: Wahl-Ammann paper on MBH-MM issues
Date: Mon, 23 Jan 2006 13:47:30 -0700
Cc: "Wahl, Eugene R" <wahle@alfred.edu>, Keith Briffa <k.briffa@uea.ac.uk>

Hi all - I'm betting that "provisional acceptance" is not good enough for inclusion in the Second Order draft, but based on what Gene has said, he should have formal acceptance soon - we really need that. Can you give us a read on when you'll have it Gene? Best make this a top priority, or we'll have to leave your important work out of the chapter. Many thanks!!
Peck

Hi Peck, I assume a provisional acceptance is OK by IPCC rules? The timing of these matters are being followed closely by McIntyre (see: <http://www.climateaudit.org/?p=503>) and we cannot afford to be caught doing anything that is not within the regulations. Thus need to consult with martin and Susan on this (see also last mail from Melinda).

Cheers,

Eystein

Thanks Gene - it is worth all the effort, and please keep us (especially Keith) posted on the updates.

best, peck

X-Sieve: CMU Sieve 2.2
Subject: RE: Wahl-Ammann paper on MBH-MM issues
Date: Thu, 19 Jan 2006 21:17:03 -0500

Thread-Topic: Wahl-Ammann paper on MBH-MM issues
Thread-Index: AcWBF2jTf69xJLFkThuHZzU6qK8tMx+kOAJUB28NG2A=

From: "Wahl, Eugene R" <wahle@alfred.edu>
To: "Jonathan Overpeck" <jto@u.arizona.edu>

Hello Jonathan and Keith:

I'm not sure that I ever sent you the updated Wahl-Ammann paper that was the basis for Steve's provisional acceptance. Here it is. As is, it contains a long appendix (# 1)

on issues with interannual statistics of merit for validation, which was not in the version I had sent you earlier in the year. All the main results and conclusions are the same.

Caspar and I are also now responding to Steve's final requests, based on independent re-review. This is primarily to address publishing Pearson's r^2 and CE calculations for verification, which Steve and the reviewer reason should be done to get the conversation off the topic of us choosing not to report these measures, and onto the science itself. We explain thoroughly in the appendix I mention above why we feel these (and other interannual-only) measures of merit are not of much use for verification in the MBH context, so that the fact we are reporting them is contextualized appropriately. IN FACT, we will be going farther than that and will be bringing this material currently in an appendix into the main text, based on the reasoning below (quoted from another message)

Caspar mentioned yesterday that he talked with Susan Solomon about this paper, and she did not see the appendix we had added concerning the issues about Pearson's r^2 etc. Based on this she therefore thought our text was weak in this area in relation to McIntyre's criticisms. Caspar thought, and I agree, that we need to bring this stuff OUT of the appendix and get it INTO the methods section, so that it won't be so easily missed!! We are working on this--which will include other material as well in the text proper.

Also, we are going ahead with an even further-expanded discussion on the issues with r^2 , which itself will probably become an appendix in the final text (it had been slated for publication as supplemental web-site material). This expanded discussion will go into additional reasoning (with graphics) concerning the basis for r^2 not being useful in this context. It will give a vector space analysis of the issues, and explicit visual demonstration of how these issues with r^2 play out in terms of false negative and false positive errors in validation.

Let me know if I can be of any further help in all this. Apologies if this message seems long. I did my best to keep it short, but I'm a bit tired and it is hard to edit well in that state!

Peace, Gene

Dr. Eugene R. Wahl

Asst. Professor of Environmental Studies

Alfred University

1 Saxon Drive

Alfred NY, 14802

607.871.2604

From: Wahl, Eugene R

Sent: Tuesday, December 13, 2005 12:55 AM

To: Jonathan Overpeck

Cc: Keith Briffa; ammann@ucar.edu

Subject: RE: Wahl-Ammann paper on MBH-MM issues

Hello Jonathan:

1) I want you to know that we heard from Steve Schneider today that our paper with Climatic Change has been provisionally accepted for publication. The provisions Steve outlined are ones we fully accept and will implement (extra statistics of merit and remaking of graphics), so this paper can be viewed as accepted, I should think.

Caspar and I are getting right on it. We wanted you to know this ASAP.

2) The Ammann-Wahl GRL comment on the MM GRL paper from early 2005 is being sent for final review along with a response by MM that GRL is soliciting. We had thought, based on info from James Famiglietti (editor), that this article had been accepted and the response from MM was just being sought. We did not realize that the entire package of comment and response would be put through a final review. We just heard about this last Friday. Sorry that we had that one mistaken.

Hope you are well. Best wishes on IPCC work.

Peace, Gene

Dr. Eugene R. Wahl

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Content-Type: application/msword;
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filename="Wahl_Ammann_3321_revised.doc"

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From: Gian-Kasper Plattner <plattner@climate.unibe.ch>
To: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Bern2.5CC IPCC-AR4 millennium simulations
Date: Fri, 27 Jan 2006 16:46:40 +0100
Reply-to: plattner@climate.unibe.ch
Cc: Fortunat Joos <joos@climate.unibe.ch>, Stefan Rahmstorf
<rahmstorf@ozean-klima.de>, Anders Levermann <levermann@pik-potsdam.de>,
Eva Bauer <eva.bauer@pik-potsdam.de>, Eystein Jansen
<eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, Christoph
Raible <raible@climate.unibe.ch>

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Dear all,

Please find attached the Bern2.5CC model output for the IPCC-AR4 millennium simulations, all spanning the period from 1000 - 1998AD. Some plots including a preliminary comparison between CLIMBER-2 and Bern2.5CC results are additionally included (see infos below).

1. The following Bern2.5CC files are attached (with the simulation tag as specified in Fortunat's readme document):

Simulation B1.1: Bern2.5CC_bard08_volcCrow_CO2_nonCO2_1000-1998_ar4.dat
Simulation B1.2: Bern2.5CC_bard25_volcCrow_CO2_nonCO2_1000-1998_ar4.dat
Simulation B2 : Bern2.5CC_WLS-2005_volcCrow_CO2_nonCO2_1000-1998_ar4.dat
Simulation B3.1: Bern2.5CC_bard08_volcCrow_CO2_anthr0_1000-1998_ar4.dat
Simulation B3.2: Bern2.5CC_bard25_volcCrow_CO2_anthr0_1000-1998_ar4.dat
Simulation B3.3: Bern2.5CC_WLS-2005_volcCrow_CO2_anthr0_1000-1998_ar4.dat
Simulation B4 : Bern2.5CC_ctrl_1000-1998_ar4.dat

The variables stored are: year AD, globally averaged surface air temperature, and northern hemispheric and southern hemispheric surface air temperature. The most important information about model setup and references is included in the extended header in each file. Please note that the information on the forcing timeseries applied are specified in the filename only!

Please let me know if something is unclear or if you want additional informations about these simulations in particular or the Bern2.5CC model in general. I can also provide more output variables if desired (such as e.g. MOC, Sea level, ...).

2. In addition, the following plots with CLIMBER-2 and Bern2.5CC results are attached:

Dgmairtnorm_millennium_Bern2.5CC-CLIMBER2_1000_1998_ipccar4.eps
Dgmairtnorm_millennium_Bern2.5CC-CLIMBER2_offset0.8_1000_1998_ipccar4.eps
Dgmairtnorm_millennium_Bern2.5CC_1000_1998_ipccar4.eps
Dgmairtnorm_millennium_CLIMBER2_1000_1998_ipccar4.eps

All these plots show the anomaly in global mean surface air temperature

with respect to the value in year 1001AD from either CLIMBER-2, Bern2.5CC, or both. Let me know if you have questions or comments about the plots.

With best regards,

Gian-Kasper

--

Gian-Kasper Plattner

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</x-flowed>

```
#          91
# IPCC AR4 Millenium Runs output (vary solar forcing)
# ++++++
#
# Model: Bern2.5CC version with active C-cycle
# -----
# Prescribed forcing timeseries as described in file
# readme_doRuns_IPCC_Chap6_millennium_21jan06.txt
# provided by F. Joos, University of Bern.
#
# Contact:
# -----
# Gian-Kasper Plattner
# Climate and Environmental Physics
# Physics Institute, University of Bern
# Sidlerstrasse 5, CH-3012 Bern, Switzerland
# plattner@climate.unibe.ch
# http://www.climate.unibe.ch/~plattner/
# tel: ++41 (0)31 631-44-67
# fax: ++41 (0)31 631-87-42
#
# Some model setup informations:
# -----
# All runs with horizontal/vertical diffusion
#
# Run with standard ocean parameters
# as used in Plattner et al. 2001/2002
```

```
#       with Kv (diffusivity) 4*10^-5 m2/s
#
# Climate sens. set to ~ 3.2 degrees C
# parameterized see Knutti et al. (Clim. Dyn. 2003)
#
# Model version is annual mean.
#
# No radiation code, CO2 radiative forcing calculated
# for as RF=5.35*ln(CO2/CO2_preind),
# Non-co2 radiative forcing prescribed according to
# Joos et al. GBC 2001 with updates for solar forcing
#
# More model description:
# -----
# Zonally averaged dynamical ocean with 3 basins and
# Southern Ocean, zonally averaged one layer energy
# and moisture balance atmosphere, thermodynamic
# sea ice (Stocker et al., J. Climate 1992).
#
# Carbon cycle components: Ocean/Atm/Terr.biosphere;
# Ocean carbon cycle is a description of the cycles
# of organic carbon and CaCO3 (Marchal et al., Tellus
# Tellus B), based on Redfield approach using PO4 as
# biolimiting nutrient.
#
# Land Biota: Lund-Jena-Postdam Dynamical Global
#               Vegetation Model (LPJ-DGVM)
# at GCM resolution (Gerber et al. 2003, Climate
# Dynamics; Sitch et al. 2003, Global Change Biology)
#
# LPJ forced by Cramer/Leemans annual mean
# climatology plus interannual climate variability
# from Hadley simulation (30-recycled climate) plus
# changes in the fields of surface temperature,
# precipitation, and cloudcover as simulated with the
# Impulse-EOF version of ECHAM-3/LSG in response to
# projected radiative forcing changes.
#
# Land use changes are not explicitly considered.
#
# Impact of climate change on terrestrial C-storage
# included
#
# References:
# -----
# Carbon cycle Ocean: Marchal et al., Tellus 1998
# Carbon cycle Terr. Bio: Sitch et al., GCB 2003
#                       Gerber et al., Clim. Dyn. 2003
# Ccycle-climate feedbacks and global warming:
#       Plattner et al., Tellus 2001
#       Plattner et al., GBC 2002
# Non-CO2 forcing: Joos et al., GBC 2001
# Climate model: Stocker et al., J. Climate 1992
# Sea level: Knutti et al., J. Climate 2000
```

Global warming Physics: Knutti et al., Nature 2002
Knutti et al., Cl. Dyn. 2003
and refs therein.

Output columns:

Time (yr AD)

Global mean air temperature (deg C)

NH-averaged air temperature (deg C)

SH-averaged air temperature (deg C)

| | | | |
|--------------|--------------|--------------|--------------|
| 0.100100E+04 | 0.159155E+02 | 0.165835E+02 | 0.152475E+02 |
| 0.100200E+04 | 0.159209E+02 | 0.165892E+02 | 0.152525E+02 |
| 0.100300E+04 | 0.159252E+02 | 0.165938E+02 | 0.152567E+02 |
| 0.100400E+04 | 0.158977E+02 | 0.165611E+02 | 0.152344E+02 |
| 0.100500E+04 | 0.158655E+02 | 0.165220E+02 | 0.152089E+02 |
| 0.100600E+04 | 0.158774E+02 | 0.165361E+02 | 0.152187E+02 |
| 0.100700E+04 | 0.158992E+02 | 0.165626E+02 | 0.152358E+02 |
| 0.100800E+04 | 0.159109E+02 | 0.165768E+02 | 0.152449E+02 |
| 0.100900E+04 | 0.159171E+02 | 0.165843E+02 | 0.152500E+02 |
| 0.101000E+04 | 0.159213E+02 | 0.165891E+02 | 0.152535E+02 |
| 0.101100E+04 | 0.159242E+02 | 0.165924E+02 | 0.152560E+02 |
| 0.101200E+04 | 0.159263E+02 | 0.165946E+02 | 0.152579E+02 |
| 0.101300E+04 | 0.159279E+02 | 0.165964E+02 | 0.152593E+02 |
| 0.101400E+04 | 0.159292E+02 | 0.165979E+02 | 0.152606E+02 |
| 0.101500E+04 | 0.158213E+02 | 0.164710E+02 | 0.151715E+02 |
| 0.101600E+04 | 0.157214E+02 | 0.163645E+02 | 0.150782E+02 |
| 0.101700E+04 | 0.157650E+02 | 0.164064E+02 | 0.151236E+02 |
| 0.101800E+04 | 0.158283E+02 | 0.164797E+02 | 0.151770E+02 |
| 0.101900E+04 | 0.158570E+02 | 0.165118E+02 | 0.152022E+02 |
| 0.102000E+04 | 0.158701E+02 | 0.165312E+02 | 0.152089E+02 |
| 0.102100E+04 | 0.158780E+02 | 0.165447E+02 | 0.152113E+02 |
| 0.102200E+04 | 0.158856E+02 | 0.165546E+02 | 0.152167E+02 |
| 0.102300E+04 | 0.158920E+02 | 0.165619E+02 | 0.152220E+02 |
| 0.102400E+04 | 0.158971E+02 | 0.165676E+02 | 0.152267E+02 |
| 0.102500E+04 | 0.159014E+02 | 0.165720E+02 | 0.152307E+02 |
| 0.102600E+04 | 0.157770E+02 | 0.164254E+02 | 0.151285E+02 |
| 0.102700E+04 | 0.156600E+02 | 0.162963E+02 | 0.150237E+02 |
| 0.102800E+04 | 0.157085E+02 | 0.163461E+02 | 0.150709E+02 |
| 0.102900E+04 | 0.157839E+02 | 0.164324E+02 | 0.151353E+02 |
| 0.103000E+04 | 0.158211E+02 | 0.164751E+02 | 0.151670E+02 |
| 0.103100E+04 | 0.158403E+02 | 0.164997E+02 | 0.151808E+02 |
| 0.103200E+04 | 0.158500E+02 | 0.165164E+02 | 0.151835E+02 |
| 0.103300E+04 | 0.158594E+02 | 0.165285E+02 | 0.151903E+02 |
| 0.103400E+04 | 0.158673E+02 | 0.165375E+02 | 0.151971E+02 |
| 0.103500E+04 | 0.158737E+02 | 0.165443E+02 | 0.152032E+02 |
| 0.103600E+04 | 0.158791E+02 | 0.165496E+02 | 0.152085E+02 |
| 0.103700E+04 | 0.158835E+02 | 0.165539E+02 | 0.152131E+02 |
| 0.103800E+04 | 0.158873E+02 | 0.165574E+02 | 0.152171E+02 |
| 0.103900E+04 | 0.158904E+02 | 0.165603E+02 | 0.152206E+02 |
| 0.104000E+04 | 0.158931E+02 | 0.165627E+02 | 0.152235E+02 |
| 0.104100E+04 | 0.158954E+02 | 0.165646E+02 | 0.152261E+02 |
| 0.104200E+04 | 0.158973E+02 | 0.165663E+02 | 0.152284E+02 |
| 0.104300E+04 | 0.158990E+02 | 0.165676E+02 | 0.152303E+02 |
| 0.104400E+04 | 0.159004E+02 | 0.165687E+02 | 0.152320E+02 |

| | | | |
|--------------|--------------|--------------|--------------|
| 0.104500E+04 | 0.159016E+02 | 0.165697E+02 | 0.152335E+02 |
| 0.104600E+04 | 0.159027E+02 | 0.165706E+02 | 0.152348E+02 |
| 0.104700E+04 | 0.159038E+02 | 0.165715E+02 | 0.152361E+02 |
| 0.104800E+04 | 0.159047E+02 | 0.165722E+02 | 0.152372E+02 |
| 0.104900E+04 | 0.159055E+02 | 0.165729E+02 | 0.152382E+02 |
| 0.105000E+04 | 0.159063E+02 | 0.165735E+02 | 0.152392E+02 |
| 0.105100E+04 | 0.159070E+02 | 0.165740E+02 | 0.152400E+02 |
| 0.105200E+04 | 0.159077E+02 | 0.165745E+02 | 0.152409E+02 |
| 0.105300E+04 | 0.159083E+02 | 0.165750E+02 | 0.152416E+02 |
| 0.105400E+04 | 0.159089E+02 | 0.165754E+02 | 0.152423E+02 |
| 0.105500E+04 | 0.159095E+02 | 0.165759E+02 | 0.152431E+02 |
| 0.105600E+04 | 0.159101E+02 | 0.165764E+02 | 0.152438E+02 |
| 0.105700E+04 | 0.159107E+02 | 0.165769E+02 | 0.152445E+02 |
| 0.105800E+04 | 0.157526E+02 | 0.163976E+02 | 0.151075E+02 |
| 0.105900E+04 | 0.155681E+02 | 0.161824E+02 | 0.149539E+02 |
| 0.106000E+04 | 0.157024E+02 | 0.162482E+02 | 0.151566E+02 |
| 0.106100E+04 | 0.158714E+02 | 0.163711E+02 | 0.153716E+02 |
| 0.106200E+04 | 0.159064E+02 | 0.163799E+02 | 0.154328E+02 |
| 0.106300E+04 | 0.158912E+02 | 0.163588E+02 | 0.154235E+02 |
| 0.106400E+04 | 0.159282E+02 | 0.164062E+02 | 0.154501E+02 |
| 0.106500E+04 | 0.159701E+02 | 0.164636E+02 | 0.154766E+02 |
| 0.106600E+04 | 0.159940E+02 | 0.164998E+02 | 0.154882E+02 |
| 0.106700E+04 | 0.160082E+02 | 0.165240E+02 | 0.154924E+02 |
| 0.106800E+04 | 0.160205E+02 | 0.165424E+02 | 0.154986E+02 |
| 0.106900E+04 | 0.160272E+02 | 0.165572E+02 | 0.154971E+02 |
| 0.107000E+04 | 0.160326E+02 | 0.165692E+02 | 0.154960E+02 |
| 0.107100E+04 | 0.160368E+02 | 0.165792E+02 | 0.154944E+02 |
| 0.107200E+04 | 0.160401E+02 | 0.165874E+02 | 0.154927E+02 |
| 0.107300E+04 | 0.160427E+02 | 0.165944E+02 | 0.154910E+02 |
| 0.107400E+04 | 0.160449E+02 | 0.166004E+02 | 0.154894E+02 |
| 0.107500E+04 | 0.160467E+02 | 0.166055E+02 | 0.154880E+02 |
| 0.107600E+04 | 0.160483E+02 | 0.166098E+02 | 0.154867E+02 |
| 0.107700E+04 | 0.160495E+02 | 0.166134E+02 | 0.154855E+02 |
| 0.107800E+04 | 0.160504E+02 | 0.166164E+02 | 0.154844E+02 |
| 0.107900E+04 | 0.160513E+02 | 0.166191E+02 | 0.154835E+02 |
| 0.108000E+04 | 0.160218E+02 | 0.165851E+02 | 0.154584E+02 |
| 0.108100E+04 | 0.159894E+02 | 0.165469E+02 | 0.154319E+02 |
| 0.108200E+04 | 0.160000E+02 | 0.165607E+02 | 0.154393E+02 |
| 0.108300E+04 | 0.160202E+02 | 0.165871E+02 | 0.154533E+02 |
| 0.108400E+04 | 0.160222E+02 | 0.166017E+02 | 0.154427E+02 |
| 0.108500E+04 | 0.160174E+02 | 0.166096E+02 | 0.154252E+02 |
| 0.108600E+04 | 0.160121E+02 | 0.166147E+02 | 0.154095E+02 |
| 0.108700E+04 | 0.159660E+02 | 0.166182E+02 | 0.153138E+02 |
| 0.108800E+04 | 0.159316E+02 | 0.166197E+02 | 0.152435E+02 |
| 0.108900E+04 | 0.159111E+02 | 0.166198E+02 | 0.152025E+02 |
| 0.109000E+04 | 0.158969E+02 | 0.166188E+02 | 0.151750E+02 |
| 0.109100E+04 | 0.158874E+02 | 0.166175E+02 | 0.151574E+02 |
| 0.109200E+04 | 0.158810E+02 | 0.166159E+02 | 0.151460E+02 |
| 0.109300E+04 | 0.158765E+02 | 0.166143E+02 | 0.151387E+02 |
| 0.109400E+04 | 0.158754E+02 | 0.166129E+02 | 0.151380E+02 |
| 0.109500E+04 | 0.158763E+02 | 0.166119E+02 | 0.151407E+02 |
| 0.109600E+04 | 0.158786E+02 | 0.166114E+02 | 0.151459E+02 |
| 0.109700E+04 | 0.158099E+02 | 0.165273E+02 | 0.150926E+02 |
| 0.109800E+04 | 0.157483E+02 | 0.164610E+02 | 0.150355E+02 |

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| 0.109900E+04 | 0.157746E+02 | 0.164875E+02 | 0.150618E+02 |
| 0.110000E+04 | 0.158230E+02 | 0.165395E+02 | 0.151065E+02 |
| 0.110100E+04 | 0.158454E+02 | 0.165578E+02 | 0.151331E+02 |
| 0.110200E+04 | 0.158613E+02 | 0.165715E+02 | 0.151512E+02 |
| 0.110300E+04 | 0.158743E+02 | 0.165824E+02 | 0.151662E+02 |
| 0.110400E+04 | 0.158852E+02 | 0.165912E+02 | 0.151792E+02 |
| 0.110500E+04 | 0.158948E+02 | 0.165988E+02 | 0.151909E+02 |
| 0.110600E+04 | 0.159034E+02 | 0.166054E+02 | 0.152014E+02 |
| 0.110700E+04 | 0.159111E+02 | 0.166114E+02 | 0.152108E+02 |
| 0.110800E+04 | 0.159182E+02 | 0.166169E+02 | 0.152195E+02 |
| 0.110900E+04 | 0.159249E+02 | 0.166223E+02 | 0.152274E+02 |
| 0.111000E+04 | 0.159314E+02 | 0.166278E+02 | 0.152350E+02 |
| 0.111100E+04 | 0.159377E+02 | 0.166333E+02 | 0.152421E+02 |
| 0.111200E+04 | 0.159437E+02 | 0.166387E+02 | 0.152487E+02 |
| 0.111300E+04 | 0.159495E+02 | 0.166441E+02 | 0.152550E+02 |
| 0.111400E+04 | 0.159551E+02 | 0.166494E+02 | 0.152609E+02 |
| 0.111500E+04 | 0.159603E+02 | 0.166543E+02 | 0.152663E+02 |
| 0.111600E+04 | 0.159649E+02 | 0.166588E+02 | 0.152710E+02 |
| 0.111700E+04 | 0.159691E+02 | 0.166628E+02 | 0.152753E+02 |
| 0.111800E+04 | 0.159729E+02 | 0.166667E+02 | 0.152792E+02 |
| 0.111900E+04 | 0.159765E+02 | 0.166703E+02 | 0.152828E+02 |
| 0.112000E+04 | 0.159799E+02 | 0.166737E+02 | 0.152861E+02 |
| 0.112100E+04 | 0.159831E+02 | 0.166770E+02 | 0.152891E+02 |
| 0.112200E+04 | 0.159861E+02 | 0.166802E+02 | 0.152919E+02 |
| 0.112300E+04 | 0.159889E+02 | 0.166833E+02 | 0.152946E+02 |
| 0.112400E+04 | 0.159916E+02 | 0.166862E+02 | 0.152970E+02 |
| 0.112500E+04 | 0.159942E+02 | 0.166891E+02 | 0.152993E+02 |
| 0.112600E+04 | 0.159967E+02 | 0.166919E+02 | 0.153015E+02 |
| 0.112700E+04 | 0.159991E+02 | 0.166946E+02 | 0.153036E+02 |
| 0.112800E+04 | 0.160014E+02 | 0.166972E+02 | 0.153055E+02 |
| 0.112900E+04 | 0.160036E+02 | 0.166997E+02 | 0.153074E+02 |
| 0.113000E+04 | 0.160057E+02 | 0.167022E+02 | 0.153092E+02 |
| 0.113100E+04 | 0.160078E+02 | 0.167046E+02 | 0.153109E+02 |
| 0.113200E+04 | 0.160097E+02 | 0.167069E+02 | 0.153126E+02 |
| 0.113300E+04 | 0.160118E+02 | 0.167093E+02 | 0.153142E+02 |
| 0.113400E+04 | 0.160140E+02 | 0.167119E+02 | 0.153160E+02 |
| 0.113500E+04 | 0.160161E+02 | 0.167144E+02 | 0.153178E+02 |
| 0.113600E+04 | 0.160182E+02 | 0.167169E+02 | 0.153195E+02 |
| 0.113700E+04 | 0.160202E+02 | 0.167192E+02 | 0.153212E+02 |
| 0.113800E+04 | 0.160222E+02 | 0.167215E+02 | 0.153228E+02 |
| 0.113900E+04 | 0.160240E+02 | 0.167236E+02 | 0.153244E+02 |
| 0.114000E+04 | 0.160256E+02 | 0.167255E+02 | 0.153258E+02 |
| 0.114100E+04 | 0.160271E+02 | 0.167272E+02 | 0.153270E+02 |
| 0.114200E+04 | 0.160285E+02 | 0.167288E+02 | 0.153282E+02 |
| 0.114300E+04 | 0.160298E+02 | 0.167303E+02 | 0.153294E+02 |
| 0.114400E+04 | 0.160311E+02 | 0.167317E+02 | 0.153304E+02 |
| 0.114500E+04 | 0.160322E+02 | 0.167330E+02 | 0.153315E+02 |
| 0.114600E+04 | 0.160333E+02 | 0.167342E+02 | 0.153325E+02 |
| 0.114700E+04 | 0.160343E+02 | 0.167353E+02 | 0.153333E+02 |
| 0.114800E+04 | 0.160351E+02 | 0.167361E+02 | 0.153341E+02 |
| 0.114900E+04 | 0.160358E+02 | 0.167368E+02 | 0.153347E+02 |
| 0.115000E+04 | 0.160363E+02 | 0.167373E+02 | 0.153353E+02 |
| 0.115100E+04 | 0.160368E+02 | 0.167377E+02 | 0.153358E+02 |
| 0.115200E+04 | 0.160372E+02 | 0.167381E+02 | 0.153363E+02 |

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| 0.115300E+04 | 0.160375E+02 | 0.167383E+02 | 0.153366E+02 |
| 0.115400E+04 | 0.160377E+02 | 0.167385E+02 | 0.153370E+02 |
| 0.115500E+04 | 0.160379E+02 | 0.167386E+02 | 0.153373E+02 |
| 0.115600E+04 | 0.160380E+02 | 0.167385E+02 | 0.153375E+02 |
| 0.115700E+04 | 0.160379E+02 | 0.167382E+02 | 0.153376E+02 |
| 0.115800E+04 | 0.160376E+02 | 0.167377E+02 | 0.153375E+02 |
| 0.115900E+04 | 0.160372E+02 | 0.167371E+02 | 0.153373E+02 |
| 0.116000E+04 | 0.160367E+02 | 0.167363E+02 | 0.153371E+02 |
| 0.116100E+04 | 0.160362E+02 | 0.167355E+02 | 0.153368E+02 |
| 0.116200E+04 | 0.160358E+02 | 0.167349E+02 | 0.153367E+02 |
| 0.116300E+04 | 0.160357E+02 | 0.167346E+02 | 0.153368E+02 |
| 0.116400E+04 | 0.160358E+02 | 0.167345E+02 | 0.153370E+02 |
| 0.116500E+04 | 0.160359E+02 | 0.167345E+02 | 0.153374E+02 |
| 0.116600E+04 | 0.159930E+02 | 0.166837E+02 | 0.153022E+02 |
| 0.116700E+04 | 0.159464E+02 | 0.166292E+02 | 0.152636E+02 |
| 0.116800E+04 | 0.159600E+02 | 0.166457E+02 | 0.152742E+02 |
| 0.116900E+04 | 0.159866E+02 | 0.166781E+02 | 0.152951E+02 |
| 0.117000E+04 | 0.160004E+02 | 0.166949E+02 | 0.153060E+02 |
| 0.117100E+04 | 0.160074E+02 | 0.167030E+02 | 0.153119E+02 |
| 0.117200E+04 | 0.160119E+02 | 0.167079E+02 | 0.153159E+02 |
| 0.117300E+04 | 0.160150E+02 | 0.167111E+02 | 0.153189E+02 |
| 0.117400E+04 | 0.160172E+02 | 0.167132E+02 | 0.153212E+02 |
| 0.117500E+04 | 0.157641E+02 | 0.164281E+02 | 0.151002E+02 |
| 0.117600E+04 | 0.156187E+02 | 0.161368E+02 | 0.151005E+02 |
| 0.117700E+04 | 0.157262E+02 | 0.162059E+02 | 0.152466E+02 |
| 0.117800E+04 | 0.158665E+02 | 0.163478E+02 | 0.153853E+02 |
| 0.117900E+04 | 0.159583E+02 | 0.164437E+02 | 0.154728E+02 |
| 0.118000E+04 | 0.160115E+02 | 0.165076E+02 | 0.155154E+02 |
| 0.118100E+04 | 0.160423E+02 | 0.165479E+02 | 0.155367E+02 |
| 0.118200E+04 | 0.160629E+02 | 0.165776E+02 | 0.155483E+02 |
| 0.118300E+04 | 0.160774E+02 | 0.166006E+02 | 0.155543E+02 |
| 0.118400E+04 | 0.160901E+02 | 0.166188E+02 | 0.155613E+02 |
| 0.118500E+04 | 0.160976E+02 | 0.166333E+02 | 0.155619E+02 |
| 0.118600E+04 | 0.161031E+02 | 0.166448E+02 | 0.155615E+02 |
| 0.118700E+04 | 0.161071E+02 | 0.166539E+02 | 0.155602E+02 |
| 0.118800E+04 | 0.161099E+02 | 0.166612E+02 | 0.155586E+02 |
| 0.118900E+04 | 0.161334E+02 | 0.167069E+02 | 0.155598E+02 |
| 0.119000E+04 | 0.161327E+02 | 0.167065E+02 | 0.155588E+02 |
| 0.119100E+04 | 0.161322E+02 | 0.167074E+02 | 0.155571E+02 |
| 0.119200E+04 | 0.161319E+02 | 0.167084E+02 | 0.155553E+02 |
| 0.119300E+04 | 0.161240E+02 | 0.167094E+02 | 0.155386E+02 |
| 0.119400E+04 | 0.160214E+02 | 0.166002E+02 | 0.154426E+02 |
| 0.119500E+04 | 0.159266E+02 | 0.165069E+02 | 0.153463E+02 |
| 0.119600E+04 | 0.159470E+02 | 0.165403E+02 | 0.153538E+02 |
| 0.119700E+04 | 0.159938E+02 | 0.166044E+02 | 0.153832E+02 |
| 0.119800E+04 | 0.159733E+02 | 0.166301E+02 | 0.153165E+02 |
| 0.119900E+04 | 0.159482E+02 | 0.166444E+02 | 0.152520E+02 |
| 0.120000E+04 | 0.159354E+02 | 0.166526E+02 | 0.152182E+02 |
| 0.120100E+04 | 0.159268E+02 | 0.166569E+02 | 0.151968E+02 |
| 0.120200E+04 | 0.159216E+02 | 0.166589E+02 | 0.151843E+02 |
| 0.120300E+04 | 0.159191E+02 | 0.166595E+02 | 0.151786E+02 |
| 0.120400E+04 | 0.159166E+02 | 0.166591E+02 | 0.151741E+02 |
| 0.120500E+04 | 0.158664E+02 | 0.165991E+02 | 0.151337E+02 |
| 0.120600E+04 | 0.158113E+02 | 0.165312E+02 | 0.150915E+02 |

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| 0.120700E+04 | 0.158427E+02 | 0.165735E+02 | 0.151118E+02 |
| 0.120800E+04 | 0.158776E+02 | 0.166097E+02 | 0.151455E+02 |
| 0.120900E+04 | 0.158981E+02 | 0.166288E+02 | 0.151674E+02 |
| 0.121000E+04 | 0.159110E+02 | 0.166387E+02 | 0.151834E+02 |
| 0.121100E+04 | 0.159211E+02 | 0.166451E+02 | 0.151971E+02 |
| 0.121200E+04 | 0.159295E+02 | 0.166497E+02 | 0.152093E+02 |
| 0.121300E+04 | 0.159368E+02 | 0.166532E+02 | 0.152203E+02 |
| 0.121400E+04 | 0.159412E+02 | 0.166523E+02 | 0.152302E+02 |
| 0.121500E+04 | 0.159417E+02 | 0.166451E+02 | 0.152383E+02 |
| 0.121600E+04 | 0.159454E+02 | 0.166451E+02 | 0.152458E+02 |
| 0.121700E+04 | 0.159497E+02 | 0.166466E+02 | 0.152527E+02 |
| 0.121800E+04 | 0.159538E+02 | 0.166485E+02 | 0.152591E+02 |
| 0.121900E+04 | 0.159578E+02 | 0.166506E+02 | 0.152649E+02 |
| 0.122000E+04 | 0.159615E+02 | 0.166528E+02 | 0.152702E+02 |
| 0.122100E+04 | 0.159650E+02 | 0.166550E+02 | 0.152751E+02 |
| 0.122200E+04 | 0.159684E+02 | 0.166573E+02 | 0.152794E+02 |
| 0.122300E+04 | 0.159715E+02 | 0.166595E+02 | 0.152834E+02 |
| 0.122400E+04 | 0.159744E+02 | 0.166618E+02 | 0.152871E+02 |
| 0.122500E+04 | 0.159772E+02 | 0.166640E+02 | 0.152904E+02 |
| 0.122600E+04 | 0.159798E+02 | 0.166662E+02 | 0.152934E+02 |
| 0.122700E+04 | 0.159512E+02 | 0.166321E+02 | 0.152703E+02 |
| 0.122800E+04 | 0.159185E+02 | 0.165928E+02 | 0.152442E+02 |
| 0.122900E+04 | 0.157488E+02 | 0.164060E+02 | 0.150916E+02 |
| 0.123000E+04 | 0.155640E+02 | 0.161868E+02 | 0.149411E+02 |
| 0.123100E+04 | 0.157756E+02 | 0.162876E+02 | 0.152636E+02 |
| 0.123200E+04 | 0.159056E+02 | 0.163980E+02 | 0.154133E+02 |
| 0.123300E+04 | 0.159822E+02 | 0.164718E+02 | 0.154927E+02 |
| 0.123400E+04 | 0.160216E+02 | 0.165128E+02 | 0.155304E+02 |
| 0.123500E+04 | 0.160469E+02 | 0.165448E+02 | 0.155489E+02 |
| 0.123600E+04 | 0.160638E+02 | 0.165696E+02 | 0.155579E+02 |
| 0.123700E+04 | 0.160753E+02 | 0.165892E+02 | 0.155615E+02 |
| 0.123800E+04 | 0.160834E+02 | 0.166051E+02 | 0.155618E+02 |
| 0.123900E+04 | 0.160918E+02 | 0.166181E+02 | 0.155654E+02 |
| 0.124000E+04 | 0.160957E+02 | 0.166290E+02 | 0.155623E+02 |
| 0.124100E+04 | 0.160987E+02 | 0.166380E+02 | 0.155594E+02 |
| 0.124200E+04 | 0.161230E+02 | 0.166886E+02 | 0.155574E+02 |
| 0.124300E+04 | 0.161230E+02 | 0.166895E+02 | 0.155566E+02 |
| 0.124400E+04 | 0.161223E+02 | 0.166907E+02 | 0.155539E+02 |
| 0.124500E+04 | 0.161217E+02 | 0.166926E+02 | 0.155509E+02 |
| 0.124600E+04 | 0.161211E+02 | 0.166942E+02 | 0.155479E+02 |
| 0.124700E+04 | 0.161202E+02 | 0.166954E+02 | 0.155449E+02 |
| 0.124800E+04 | 0.161119E+02 | 0.166962E+02 | 0.155277E+02 |
| 0.124900E+04 | 0.161002E+02 | 0.166964E+02 | 0.155039E+02 |
| 0.125000E+04 | 0.160896E+02 | 0.166961E+02 | 0.154831E+02 |
| 0.125100E+04 | 0.160803E+02 | 0.166953E+02 | 0.154653E+02 |
| 0.125200E+04 | 0.160221E+02 | 0.166938E+02 | 0.153503E+02 |
| 0.125300E+04 | 0.159889E+02 | 0.166911E+02 | 0.152866E+02 |
| 0.125400E+04 | 0.159666E+02 | 0.166870E+02 | 0.152462E+02 |
| 0.125500E+04 | 0.159499E+02 | 0.166818E+02 | 0.152181E+02 |
| 0.125600E+04 | 0.159376E+02 | 0.166759E+02 | 0.151992E+02 |
| 0.125700E+04 | 0.159286E+02 | 0.166699E+02 | 0.151873E+02 |
| 0.125800E+04 | 0.159206E+02 | 0.166642E+02 | 0.151771E+02 |
| 0.125900E+04 | 0.154807E+02 | 0.161494E+02 | 0.148121E+02 |
| 0.126000E+04 | 0.152572E+02 | 0.160683E+02 | 0.144461E+02 |

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| 0.126100E+04 | 0.154588E+02 | 0.163821E+02 | 0.145355E+02 |
| 0.126200E+04 | 0.156558E+02 | 0.165738E+02 | 0.147379E+02 |
| 0.126300E+04 | 0.157923E+02 | 0.166953E+02 | 0.148893E+02 |
| 0.126400E+04 | 0.158568E+02 | 0.167406E+02 | 0.149730E+02 |
| 0.126500E+04 | 0.158900E+02 | 0.167545E+02 | 0.150255E+02 |
| 0.126600E+04 | 0.159115E+02 | 0.167589E+02 | 0.150640E+02 |
| 0.126700E+04 | 0.159227E+02 | 0.167575E+02 | 0.150880E+02 |
| 0.126800E+04 | 0.159307E+02 | 0.167536E+02 | 0.151077E+02 |
| 0.126900E+04 | 0.159367E+02 | 0.167486E+02 | 0.151247E+02 |
| 0.127000E+04 | 0.159415E+02 | 0.167434E+02 | 0.151395E+02 |
| 0.127100E+04 | 0.159453E+02 | 0.167383E+02 | 0.151523E+02 |
| 0.127200E+04 | 0.159485E+02 | 0.167336E+02 | 0.151634E+02 |
| 0.127300E+04 | 0.159510E+02 | 0.167292E+02 | 0.151729E+02 |
| 0.127400E+04 | 0.159530E+02 | 0.167251E+02 | 0.151809E+02 |
| 0.127500E+04 | 0.158544E+02 | 0.166053E+02 | 0.151035E+02 |
| 0.127600E+04 | 0.157431E+02 | 0.164816E+02 | 0.150046E+02 |
| 0.127700E+04 | 0.157788E+02 | 0.165214E+02 | 0.150362E+02 |
| 0.127800E+04 | 0.158412E+02 | 0.165867E+02 | 0.150956E+02 |
| 0.127900E+04 | 0.158710E+02 | 0.166153E+02 | 0.151268E+02 |
| 0.128000E+04 | 0.158755E+02 | 0.166076E+02 | 0.151434E+02 |
| 0.128100E+04 | 0.158771E+02 | 0.165999E+02 | 0.151544E+02 |
| 0.128200E+04 | 0.158361E+02 | 0.165112E+02 | 0.151610E+02 |
| 0.128300E+04 | 0.158180E+02 | 0.164730E+02 | 0.151631E+02 |
| 0.128400E+04 | 0.158099E+02 | 0.164557E+02 | 0.151641E+02 |
| 0.128500E+04 | 0.156680E+02 | 0.162921E+02 | 0.150440E+02 |
| 0.128600E+04 | 0.155212E+02 | 0.161200E+02 | 0.149223E+02 |
| 0.128700E+04 | 0.156037E+02 | 0.162401E+02 | 0.149673E+02 |
| 0.128800E+04 | 0.158194E+02 | 0.163373E+02 | 0.153015E+02 |
| 0.128900E+04 | 0.158806E+02 | 0.163862E+02 | 0.153750E+02 |
| 0.129000E+04 | 0.159281E+02 | 0.164155E+02 | 0.154407E+02 |
| 0.129100E+04 | 0.159485E+02 | 0.164389E+02 | 0.154580E+02 |
| 0.129200E+04 | 0.159630E+02 | 0.164605E+02 | 0.154654E+02 |
| 0.129300E+04 | 0.159730E+02 | 0.164790E+02 | 0.154671E+02 |
| 0.129400E+04 | 0.159836E+02 | 0.164952E+02 | 0.154720E+02 |
| 0.129500E+04 | 0.158516E+02 | 0.163463E+02 | 0.153569E+02 |
| 0.129600E+04 | 0.157273E+02 | 0.162135E+02 | 0.152410E+02 |
| 0.129700E+04 | 0.157762E+02 | 0.162788E+02 | 0.152737E+02 |
| 0.129800E+04 | 0.158534E+02 | 0.163750E+02 | 0.153318E+02 |
| 0.129900E+04 | 0.158826E+02 | 0.164088E+02 | 0.153563E+02 |
| 0.130000E+04 | 0.159022E+02 | 0.164376E+02 | 0.153668E+02 |
| 0.130100E+04 | 0.159166E+02 | 0.164602E+02 | 0.153729E+02 |
| 0.130200E+04 | 0.159277E+02 | 0.164786E+02 | 0.153768E+02 |
| 0.130300E+04 | 0.159365E+02 | 0.164937E+02 | 0.153794E+02 |
| 0.130400E+04 | 0.159437E+02 | 0.165063E+02 | 0.153812E+02 |
| 0.130500E+04 | 0.159498E+02 | 0.165171E+02 | 0.153825E+02 |
| 0.130600E+04 | 0.159550E+02 | 0.165265E+02 | 0.153835E+02 |
| 0.130700E+04 | 0.159595E+02 | 0.165348E+02 | 0.153843E+02 |
| 0.130800E+04 | 0.159634E+02 | 0.165420E+02 | 0.153849E+02 |
| 0.130900E+04 | 0.159669E+02 | 0.165483E+02 | 0.153854E+02 |
| 0.131000E+04 | 0.159925E+02 | 0.165984E+02 | 0.153867E+02 |
| 0.131100E+04 | 0.159960E+02 | 0.166017E+02 | 0.153903E+02 |
| 0.131200E+04 | 0.159968E+02 | 0.166020E+02 | 0.153916E+02 |
| 0.131300E+04 | 0.159915E+02 | 0.166036E+02 | 0.153793E+02 |
| 0.131400E+04 | 0.159816E+02 | 0.166054E+02 | 0.153578E+02 |

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| 0.131500E+04 | 0.159730E+02 | 0.166070E+02 | 0.153391E+02 |
| 0.131600E+04 | 0.159365E+02 | 0.166080E+02 | 0.152649E+02 |
| 0.131700E+04 | 0.158919E+02 | 0.166079E+02 | 0.151758E+02 |
| 0.131800E+04 | 0.158676E+02 | 0.166061E+02 | 0.151291E+02 |
| 0.131900E+04 | 0.158506E+02 | 0.166035E+02 | 0.150976E+02 |
| 0.132000E+04 | 0.158387E+02 | 0.166006E+02 | 0.150767E+02 |
| 0.132100E+04 | 0.158288E+02 | 0.165975E+02 | 0.150601E+02 |
| 0.132200E+04 | 0.158235E+02 | 0.165944E+02 | 0.150526E+02 |
| 0.132300E+04 | 0.158210E+02 | 0.165916E+02 | 0.150504E+02 |
| 0.132400E+04 | 0.158207E+02 | 0.165893E+02 | 0.150520E+02 |
| 0.132500E+04 | 0.158220E+02 | 0.165876E+02 | 0.150563E+02 |
| 0.132600E+04 | 0.158244E+02 | 0.165863E+02 | 0.150624E+02 |
| 0.132700E+04 | 0.158276E+02 | 0.165856E+02 | 0.150696E+02 |
| 0.132800E+04 | 0.158312E+02 | 0.165849E+02 | 0.150774E+02 |
| 0.132900E+04 | 0.157124E+02 | 0.164412E+02 | 0.149837E+02 |
| 0.133000E+04 | 0.156000E+02 | 0.163193E+02 | 0.148807E+02 |
| 0.133100E+04 | 0.156443E+02 | 0.163665E+02 | 0.149222E+02 |
| 0.133200E+04 | 0.157169E+02 | 0.164435E+02 | 0.149904E+02 |
| 0.133300E+04 | 0.157558E+02 | 0.164836E+02 | 0.150280E+02 |
| 0.133400E+04 | 0.157793E+02 | 0.165071E+02 | 0.150516E+02 |
| 0.133500E+04 | 0.157970E+02 | 0.165239E+02 | 0.150701E+02 |
| 0.133600E+04 | 0.158117E+02 | 0.165374E+02 | 0.150860E+02 |
| 0.133700E+04 | 0.158245E+02 | 0.165490E+02 | 0.151000E+02 |
| 0.133800E+04 | 0.158357E+02 | 0.165590E+02 | 0.151124E+02 |
| 0.133900E+04 | 0.158458E+02 | 0.165679E+02 | 0.151236E+02 |
| 0.134000E+04 | 0.158546E+02 | 0.165757E+02 | 0.151335E+02 |
| 0.134100E+04 | 0.158623E+02 | 0.165823E+02 | 0.151422E+02 |
| 0.134200E+04 | 0.158690E+02 | 0.165880E+02 | 0.151499E+02 |
| 0.134300E+04 | 0.158749E+02 | 0.165931E+02 | 0.151568E+02 |
| 0.134400E+04 | 0.158803E+02 | 0.165976E+02 | 0.151630E+02 |
| 0.134500E+04 | 0.157810E+02 | 0.164796E+02 | 0.150824E+02 |
| 0.134600E+04 | 0.156860E+02 | 0.163782E+02 | 0.149939E+02 |
| 0.134700E+04 | 0.157263E+02 | 0.164183E+02 | 0.150343E+02 |
| 0.134800E+04 | 0.157903E+02 | 0.164873E+02 | 0.150932E+02 |
| 0.134900E+04 | 0.158240E+02 | 0.165247E+02 | 0.151232E+02 |
| 0.135000E+04 | 0.158390E+02 | 0.165469E+02 | 0.151311E+02 |
| 0.135100E+04 | 0.158509E+02 | 0.165625E+02 | 0.151392E+02 |
| 0.135200E+04 | 0.158613E+02 | 0.165743E+02 | 0.151483E+02 |
| 0.135300E+04 | 0.158701E+02 | 0.165835E+02 | 0.151567E+02 |
| 0.135400E+04 | 0.158775E+02 | 0.165910E+02 | 0.151640E+02 |
| 0.135500E+04 | 0.158838E+02 | 0.165971E+02 | 0.151705E+02 |
| 0.135600E+04 | 0.158893E+02 | 0.166023E+02 | 0.151763E+02 |
| 0.135700E+04 | 0.158942E+02 | 0.166068E+02 | 0.151815E+02 |
| 0.135800E+04 | 0.158986E+02 | 0.166109E+02 | 0.151863E+02 |
| 0.135900E+04 | 0.159026E+02 | 0.166145E+02 | 0.151906E+02 |
| 0.136000E+04 | 0.159062E+02 | 0.166178E+02 | 0.151946E+02 |
| 0.136100E+04 | 0.159095E+02 | 0.166209E+02 | 0.151982E+02 |
| 0.136200E+04 | 0.159126E+02 | 0.166236E+02 | 0.152016E+02 |
| 0.136300E+04 | 0.159155E+02 | 0.166262E+02 | 0.152047E+02 |
| 0.136400E+04 | 0.159181E+02 | 0.166286E+02 | 0.152076E+02 |
| 0.136500E+04 | 0.159206E+02 | 0.166308E+02 | 0.152104E+02 |
| 0.136600E+04 | 0.159229E+02 | 0.166328E+02 | 0.152130E+02 |
| 0.136700E+04 | 0.159251E+02 | 0.166348E+02 | 0.152155E+02 |
| 0.136800E+04 | 0.159272E+02 | 0.166366E+02 | 0.152178E+02 |

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| 0.136900E+04 | 0.159292E+02 | 0.166383E+02 | 0.152201E+02 |
| 0.137000E+04 | 0.159311E+02 | 0.166400E+02 | 0.152223E+02 |
| 0.137100E+04 | 0.159330E+02 | 0.166415E+02 | 0.152244E+02 |
| 0.137200E+04 | 0.159347E+02 | 0.166430E+02 | 0.152265E+02 |
| 0.137300E+04 | 0.159364E+02 | 0.166444E+02 | 0.152285E+02 |
| 0.137400E+04 | 0.159380E+02 | 0.166457E+02 | 0.152303E+02 |
| 0.137500E+04 | 0.158966E+02 | 0.165967E+02 | 0.151964E+02 |
| 0.137600E+04 | 0.158502E+02 | 0.165415E+02 | 0.151589E+02 |
| 0.137700E+04 | 0.158650E+02 | 0.165592E+02 | 0.151707E+02 |
| 0.137800E+04 | 0.158934E+02 | 0.165937E+02 | 0.151931E+02 |
| 0.137900E+04 | 0.159088E+02 | 0.166121E+02 | 0.152055E+02 |
| 0.138000E+04 | 0.159172E+02 | 0.166217E+02 | 0.152128E+02 |
| 0.138100E+04 | 0.159231E+02 | 0.166279E+02 | 0.152182E+02 |
| 0.138200E+04 | 0.159275E+02 | 0.166324E+02 | 0.152227E+02 |
| 0.138300E+04 | 0.159311E+02 | 0.166357E+02 | 0.152264E+02 |
| 0.138400E+04 | 0.159339E+02 | 0.166382E+02 | 0.152295E+02 |
| 0.138500E+04 | 0.159361E+02 | 0.166400E+02 | 0.152322E+02 |
| 0.138600E+04 | 0.159378E+02 | 0.166412E+02 | 0.152344E+02 |
| 0.138700E+04 | 0.158964E+02 | 0.165921E+02 | 0.152007E+02 |
| 0.138800E+04 | 0.158500E+02 | 0.165366E+02 | 0.151633E+02 |
| 0.138900E+04 | 0.158645E+02 | 0.165539E+02 | 0.151751E+02 |
| 0.139000E+04 | 0.158926E+02 | 0.165878E+02 | 0.151973E+02 |
| 0.139100E+04 | 0.159075E+02 | 0.166056E+02 | 0.152094E+02 |
| 0.139200E+04 | 0.159154E+02 | 0.166145E+02 | 0.152164E+02 |
| 0.139300E+04 | 0.159206E+02 | 0.166199E+02 | 0.152213E+02 |
| 0.139400E+04 | 0.159242E+02 | 0.166233E+02 | 0.152250E+02 |
| 0.139500E+04 | 0.159268E+02 | 0.166256E+02 | 0.152280E+02 |
| 0.139600E+04 | 0.159285E+02 | 0.166268E+02 | 0.152302E+02 |
| 0.139700E+04 | 0.159293E+02 | 0.166270E+02 | 0.152317E+02 |
| 0.139800E+04 | 0.159296E+02 | 0.166266E+02 | 0.152327E+02 |
| 0.139900E+04 | 0.159295E+02 | 0.166257E+02 | 0.152332E+02 |
| 0.140000E+04 | 0.159290E+02 | 0.166246E+02 | 0.152335E+02 |
| 0.140100E+04 | 0.159285E+02 | 0.166234E+02 | 0.152336E+02 |
| 0.140200E+04 | 0.159281E+02 | 0.166224E+02 | 0.152338E+02 |
| 0.140300E+04 | 0.159278E+02 | 0.166215E+02 | 0.152341E+02 |
| 0.140400E+04 | 0.159274E+02 | 0.166206E+02 | 0.152342E+02 |
| 0.140500E+04 | 0.159270E+02 | 0.166197E+02 | 0.152344E+02 |
| 0.140600E+04 | 0.159266E+02 | 0.166188E+02 | 0.152345E+02 |
| 0.140700E+04 | 0.159262E+02 | 0.166178E+02 | 0.152345E+02 |
| 0.140800E+04 | 0.158868E+02 | 0.165715E+02 | 0.152022E+02 |
| 0.140900E+04 | 0.158420E+02 | 0.165183E+02 | 0.151656E+02 |
| 0.141000E+04 | 0.158664E+02 | 0.165566E+02 | 0.151763E+02 |
| 0.141100E+04 | 0.158905E+02 | 0.165843E+02 | 0.151967E+02 |
| 0.141200E+04 | 0.159030E+02 | 0.165986E+02 | 0.152075E+02 |
| 0.141300E+04 | 0.159094E+02 | 0.166055E+02 | 0.152134E+02 |
| 0.141400E+04 | 0.159135E+02 | 0.166096E+02 | 0.152175E+02 |
| 0.141500E+04 | 0.159161E+02 | 0.166118E+02 | 0.152204E+02 |
| 0.141600E+04 | 0.159174E+02 | 0.166124E+02 | 0.152223E+02 |
| 0.141700E+04 | 0.159178E+02 | 0.166121E+02 | 0.152234E+02 |
| 0.141800E+04 | 0.159176E+02 | 0.166111E+02 | 0.152240E+02 |
| 0.141900E+04 | 0.159169E+02 | 0.166096E+02 | 0.152241E+02 |
| 0.142000E+04 | 0.159159E+02 | 0.166079E+02 | 0.152239E+02 |
| 0.142100E+04 | 0.159149E+02 | 0.166062E+02 | 0.152236E+02 |
| 0.142200E+04 | 0.159140E+02 | 0.166047E+02 | 0.152234E+02 |

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| 0.142300E+04 | 0.159132E+02 | 0.166033E+02 | 0.152231E+02 |
| 0.142400E+04 | 0.159124E+02 | 0.166020E+02 | 0.152228E+02 |
| 0.142500E+04 | 0.159116E+02 | 0.166007E+02 | 0.152225E+02 |
| 0.142600E+04 | 0.159108E+02 | 0.165994E+02 | 0.152222E+02 |
| 0.142700E+04 | 0.159100E+02 | 0.165981E+02 | 0.152218E+02 |
| 0.142800E+04 | 0.159089E+02 | 0.165966E+02 | 0.152212E+02 |
| 0.142900E+04 | 0.159073E+02 | 0.165945E+02 | 0.152201E+02 |
| 0.143000E+04 | 0.159054E+02 | 0.165921E+02 | 0.152188E+02 |
| 0.143100E+04 | 0.159033E+02 | 0.165894E+02 | 0.152172E+02 |
| 0.143200E+04 | 0.159011E+02 | 0.165867E+02 | 0.152155E+02 |
| 0.143300E+04 | 0.158987E+02 | 0.165838E+02 | 0.152137E+02 |
| 0.143400E+04 | 0.158695E+02 | 0.165499E+02 | 0.151891E+02 |
| 0.143500E+04 | 0.158398E+02 | 0.165154E+02 | 0.151643E+02 |
| 0.143600E+04 | 0.158475E+02 | 0.165247E+02 | 0.151703E+02 |
| 0.143700E+04 | 0.158607E+02 | 0.165407E+02 | 0.151806E+02 |
| 0.143800E+04 | 0.158662E+02 | 0.165476E+02 | 0.151849E+02 |
| 0.143900E+04 | 0.158682E+02 | 0.165500E+02 | 0.151863E+02 |
| 0.144000E+04 | 0.158687E+02 | 0.165506E+02 | 0.151867E+02 |
| 0.144100E+04 | 0.158684E+02 | 0.165502E+02 | 0.151865E+02 |
| 0.144200E+04 | 0.158678E+02 | 0.165494E+02 | 0.151862E+02 |
| 0.144300E+04 | 0.158674E+02 | 0.165489E+02 | 0.151860E+02 |
| 0.144400E+04 | 0.158671E+02 | 0.165484E+02 | 0.151859E+02 |
| 0.144500E+04 | 0.158668E+02 | 0.165480E+02 | 0.151857E+02 |
| 0.144600E+04 | 0.158666E+02 | 0.165475E+02 | 0.151856E+02 |
| 0.144700E+04 | 0.158663E+02 | 0.165471E+02 | 0.151855E+02 |
| 0.144800E+04 | 0.158660E+02 | 0.165467E+02 | 0.151853E+02 |
| 0.144900E+04 | 0.158657E+02 | 0.165463E+02 | 0.151852E+02 |
| 0.145000E+04 | 0.158655E+02 | 0.165459E+02 | 0.151850E+02 |
| 0.145100E+04 | 0.158652E+02 | 0.165456E+02 | 0.151849E+02 |
| 0.145200E+04 | 0.158649E+02 | 0.165451E+02 | 0.151847E+02 |
| 0.145300E+04 | 0.156986E+02 | 0.163494E+02 | 0.150478E+02 |
| 0.145400E+04 | 0.155209E+02 | 0.161407E+02 | 0.149010E+02 |
| 0.145500E+04 | 0.157223E+02 | 0.162074E+02 | 0.152372E+02 |
| 0.145600E+04 | 0.158517E+02 | 0.163244E+02 | 0.153790E+02 |
| 0.145700E+04 | 0.159077E+02 | 0.163858E+02 | 0.154296E+02 |
| 0.145800E+04 | 0.159363E+02 | 0.164214E+02 | 0.154512E+02 |
| 0.145900E+04 | 0.157969E+02 | 0.162550E+02 | 0.153387E+02 |
| 0.146000E+04 | 0.155383E+02 | 0.159649E+02 | 0.151117E+02 |
| 0.146100E+04 | 0.154816E+02 | 0.159199E+02 | 0.150434E+02 |
| 0.146200E+04 | 0.156008E+02 | 0.160635E+02 | 0.151380E+02 |
| 0.146300E+04 | 0.157161E+02 | 0.161978E+02 | 0.152344E+02 |
| 0.146400E+04 | 0.157779E+02 | 0.162756E+02 | 0.152803E+02 |
| 0.146500E+04 | 0.158130E+02 | 0.163221E+02 | 0.153038E+02 |
| 0.146600E+04 | 0.157942E+02 | 0.163031E+02 | 0.152853E+02 |
| 0.146700E+04 | 0.157648E+02 | 0.162694E+02 | 0.152601E+02 |
| 0.146800E+04 | 0.157932E+02 | 0.163077E+02 | 0.152787E+02 |
| 0.146900E+04 | 0.158317E+02 | 0.163593E+02 | 0.153042E+02 |
| 0.147000E+04 | 0.158545E+02 | 0.163903E+02 | 0.153187E+02 |
| 0.147100E+04 | 0.158691E+02 | 0.164102E+02 | 0.153280E+02 |
| 0.147200E+04 | 0.158802E+02 | 0.164253E+02 | 0.153350E+02 |
| 0.147300E+04 | 0.159115E+02 | 0.164797E+02 | 0.153434E+02 |
| 0.147400E+04 | 0.159169E+02 | 0.164839E+02 | 0.153498E+02 |
| 0.147500E+04 | 0.159220E+02 | 0.164894E+02 | 0.153546E+02 |
| 0.147600E+04 | 0.159266E+02 | 0.164947E+02 | 0.153586E+02 |

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| 0.147700E+04 | 0.159309E+02 | 0.164996E+02 | 0.153622E+02 |
| 0.147800E+04 | 0.159350E+02 | 0.165043E+02 | 0.153657E+02 |
| 0.147900E+04 | 0.159388E+02 | 0.165085E+02 | 0.153690E+02 |
| 0.148000E+04 | 0.159372E+02 | 0.165125E+02 | 0.153620E+02 |
| 0.148100E+04 | 0.158914E+02 | 0.164707E+02 | 0.153122E+02 |
| 0.148200E+04 | 0.158569E+02 | 0.164401E+02 | 0.152738E+02 |
| 0.148300E+04 | 0.158195E+02 | 0.164051E+02 | 0.152338E+02 |
| 0.148400E+04 | 0.158050E+02 | 0.164048E+02 | 0.152052E+02 |
| 0.148500E+04 | 0.158148E+02 | 0.164485E+02 | 0.151811E+02 |
| 0.148600E+04 | 0.157900E+02 | 0.164782E+02 | 0.151018E+02 |
| 0.148700E+04 | 0.157779E+02 | 0.164903E+02 | 0.150655E+02 |
| 0.148800E+04 | 0.157811E+02 | 0.164964E+02 | 0.150659E+02 |
| 0.148900E+04 | 0.157809E+02 | 0.164998E+02 | 0.150620E+02 |
| 0.149000E+04 | 0.157796E+02 | 0.165015E+02 | 0.150577E+02 |
| 0.149100E+04 | 0.157791E+02 | 0.165020E+02 | 0.150562E+02 |
| 0.149200E+04 | 0.157799E+02 | 0.165020E+02 | 0.150578E+02 |
| 0.149300E+04 | 0.157818E+02 | 0.165018E+02 | 0.150619E+02 |
| 0.149400E+04 | 0.157847E+02 | 0.165016E+02 | 0.150678E+02 |
| 0.149500E+04 | 0.157421E+02 | 0.164482E+02 | 0.150361E+02 |
| 0.149600E+04 | 0.156978E+02 | 0.163923E+02 | 0.150032E+02 |
| 0.149700E+04 | 0.157180E+02 | 0.164116E+02 | 0.150243E+02 |
| 0.149800E+04 | 0.157474E+02 | 0.164401E+02 | 0.150546E+02 |
| 0.149900E+04 | 0.157617E+02 | 0.164504E+02 | 0.150730E+02 |
| 0.150000E+04 | 0.157724E+02 | 0.164588E+02 | 0.150861E+02 |
| 0.150100E+04 | 0.157814E+02 | 0.164655E+02 | 0.150973E+02 |
| 0.150200E+04 | 0.157892E+02 | 0.164712E+02 | 0.151072E+02 |
| 0.150300E+04 | 0.157961E+02 | 0.164760E+02 | 0.151162E+02 |
| 0.150400E+04 | 0.157634E+02 | 0.164350E+02 | 0.150919E+02 |
| 0.150500E+04 | 0.157249E+02 | 0.163868E+02 | 0.150630E+02 |
| 0.150600E+04 | 0.157419E+02 | 0.164051E+02 | 0.150788E+02 |
| 0.150700E+04 | 0.157725E+02 | 0.164402E+02 | 0.151047E+02 |
| 0.150800E+04 | 0.157905E+02 | 0.164605E+02 | 0.151205E+02 |
| 0.150900E+04 | 0.158017E+02 | 0.164724E+02 | 0.151309E+02 |
| 0.151000E+04 | 0.158103E+02 | 0.164812E+02 | 0.151393E+02 |
| 0.151100E+04 | 0.158174E+02 | 0.164884E+02 | 0.151463E+02 |
| 0.151200E+04 | 0.158233E+02 | 0.164944E+02 | 0.151523E+02 |
| 0.151300E+04 | 0.158283E+02 | 0.164993E+02 | 0.151573E+02 |
| 0.151400E+04 | 0.158325E+02 | 0.165034E+02 | 0.151616E+02 |
| 0.151500E+04 | 0.158360E+02 | 0.165068E+02 | 0.151652E+02 |
| 0.151600E+04 | 0.158391E+02 | 0.165099E+02 | 0.151684E+02 |
| 0.151700E+04 | 0.158418E+02 | 0.165126E+02 | 0.151711E+02 |
| 0.151800E+04 | 0.158442E+02 | 0.165151E+02 | 0.151734E+02 |
| 0.151900E+04 | 0.158464E+02 | 0.165174E+02 | 0.151754E+02 |
| 0.152000E+04 | 0.158485E+02 | 0.165197E+02 | 0.151773E+02 |
| 0.152100E+04 | 0.158505E+02 | 0.165220E+02 | 0.151790E+02 |
| 0.152200E+04 | 0.158523E+02 | 0.165241E+02 | 0.151805E+02 |
| 0.152300E+04 | 0.158541E+02 | 0.165262E+02 | 0.151819E+02 |
| 0.152400E+04 | 0.158556E+02 | 0.165280E+02 | 0.151832E+02 |
| 0.152500E+04 | 0.158570E+02 | 0.165298E+02 | 0.151843E+02 |
| 0.152600E+04 | 0.158584E+02 | 0.165314E+02 | 0.151853E+02 |
| 0.152700E+04 | 0.157598E+02 | 0.164234E+02 | 0.150961E+02 |
| 0.152800E+04 | 0.156533E+02 | 0.162994E+02 | 0.150073E+02 |
| 0.152900E+04 | 0.156942E+02 | 0.163413E+02 | 0.150471E+02 |
| 0.153000E+04 | 0.157584E+02 | 0.164157E+02 | 0.151010E+02 |

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| 0.153100E+04 | 0.157864E+02 | 0.164467E+02 | 0.151262E+02 |
| 0.153200E+04 | 0.157988E+02 | 0.164660E+02 | 0.151317E+02 |
| 0.153300E+04 | 0.158070E+02 | 0.164795E+02 | 0.151346E+02 |
| 0.153400E+04 | 0.158147E+02 | 0.164895E+02 | 0.151400E+02 |
| 0.153500E+04 | 0.158212E+02 | 0.164971E+02 | 0.151452E+02 |
| 0.153600E+04 | 0.158266E+02 | 0.165033E+02 | 0.151499E+02 |
| 0.153700E+04 | 0.158312E+02 | 0.165085E+02 | 0.151540E+02 |
| 0.153800E+04 | 0.158351E+02 | 0.165127E+02 | 0.151575E+02 |
| 0.153900E+04 | 0.158383E+02 | 0.165161E+02 | 0.151606E+02 |
| 0.154000E+04 | 0.158410E+02 | 0.165189E+02 | 0.151632E+02 |
| 0.154100E+04 | 0.158435E+02 | 0.165214E+02 | 0.151656E+02 |
| 0.154200E+04 | 0.158457E+02 | 0.165237E+02 | 0.151677E+02 |
| 0.154300E+04 | 0.158477E+02 | 0.165258E+02 | 0.151696E+02 |
| 0.154400E+04 | 0.158495E+02 | 0.165277E+02 | 0.151714E+02 |
| 0.154500E+04 | 0.158512E+02 | 0.165294E+02 | 0.151730E+02 |
| 0.154600E+04 | 0.158527E+02 | 0.165310E+02 | 0.151744E+02 |
| 0.154700E+04 | 0.158541E+02 | 0.165324E+02 | 0.151757E+02 |
| 0.154800E+04 | 0.158553E+02 | 0.165337E+02 | 0.151769E+02 |
| 0.154900E+04 | 0.158564E+02 | 0.165349E+02 | 0.151780E+02 |
| 0.155000E+04 | 0.158574E+02 | 0.165359E+02 | 0.151790E+02 |
| 0.155100E+04 | 0.158583E+02 | 0.165368E+02 | 0.151798E+02 |
| 0.155200E+04 | 0.158591E+02 | 0.165375E+02 | 0.151806E+02 |
| 0.155300E+04 | 0.158597E+02 | 0.165381E+02 | 0.151813E+02 |
| 0.155400E+04 | 0.158603E+02 | 0.165387E+02 | 0.151819E+02 |
| 0.155500E+04 | 0.158608E+02 | 0.165391E+02 | 0.151825E+02 |
| 0.155600E+04 | 0.158612E+02 | 0.165395E+02 | 0.151830E+02 |
| 0.155700E+04 | 0.158616E+02 | 0.165398E+02 | 0.151834E+02 |
| 0.155800E+04 | 0.158620E+02 | 0.165401E+02 | 0.151839E+02 |
| 0.155900E+04 | 0.158623E+02 | 0.165402E+02 | 0.151843E+02 |
| 0.156000E+04 | 0.158626E+02 | 0.165405E+02 | 0.151848E+02 |
| 0.156100E+04 | 0.158631E+02 | 0.165409E+02 | 0.151853E+02 |
| 0.156200E+04 | 0.158636E+02 | 0.165413E+02 | 0.151860E+02 |
| 0.156300E+04 | 0.158642E+02 | 0.165418E+02 | 0.151866E+02 |
| 0.156400E+04 | 0.157724E+02 | 0.164418E+02 | 0.151029E+02 |
| 0.156500E+04 | 0.156692E+02 | 0.163206E+02 | 0.150179E+02 |
| 0.156600E+04 | 0.157063E+02 | 0.163577E+02 | 0.150550E+02 |
| 0.156700E+04 | 0.157687E+02 | 0.164302E+02 | 0.151072E+02 |
| 0.156800E+04 | 0.157953E+02 | 0.164588E+02 | 0.151319E+02 |
| 0.156900E+04 | 0.158074E+02 | 0.164769E+02 | 0.151379E+02 |
| 0.157000E+04 | 0.157880E+02 | 0.164580E+02 | 0.151180E+02 |
| 0.157100E+04 | 0.157674E+02 | 0.164338E+02 | 0.151011E+02 |
| 0.157200E+04 | 0.157842E+02 | 0.164534E+02 | 0.151150E+02 |
| 0.157300E+04 | 0.158059E+02 | 0.164792E+02 | 0.151326E+02 |
| 0.157400E+04 | 0.158187E+02 | 0.164940E+02 | 0.151434E+02 |
| 0.157500E+04 | 0.158270E+02 | 0.165031E+02 | 0.151509E+02 |
| 0.157600E+04 | 0.158334E+02 | 0.165097E+02 | 0.151570E+02 |
| 0.157700E+04 | 0.158386E+02 | 0.165150E+02 | 0.151622E+02 |
| 0.157800E+04 | 0.158433E+02 | 0.165196E+02 | 0.151670E+02 |
| 0.157900E+04 | 0.158475E+02 | 0.165236E+02 | 0.151713E+02 |
| 0.158000E+04 | 0.158512E+02 | 0.165272E+02 | 0.151753E+02 |
| 0.158100E+04 | 0.158547E+02 | 0.165305E+02 | 0.151789E+02 |
| 0.158200E+04 | 0.158579E+02 | 0.165334E+02 | 0.151823E+02 |
| 0.158300E+04 | 0.158609E+02 | 0.165362E+02 | 0.151855E+02 |
| 0.158400E+04 | 0.158637E+02 | 0.165389E+02 | 0.151885E+02 |

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| 0.158500E+04 | 0.158663E+02 | 0.165412E+02 | 0.151913E+02 |
| 0.158600E+04 | 0.158686E+02 | 0.165433E+02 | 0.151938E+02 |
| 0.158700E+04 | 0.157132E+02 | 0.163666E+02 | 0.150598E+02 |
| 0.158800E+04 | 0.154744E+02 | 0.160886E+02 | 0.148602E+02 |
| 0.158900E+04 | 0.156158E+02 | 0.161042E+02 | 0.151275E+02 |
| 0.159000E+04 | 0.157461E+02 | 0.162183E+02 | 0.152740E+02 |
| 0.159100E+04 | 0.158410E+02 | 0.163176E+02 | 0.153645E+02 |
| 0.159200E+04 | 0.158870E+02 | 0.163685E+02 | 0.154054E+02 |
| 0.159300E+04 | 0.159142E+02 | 0.164041E+02 | 0.154242E+02 |
| 0.159400E+04 | 0.159325E+02 | 0.164313E+02 | 0.154336E+02 |
| 0.159500E+04 | 0.159455E+02 | 0.164532E+02 | 0.154379E+02 |
| 0.159600E+04 | 0.159551E+02 | 0.164710E+02 | 0.154392E+02 |
| 0.159700E+04 | 0.159644E+02 | 0.164859E+02 | 0.154430E+02 |
| 0.159800E+04 | 0.159701E+02 | 0.164982E+02 | 0.154419E+02 |
| 0.159900E+04 | 0.159745E+02 | 0.165086E+02 | 0.154403E+02 |
| 0.160000E+04 | 0.160005E+02 | 0.165604E+02 | 0.154407E+02 |
| 0.160100E+04 | 0.158122E+02 | 0.163475E+02 | 0.152769E+02 |
| 0.160200E+04 | 0.155901E+02 | 0.160843E+02 | 0.150960E+02 |
| 0.160300E+04 | 0.156584E+02 | 0.161822E+02 | 0.151345E+02 |
| 0.160400E+04 | 0.157541E+02 | 0.162916E+02 | 0.152167E+02 |
| 0.160500E+04 | 0.158219E+02 | 0.163701E+02 | 0.152738E+02 |
| 0.160600E+04 | 0.158609E+02 | 0.164181E+02 | 0.153037E+02 |
| 0.160700E+04 | 0.158846E+02 | 0.164478E+02 | 0.153213E+02 |
| 0.160800E+04 | 0.159013E+02 | 0.164688E+02 | 0.153337E+02 |
| 0.160900E+04 | 0.159137E+02 | 0.164844E+02 | 0.153430E+02 |
| 0.161000E+04 | 0.159238E+02 | 0.164969E+02 | 0.153507E+02 |
| 0.161100E+04 | 0.159318E+02 | 0.165068E+02 | 0.153569E+02 |
| 0.161200E+04 | 0.159390E+02 | 0.165186E+02 | 0.153594E+02 |
| 0.161300E+04 | 0.158339E+02 | 0.164063E+02 | 0.152615E+02 |
| 0.161400E+04 | 0.157340E+02 | 0.163069E+02 | 0.151611E+02 |
| 0.161500E+04 | 0.157652E+02 | 0.163531E+02 | 0.151773E+02 |
| 0.161600E+04 | 0.157956E+02 | 0.164189E+02 | 0.151722E+02 |
| 0.161700E+04 | 0.157667E+02 | 0.164460E+02 | 0.150875E+02 |
| 0.161800E+04 | 0.157532E+02 | 0.164578E+02 | 0.150485E+02 |
| 0.161900E+04 | 0.157451E+02 | 0.164659E+02 | 0.150243E+02 |
| 0.162000E+04 | 0.157411E+02 | 0.164719E+02 | 0.150103E+02 |
| 0.162100E+04 | 0.157380E+02 | 0.164753E+02 | 0.150008E+02 |
| 0.162200E+04 | 0.156322E+02 | 0.163359E+02 | 0.149285E+02 |
| 0.162300E+04 | 0.155198E+02 | 0.162136E+02 | 0.148260E+02 |
| 0.162400E+04 | 0.155648E+02 | 0.162614E+02 | 0.148681E+02 |
| 0.162500E+04 | 0.156398E+02 | 0.163442E+02 | 0.149354E+02 |
| 0.162600E+04 | 0.156795E+02 | 0.163859E+02 | 0.149732E+02 |
| 0.162700E+04 | 0.157020E+02 | 0.164082E+02 | 0.149957E+02 |
| 0.162800E+04 | 0.157181E+02 | 0.164227E+02 | 0.150135E+02 |
| 0.162900E+04 | 0.157311E+02 | 0.164333E+02 | 0.150289E+02 |
| 0.163000E+04 | 0.157403E+02 | 0.164394E+02 | 0.150413E+02 |
| 0.163100E+04 | 0.157475E+02 | 0.164431E+02 | 0.150518E+02 |
| 0.163200E+04 | 0.157543E+02 | 0.164467E+02 | 0.150619E+02 |
| 0.163300E+04 | 0.157616E+02 | 0.164512E+02 | 0.150719E+02 |
| 0.163400E+04 | 0.157685E+02 | 0.164556E+02 | 0.150813E+02 |
| 0.163500E+04 | 0.157740E+02 | 0.164587E+02 | 0.150892E+02 |
| 0.163600E+04 | 0.157788E+02 | 0.164614E+02 | 0.150962E+02 |
| 0.163700E+04 | 0.157831E+02 | 0.164638E+02 | 0.151025E+02 |
| 0.163800E+04 | 0.157918E+02 | 0.164716E+02 | 0.151120E+02 |

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| 0.163900E+04 | 0.158041E+02 | 0.164840E+02 | 0.151243E+02 |
| 0.164000E+04 | 0.158098E+02 | 0.164888E+02 | 0.151308E+02 |
| 0.164100E+04 | 0.156118E+02 | 0.162614E+02 | 0.149622E+02 |
| 0.164200E+04 | 0.153879E+02 | 0.160021E+02 | 0.147737E+02 |
| 0.164300E+04 | 0.154603E+02 | 0.160982E+02 | 0.148224E+02 |
| 0.164400E+04 | 0.156929E+02 | 0.162056E+02 | 0.151802E+02 |
| 0.164500E+04 | 0.157847E+02 | 0.162687E+02 | 0.153006E+02 |
| 0.164600E+04 | 0.158189E+02 | 0.163024E+02 | 0.153354E+02 |
| 0.164700E+04 | 0.158534E+02 | 0.163439E+02 | 0.153628E+02 |
| 0.164800E+04 | 0.158805E+02 | 0.163807E+02 | 0.153803E+02 |
| 0.164900E+04 | 0.158974E+02 | 0.164068E+02 | 0.153879E+02 |
| 0.165000E+04 | 0.159084E+02 | 0.164265E+02 | 0.153903E+02 |
| 0.165100E+04 | 0.159161E+02 | 0.164422E+02 | 0.153899E+02 |
| 0.165200E+04 | 0.159217E+02 | 0.164552E+02 | 0.153881E+02 |
| 0.165300E+04 | 0.159279E+02 | 0.164660E+02 | 0.153898E+02 |
| 0.165400E+04 | 0.159304E+02 | 0.164744E+02 | 0.153863E+02 |
| 0.165500E+04 | 0.159319E+02 | 0.164811E+02 | 0.153827E+02 |
| 0.165600E+04 | 0.159327E+02 | 0.164865E+02 | 0.153790E+02 |
| 0.165700E+04 | 0.159331E+02 | 0.164909E+02 | 0.153754E+02 |
| 0.165800E+04 | 0.159331E+02 | 0.164944E+02 | 0.153718E+02 |
| 0.165900E+04 | 0.159327E+02 | 0.164971E+02 | 0.153683E+02 |
| 0.166000E+04 | 0.159323E+02 | 0.164995E+02 | 0.153652E+02 |
| 0.166100E+04 | 0.159318E+02 | 0.165014E+02 | 0.153622E+02 |
| 0.166200E+04 | 0.159311E+02 | 0.165028E+02 | 0.153594E+02 |
| 0.166300E+04 | 0.159302E+02 | 0.165038E+02 | 0.153566E+02 |
| 0.166400E+04 | 0.159293E+02 | 0.165046E+02 | 0.153541E+02 |
| 0.166500E+04 | 0.159285E+02 | 0.165052E+02 | 0.153517E+02 |
| 0.166600E+04 | 0.159276E+02 | 0.165056E+02 | 0.153495E+02 |
| 0.166700E+04 | 0.158344E+02 | 0.163951E+02 | 0.152737E+02 |
| 0.166800E+04 | 0.157492E+02 | 0.163029E+02 | 0.151955E+02 |
| 0.166900E+04 | 0.157781E+02 | 0.163374E+02 | 0.152187E+02 |
| 0.167000E+04 | 0.158318E+02 | 0.164032E+02 | 0.152604E+02 |
| 0.167100E+04 | 0.158537E+02 | 0.164279E+02 | 0.152796E+02 |
| 0.167200E+04 | 0.158664E+02 | 0.164438E+02 | 0.152889E+02 |
| 0.167300E+04 | 0.158750E+02 | 0.164551E+02 | 0.152949E+02 |
| 0.167400E+04 | 0.157526E+02 | 0.163090E+02 | 0.151962E+02 |
| 0.167500E+04 | 0.156341E+02 | 0.161792E+02 | 0.150889E+02 |
| 0.167600E+04 | 0.156827E+02 | 0.162345E+02 | 0.151310E+02 |
| 0.167700E+04 | 0.157585E+02 | 0.163240E+02 | 0.151930E+02 |
| 0.167800E+04 | 0.157964E+02 | 0.163696E+02 | 0.152232E+02 |
| 0.167900E+04 | 0.158178E+02 | 0.163960E+02 | 0.152395E+02 |
| 0.168000E+04 | 0.158326E+02 | 0.164144E+02 | 0.152509E+02 |
| 0.168100E+04 | 0.157374E+02 | 0.163006E+02 | 0.151742E+02 |
| 0.168200E+04 | 0.156461E+02 | 0.161991E+02 | 0.150931E+02 |
| 0.168300E+04 | 0.156884E+02 | 0.162485E+02 | 0.151283E+02 |
| 0.168400E+04 | 0.157531E+02 | 0.163242E+02 | 0.151820E+02 |
| 0.168500E+04 | 0.157872E+02 | 0.163648E+02 | 0.152095E+02 |
| 0.168600E+04 | 0.158072E+02 | 0.163891E+02 | 0.152252E+02 |
| 0.168700E+04 | 0.158181E+02 | 0.164063E+02 | 0.152299E+02 |
| 0.168800E+04 | 0.158173E+02 | 0.164190E+02 | 0.152155E+02 |
| 0.168900E+04 | 0.157868E+02 | 0.163945E+02 | 0.151790E+02 |
| 0.169000E+04 | 0.157641E+02 | 0.163768E+02 | 0.151515E+02 |
| 0.169100E+04 | 0.157200E+02 | 0.164022E+02 | 0.150378E+02 |
| 0.169200E+04 | 0.157075E+02 | 0.164202E+02 | 0.149947E+02 |

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| 0.169300E+04 | 0.156965E+02 | 0.164278E+02 | 0.149651E+02 |
| 0.169400E+04 | 0.156887E+02 | 0.164316E+02 | 0.149458E+02 |
| 0.169500E+04 | 0.155731E+02 | 0.162939E+02 | 0.148524E+02 |
| 0.169600E+04 | 0.154275E+02 | 0.161307E+02 | 0.147242E+02 |
| 0.169700E+04 | 0.154729E+02 | 0.161823E+02 | 0.147636E+02 |
| 0.169800E+04 | 0.155553E+02 | 0.162755E+02 | 0.148352E+02 |
| 0.169900E+04 | 0.155967E+02 | 0.163202E+02 | 0.148733E+02 |
| 0.170000E+04 | 0.156214E+02 | 0.163459E+02 | 0.148968E+02 |
| 0.170100E+04 | 0.156400E+02 | 0.163638E+02 | 0.149161E+02 |
| 0.170200E+04 | 0.156552E+02 | 0.163771E+02 | 0.149332E+02 |
| 0.170300E+04 | 0.156684E+02 | 0.163878E+02 | 0.149489E+02 |
| 0.170400E+04 | 0.156802E+02 | 0.163968E+02 | 0.149636E+02 |
| 0.170500E+04 | 0.156909E+02 | 0.164047E+02 | 0.149772E+02 |
| 0.170600E+04 | 0.157005E+02 | 0.164113E+02 | 0.149897E+02 |
| 0.170700E+04 | 0.157091E+02 | 0.164172E+02 | 0.150011E+02 |
| 0.170800E+04 | 0.157169E+02 | 0.164224E+02 | 0.150115E+02 |
| 0.170900E+04 | 0.157239E+02 | 0.164268E+02 | 0.150209E+02 |
| 0.171000E+04 | 0.157302E+02 | 0.164310E+02 | 0.150294E+02 |
| 0.171100E+04 | 0.157362E+02 | 0.164351E+02 | 0.150373E+02 |
| 0.171200E+04 | 0.157419E+02 | 0.164391E+02 | 0.150447E+02 |
| 0.171300E+04 | 0.157472E+02 | 0.164429E+02 | 0.150514E+02 |
| 0.171400E+04 | 0.157522E+02 | 0.164468E+02 | 0.150577E+02 |
| 0.171500E+04 | 0.157573E+02 | 0.164508E+02 | 0.150637E+02 |
| 0.171600E+04 | 0.157624E+02 | 0.164552E+02 | 0.150696E+02 |
| 0.171700E+04 | 0.157679E+02 | 0.164602E+02 | 0.150755E+02 |
| 0.171800E+04 | 0.157726E+02 | 0.164645E+02 | 0.150807E+02 |
| 0.171900E+04 | 0.157778E+02 | 0.164695E+02 | 0.150861E+02 |
| 0.172000E+04 | 0.157834E+02 | 0.164751E+02 | 0.150917E+02 |
| 0.172100E+04 | 0.157875E+02 | 0.164790E+02 | 0.150960E+02 |
| 0.172200E+04 | 0.157904E+02 | 0.164816E+02 | 0.150992E+02 |
| 0.172300E+04 | 0.157922E+02 | 0.164831E+02 | 0.151014E+02 |
| 0.172400E+04 | 0.157947E+02 | 0.164854E+02 | 0.151041E+02 |
| 0.172500E+04 | 0.157980E+02 | 0.164887E+02 | 0.151073E+02 |
| 0.172600E+04 | 0.158025E+02 | 0.164935E+02 | 0.151114E+02 |
| 0.172700E+04 | 0.158079E+02 | 0.164995E+02 | 0.151162E+02 |
| 0.172800E+04 | 0.158140E+02 | 0.165064E+02 | 0.151217E+02 |
| 0.172900E+04 | 0.156973E+02 | 0.163766E+02 | 0.150180E+02 |
| 0.173000E+04 | 0.156142E+02 | 0.162820E+02 | 0.149463E+02 |
| 0.173100E+04 | 0.156949E+02 | 0.163697E+02 | 0.150201E+02 |
| 0.173200E+04 | 0.157397E+02 | 0.164162E+02 | 0.150632E+02 |
| 0.173300E+04 | 0.157571E+02 | 0.164387E+02 | 0.150755E+02 |
| 0.173400E+04 | 0.157614E+02 | 0.164530E+02 | 0.150697E+02 |
| 0.173500E+04 | 0.157679E+02 | 0.164650E+02 | 0.150708E+02 |
| 0.173600E+04 | 0.157772E+02 | 0.164781E+02 | 0.150763E+02 |
| 0.173700E+04 | 0.157852E+02 | 0.164885E+02 | 0.150818E+02 |
| 0.173800E+04 | 0.157893E+02 | 0.164938E+02 | 0.150848E+02 |
| 0.173900E+04 | 0.157441E+02 | 0.164411E+02 | 0.150472E+02 |
| 0.174000E+04 | 0.157109E+02 | 0.164014E+02 | 0.150203E+02 |
| 0.174100E+04 | 0.157445E+02 | 0.164414E+02 | 0.150475E+02 |
| 0.174200E+04 | 0.157701E+02 | 0.164725E+02 | 0.150677E+02 |
| 0.174300E+04 | 0.157796E+02 | 0.164836E+02 | 0.150755E+02 |
| 0.174400E+04 | 0.157848E+02 | 0.164894E+02 | 0.150802E+02 |
| 0.174500E+04 | 0.157889E+02 | 0.164937E+02 | 0.150841E+02 |
| 0.174600E+04 | 0.157926E+02 | 0.164975E+02 | 0.150877E+02 |

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| 0.174700E+04 | 0.157961E+02 | 0.165011E+02 | 0.150911E+02 |
| 0.174800E+04 | 0.158034E+02 | 0.165091E+02 | 0.150977E+02 |
| 0.174900E+04 | 0.158135E+02 | 0.165204E+02 | 0.151066E+02 |
| 0.175000E+04 | 0.158207E+02 | 0.165282E+02 | 0.151131E+02 |
| 0.175100E+04 | 0.158239E+02 | 0.165313E+02 | 0.151164E+02 |
| 0.175200E+04 | 0.158247E+02 | 0.165316E+02 | 0.151178E+02 |
| 0.175300E+04 | 0.158257E+02 | 0.165321E+02 | 0.151192E+02 |
| 0.175400E+04 | 0.158261E+02 | 0.165320E+02 | 0.151202E+02 |
| 0.175500E+04 | 0.158260E+02 | 0.165314E+02 | 0.151205E+02 |
| 0.175600E+04 | 0.158265E+02 | 0.165316E+02 | 0.151215E+02 |
| 0.175700E+04 | 0.158289E+02 | 0.165339E+02 | 0.151239E+02 |
| 0.175800E+04 | 0.158332E+02 | 0.165386E+02 | 0.151279E+02 |
| 0.175900E+04 | 0.158383E+02 | 0.165441E+02 | 0.151325E+02 |
| 0.176000E+04 | 0.158424E+02 | 0.165484E+02 | 0.151364E+02 |
| 0.176100E+04 | 0.158468E+02 | 0.165530E+02 | 0.151406E+02 |
| 0.176200E+04 | 0.158506E+02 | 0.165569E+02 | 0.151443E+02 |
| 0.176300E+04 | 0.158516E+02 | 0.165575E+02 | 0.151458E+02 |
| 0.176400E+04 | 0.158518E+02 | 0.165571E+02 | 0.151465E+02 |
| 0.176500E+04 | 0.158509E+02 | 0.165554E+02 | 0.151463E+02 |
| 0.176600E+04 | 0.158489E+02 | 0.165526E+02 | 0.151452E+02 |
| 0.176700E+04 | 0.158496E+02 | 0.165530E+02 | 0.151462E+02 |
| 0.176800E+04 | 0.158551E+02 | 0.165590E+02 | 0.151512E+02 |
| 0.176900E+04 | 0.158634E+02 | 0.165682E+02 | 0.151585E+02 |
| 0.177000E+04 | 0.158709E+02 | 0.165766E+02 | 0.151653E+02 |
| 0.177100E+04 | 0.158751E+02 | 0.165809E+02 | 0.151694E+02 |
| 0.177200E+04 | 0.158766E+02 | 0.165819E+02 | 0.151713E+02 |
| 0.177300E+04 | 0.158752E+02 | 0.165796E+02 | 0.151709E+02 |
| 0.177400E+04 | 0.158724E+02 | 0.165756E+02 | 0.151692E+02 |
| 0.177500E+04 | 0.158693E+02 | 0.165714E+02 | 0.151672E+02 |
| 0.177600E+04 | 0.158669E+02 | 0.165681E+02 | 0.151656E+02 |
| 0.177700E+04 | 0.158677E+02 | 0.165687E+02 | 0.151667E+02 |
| 0.177800E+04 | 0.158724E+02 | 0.165738E+02 | 0.151709E+02 |
| 0.177900E+04 | 0.158785E+02 | 0.165807E+02 | 0.151764E+02 |
| 0.178000E+04 | 0.158816E+02 | 0.165838E+02 | 0.151794E+02 |
| 0.178100E+04 | 0.158830E+02 | 0.165849E+02 | 0.151811E+02 |
| 0.178200E+04 | 0.158829E+02 | 0.165842E+02 | 0.151816E+02 |
| 0.178300E+04 | 0.157636E+02 | 0.164499E+02 | 0.150773E+02 |
| 0.178400E+04 | 0.156658E+02 | 0.163405E+02 | 0.149910E+02 |
| 0.178500E+04 | 0.157445E+02 | 0.164247E+02 | 0.150644E+02 |
| 0.178600E+04 | 0.157928E+02 | 0.164771E+02 | 0.151084E+02 |
| 0.178700E+04 | 0.158182E+02 | 0.165069E+02 | 0.151295E+02 |
| 0.178800E+04 | 0.158312E+02 | 0.165278E+02 | 0.151346E+02 |
| 0.178900E+04 | 0.157948E+02 | 0.164865E+02 | 0.151030E+02 |
| 0.179000E+04 | 0.157519E+02 | 0.164358E+02 | 0.150681E+02 |
| 0.179100E+04 | 0.157715E+02 | 0.164588E+02 | 0.150843E+02 |
| 0.179200E+04 | 0.158025E+02 | 0.164957E+02 | 0.151093E+02 |
| 0.179300E+04 | 0.158196E+02 | 0.165156E+02 | 0.151236E+02 |
| 0.179400E+04 | 0.158288E+02 | 0.165256E+02 | 0.151320E+02 |
| 0.179500E+04 | 0.158336E+02 | 0.165302E+02 | 0.151369E+02 |
| 0.179600E+04 | 0.158362E+02 | 0.165323E+02 | 0.151400E+02 |
| 0.179700E+04 | 0.158380E+02 | 0.165335E+02 | 0.151425E+02 |
| 0.179800E+04 | 0.158392E+02 | 0.165340E+02 | 0.151444E+02 |
| 0.179900E+04 | 0.158405E+02 | 0.165347E+02 | 0.151462E+02 |
| 0.180000E+04 | 0.158423E+02 | 0.165361E+02 | 0.151484E+02 |

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| 0.180100E+04 | 0.158470E+02 | 0.165409E+02 | 0.151530E+02 |
| 0.180200E+04 | 0.158521E+02 | 0.165463E+02 | 0.151578E+02 |
| 0.180300E+04 | 0.158532E+02 | 0.165470E+02 | 0.151594E+02 |
| 0.180400E+04 | 0.158529E+02 | 0.165459E+02 | 0.151598E+02 |
| 0.180500E+04 | 0.158533E+02 | 0.165458E+02 | 0.151607E+02 |
| 0.180600E+04 | 0.158532E+02 | 0.165452E+02 | 0.151611E+02 |
| 0.180700E+04 | 0.158517E+02 | 0.165430E+02 | 0.151604E+02 |
| 0.180800E+04 | 0.158500E+02 | 0.165405E+02 | 0.151594E+02 |
| 0.180900E+04 | 0.156519E+02 | 0.163164E+02 | 0.149873E+02 |
| 0.181000E+04 | 0.154280E+02 | 0.160537E+02 | 0.148024E+02 |
| 0.181100E+04 | 0.156399E+02 | 0.161472E+02 | 0.151326E+02 |
| 0.181200E+04 | 0.157679E+02 | 0.162559E+02 | 0.152799E+02 |
| 0.181300E+04 | 0.158479E+02 | 0.163399E+02 | 0.153558E+02 |
| 0.181400E+04 | 0.158826E+02 | 0.163737E+02 | 0.153914E+02 |
| 0.181500E+04 | 0.156966E+02 | 0.161616E+02 | 0.152316E+02 |
| 0.181600E+04 | 0.154872E+02 | 0.159275E+02 | 0.150469E+02 |
| 0.181700E+04 | 0.155465E+02 | 0.160009E+02 | 0.150920E+02 |
| 0.181800E+04 | 0.156674E+02 | 0.161506E+02 | 0.151841E+02 |
| 0.181900E+04 | 0.157479E+02 | 0.162478E+02 | 0.152480E+02 |
| 0.182000E+04 | 0.157966E+02 | 0.163125E+02 | 0.152807E+02 |
| 0.182100E+04 | 0.158258E+02 | 0.163527E+02 | 0.152989E+02 |
| 0.182200E+04 | 0.158467E+02 | 0.163822E+02 | 0.153111E+02 |
| 0.182300E+04 | 0.158627E+02 | 0.164052E+02 | 0.153202E+02 |
| 0.182400E+04 | 0.158755E+02 | 0.164236E+02 | 0.153273E+02 |
| 0.182500E+04 | 0.158865E+02 | 0.164394E+02 | 0.153335E+02 |
| 0.182600E+04 | 0.158967E+02 | 0.164537E+02 | 0.153396E+02 |
| 0.182700E+04 | 0.159288E+02 | 0.165105E+02 | 0.153471E+02 |
| 0.182800E+04 | 0.159366E+02 | 0.165175E+02 | 0.153556E+02 |
| 0.182900E+04 | 0.159429E+02 | 0.165243E+02 | 0.153615E+02 |
| 0.183000E+04 | 0.159029E+02 | 0.164761E+02 | 0.153297E+02 |
| 0.183100E+04 | 0.157047E+02 | 0.162489E+02 | 0.151605E+02 |
| 0.183200E+04 | 0.155328E+02 | 0.160448E+02 | 0.150207E+02 |
| 0.183300E+04 | 0.156347E+02 | 0.161797E+02 | 0.150896E+02 |
| 0.183400E+04 | 0.157406E+02 | 0.162986E+02 | 0.151826E+02 |
| 0.183500E+04 | 0.156834E+02 | 0.162272E+02 | 0.151396E+02 |
| 0.183600E+04 | 0.156094E+02 | 0.161435E+02 | 0.150753E+02 |
| 0.183700E+04 | 0.156759E+02 | 0.162219E+02 | 0.151299E+02 |
| 0.183800E+04 | 0.157538E+02 | 0.163093E+02 | 0.151983E+02 |
| 0.183900E+04 | 0.157944E+02 | 0.163556E+02 | 0.152332E+02 |
| 0.184000E+04 | 0.157790E+02 | 0.163355E+02 | 0.152225E+02 |
| 0.184100E+04 | 0.157509E+02 | 0.162995E+02 | 0.152022E+02 |
| 0.184200E+04 | 0.157738E+02 | 0.163266E+02 | 0.152209E+02 |
| 0.184300E+04 | 0.157718E+02 | 0.163414E+02 | 0.152023E+02 |
| 0.184400E+04 | 0.157430E+02 | 0.163138E+02 | 0.151721E+02 |
| 0.184500E+04 | 0.157721E+02 | 0.163440E+02 | 0.152002E+02 |
| 0.184600E+04 | 0.158152E+02 | 0.163961E+02 | 0.152343E+02 |
| 0.184700E+04 | 0.158279E+02 | 0.164210E+02 | 0.152349E+02 |
| 0.184800E+04 | 0.158034E+02 | 0.164430E+02 | 0.151638E+02 |
| 0.184900E+04 | 0.157776E+02 | 0.164604E+02 | 0.150948E+02 |
| 0.185000E+04 | 0.157634E+02 | 0.164694E+02 | 0.150575E+02 |
| 0.185100E+04 | 0.157519E+02 | 0.164728E+02 | 0.150310E+02 |
| 0.185200E+04 | 0.157438E+02 | 0.164741E+02 | 0.150135E+02 |
| 0.185300E+04 | 0.157375E+02 | 0.164730E+02 | 0.150021E+02 |
| 0.185400E+04 | 0.156986E+02 | 0.164328E+02 | 0.149644E+02 |

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| 0.185500E+04 | 0.156712E+02 | 0.164014E+02 | 0.149410E+02 |
| 0.185600E+04 | 0.156871E+02 | 0.164191E+02 | 0.149551E+02 |
| 0.185700E+04 | 0.157018E+02 | 0.164343E+02 | 0.149692E+02 |
| 0.185800E+04 | 0.157132E+02 | 0.164443E+02 | 0.149820E+02 |
| 0.185900E+04 | 0.157263E+02 | 0.164556E+02 | 0.149970E+02 |
| 0.186000E+04 | 0.157399E+02 | 0.164670E+02 | 0.150128E+02 |
| 0.186100E+04 | 0.157505E+02 | 0.164746E+02 | 0.150263E+02 |
| 0.186200E+04 | 0.157570E+02 | 0.164775E+02 | 0.150365E+02 |
| 0.186300E+04 | 0.157612E+02 | 0.164779E+02 | 0.150445E+02 |
| 0.186400E+04 | 0.157649E+02 | 0.164779E+02 | 0.150518E+02 |
| 0.186500E+04 | 0.157679E+02 | 0.164776E+02 | 0.150581E+02 |
| 0.186600E+04 | 0.157702E+02 | 0.164769E+02 | 0.150636E+02 |
| 0.186700E+04 | 0.157723E+02 | 0.164762E+02 | 0.150684E+02 |
| 0.186800E+04 | 0.157763E+02 | 0.164782E+02 | 0.150745E+02 |
| 0.186900E+04 | 0.157844E+02 | 0.164853E+02 | 0.150835E+02 |
| 0.187000E+04 | 0.157959E+02 | 0.164966E+02 | 0.150951E+02 |
| 0.187100E+04 | 0.158066E+02 | 0.165073E+02 | 0.151059E+02 |
| 0.187200E+04 | 0.158138E+02 | 0.165140E+02 | 0.151136E+02 |
| 0.187300E+04 | 0.158175E+02 | 0.165167E+02 | 0.151183E+02 |
| 0.187400E+04 | 0.158181E+02 | 0.165161E+02 | 0.151202E+02 |
| 0.187500E+04 | 0.158172E+02 | 0.165139E+02 | 0.151206E+02 |
| 0.187600E+04 | 0.158160E+02 | 0.165116E+02 | 0.151204E+02 |
| 0.187700E+04 | 0.158157E+02 | 0.165106E+02 | 0.151209E+02 |
| 0.187800E+04 | 0.158163E+02 | 0.165108E+02 | 0.151218E+02 |
| 0.187900E+04 | 0.158174E+02 | 0.165118E+02 | 0.151230E+02 |
| 0.188000E+04 | 0.158205E+02 | 0.165152E+02 | 0.151257E+02 |
| 0.188100E+04 | 0.158262E+02 | 0.165218E+02 | 0.151305E+02 |
| 0.188200E+04 | 0.158323E+02 | 0.165290E+02 | 0.151357E+02 |
| 0.188300E+04 | 0.157052E+02 | 0.163900E+02 | 0.150204E+02 |
| 0.188400E+04 | 0.155552E+02 | 0.162176E+02 | 0.148927E+02 |
| 0.188500E+04 | 0.156085E+02 | 0.162716E+02 | 0.149454E+02 |
| 0.188600E+04 | 0.156927E+02 | 0.163696E+02 | 0.150159E+02 |
| 0.188700E+04 | 0.157312E+02 | 0.164129E+02 | 0.150494E+02 |
| 0.188800E+04 | 0.157496E+02 | 0.164376E+02 | 0.150615E+02 |
| 0.188900E+04 | 0.157602E+02 | 0.164547E+02 | 0.150657E+02 |
| 0.189000E+04 | 0.157706E+02 | 0.164679E+02 | 0.150734E+02 |
| 0.189100E+04 | 0.157826E+02 | 0.164819E+02 | 0.150833E+02 |
| 0.189200E+04 | 0.157959E+02 | 0.164973E+02 | 0.150946E+02 |
| 0.189300E+04 | 0.158088E+02 | 0.165119E+02 | 0.151057E+02 |
| 0.189400E+04 | 0.158210E+02 | 0.165257E+02 | 0.151164E+02 |
| 0.189500E+04 | 0.158304E+02 | 0.165361E+02 | 0.151248E+02 |
| 0.189600E+04 | 0.158351E+02 | 0.165409E+02 | 0.151292E+02 |
| 0.189700E+04 | 0.158362E+02 | 0.165417E+02 | 0.151308E+02 |
| 0.189800E+04 | 0.158366E+02 | 0.165416E+02 | 0.151316E+02 |
| 0.189900E+04 | 0.158373E+02 | 0.165421E+02 | 0.151325E+02 |
| 0.190000E+04 | 0.158379E+02 | 0.165425E+02 | 0.151333E+02 |
| 0.190100E+04 | 0.158379E+02 | 0.165424E+02 | 0.151335E+02 |
| 0.190200E+04 | 0.157089E+02 | 0.164009E+02 | 0.150169E+02 |
| 0.190300E+04 | 0.155666E+02 | 0.162342E+02 | 0.148989E+02 |
| 0.190400E+04 | 0.156233E+02 | 0.162943E+02 | 0.149522E+02 |
| 0.190500E+04 | 0.157090E+02 | 0.163933E+02 | 0.150246E+02 |
| 0.190600E+04 | 0.157524E+02 | 0.164431E+02 | 0.150618E+02 |
| 0.190700E+04 | 0.157604E+02 | 0.164561E+02 | 0.150647E+02 |
| 0.190800E+04 | 0.157641E+02 | 0.164641E+02 | 0.150641E+02 |

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| 0.190900E+04 | 0.157846E+02 | 0.164886E+02 | 0.150805E+02 |
| 0.191000E+04 | 0.157986E+02 | 0.165048E+02 | 0.150923E+02 |
| 0.191100E+04 | 0.158060E+02 | 0.165127E+02 | 0.150993E+02 |
| 0.191200E+04 | 0.157299E+02 | 0.164230E+02 | 0.150368E+02 |
| 0.191300E+04 | 0.156573E+02 | 0.163470E+02 | 0.149677E+02 |
| 0.191400E+04 | 0.156577E+02 | 0.163447E+02 | 0.149707E+02 |
| 0.191500E+04 | 0.156980E+02 | 0.163862E+02 | 0.150098E+02 |
| 0.191600E+04 | 0.157493E+02 | 0.164422E+02 | 0.150565E+02 |
| 0.191700E+04 | 0.157825E+02 | 0.164811E+02 | 0.150839E+02 |
| 0.191800E+04 | 0.158015E+02 | 0.165074E+02 | 0.150956E+02 |
| 0.191900E+04 | 0.158147E+02 | 0.165230E+02 | 0.151064E+02 |
| 0.192000E+04 | 0.158217E+02 | 0.165301E+02 | 0.151132E+02 |
| 0.192100E+04 | 0.158256E+02 | 0.165334E+02 | 0.151179E+02 |
| 0.192200E+04 | 0.158278E+02 | 0.165345E+02 | 0.151210E+02 |
| 0.192300E+04 | 0.158298E+02 | 0.165355E+02 | 0.151240E+02 |
| 0.192400E+04 | 0.157817E+02 | 0.164783E+02 | 0.150852E+02 |
| 0.192500E+04 | 0.157295E+02 | 0.164157E+02 | 0.150433E+02 |
| 0.192600E+04 | 0.157652E+02 | 0.164664E+02 | 0.150641E+02 |
| 0.192700E+04 | 0.158039E+02 | 0.165099E+02 | 0.150979E+02 |
| 0.192800E+04 | 0.158210E+02 | 0.165249E+02 | 0.151171E+02 |
| 0.192900E+04 | 0.158315E+02 | 0.165354E+02 | 0.151276E+02 |
| 0.193000E+04 | 0.158391E+02 | 0.165430E+02 | 0.151352E+02 |
| 0.193100E+04 | 0.158441E+02 | 0.165476E+02 | 0.151406E+02 |
| 0.193200E+04 | 0.158462E+02 | 0.165489E+02 | 0.151435E+02 |
| 0.193300E+04 | 0.158463E+02 | 0.165479E+02 | 0.151446E+02 |
| 0.193400E+04 | 0.158470E+02 | 0.165478E+02 | 0.151462E+02 |
| 0.193500E+04 | 0.158515E+02 | 0.165522E+02 | 0.151508E+02 |
| 0.193600E+04 | 0.158611E+02 | 0.165627E+02 | 0.151596E+02 |
| 0.193700E+04 | 0.158713E+02 | 0.165738E+02 | 0.151687E+02 |
| 0.193800E+04 | 0.158771E+02 | 0.165798E+02 | 0.151744E+02 |
| 0.193900E+04 | 0.158805E+02 | 0.165829E+02 | 0.151782E+02 |
| 0.194000E+04 | 0.158829E+02 | 0.165847E+02 | 0.151810E+02 |
| 0.194100E+04 | 0.158838E+02 | 0.165850E+02 | 0.151826E+02 |
| 0.194200E+04 | 0.158832E+02 | 0.165835E+02 | 0.151829E+02 |
| 0.194300E+04 | 0.158808E+02 | 0.165800E+02 | 0.151816E+02 |
| 0.194400E+04 | 0.158785E+02 | 0.165766E+02 | 0.151803E+02 |
| 0.194500E+04 | 0.158803E+02 | 0.165782E+02 | 0.151823E+02 |
| 0.194600E+04 | 0.158856E+02 | 0.165840E+02 | 0.151871E+02 |
| 0.194700E+04 | 0.158926E+02 | 0.165918E+02 | 0.151934E+02 |
| 0.194800E+04 | 0.159012E+02 | 0.166013E+02 | 0.152010E+02 |
| 0.194900E+04 | 0.159076E+02 | 0.166083E+02 | 0.152070E+02 |
| 0.195000E+04 | 0.159090E+02 | 0.166092E+02 | 0.152088E+02 |
| 0.195100E+04 | 0.159057E+02 | 0.166046E+02 | 0.152068E+02 |
| 0.195200E+04 | 0.159016E+02 | 0.165992E+02 | 0.152041E+02 |
| 0.195300E+04 | 0.158983E+02 | 0.165947E+02 | 0.152018E+02 |
| 0.195400E+04 | 0.158953E+02 | 0.165910E+02 | 0.151997E+02 |
| 0.195500E+04 | 0.158951E+02 | 0.165904E+02 | 0.151999E+02 |
| 0.195600E+04 | 0.158857E+02 | 0.165791E+02 | 0.151922E+02 |
| 0.195700E+04 | 0.158874E+02 | 0.165809E+02 | 0.151940E+02 |
| 0.195800E+04 | 0.159090E+02 | 0.166061E+02 | 0.152120E+02 |
| 0.195900E+04 | 0.159212E+02 | 0.166203E+02 | 0.152222E+02 |
| 0.196000E+04 | 0.159246E+02 | 0.166236E+02 | 0.152256E+02 |
| 0.196100E+04 | 0.159074E+02 | 0.166029E+02 | 0.152119E+02 |
| 0.196200E+04 | 0.158797E+02 | 0.165698E+02 | 0.151895E+02 |

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| 0.196300E+04 | 0.158506E+02 | 0.165356E+02 | 0.151656E+02 |
| 0.196400E+04 | 0.158127E+02 | 0.164970E+02 | 0.151284E+02 |
| 0.196500E+04 | 0.157605E+02 | 0.164380E+02 | 0.150830E+02 |
| 0.196600E+04 | 0.157488E+02 | 0.164254E+02 | 0.150721E+02 |
| 0.196700E+04 | 0.157656E+02 | 0.164461E+02 | 0.150852E+02 |
| 0.196800E+04 | 0.157688E+02 | 0.164506E+02 | 0.150869E+02 |
| 0.196900E+04 | 0.157444E+02 | 0.164232E+02 | 0.150657E+02 |
| 0.197000E+04 | 0.157400E+02 | 0.164187E+02 | 0.150613E+02 |
| 0.197100E+04 | 0.157682E+02 | 0.164519E+02 | 0.150845E+02 |
| 0.197200E+04 | 0.157945E+02 | 0.164808E+02 | 0.151082E+02 |
| 0.197300E+04 | 0.158068E+02 | 0.164936E+02 | 0.151200E+02 |
| 0.197400E+04 | 0.158017E+02 | 0.164869E+02 | 0.151165E+02 |
| 0.197500E+04 | 0.157696E+02 | 0.164485E+02 | 0.150908E+02 |
| 0.197600E+04 | 0.157467E+02 | 0.164213E+02 | 0.150720E+02 |
| 0.197700E+04 | 0.157658E+02 | 0.164439E+02 | 0.150877E+02 |
| 0.197800E+04 | 0.157938E+02 | 0.164771E+02 | 0.151104E+02 |
| 0.197900E+04 | 0.158134E+02 | 0.165001E+02 | 0.151267E+02 |
| 0.198000E+04 | 0.158318E+02 | 0.165211E+02 | 0.151425E+02 |
| 0.198100E+04 | 0.158499E+02 | 0.165415E+02 | 0.151582E+02 |
| 0.198200E+04 | 0.157715E+02 | 0.164493E+02 | 0.150938E+02 |
| 0.198300E+04 | 0.156124E+02 | 0.162730E+02 | 0.149517E+02 |
| 0.198400E+04 | 0.155485E+02 | 0.161923E+02 | 0.149046E+02 |
| 0.198500E+04 | 0.156109E+02 | 0.162601E+02 | 0.149617E+02 |
| 0.198600E+04 | 0.158011E+02 | 0.163328E+02 | 0.152694E+02 |
| 0.198700E+04 | 0.158724E+02 | 0.163760E+02 | 0.153688E+02 |
| 0.198800E+04 | 0.159084E+02 | 0.164078E+02 | 0.154090E+02 |
| 0.198900E+04 | 0.159455E+02 | 0.164509E+02 | 0.154401E+02 |
| 0.199000E+04 | 0.159758E+02 | 0.164907E+02 | 0.154609E+02 |
| 0.199100E+04 | 0.159382E+02 | 0.164510E+02 | 0.154255E+02 |
| 0.199200E+04 | 0.157860E+02 | 0.162794E+02 | 0.152926E+02 |
| 0.199300E+04 | 0.156819E+02 | 0.161604E+02 | 0.152034E+02 |
| 0.199400E+04 | 0.157312E+02 | 0.162260E+02 | 0.152365E+02 |
| 0.199500E+04 | 0.157990E+02 | 0.163122E+02 | 0.152857E+02 |
| 0.199600E+04 | 0.158415E+02 | 0.163674E+02 | 0.153155E+02 |
| 0.199700E+04 | 0.158711E+02 | 0.164086E+02 | 0.153337E+02 |
| 0.199800E+04 | 0.158986E+02 | 0.164463E+02 | 0.153509E+02 |

```
#          91
# IPCC AR4 Millenium Runs output (vary solar forcing)
# ++++++
#
# Model: Bern2.5CC version with active C-cycle
# -----
# Prescribed forcing timeseries as described in file
# readme_doRuns_IPCC_Chap6_millennium_21jan06.txt
# provided by F. Joos, University of Bern.
#
# Contact:
# -----
# Gian-Kasper Plattner
# Climate and Environmental Physics
# Physics Institute, University of Bern
# Sidlerstrasse 5, CH-3012 Bern, Switzerland
# plattner@climate.unibe.ch
```

```
# http://www.climate.unibe.ch/~plattner/
# tel: ++41 (0)31 631-44-67
# fax: ++41 (0)31 631-87-42
#
# Some model setup informations:
# -----
# All runs with horizontal/vertical diffusion
#
# Run with standard ocean parameters
#   as used in Plattner et al. 2001/2002
#   with Kv (diffusivity)  $4 \cdot 10^{-5}$  m2/s
#
# Climate sens. set to ~ 3.2 degrees C
# parameterized see Knutti et al. (Clim. Dyn. 2003)
#
# Model version is annual mean.
#
# No radiation code, CO2 radiative forcing calculated
# for as  $RF = 5.35 \cdot \ln(CO2/CO2\_preind)$ ,
# Non-co2 radiative forcing prescribed according to
# Joos et al. GBC 2001 with updates for solar forcing
#
# More model description:
# -----
# Zonally averaged dynamical ocean with 3 basins and
# Southern Ocean, zonally averaged one layer energy
# and moisture balance atmosphere, thermodynamic
# sea ice (Stocker et al., J. Climate 1992).
#
# Carbon cycle components: Ocean/Atm/Terr.biosphere;
# Ocean carbon cycle is a description of the cycles
# of organic carbon and CaCO3 (Marchal et al., Tellus
# Tellus B), based on Redfield approach using PO4 as
# biolimiting nutrient.
#
# Land Biota: Lund-Jena-Postdam Dynamical Global
#               Vegetation Model (LPJ-DGVM)
# at GCM resolution (Gerber et al. 2003, Climate
# Dynamics; Sitch et al. 2003, Global Change Biology)
#
# LPJ forced by Cramer/Leemans annual mean
# climatology plus interannual climate variability
# from Hadley simulation (30-recycled climate) plus
# changes in the fields of surface temperature,
# precipitation, and cloudcover as simulated with the
# Impulse-EOF version of ECHAM-3/LSG in response to
# projected radiative forcing changes.
#
# Land use changes are not explicitly considered.
#
# Impact of climate change on terrestrial C-storage
# included
#
# References:
```

```

# -----
# Carbon cycle Ocean: Marchal et al., Tellus 1998
# Carbon cycle Terr. Bio: Sitch et al., GCB 2003
#           Gerber et al., Clim. Dyn. 2003
# Ccycle-climate feedbacks and global warming:
#           Plattner et al., Tellus 2001
#           Plattner et al., GCB 2002
# Non-CO2 forcing: Joos et al., GCB 2001
# Climate model: Stocker et al., J. Climate 1992
# Sea level: Knutti et al., J. Climate 2000
# Global warming Physics: Knutti et al., Nature 2002
#           Knutti et al., Cl. Dyn. 2003
#           and refs therein.
#
# Output columns:
# -----
# Time (yr AD)
# Global mean air temperature (deg C)
# NH-averaged air temperature (deg C)
# SH-averaged air temperature (deg C)
0.100100E+04 0.159155E+02 0.165835E+02 0.152475E+02
0.100200E+04 0.159209E+02 0.165892E+02 0.152525E+02
0.100300E+04 0.159252E+02 0.165938E+02 0.152567E+02
0.100400E+04 0.158977E+02 0.165611E+02 0.152344E+02
0.100500E+04 0.158655E+02 0.165220E+02 0.152089E+02
0.100600E+04 0.158774E+02 0.165361E+02 0.152187E+02
0.100700E+04 0.158992E+02 0.165626E+02 0.152358E+02
0.100800E+04 0.159109E+02 0.165768E+02 0.152449E+02
0.100900E+04 0.159171E+02 0.165843E+02 0.152500E+02
0.101000E+04 0.159213E+02 0.165891E+02 0.152535E+02
0.101100E+04 0.159242E+02 0.165924E+02 0.152560E+02
0.101200E+04 0.159263E+02 0.165946E+02 0.152579E+02
0.101300E+04 0.159279E+02 0.165964E+02 0.152593E+02
0.101400E+04 0.159292E+02 0.165979E+02 0.152606E+02
0.101500E+04 0.158213E+02 0.164710E+02 0.151715E+02
0.101600E+04 0.157214E+02 0.163645E+02 0.150782E+02
0.101700E+04 0.157650E+02 0.164064E+02 0.151236E+02
0.101800E+04 0.158283E+02 0.164797E+02 0.151770E+02
0.101900E+04 0.158570E+02 0.165118E+02 0.152022E+02
0.102000E+04 0.158701E+02 0.165312E+02 0.152089E+02
0.102100E+04 0.158780E+02 0.165447E+02 0.152113E+02
0.102200E+04 0.158856E+02 0.165546E+02 0.152167E+02
0.102300E+04 0.158920E+02 0.165619E+02 0.152220E+02
0.102400E+04 0.158971E+02 0.165676E+02 0.152267E+02
0.102500E+04 0.159014E+02 0.165720E+02 0.152307E+02
0.102600E+04 0.157770E+02 0.164254E+02 0.151285E+02
0.102700E+04 0.156600E+02 0.162963E+02 0.150237E+02
0.102800E+04 0.157085E+02 0.163461E+02 0.150709E+02
0.102900E+04 0.157839E+02 0.164324E+02 0.151353E+02
0.103000E+04 0.158211E+02 0.164751E+02 0.151670E+02
0.103100E+04 0.158403E+02 0.164997E+02 0.151808E+02
0.103200E+04 0.158500E+02 0.165164E+02 0.151835E+02
0.103300E+04 0.158594E+02 0.165285E+02 0.151903E+02
0.103400E+04 0.158673E+02 0.165375E+02 0.151971E+02

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| 0.103500E+04 | 0.158737E+02 | 0.165443E+02 | 0.152032E+02 |
| 0.103600E+04 | 0.158791E+02 | 0.165496E+02 | 0.152085E+02 |
| 0.103700E+04 | 0.158835E+02 | 0.165539E+02 | 0.152131E+02 |
| 0.103800E+04 | 0.158873E+02 | 0.165574E+02 | 0.152171E+02 |
| 0.103900E+04 | 0.158904E+02 | 0.165603E+02 | 0.152206E+02 |
| 0.104000E+04 | 0.158931E+02 | 0.165627E+02 | 0.152235E+02 |
| 0.104100E+04 | 0.158954E+02 | 0.165646E+02 | 0.152261E+02 |
| 0.104200E+04 | 0.158973E+02 | 0.165663E+02 | 0.152284E+02 |
| 0.104300E+04 | 0.158990E+02 | 0.165676E+02 | 0.152303E+02 |
| 0.104400E+04 | 0.159004E+02 | 0.165687E+02 | 0.152320E+02 |
| 0.104500E+04 | 0.159016E+02 | 0.165697E+02 | 0.152335E+02 |
| 0.104600E+04 | 0.159027E+02 | 0.165706E+02 | 0.152348E+02 |
| 0.104700E+04 | 0.159038E+02 | 0.165715E+02 | 0.152361E+02 |
| 0.104800E+04 | 0.159047E+02 | 0.165722E+02 | 0.152372E+02 |
| 0.104900E+04 | 0.159055E+02 | 0.165729E+02 | 0.152382E+02 |
| 0.105000E+04 | 0.159063E+02 | 0.165735E+02 | 0.152392E+02 |
| 0.105100E+04 | 0.159070E+02 | 0.165740E+02 | 0.152400E+02 |
| 0.105200E+04 | 0.159077E+02 | 0.165745E+02 | 0.152409E+02 |
| 0.105300E+04 | 0.159083E+02 | 0.165750E+02 | 0.152416E+02 |
| 0.105400E+04 | 0.159089E+02 | 0.165754E+02 | 0.152423E+02 |
| 0.105500E+04 | 0.159095E+02 | 0.165759E+02 | 0.152431E+02 |
| 0.105600E+04 | 0.159101E+02 | 0.165764E+02 | 0.152438E+02 |
| 0.105700E+04 | 0.159107E+02 | 0.165769E+02 | 0.152445E+02 |
| 0.105800E+04 | 0.157526E+02 | 0.163976E+02 | 0.151075E+02 |
| 0.105900E+04 | 0.155681E+02 | 0.161824E+02 | 0.149539E+02 |
| 0.106000E+04 | 0.157024E+02 | 0.162482E+02 | 0.151566E+02 |
| 0.106100E+04 | 0.158714E+02 | 0.163711E+02 | 0.153716E+02 |
| 0.106200E+04 | 0.159064E+02 | 0.163799E+02 | 0.154328E+02 |
| 0.106300E+04 | 0.158912E+02 | 0.163588E+02 | 0.154235E+02 |
| 0.106400E+04 | 0.159282E+02 | 0.164062E+02 | 0.154501E+02 |
| 0.106500E+04 | 0.159701E+02 | 0.164636E+02 | 0.154766E+02 |
| 0.106600E+04 | 0.159940E+02 | 0.164998E+02 | 0.154882E+02 |
| 0.106700E+04 | 0.160082E+02 | 0.165240E+02 | 0.154924E+02 |
| 0.106800E+04 | 0.160205E+02 | 0.165424E+02 | 0.154986E+02 |
| 0.106900E+04 | 0.160272E+02 | 0.165572E+02 | 0.154971E+02 |
| 0.107000E+04 | 0.160326E+02 | 0.165692E+02 | 0.154960E+02 |
| 0.107100E+04 | 0.160368E+02 | 0.165792E+02 | 0.154944E+02 |
| 0.107200E+04 | 0.160401E+02 | 0.165874E+02 | 0.154927E+02 |
| 0.107300E+04 | 0.160427E+02 | 0.165944E+02 | 0.154910E+02 |
| 0.107400E+04 | 0.160449E+02 | 0.166004E+02 | 0.154894E+02 |
| 0.107500E+04 | 0.160467E+02 | 0.166055E+02 | 0.154880E+02 |
| 0.107600E+04 | 0.160483E+02 | 0.166098E+02 | 0.154867E+02 |
| 0.107700E+04 | 0.160495E+02 | 0.166134E+02 | 0.154855E+02 |
| 0.107800E+04 | 0.160504E+02 | 0.166164E+02 | 0.154844E+02 |
| 0.107900E+04 | 0.160513E+02 | 0.166191E+02 | 0.154835E+02 |
| 0.108000E+04 | 0.160218E+02 | 0.165851E+02 | 0.154584E+02 |
| 0.108100E+04 | 0.159894E+02 | 0.165469E+02 | 0.154319E+02 |
| 0.108200E+04 | 0.160000E+02 | 0.165607E+02 | 0.154393E+02 |
| 0.108300E+04 | 0.160202E+02 | 0.165871E+02 | 0.154533E+02 |
| 0.108400E+04 | 0.160222E+02 | 0.166017E+02 | 0.154427E+02 |
| 0.108500E+04 | 0.160174E+02 | 0.166096E+02 | 0.154252E+02 |
| 0.108600E+04 | 0.160121E+02 | 0.166147E+02 | 0.154095E+02 |
| 0.108700E+04 | 0.159660E+02 | 0.166182E+02 | 0.153138E+02 |
| 0.108800E+04 | 0.159316E+02 | 0.166197E+02 | 0.152435E+02 |

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| 0.108900E+04 | 0.159111E+02 | 0.166198E+02 | 0.152025E+02 |
| 0.109000E+04 | 0.158969E+02 | 0.166188E+02 | 0.151750E+02 |
| 0.109100E+04 | 0.158874E+02 | 0.166175E+02 | 0.151574E+02 |
| 0.109200E+04 | 0.158810E+02 | 0.166159E+02 | 0.151460E+02 |
| 0.109300E+04 | 0.158765E+02 | 0.166143E+02 | 0.151387E+02 |
| 0.109400E+04 | 0.158754E+02 | 0.166129E+02 | 0.151380E+02 |
| 0.109500E+04 | 0.158763E+02 | 0.166119E+02 | 0.151407E+02 |
| 0.109600E+04 | 0.158786E+02 | 0.166114E+02 | 0.151459E+02 |
| 0.109700E+04 | 0.158099E+02 | 0.165273E+02 | 0.150926E+02 |
| 0.109800E+04 | 0.157483E+02 | 0.164610E+02 | 0.150355E+02 |
| 0.109900E+04 | 0.157746E+02 | 0.164875E+02 | 0.150618E+02 |
| 0.110000E+04 | 0.158230E+02 | 0.165395E+02 | 0.151065E+02 |
| 0.110100E+04 | 0.158454E+02 | 0.165578E+02 | 0.151331E+02 |
| 0.110200E+04 | 0.158613E+02 | 0.165715E+02 | 0.151512E+02 |
| 0.110300E+04 | 0.158743E+02 | 0.165824E+02 | 0.151662E+02 |
| 0.110400E+04 | 0.158852E+02 | 0.165912E+02 | 0.151792E+02 |
| 0.110500E+04 | 0.158948E+02 | 0.165988E+02 | 0.151909E+02 |
| 0.110600E+04 | 0.159034E+02 | 0.166054E+02 | 0.152014E+02 |
| 0.110700E+04 | 0.159111E+02 | 0.166114E+02 | 0.152108E+02 |
| 0.110800E+04 | 0.159182E+02 | 0.166169E+02 | 0.152195E+02 |
| 0.110900E+04 | 0.159249E+02 | 0.166223E+02 | 0.152274E+02 |
| 0.111000E+04 | 0.159314E+02 | 0.166278E+02 | 0.152350E+02 |
| 0.111100E+04 | 0.159377E+02 | 0.166333E+02 | 0.152421E+02 |
| 0.111200E+04 | 0.159437E+02 | 0.166387E+02 | 0.152487E+02 |
| 0.111300E+04 | 0.159495E+02 | 0.166441E+02 | 0.152550E+02 |
| 0.111400E+04 | 0.159551E+02 | 0.166494E+02 | 0.152609E+02 |
| 0.111500E+04 | 0.159603E+02 | 0.166543E+02 | 0.152663E+02 |
| 0.111600E+04 | 0.159649E+02 | 0.166588E+02 | 0.152710E+02 |
| 0.111700E+04 | 0.159691E+02 | 0.166628E+02 | 0.152753E+02 |
| 0.111800E+04 | 0.159729E+02 | 0.166667E+02 | 0.152792E+02 |
| 0.111900E+04 | 0.159765E+02 | 0.166703E+02 | 0.152828E+02 |
| 0.112000E+04 | 0.159799E+02 | 0.166737E+02 | 0.152861E+02 |
| 0.112100E+04 | 0.159831E+02 | 0.166770E+02 | 0.152891E+02 |
| 0.112200E+04 | 0.159861E+02 | 0.166802E+02 | 0.152919E+02 |
| 0.112300E+04 | 0.159889E+02 | 0.166833E+02 | 0.152946E+02 |
| 0.112400E+04 | 0.159916E+02 | 0.166862E+02 | 0.152970E+02 |
| 0.112500E+04 | 0.159942E+02 | 0.166891E+02 | 0.152993E+02 |
| 0.112600E+04 | 0.159967E+02 | 0.166919E+02 | 0.153015E+02 |
| 0.112700E+04 | 0.159991E+02 | 0.166946E+02 | 0.153036E+02 |
| 0.112800E+04 | 0.160014E+02 | 0.166972E+02 | 0.153055E+02 |
| 0.112900E+04 | 0.160036E+02 | 0.166997E+02 | 0.153074E+02 |
| 0.113000E+04 | 0.160057E+02 | 0.167022E+02 | 0.153092E+02 |
| 0.113100E+04 | 0.160078E+02 | 0.167046E+02 | 0.153109E+02 |
| 0.113200E+04 | 0.160097E+02 | 0.167069E+02 | 0.153126E+02 |
| 0.113300E+04 | 0.160118E+02 | 0.167093E+02 | 0.153142E+02 |
| 0.113400E+04 | 0.160140E+02 | 0.167119E+02 | 0.153160E+02 |
| 0.113500E+04 | 0.160161E+02 | 0.167144E+02 | 0.153178E+02 |
| 0.113600E+04 | 0.160182E+02 | 0.167169E+02 | 0.153195E+02 |
| 0.113700E+04 | 0.160202E+02 | 0.167192E+02 | 0.153212E+02 |
| 0.113800E+04 | 0.160222E+02 | 0.167215E+02 | 0.153228E+02 |
| 0.113900E+04 | 0.160240E+02 | 0.167236E+02 | 0.153244E+02 |
| 0.114000E+04 | 0.160256E+02 | 0.167255E+02 | 0.153258E+02 |
| 0.114100E+04 | 0.160271E+02 | 0.167272E+02 | 0.153270E+02 |
| 0.114200E+04 | 0.160285E+02 | 0.167288E+02 | 0.153282E+02 |

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| 0.114300E+04 | 0.160298E+02 | 0.167303E+02 | 0.153294E+02 |
| 0.114400E+04 | 0.160311E+02 | 0.167317E+02 | 0.153304E+02 |
| 0.114500E+04 | 0.160322E+02 | 0.167330E+02 | 0.153315E+02 |
| 0.114600E+04 | 0.160333E+02 | 0.167342E+02 | 0.153325E+02 |
| 0.114700E+04 | 0.160343E+02 | 0.167353E+02 | 0.153333E+02 |
| 0.114800E+04 | 0.160351E+02 | 0.167361E+02 | 0.153341E+02 |
| 0.114900E+04 | 0.160358E+02 | 0.167368E+02 | 0.153347E+02 |
| 0.115000E+04 | 0.160363E+02 | 0.167373E+02 | 0.153353E+02 |
| 0.115100E+04 | 0.160368E+02 | 0.167377E+02 | 0.153358E+02 |
| 0.115200E+04 | 0.160372E+02 | 0.167381E+02 | 0.153363E+02 |
| 0.115300E+04 | 0.160375E+02 | 0.167383E+02 | 0.153366E+02 |
| 0.115400E+04 | 0.160377E+02 | 0.167385E+02 | 0.153370E+02 |
| 0.115500E+04 | 0.160379E+02 | 0.167386E+02 | 0.153373E+02 |
| 0.115600E+04 | 0.160380E+02 | 0.167385E+02 | 0.153375E+02 |
| 0.115700E+04 | 0.160379E+02 | 0.167382E+02 | 0.153376E+02 |
| 0.115800E+04 | 0.160376E+02 | 0.167377E+02 | 0.153375E+02 |
| 0.115900E+04 | 0.160372E+02 | 0.167371E+02 | 0.153373E+02 |
| 0.116000E+04 | 0.160367E+02 | 0.167363E+02 | 0.153371E+02 |
| 0.116100E+04 | 0.160362E+02 | 0.167355E+02 | 0.153368E+02 |
| 0.116200E+04 | 0.160358E+02 | 0.167349E+02 | 0.153367E+02 |
| 0.116300E+04 | 0.160357E+02 | 0.167346E+02 | 0.153368E+02 |
| 0.116400E+04 | 0.160358E+02 | 0.167345E+02 | 0.153370E+02 |
| 0.116500E+04 | 0.160359E+02 | 0.167345E+02 | 0.153374E+02 |
| 0.116600E+04 | 0.159930E+02 | 0.166837E+02 | 0.153022E+02 |
| 0.116700E+04 | 0.159464E+02 | 0.166292E+02 | 0.152636E+02 |
| 0.116800E+04 | 0.159600E+02 | 0.166457E+02 | 0.152742E+02 |
| 0.116900E+04 | 0.159866E+02 | 0.166781E+02 | 0.152951E+02 |
| 0.117000E+04 | 0.160004E+02 | 0.166949E+02 | 0.153060E+02 |
| 0.117100E+04 | 0.160074E+02 | 0.167030E+02 | 0.153119E+02 |
| 0.117200E+04 | 0.160119E+02 | 0.167079E+02 | 0.153159E+02 |
| 0.117300E+04 | 0.160150E+02 | 0.167111E+02 | 0.153189E+02 |
| 0.117400E+04 | 0.160172E+02 | 0.167132E+02 | 0.153212E+02 |
| 0.117500E+04 | 0.157641E+02 | 0.164281E+02 | 0.151002E+02 |
| 0.117600E+04 | 0.156187E+02 | 0.161368E+02 | 0.151005E+02 |
| 0.117700E+04 | 0.157262E+02 | 0.162059E+02 | 0.152466E+02 |
| 0.117800E+04 | 0.158665E+02 | 0.163478E+02 | 0.153853E+02 |
| 0.117900E+04 | 0.159583E+02 | 0.164437E+02 | 0.154728E+02 |
| 0.118000E+04 | 0.160115E+02 | 0.165076E+02 | 0.155154E+02 |
| 0.118100E+04 | 0.160423E+02 | 0.165479E+02 | 0.155367E+02 |
| 0.118200E+04 | 0.160629E+02 | 0.165776E+02 | 0.155483E+02 |
| 0.118300E+04 | 0.160774E+02 | 0.166006E+02 | 0.155543E+02 |
| 0.118400E+04 | 0.160901E+02 | 0.166188E+02 | 0.155613E+02 |
| 0.118500E+04 | 0.160976E+02 | 0.166333E+02 | 0.155619E+02 |
| 0.118600E+04 | 0.161031E+02 | 0.166448E+02 | 0.155615E+02 |
| 0.118700E+04 | 0.161071E+02 | 0.166539E+02 | 0.155602E+02 |
| 0.118800E+04 | 0.161099E+02 | 0.166612E+02 | 0.155586E+02 |
| 0.118900E+04 | 0.161334E+02 | 0.167069E+02 | 0.155598E+02 |
| 0.119000E+04 | 0.161327E+02 | 0.167065E+02 | 0.155588E+02 |
| 0.119100E+04 | 0.161322E+02 | 0.167074E+02 | 0.155571E+02 |
| 0.119200E+04 | 0.161319E+02 | 0.167084E+02 | 0.155553E+02 |
| 0.119300E+04 | 0.161240E+02 | 0.167094E+02 | 0.155386E+02 |
| 0.119400E+04 | 0.160214E+02 | 0.166002E+02 | 0.154426E+02 |
| 0.119500E+04 | 0.159266E+02 | 0.165069E+02 | 0.153463E+02 |
| 0.119600E+04 | 0.159470E+02 | 0.165403E+02 | 0.153538E+02 |

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| 0.119700E+04 | 0.159938E+02 | 0.166044E+02 | 0.153832E+02 |
| 0.119800E+04 | 0.159733E+02 | 0.166301E+02 | 0.153165E+02 |
| 0.119900E+04 | 0.159482E+02 | 0.166444E+02 | 0.152520E+02 |
| 0.120000E+04 | 0.159354E+02 | 0.166526E+02 | 0.152182E+02 |
| 0.120100E+04 | 0.159268E+02 | 0.166569E+02 | 0.151968E+02 |
| 0.120200E+04 | 0.159216E+02 | 0.166589E+02 | 0.151843E+02 |
| 0.120300E+04 | 0.159191E+02 | 0.166595E+02 | 0.151786E+02 |
| 0.120400E+04 | 0.159166E+02 | 0.166591E+02 | 0.151741E+02 |
| 0.120500E+04 | 0.158664E+02 | 0.165991E+02 | 0.151337E+02 |
| 0.120600E+04 | 0.158113E+02 | 0.165312E+02 | 0.150915E+02 |
| 0.120700E+04 | 0.158427E+02 | 0.165735E+02 | 0.151118E+02 |
| 0.120800E+04 | 0.158776E+02 | 0.166097E+02 | 0.151455E+02 |
| 0.120900E+04 | 0.158981E+02 | 0.166288E+02 | 0.151674E+02 |
| 0.121000E+04 | 0.159110E+02 | 0.166387E+02 | 0.151834E+02 |
| 0.121100E+04 | 0.159211E+02 | 0.166451E+02 | 0.151971E+02 |
| 0.121200E+04 | 0.159295E+02 | 0.166497E+02 | 0.152093E+02 |
| 0.121300E+04 | 0.159368E+02 | 0.166532E+02 | 0.152203E+02 |
| 0.121400E+04 | 0.159412E+02 | 0.166523E+02 | 0.152302E+02 |
| 0.121500E+04 | 0.159417E+02 | 0.166451E+02 | 0.152383E+02 |
| 0.121600E+04 | 0.159454E+02 | 0.166451E+02 | 0.152458E+02 |
| 0.121700E+04 | 0.159497E+02 | 0.166466E+02 | 0.152527E+02 |
| 0.121800E+04 | 0.159538E+02 | 0.166485E+02 | 0.152591E+02 |
| 0.121900E+04 | 0.159578E+02 | 0.166506E+02 | 0.152649E+02 |
| 0.122000E+04 | 0.159615E+02 | 0.166528E+02 | 0.152702E+02 |
| 0.122100E+04 | 0.159650E+02 | 0.166550E+02 | 0.152751E+02 |
| 0.122200E+04 | 0.159684E+02 | 0.166573E+02 | 0.152794E+02 |
| 0.122300E+04 | 0.159715E+02 | 0.166595E+02 | 0.152834E+02 |
| 0.122400E+04 | 0.159744E+02 | 0.166618E+02 | 0.152871E+02 |
| 0.122500E+04 | 0.159772E+02 | 0.166640E+02 | 0.152904E+02 |
| 0.122600E+04 | 0.159798E+02 | 0.166662E+02 | 0.152934E+02 |
| 0.122700E+04 | 0.159512E+02 | 0.166321E+02 | 0.152703E+02 |
| 0.122800E+04 | 0.159185E+02 | 0.165928E+02 | 0.152442E+02 |
| 0.122900E+04 | 0.157488E+02 | 0.164060E+02 | 0.150916E+02 |
| 0.123000E+04 | 0.155640E+02 | 0.161868E+02 | 0.149411E+02 |
| 0.123100E+04 | 0.157756E+02 | 0.162876E+02 | 0.152636E+02 |
| 0.123200E+04 | 0.159056E+02 | 0.163980E+02 | 0.154133E+02 |
| 0.123300E+04 | 0.159822E+02 | 0.164718E+02 | 0.154927E+02 |
| 0.123400E+04 | 0.160216E+02 | 0.165128E+02 | 0.155304E+02 |
| 0.123500E+04 | 0.160469E+02 | 0.165448E+02 | 0.155489E+02 |
| 0.123600E+04 | 0.160638E+02 | 0.165696E+02 | 0.155579E+02 |
| 0.123700E+04 | 0.160753E+02 | 0.165892E+02 | 0.155615E+02 |
| 0.123800E+04 | 0.160834E+02 | 0.166051E+02 | 0.155618E+02 |
| 0.123900E+04 | 0.160918E+02 | 0.166181E+02 | 0.155654E+02 |
| 0.124000E+04 | 0.160957E+02 | 0.166290E+02 | 0.155623E+02 |
| 0.124100E+04 | 0.160987E+02 | 0.166380E+02 | 0.155594E+02 |
| 0.124200E+04 | 0.161230E+02 | 0.166886E+02 | 0.155574E+02 |
| 0.124300E+04 | 0.161230E+02 | 0.166895E+02 | 0.155566E+02 |
| 0.124400E+04 | 0.161223E+02 | 0.166907E+02 | 0.155539E+02 |
| 0.124500E+04 | 0.161217E+02 | 0.166926E+02 | 0.155509E+02 |
| 0.124600E+04 | 0.161211E+02 | 0.166942E+02 | 0.155479E+02 |
| 0.124700E+04 | 0.161202E+02 | 0.166954E+02 | 0.155449E+02 |
| 0.124800E+04 | 0.161119E+02 | 0.166962E+02 | 0.155277E+02 |
| 0.124900E+04 | 0.161002E+02 | 0.166964E+02 | 0.155039E+02 |
| 0.125000E+04 | 0.160896E+02 | 0.166961E+02 | 0.154831E+02 |

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| 0.125100E+04 | 0.160803E+02 | 0.166953E+02 | 0.154653E+02 |
| 0.125200E+04 | 0.160221E+02 | 0.166938E+02 | 0.153503E+02 |
| 0.125300E+04 | 0.159889E+02 | 0.166911E+02 | 0.152866E+02 |
| 0.125400E+04 | 0.159666E+02 | 0.166870E+02 | 0.152462E+02 |
| 0.125500E+04 | 0.159499E+02 | 0.166818E+02 | 0.152181E+02 |
| 0.125600E+04 | 0.159376E+02 | 0.166759E+02 | 0.151992E+02 |
| 0.125700E+04 | 0.159286E+02 | 0.166699E+02 | 0.151873E+02 |
| 0.125800E+04 | 0.159206E+02 | 0.166642E+02 | 0.151771E+02 |
| 0.125900E+04 | 0.154807E+02 | 0.161494E+02 | 0.148121E+02 |
| 0.126000E+04 | 0.152572E+02 | 0.160683E+02 | 0.144461E+02 |
| 0.126100E+04 | 0.154588E+02 | 0.163821E+02 | 0.145355E+02 |
| 0.126200E+04 | 0.156558E+02 | 0.165738E+02 | 0.147379E+02 |
| 0.126300E+04 | 0.157923E+02 | 0.166953E+02 | 0.148893E+02 |
| 0.126400E+04 | 0.158568E+02 | 0.167406E+02 | 0.149730E+02 |
| 0.126500E+04 | 0.158900E+02 | 0.167545E+02 | 0.150255E+02 |
| 0.126600E+04 | 0.159115E+02 | 0.167589E+02 | 0.150640E+02 |
| 0.126700E+04 | 0.159227E+02 | 0.167575E+02 | 0.150880E+02 |
| 0.126800E+04 | 0.159307E+02 | 0.167536E+02 | 0.151077E+02 |
| 0.126900E+04 | 0.159367E+02 | 0.167486E+02 | 0.151247E+02 |
| 0.127000E+04 | 0.159415E+02 | 0.167434E+02 | 0.151395E+02 |
| 0.127100E+04 | 0.159453E+02 | 0.167383E+02 | 0.151523E+02 |
| 0.127200E+04 | 0.159485E+02 | 0.167336E+02 | 0.151634E+02 |
| 0.127300E+04 | 0.159510E+02 | 0.167292E+02 | 0.151729E+02 |
| 0.127400E+04 | 0.159530E+02 | 0.167251E+02 | 0.151809E+02 |
| 0.127500E+04 | 0.158544E+02 | 0.166053E+02 | 0.151035E+02 |
| 0.127600E+04 | 0.157431E+02 | 0.164816E+02 | 0.150046E+02 |
| 0.127700E+04 | 0.157788E+02 | 0.165214E+02 | 0.150362E+02 |
| 0.127800E+04 | 0.158412E+02 | 0.165867E+02 | 0.150956E+02 |
| 0.127900E+04 | 0.158710E+02 | 0.166153E+02 | 0.151268E+02 |
| 0.128000E+04 | 0.158755E+02 | 0.166076E+02 | 0.151434E+02 |
| 0.128100E+04 | 0.158771E+02 | 0.165999E+02 | 0.151544E+02 |
| 0.128200E+04 | 0.158361E+02 | 0.165112E+02 | 0.151610E+02 |
| 0.128300E+04 | 0.158180E+02 | 0.164730E+02 | 0.151631E+02 |
| 0.128400E+04 | 0.158099E+02 | 0.164557E+02 | 0.151641E+02 |
| 0.128500E+04 | 0.156680E+02 | 0.162921E+02 | 0.150440E+02 |
| 0.128600E+04 | 0.155212E+02 | 0.161200E+02 | 0.149223E+02 |
| 0.128700E+04 | 0.156037E+02 | 0.162401E+02 | 0.149673E+02 |
| 0.128800E+04 | 0.158194E+02 | 0.163373E+02 | 0.153015E+02 |
| 0.128900E+04 | 0.158806E+02 | 0.163862E+02 | 0.153750E+02 |
| 0.129000E+04 | 0.159281E+02 | 0.164155E+02 | 0.154407E+02 |
| 0.129100E+04 | 0.159485E+02 | 0.164389E+02 | 0.154580E+02 |
| 0.129200E+04 | 0.159630E+02 | 0.164605E+02 | 0.154654E+02 |
| 0.129300E+04 | 0.159730E+02 | 0.164790E+02 | 0.154671E+02 |
| 0.129400E+04 | 0.159836E+02 | 0.164952E+02 | 0.154720E+02 |
| 0.129500E+04 | 0.158516E+02 | 0.163463E+02 | 0.153569E+02 |
| 0.129600E+04 | 0.157273E+02 | 0.162135E+02 | 0.152410E+02 |
| 0.129700E+04 | 0.157762E+02 | 0.162788E+02 | 0.152737E+02 |
| 0.129800E+04 | 0.158534E+02 | 0.163750E+02 | 0.153318E+02 |
| 0.129900E+04 | 0.158826E+02 | 0.164088E+02 | 0.153563E+02 |
| 0.130000E+04 | 0.159022E+02 | 0.164376E+02 | 0.153668E+02 |
| 0.130100E+04 | 0.159166E+02 | 0.164602E+02 | 0.153729E+02 |
| 0.130200E+04 | 0.159277E+02 | 0.164786E+02 | 0.153768E+02 |
| 0.130300E+04 | 0.159365E+02 | 0.164937E+02 | 0.153794E+02 |
| 0.130400E+04 | 0.159437E+02 | 0.165063E+02 | 0.153812E+02 |

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| 0.130500E+04 | 0.159498E+02 | 0.165171E+02 | 0.153825E+02 |
| 0.130600E+04 | 0.159550E+02 | 0.165265E+02 | 0.153835E+02 |
| 0.130700E+04 | 0.159595E+02 | 0.165348E+02 | 0.153843E+02 |
| 0.130800E+04 | 0.159634E+02 | 0.165420E+02 | 0.153849E+02 |
| 0.130900E+04 | 0.159669E+02 | 0.165483E+02 | 0.153854E+02 |
| 0.131000E+04 | 0.159925E+02 | 0.165984E+02 | 0.153867E+02 |
| 0.131100E+04 | 0.159960E+02 | 0.166017E+02 | 0.153903E+02 |
| 0.131200E+04 | 0.159968E+02 | 0.166020E+02 | 0.153916E+02 |
| 0.131300E+04 | 0.159915E+02 | 0.166036E+02 | 0.153793E+02 |
| 0.131400E+04 | 0.159816E+02 | 0.166054E+02 | 0.153578E+02 |
| 0.131500E+04 | 0.159730E+02 | 0.166070E+02 | 0.153391E+02 |
| 0.131600E+04 | 0.159365E+02 | 0.166080E+02 | 0.152649E+02 |
| 0.131700E+04 | 0.158919E+02 | 0.166079E+02 | 0.151758E+02 |
| 0.131800E+04 | 0.158676E+02 | 0.166061E+02 | 0.151291E+02 |
| 0.131900E+04 | 0.158506E+02 | 0.166035E+02 | 0.150976E+02 |
| 0.132000E+04 | 0.158387E+02 | 0.166006E+02 | 0.150767E+02 |
| 0.132100E+04 | 0.158288E+02 | 0.165975E+02 | 0.150601E+02 |
| 0.132200E+04 | 0.158235E+02 | 0.165944E+02 | 0.150526E+02 |
| 0.132300E+04 | 0.158210E+02 | 0.165916E+02 | 0.150504E+02 |
| 0.132400E+04 | 0.158207E+02 | 0.165893E+02 | 0.150520E+02 |
| 0.132500E+04 | 0.158220E+02 | 0.165876E+02 | 0.150563E+02 |
| 0.132600E+04 | 0.158244E+02 | 0.165863E+02 | 0.150624E+02 |
| 0.132700E+04 | 0.158276E+02 | 0.165856E+02 | 0.150696E+02 |
| 0.132800E+04 | 0.158312E+02 | 0.165849E+02 | 0.150774E+02 |
| 0.132900E+04 | 0.157124E+02 | 0.164412E+02 | 0.149837E+02 |
| 0.133000E+04 | 0.156000E+02 | 0.163193E+02 | 0.148807E+02 |
| 0.133100E+04 | 0.156443E+02 | 0.163665E+02 | 0.149222E+02 |
| 0.133200E+04 | 0.157169E+02 | 0.164435E+02 | 0.149904E+02 |
| 0.133300E+04 | 0.157558E+02 | 0.164836E+02 | 0.150280E+02 |
| 0.133400E+04 | 0.157793E+02 | 0.165071E+02 | 0.150516E+02 |
| 0.133500E+04 | 0.157970E+02 | 0.165239E+02 | 0.150701E+02 |
| 0.133600E+04 | 0.158117E+02 | 0.165374E+02 | 0.150860E+02 |
| 0.133700E+04 | 0.158245E+02 | 0.165490E+02 | 0.151000E+02 |
| 0.133800E+04 | 0.158357E+02 | 0.165590E+02 | 0.151124E+02 |
| 0.133900E+04 | 0.158458E+02 | 0.165679E+02 | 0.151236E+02 |
| 0.134000E+04 | 0.158546E+02 | 0.165757E+02 | 0.151335E+02 |
| 0.134100E+04 | 0.158623E+02 | 0.165823E+02 | 0.151422E+02 |
| 0.134200E+04 | 0.158690E+02 | 0.165880E+02 | 0.151499E+02 |
| 0.134300E+04 | 0.158749E+02 | 0.165931E+02 | 0.151568E+02 |
| 0.134400E+04 | 0.158803E+02 | 0.165976E+02 | 0.151630E+02 |
| 0.134500E+04 | 0.157810E+02 | 0.164796E+02 | 0.150824E+02 |
| 0.134600E+04 | 0.156860E+02 | 0.163782E+02 | 0.149939E+02 |
| 0.134700E+04 | 0.157263E+02 | 0.164183E+02 | 0.150343E+02 |
| 0.134800E+04 | 0.157903E+02 | 0.164873E+02 | 0.150932E+02 |
| 0.134900E+04 | 0.158240E+02 | 0.165247E+02 | 0.151232E+02 |
| 0.135000E+04 | 0.158390E+02 | 0.165469E+02 | 0.151311E+02 |
| 0.135100E+04 | 0.158509E+02 | 0.165625E+02 | 0.151392E+02 |
| 0.135200E+04 | 0.158613E+02 | 0.165743E+02 | 0.151483E+02 |
| 0.135300E+04 | 0.158701E+02 | 0.165835E+02 | 0.151567E+02 |
| 0.135400E+04 | 0.158775E+02 | 0.165910E+02 | 0.151640E+02 |
| 0.135500E+04 | 0.158838E+02 | 0.165971E+02 | 0.151705E+02 |
| 0.135600E+04 | 0.158893E+02 | 0.166023E+02 | 0.151763E+02 |
| 0.135700E+04 | 0.158942E+02 | 0.166068E+02 | 0.151815E+02 |
| 0.135800E+04 | 0.158986E+02 | 0.166109E+02 | 0.151863E+02 |

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| 0.135900E+04 | 0.159026E+02 | 0.166145E+02 | 0.151906E+02 |
| 0.136000E+04 | 0.159062E+02 | 0.166178E+02 | 0.151946E+02 |
| 0.136100E+04 | 0.159095E+02 | 0.166209E+02 | 0.151982E+02 |
| 0.136200E+04 | 0.159126E+02 | 0.166236E+02 | 0.152016E+02 |
| 0.136300E+04 | 0.159155E+02 | 0.166262E+02 | 0.152047E+02 |
| 0.136400E+04 | 0.159181E+02 | 0.166286E+02 | 0.152076E+02 |
| 0.136500E+04 | 0.159206E+02 | 0.166308E+02 | 0.152104E+02 |
| 0.136600E+04 | 0.159229E+02 | 0.166328E+02 | 0.152130E+02 |
| 0.136700E+04 | 0.159251E+02 | 0.166348E+02 | 0.152155E+02 |
| 0.136800E+04 | 0.159272E+02 | 0.166366E+02 | 0.152178E+02 |
| 0.136900E+04 | 0.159292E+02 | 0.166383E+02 | 0.152201E+02 |
| 0.137000E+04 | 0.159311E+02 | 0.166400E+02 | 0.152223E+02 |
| 0.137100E+04 | 0.159330E+02 | 0.166415E+02 | 0.152244E+02 |
| 0.137200E+04 | 0.159347E+02 | 0.166430E+02 | 0.152265E+02 |
| 0.137300E+04 | 0.159364E+02 | 0.166444E+02 | 0.152285E+02 |
| 0.137400E+04 | 0.159380E+02 | 0.166457E+02 | 0.152303E+02 |
| 0.137500E+04 | 0.158966E+02 | 0.165967E+02 | 0.151964E+02 |
| 0.137600E+04 | 0.158502E+02 | 0.165415E+02 | 0.151589E+02 |
| 0.137700E+04 | 0.158650E+02 | 0.165592E+02 | 0.151707E+02 |
| 0.137800E+04 | 0.158934E+02 | 0.165937E+02 | 0.151931E+02 |
| 0.137900E+04 | 0.159088E+02 | 0.166121E+02 | 0.152055E+02 |
| 0.138000E+04 | 0.159172E+02 | 0.166217E+02 | 0.152128E+02 |
| 0.138100E+04 | 0.159231E+02 | 0.166279E+02 | 0.152182E+02 |
| 0.138200E+04 | 0.159275E+02 | 0.166324E+02 | 0.152227E+02 |
| 0.138300E+04 | 0.159311E+02 | 0.166357E+02 | 0.152264E+02 |
| 0.138400E+04 | 0.159339E+02 | 0.166382E+02 | 0.152295E+02 |
| 0.138500E+04 | 0.159361E+02 | 0.166400E+02 | 0.152322E+02 |
| 0.138600E+04 | 0.159378E+02 | 0.166412E+02 | 0.152344E+02 |
| 0.138700E+04 | 0.158964E+02 | 0.165921E+02 | 0.152007E+02 |
| 0.138800E+04 | 0.158500E+02 | 0.165366E+02 | 0.151633E+02 |
| 0.138900E+04 | 0.158645E+02 | 0.165539E+02 | 0.151751E+02 |
| 0.139000E+04 | 0.158926E+02 | 0.165878E+02 | 0.151973E+02 |
| 0.139100E+04 | 0.159075E+02 | 0.166056E+02 | 0.152094E+02 |
| 0.139200E+04 | 0.159154E+02 | 0.166145E+02 | 0.152164E+02 |
| 0.139300E+04 | 0.159206E+02 | 0.166199E+02 | 0.152213E+02 |
| 0.139400E+04 | 0.159242E+02 | 0.166233E+02 | 0.152250E+02 |
| 0.139500E+04 | 0.159268E+02 | 0.166256E+02 | 0.152280E+02 |
| 0.139600E+04 | 0.159285E+02 | 0.166268E+02 | 0.152302E+02 |
| 0.139700E+04 | 0.159293E+02 | 0.166270E+02 | 0.152317E+02 |
| 0.139800E+04 | 0.159296E+02 | 0.166266E+02 | 0.152327E+02 |
| 0.139900E+04 | 0.159295E+02 | 0.166257E+02 | 0.152332E+02 |
| 0.140000E+04 | 0.159290E+02 | 0.166246E+02 | 0.152335E+02 |
| 0.140100E+04 | 0.159285E+02 | 0.166234E+02 | 0.152336E+02 |
| 0.140200E+04 | 0.159281E+02 | 0.166224E+02 | 0.152338E+02 |
| 0.140300E+04 | 0.159278E+02 | 0.166215E+02 | 0.152341E+02 |
| 0.140400E+04 | 0.159274E+02 | 0.166206E+02 | 0.152342E+02 |
| 0.140500E+04 | 0.159270E+02 | 0.166197E+02 | 0.152344E+02 |
| 0.140600E+04 | 0.159266E+02 | 0.166188E+02 | 0.152345E+02 |
| 0.140700E+04 | 0.159262E+02 | 0.166178E+02 | 0.152345E+02 |
| 0.140800E+04 | 0.158868E+02 | 0.165715E+02 | 0.152022E+02 |
| 0.140900E+04 | 0.158420E+02 | 0.165183E+02 | 0.151656E+02 |
| 0.141000E+04 | 0.158664E+02 | 0.165566E+02 | 0.151763E+02 |
| 0.141100E+04 | 0.158905E+02 | 0.165843E+02 | 0.151967E+02 |
| 0.141200E+04 | 0.159030E+02 | 0.165986E+02 | 0.152075E+02 |

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| 0.141300E+04 | 0.159094E+02 | 0.166055E+02 | 0.152134E+02 |
| 0.141400E+04 | 0.159135E+02 | 0.166096E+02 | 0.152175E+02 |
| 0.141500E+04 | 0.159161E+02 | 0.166118E+02 | 0.152204E+02 |
| 0.141600E+04 | 0.159174E+02 | 0.166124E+02 | 0.152223E+02 |
| 0.141700E+04 | 0.159178E+02 | 0.166121E+02 | 0.152234E+02 |
| 0.141800E+04 | 0.159176E+02 | 0.166111E+02 | 0.152240E+02 |
| 0.141900E+04 | 0.159169E+02 | 0.166096E+02 | 0.152241E+02 |
| 0.142000E+04 | 0.159159E+02 | 0.166079E+02 | 0.152239E+02 |
| 0.142100E+04 | 0.159149E+02 | 0.166062E+02 | 0.152236E+02 |
| 0.142200E+04 | 0.159140E+02 | 0.166047E+02 | 0.152234E+02 |
| 0.142300E+04 | 0.159132E+02 | 0.166033E+02 | 0.152231E+02 |
| 0.142400E+04 | 0.159124E+02 | 0.166020E+02 | 0.152228E+02 |
| 0.142500E+04 | 0.159116E+02 | 0.166007E+02 | 0.152225E+02 |
| 0.142600E+04 | 0.159108E+02 | 0.165994E+02 | 0.152222E+02 |
| 0.142700E+04 | 0.159100E+02 | 0.165981E+02 | 0.152218E+02 |
| 0.142800E+04 | 0.159089E+02 | 0.165966E+02 | 0.152212E+02 |
| 0.142900E+04 | 0.159073E+02 | 0.165945E+02 | 0.152201E+02 |
| 0.143000E+04 | 0.159054E+02 | 0.165921E+02 | 0.152188E+02 |
| 0.143100E+04 | 0.159033E+02 | 0.165894E+02 | 0.152172E+02 |
| 0.143200E+04 | 0.159011E+02 | 0.165867E+02 | 0.152155E+02 |
| 0.143300E+04 | 0.158987E+02 | 0.165838E+02 | 0.152137E+02 |
| 0.143400E+04 | 0.158695E+02 | 0.165499E+02 | 0.151891E+02 |
| 0.143500E+04 | 0.158398E+02 | 0.165154E+02 | 0.151643E+02 |
| 0.143600E+04 | 0.158475E+02 | 0.165247E+02 | 0.151703E+02 |
| 0.143700E+04 | 0.158607E+02 | 0.165407E+02 | 0.151806E+02 |
| 0.143800E+04 | 0.158662E+02 | 0.165476E+02 | 0.151849E+02 |
| 0.143900E+04 | 0.158682E+02 | 0.165500E+02 | 0.151863E+02 |
| 0.144000E+04 | 0.158687E+02 | 0.165506E+02 | 0.151867E+02 |
| 0.144100E+04 | 0.158684E+02 | 0.165502E+02 | 0.151865E+02 |
| 0.144200E+04 | 0.158678E+02 | 0.165494E+02 | 0.151862E+02 |
| 0.144300E+04 | 0.158674E+02 | 0.165489E+02 | 0.151860E+02 |
| 0.144400E+04 | 0.158671E+02 | 0.165484E+02 | 0.151859E+02 |
| 0.144500E+04 | 0.158668E+02 | 0.165480E+02 | 0.151857E+02 |
| 0.144600E+04 | 0.158666E+02 | 0.165475E+02 | 0.151856E+02 |
| 0.144700E+04 | 0.158663E+02 | 0.165471E+02 | 0.151855E+02 |
| 0.144800E+04 | 0.158660E+02 | 0.165467E+02 | 0.151853E+02 |
| 0.144900E+04 | 0.158657E+02 | 0.165463E+02 | 0.151852E+02 |
| 0.145000E+04 | 0.158655E+02 | 0.165459E+02 | 0.151850E+02 |
| 0.145100E+04 | 0.158652E+02 | 0.165456E+02 | 0.151849E+02 |
| 0.145200E+04 | 0.158649E+02 | 0.165451E+02 | 0.151847E+02 |
| 0.145300E+04 | 0.156986E+02 | 0.163494E+02 | 0.150478E+02 |
| 0.145400E+04 | 0.155209E+02 | 0.161407E+02 | 0.149010E+02 |
| 0.145500E+04 | 0.157223E+02 | 0.162074E+02 | 0.152372E+02 |
| 0.145600E+04 | 0.158517E+02 | 0.163244E+02 | 0.153790E+02 |
| 0.145700E+04 | 0.159077E+02 | 0.163858E+02 | 0.154296E+02 |
| 0.145800E+04 | 0.159363E+02 | 0.164214E+02 | 0.154512E+02 |
| 0.145900E+04 | 0.157969E+02 | 0.162550E+02 | 0.153387E+02 |
| 0.146000E+04 | 0.155383E+02 | 0.159649E+02 | 0.151117E+02 |
| 0.146100E+04 | 0.154816E+02 | 0.159199E+02 | 0.150434E+02 |
| 0.146200E+04 | 0.156008E+02 | 0.160635E+02 | 0.151380E+02 |
| 0.146300E+04 | 0.157161E+02 | 0.161978E+02 | 0.152344E+02 |
| 0.146400E+04 | 0.157779E+02 | 0.162756E+02 | 0.152803E+02 |
| 0.146500E+04 | 0.158130E+02 | 0.163221E+02 | 0.153038E+02 |
| 0.146600E+04 | 0.157942E+02 | 0.163031E+02 | 0.152853E+02 |

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| 0.146700E+04 | 0.157648E+02 | 0.162694E+02 | 0.152601E+02 |
| 0.146800E+04 | 0.157932E+02 | 0.163077E+02 | 0.152787E+02 |
| 0.146900E+04 | 0.158317E+02 | 0.163593E+02 | 0.153042E+02 |
| 0.147000E+04 | 0.158545E+02 | 0.163903E+02 | 0.153187E+02 |
| 0.147100E+04 | 0.158691E+02 | 0.164102E+02 | 0.153280E+02 |
| 0.147200E+04 | 0.158802E+02 | 0.164253E+02 | 0.153350E+02 |
| 0.147300E+04 | 0.159115E+02 | 0.164797E+02 | 0.153434E+02 |
| 0.147400E+04 | 0.159169E+02 | 0.164839E+02 | 0.153498E+02 |
| 0.147500E+04 | 0.159220E+02 | 0.164894E+02 | 0.153546E+02 |
| 0.147600E+04 | 0.159266E+02 | 0.164947E+02 | 0.153586E+02 |
| 0.147700E+04 | 0.159309E+02 | 0.164996E+02 | 0.153622E+02 |
| 0.147800E+04 | 0.159350E+02 | 0.165043E+02 | 0.153657E+02 |
| 0.147900E+04 | 0.159388E+02 | 0.165085E+02 | 0.153690E+02 |
| 0.148000E+04 | 0.159372E+02 | 0.165125E+02 | 0.153620E+02 |
| 0.148100E+04 | 0.158914E+02 | 0.164707E+02 | 0.153122E+02 |
| 0.148200E+04 | 0.158569E+02 | 0.164401E+02 | 0.152738E+02 |
| 0.148300E+04 | 0.158195E+02 | 0.164051E+02 | 0.152338E+02 |
| 0.148400E+04 | 0.158050E+02 | 0.164048E+02 | 0.152052E+02 |
| 0.148500E+04 | 0.158148E+02 | 0.164485E+02 | 0.151811E+02 |
| 0.148600E+04 | 0.157900E+02 | 0.164782E+02 | 0.151018E+02 |
| 0.148700E+04 | 0.157779E+02 | 0.164903E+02 | 0.150655E+02 |
| 0.148800E+04 | 0.157811E+02 | 0.164964E+02 | 0.150659E+02 |
| 0.148900E+04 | 0.157809E+02 | 0.164998E+02 | 0.150620E+02 |
| 0.149000E+04 | 0.157796E+02 | 0.165015E+02 | 0.150577E+02 |
| 0.149100E+04 | 0.157791E+02 | 0.165020E+02 | 0.150562E+02 |
| 0.149200E+04 | 0.157799E+02 | 0.165020E+02 | 0.150578E+02 |
| 0.149300E+04 | 0.157818E+02 | 0.165018E+02 | 0.150619E+02 |
| 0.149400E+04 | 0.157847E+02 | 0.165016E+02 | 0.150678E+02 |
| 0.149500E+04 | 0.157421E+02 | 0.164482E+02 | 0.150361E+02 |
| 0.149600E+04 | 0.156978E+02 | 0.163923E+02 | 0.150032E+02 |
| 0.149700E+04 | 0.157180E+02 | 0.164116E+02 | 0.150243E+02 |
| 0.149800E+04 | 0.157474E+02 | 0.164401E+02 | 0.150546E+02 |
| 0.149900E+04 | 0.157617E+02 | 0.164504E+02 | 0.150730E+02 |
| 0.150000E+04 | 0.157724E+02 | 0.164588E+02 | 0.150861E+02 |
| 0.150100E+04 | 0.157814E+02 | 0.164655E+02 | 0.150973E+02 |
| 0.150200E+04 | 0.157892E+02 | 0.164712E+02 | 0.151072E+02 |
| 0.150300E+04 | 0.157961E+02 | 0.164760E+02 | 0.151162E+02 |
| 0.150400E+04 | 0.157634E+02 | 0.164350E+02 | 0.150919E+02 |
| 0.150500E+04 | 0.157249E+02 | 0.163868E+02 | 0.150630E+02 |
| 0.150600E+04 | 0.157419E+02 | 0.164051E+02 | 0.150788E+02 |
| 0.150700E+04 | 0.157725E+02 | 0.164402E+02 | 0.151047E+02 |
| 0.150800E+04 | 0.157905E+02 | 0.164605E+02 | 0.151205E+02 |
| 0.150900E+04 | 0.158017E+02 | 0.164724E+02 | 0.151309E+02 |
| 0.151000E+04 | 0.158103E+02 | 0.164812E+02 | 0.151393E+02 |
| 0.151100E+04 | 0.158174E+02 | 0.164884E+02 | 0.151463E+02 |
| 0.151200E+04 | 0.158233E+02 | 0.164944E+02 | 0.151523E+02 |
| 0.151300E+04 | 0.158283E+02 | 0.164993E+02 | 0.151573E+02 |
| 0.151400E+04 | 0.158325E+02 | 0.165034E+02 | 0.151616E+02 |
| 0.151500E+04 | 0.158360E+02 | 0.165068E+02 | 0.151652E+02 |
| 0.151600E+04 | 0.158391E+02 | 0.165099E+02 | 0.151684E+02 |
| 0.151700E+04 | 0.158418E+02 | 0.165126E+02 | 0.151711E+02 |
| 0.151800E+04 | 0.158442E+02 | 0.165151E+02 | 0.151734E+02 |
| 0.151900E+04 | 0.158464E+02 | 0.165174E+02 | 0.151754E+02 |
| 0.152000E+04 | 0.158485E+02 | 0.165197E+02 | 0.151773E+02 |

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| 0.152100E+04 | 0.158505E+02 | 0.165220E+02 | 0.151790E+02 |
| 0.152200E+04 | 0.158523E+02 | 0.165241E+02 | 0.151805E+02 |
| 0.152300E+04 | 0.158541E+02 | 0.165262E+02 | 0.151819E+02 |
| 0.152400E+04 | 0.158556E+02 | 0.165280E+02 | 0.151832E+02 |
| 0.152500E+04 | 0.158570E+02 | 0.165298E+02 | 0.151843E+02 |
| 0.152600E+04 | 0.158584E+02 | 0.165314E+02 | 0.151853E+02 |
| 0.152700E+04 | 0.157598E+02 | 0.164234E+02 | 0.150961E+02 |
| 0.152800E+04 | 0.156533E+02 | 0.162994E+02 | 0.150073E+02 |
| 0.152900E+04 | 0.156942E+02 | 0.163413E+02 | 0.150471E+02 |
| 0.153000E+04 | 0.157584E+02 | 0.164157E+02 | 0.151010E+02 |
| 0.153100E+04 | 0.157864E+02 | 0.164467E+02 | 0.151262E+02 |
| 0.153200E+04 | 0.157988E+02 | 0.164660E+02 | 0.151317E+02 |
| 0.153300E+04 | 0.158070E+02 | 0.164795E+02 | 0.151346E+02 |
| 0.153400E+04 | 0.158147E+02 | 0.164895E+02 | 0.151400E+02 |
| 0.153500E+04 | 0.158212E+02 | 0.164971E+02 | 0.151452E+02 |
| 0.153600E+04 | 0.158266E+02 | 0.165033E+02 | 0.151499E+02 |
| 0.153700E+04 | 0.158312E+02 | 0.165085E+02 | 0.151540E+02 |
| 0.153800E+04 | 0.158351E+02 | 0.165127E+02 | 0.151575E+02 |
| 0.153900E+04 | 0.158383E+02 | 0.165161E+02 | 0.151606E+02 |
| 0.154000E+04 | 0.158410E+02 | 0.165189E+02 | 0.151632E+02 |
| 0.154100E+04 | 0.158435E+02 | 0.165214E+02 | 0.151656E+02 |
| 0.154200E+04 | 0.158457E+02 | 0.165237E+02 | 0.151677E+02 |
| 0.154300E+04 | 0.158477E+02 | 0.165258E+02 | 0.151696E+02 |
| 0.154400E+04 | 0.158495E+02 | 0.165277E+02 | 0.151714E+02 |
| 0.154500E+04 | 0.158512E+02 | 0.165294E+02 | 0.151730E+02 |
| 0.154600E+04 | 0.158527E+02 | 0.165310E+02 | 0.151744E+02 |
| 0.154700E+04 | 0.158541E+02 | 0.165324E+02 | 0.151757E+02 |
| 0.154800E+04 | 0.158553E+02 | 0.165337E+02 | 0.151769E+02 |
| 0.154900E+04 | 0.158564E+02 | 0.165349E+02 | 0.151780E+02 |
| 0.155000E+04 | 0.158574E+02 | 0.165359E+02 | 0.151790E+02 |
| 0.155100E+04 | 0.158583E+02 | 0.165368E+02 | 0.151798E+02 |
| 0.155200E+04 | 0.158591E+02 | 0.165375E+02 | 0.151806E+02 |
| 0.155300E+04 | 0.158597E+02 | 0.165381E+02 | 0.151813E+02 |
| 0.155400E+04 | 0.158603E+02 | 0.165387E+02 | 0.151819E+02 |
| 0.155500E+04 | 0.158608E+02 | 0.165391E+02 | 0.151825E+02 |
| 0.155600E+04 | 0.158612E+02 | 0.165395E+02 | 0.151830E+02 |
| 0.155700E+04 | 0.158616E+02 | 0.165398E+02 | 0.151834E+02 |
| 0.155800E+04 | 0.158620E+02 | 0.165401E+02 | 0.151839E+02 |
| 0.155900E+04 | 0.158623E+02 | 0.165402E+02 | 0.151843E+02 |
| 0.156000E+04 | 0.158626E+02 | 0.165405E+02 | 0.151848E+02 |
| 0.156100E+04 | 0.158631E+02 | 0.165409E+02 | 0.151853E+02 |
| 0.156200E+04 | 0.158636E+02 | 0.165413E+02 | 0.151860E+02 |
| 0.156300E+04 | 0.158642E+02 | 0.165418E+02 | 0.151866E+02 |
| 0.156400E+04 | 0.157724E+02 | 0.164418E+02 | 0.151029E+02 |
| 0.156500E+04 | 0.156692E+02 | 0.163206E+02 | 0.150179E+02 |
| 0.156600E+04 | 0.157063E+02 | 0.163577E+02 | 0.150550E+02 |
| 0.156700E+04 | 0.157687E+02 | 0.164302E+02 | 0.151072E+02 |
| 0.156800E+04 | 0.157953E+02 | 0.164588E+02 | 0.151319E+02 |
| 0.156900E+04 | 0.158074E+02 | 0.164769E+02 | 0.151379E+02 |
| 0.157000E+04 | 0.157880E+02 | 0.164580E+02 | 0.151180E+02 |
| 0.157100E+04 | 0.157674E+02 | 0.164338E+02 | 0.151011E+02 |
| 0.157200E+04 | 0.157842E+02 | 0.164534E+02 | 0.151150E+02 |
| 0.157300E+04 | 0.158059E+02 | 0.164792E+02 | 0.151326E+02 |
| 0.157400E+04 | 0.158187E+02 | 0.164940E+02 | 0.151434E+02 |

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| 0.157500E+04 | 0.158270E+02 | 0.165031E+02 | 0.151509E+02 |
| 0.157600E+04 | 0.158334E+02 | 0.165097E+02 | 0.151570E+02 |
| 0.157700E+04 | 0.158386E+02 | 0.165150E+02 | 0.151622E+02 |
| 0.157800E+04 | 0.158433E+02 | 0.165196E+02 | 0.151670E+02 |
| 0.157900E+04 | 0.158475E+02 | 0.165236E+02 | 0.151713E+02 |
| 0.158000E+04 | 0.158512E+02 | 0.165272E+02 | 0.151753E+02 |
| 0.158100E+04 | 0.158547E+02 | 0.165305E+02 | 0.151789E+02 |
| 0.158200E+04 | 0.158579E+02 | 0.165334E+02 | 0.151823E+02 |
| 0.158300E+04 | 0.158609E+02 | 0.165362E+02 | 0.151855E+02 |
| 0.158400E+04 | 0.158637E+02 | 0.165389E+02 | 0.151885E+02 |
| 0.158500E+04 | 0.158663E+02 | 0.165412E+02 | 0.151913E+02 |
| 0.158600E+04 | 0.158686E+02 | 0.165433E+02 | 0.151938E+02 |
| 0.158700E+04 | 0.157132E+02 | 0.163666E+02 | 0.150598E+02 |
| 0.158800E+04 | 0.154744E+02 | 0.160886E+02 | 0.148602E+02 |
| 0.158900E+04 | 0.156158E+02 | 0.161042E+02 | 0.151275E+02 |
| 0.159000E+04 | 0.157461E+02 | 0.162183E+02 | 0.152740E+02 |
| 0.159100E+04 | 0.158410E+02 | 0.163176E+02 | 0.153645E+02 |
| 0.159200E+04 | 0.158870E+02 | 0.163685E+02 | 0.154054E+02 |
| 0.159300E+04 | 0.159142E+02 | 0.164041E+02 | 0.154242E+02 |
| 0.159400E+04 | 0.159325E+02 | 0.164313E+02 | 0.154336E+02 |
| 0.159500E+04 | 0.159455E+02 | 0.164532E+02 | 0.154379E+02 |
| 0.159600E+04 | 0.159551E+02 | 0.164710E+02 | 0.154392E+02 |
| 0.159700E+04 | 0.159644E+02 | 0.164859E+02 | 0.154430E+02 |
| 0.159800E+04 | 0.159701E+02 | 0.164982E+02 | 0.154419E+02 |
| 0.159900E+04 | 0.159745E+02 | 0.165086E+02 | 0.154403E+02 |
| 0.160000E+04 | 0.160005E+02 | 0.165604E+02 | 0.154407E+02 |
| 0.160100E+04 | 0.158122E+02 | 0.163475E+02 | 0.152769E+02 |
| 0.160200E+04 | 0.155901E+02 | 0.160843E+02 | 0.150960E+02 |
| 0.160300E+04 | 0.156584E+02 | 0.161822E+02 | 0.151345E+02 |
| 0.160400E+04 | 0.157541E+02 | 0.162916E+02 | 0.152167E+02 |
| 0.160500E+04 | 0.158219E+02 | 0.163701E+02 | 0.152738E+02 |
| 0.160600E+04 | 0.158609E+02 | 0.164181E+02 | 0.153037E+02 |
| 0.160700E+04 | 0.158846E+02 | 0.164478E+02 | 0.153213E+02 |
| 0.160800E+04 | 0.159013E+02 | 0.164688E+02 | 0.153337E+02 |
| 0.160900E+04 | 0.159137E+02 | 0.164844E+02 | 0.153430E+02 |
| 0.161000E+04 | 0.159238E+02 | 0.164969E+02 | 0.153507E+02 |
| 0.161100E+04 | 0.159318E+02 | 0.165068E+02 | 0.153569E+02 |
| 0.161200E+04 | 0.159390E+02 | 0.165186E+02 | 0.153594E+02 |
| 0.161300E+04 | 0.158339E+02 | 0.164063E+02 | 0.152615E+02 |
| 0.161400E+04 | 0.157340E+02 | 0.163069E+02 | 0.151611E+02 |
| 0.161500E+04 | 0.157652E+02 | 0.163531E+02 | 0.151773E+02 |
| 0.161600E+04 | 0.157956E+02 | 0.164189E+02 | 0.151722E+02 |
| 0.161700E+04 | 0.157667E+02 | 0.164460E+02 | 0.150875E+02 |
| 0.161800E+04 | 0.157532E+02 | 0.164578E+02 | 0.150485E+02 |
| 0.161900E+04 | 0.157451E+02 | 0.164659E+02 | 0.150243E+02 |
| 0.162000E+04 | 0.157411E+02 | 0.164719E+02 | 0.150103E+02 |
| 0.162100E+04 | 0.157380E+02 | 0.164753E+02 | 0.150008E+02 |
| 0.162200E+04 | 0.156322E+02 | 0.163359E+02 | 0.149285E+02 |
| 0.162300E+04 | 0.155198E+02 | 0.162136E+02 | 0.148260E+02 |
| 0.162400E+04 | 0.155648E+02 | 0.162614E+02 | 0.148681E+02 |
| 0.162500E+04 | 0.156398E+02 | 0.163442E+02 | 0.149354E+02 |
| 0.162600E+04 | 0.156795E+02 | 0.163859E+02 | 0.149732E+02 |
| 0.162700E+04 | 0.157020E+02 | 0.164082E+02 | 0.149957E+02 |
| 0.162800E+04 | 0.157181E+02 | 0.164227E+02 | 0.150135E+02 |

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| 0.162900E+04 | 0.157311E+02 | 0.164333E+02 | 0.150289E+02 |
| 0.163000E+04 | 0.157403E+02 | 0.164394E+02 | 0.150413E+02 |
| 0.163100E+04 | 0.157475E+02 | 0.164431E+02 | 0.150518E+02 |
| 0.163200E+04 | 0.157543E+02 | 0.164467E+02 | 0.150619E+02 |
| 0.163300E+04 | 0.157616E+02 | 0.164512E+02 | 0.150719E+02 |
| 0.163400E+04 | 0.157685E+02 | 0.164556E+02 | 0.150813E+02 |
| 0.163500E+04 | 0.157740E+02 | 0.164587E+02 | 0.150892E+02 |
| 0.163600E+04 | 0.157788E+02 | 0.164614E+02 | 0.150962E+02 |
| 0.163700E+04 | 0.157831E+02 | 0.164638E+02 | 0.151025E+02 |
| 0.163800E+04 | 0.157918E+02 | 0.164716E+02 | 0.151120E+02 |
| 0.163900E+04 | 0.158041E+02 | 0.164840E+02 | 0.151243E+02 |
| 0.164000E+04 | 0.158098E+02 | 0.164888E+02 | 0.151308E+02 |
| 0.164100E+04 | 0.156118E+02 | 0.162614E+02 | 0.149622E+02 |
| 0.164200E+04 | 0.153879E+02 | 0.160021E+02 | 0.147737E+02 |
| 0.164300E+04 | 0.154603E+02 | 0.160982E+02 | 0.148224E+02 |
| 0.164400E+04 | 0.156929E+02 | 0.162056E+02 | 0.151802E+02 |
| 0.164500E+04 | 0.157847E+02 | 0.162687E+02 | 0.153006E+02 |
| 0.164600E+04 | 0.158189E+02 | 0.163024E+02 | 0.153354E+02 |
| 0.164700E+04 | 0.158534E+02 | 0.163439E+02 | 0.153628E+02 |
| 0.164800E+04 | 0.158805E+02 | 0.163807E+02 | 0.153803E+02 |
| 0.164900E+04 | 0.158974E+02 | 0.164068E+02 | 0.153879E+02 |
| 0.165000E+04 | 0.159084E+02 | 0.164265E+02 | 0.153903E+02 |
| 0.165100E+04 | 0.159161E+02 | 0.164422E+02 | 0.153899E+02 |
| 0.165200E+04 | 0.159217E+02 | 0.164552E+02 | 0.153881E+02 |
| 0.165300E+04 | 0.159279E+02 | 0.164660E+02 | 0.153898E+02 |
| 0.165400E+04 | 0.159304E+02 | 0.164744E+02 | 0.153863E+02 |
| 0.165500E+04 | 0.159319E+02 | 0.164811E+02 | 0.153827E+02 |
| 0.165600E+04 | 0.159327E+02 | 0.164865E+02 | 0.153790E+02 |
| 0.165700E+04 | 0.159331E+02 | 0.164909E+02 | 0.153754E+02 |
| 0.165800E+04 | 0.159331E+02 | 0.164944E+02 | 0.153718E+02 |
| 0.165900E+04 | 0.159327E+02 | 0.164971E+02 | 0.153683E+02 |
| 0.166000E+04 | 0.159323E+02 | 0.164995E+02 | 0.153652E+02 |
| 0.166100E+04 | 0.159318E+02 | 0.165014E+02 | 0.153622E+02 |
| 0.166200E+04 | 0.159311E+02 | 0.165028E+02 | 0.153594E+02 |
| 0.166300E+04 | 0.159302E+02 | 0.165038E+02 | 0.153566E+02 |
| 0.166400E+04 | 0.159293E+02 | 0.165046E+02 | 0.153541E+02 |
| 0.166500E+04 | 0.159285E+02 | 0.165052E+02 | 0.153517E+02 |
| 0.166600E+04 | 0.159276E+02 | 0.165056E+02 | 0.153495E+02 |
| 0.166700E+04 | 0.158344E+02 | 0.163951E+02 | 0.152737E+02 |
| 0.166800E+04 | 0.157492E+02 | 0.163029E+02 | 0.151955E+02 |
| 0.166900E+04 | 0.157781E+02 | 0.163374E+02 | 0.152187E+02 |
| 0.167000E+04 | 0.158318E+02 | 0.164032E+02 | 0.152604E+02 |
| 0.167100E+04 | 0.158537E+02 | 0.164279E+02 | 0.152796E+02 |
| 0.167200E+04 | 0.158664E+02 | 0.164438E+02 | 0.152889E+02 |
| 0.167300E+04 | 0.158750E+02 | 0.164551E+02 | 0.152949E+02 |
| 0.167400E+04 | 0.157526E+02 | 0.163090E+02 | 0.151962E+02 |
| 0.167500E+04 | 0.156341E+02 | 0.161792E+02 | 0.150889E+02 |
| 0.167600E+04 | 0.156827E+02 | 0.162345E+02 | 0.151310E+02 |
| 0.167700E+04 | 0.157585E+02 | 0.163240E+02 | 0.151930E+02 |
| 0.167800E+04 | 0.157964E+02 | 0.163696E+02 | 0.152232E+02 |
| 0.167900E+04 | 0.158178E+02 | 0.163960E+02 | 0.152395E+02 |
| 0.168000E+04 | 0.158326E+02 | 0.164144E+02 | 0.152509E+02 |
| 0.168100E+04 | 0.157374E+02 | 0.163006E+02 | 0.151742E+02 |
| 0.168200E+04 | 0.156461E+02 | 0.161991E+02 | 0.150931E+02 |

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| 0.168300E+04 | 0.156884E+02 | 0.162485E+02 | 0.151283E+02 |
| 0.168400E+04 | 0.157531E+02 | 0.163242E+02 | 0.151820E+02 |
| 0.168500E+04 | 0.157872E+02 | 0.163648E+02 | 0.152095E+02 |
| 0.168600E+04 | 0.158072E+02 | 0.163891E+02 | 0.152252E+02 |
| 0.168700E+04 | 0.158181E+02 | 0.164063E+02 | 0.152299E+02 |
| 0.168800E+04 | 0.158173E+02 | 0.164190E+02 | 0.152155E+02 |
| 0.168900E+04 | 0.157868E+02 | 0.163945E+02 | 0.151790E+02 |
| 0.169000E+04 | 0.157641E+02 | 0.163768E+02 | 0.151515E+02 |
| 0.169100E+04 | 0.157200E+02 | 0.164022E+02 | 0.150378E+02 |
| 0.169200E+04 | 0.157075E+02 | 0.164202E+02 | 0.149947E+02 |
| 0.169300E+04 | 0.156965E+02 | 0.164278E+02 | 0.149651E+02 |
| 0.169400E+04 | 0.156887E+02 | 0.164316E+02 | 0.149458E+02 |
| 0.169500E+04 | 0.155731E+02 | 0.162939E+02 | 0.148524E+02 |
| 0.169600E+04 | 0.154275E+02 | 0.161307E+02 | 0.147242E+02 |
| 0.169700E+04 | 0.154729E+02 | 0.161823E+02 | 0.147636E+02 |
| 0.169800E+04 | 0.155553E+02 | 0.162755E+02 | 0.148352E+02 |
| 0.169900E+04 | 0.155967E+02 | 0.163202E+02 | 0.148733E+02 |
| 0.170000E+04 | 0.156214E+02 | 0.163459E+02 | 0.148968E+02 |
| 0.170100E+04 | 0.156400E+02 | 0.163638E+02 | 0.149161E+02 |
| 0.170200E+04 | 0.156552E+02 | 0.163771E+02 | 0.149332E+02 |
| 0.170300E+04 | 0.156684E+02 | 0.163878E+02 | 0.149489E+02 |
| 0.170400E+04 | 0.156802E+02 | 0.163968E+02 | 0.149636E+02 |
| 0.170500E+04 | 0.156909E+02 | 0.164047E+02 | 0.149772E+02 |
| 0.170600E+04 | 0.157005E+02 | 0.164113E+02 | 0.149897E+02 |
| 0.170700E+04 | 0.157091E+02 | 0.164172E+02 | 0.150011E+02 |
| 0.170800E+04 | 0.157169E+02 | 0.164224E+02 | 0.150115E+02 |
| 0.170900E+04 | 0.157239E+02 | 0.164268E+02 | 0.150209E+02 |
| 0.171000E+04 | 0.157302E+02 | 0.164310E+02 | 0.150294E+02 |
| 0.171100E+04 | 0.157362E+02 | 0.164351E+02 | 0.150373E+02 |
| 0.171200E+04 | 0.157419E+02 | 0.164391E+02 | 0.150447E+02 |
| 0.171300E+04 | 0.157472E+02 | 0.164429E+02 | 0.150514E+02 |
| 0.171400E+04 | 0.157522E+02 | 0.164468E+02 | 0.150577E+02 |
| 0.171500E+04 | 0.157573E+02 | 0.164508E+02 | 0.150637E+02 |
| 0.171600E+04 | 0.157624E+02 | 0.164552E+02 | 0.150696E+02 |
| 0.171700E+04 | 0.157679E+02 | 0.164602E+02 | 0.150755E+02 |
| 0.171800E+04 | 0.157726E+02 | 0.164645E+02 | 0.150807E+02 |
| 0.171900E+04 | 0.157778E+02 | 0.164695E+02 | 0.150861E+02 |
| 0.172000E+04 | 0.157834E+02 | 0.164751E+02 | 0.150917E+02 |
| 0.172100E+04 | 0.157875E+02 | 0.164790E+02 | 0.150960E+02 |
| 0.172200E+04 | 0.157904E+02 | 0.164816E+02 | 0.150992E+02 |
| 0.172300E+04 | 0.157922E+02 | 0.164831E+02 | 0.151014E+02 |
| 0.172400E+04 | 0.157947E+02 | 0.164854E+02 | 0.151041E+02 |
| 0.172500E+04 | 0.157980E+02 | 0.164887E+02 | 0.151073E+02 |
| 0.172600E+04 | 0.158025E+02 | 0.164935E+02 | 0.151114E+02 |
| 0.172700E+04 | 0.158079E+02 | 0.164995E+02 | 0.151162E+02 |
| 0.172800E+04 | 0.158140E+02 | 0.165064E+02 | 0.151217E+02 |
| 0.172900E+04 | 0.156973E+02 | 0.163766E+02 | 0.150180E+02 |
| 0.173000E+04 | 0.156142E+02 | 0.162820E+02 | 0.149463E+02 |
| 0.173100E+04 | 0.156949E+02 | 0.163697E+02 | 0.150201E+02 |
| 0.173200E+04 | 0.157397E+02 | 0.164162E+02 | 0.150632E+02 |
| 0.173300E+04 | 0.157571E+02 | 0.164387E+02 | 0.150755E+02 |
| 0.173400E+04 | 0.157614E+02 | 0.164530E+02 | 0.150697E+02 |
| 0.173500E+04 | 0.157679E+02 | 0.164650E+02 | 0.150708E+02 |
| 0.173600E+04 | 0.157772E+02 | 0.164781E+02 | 0.150763E+02 |

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| 0.173700E+04 | 0.157852E+02 | 0.164885E+02 | 0.150818E+02 |
| 0.173800E+04 | 0.157893E+02 | 0.164938E+02 | 0.150848E+02 |
| 0.173900E+04 | 0.157441E+02 | 0.164411E+02 | 0.150472E+02 |
| 0.174000E+04 | 0.157109E+02 | 0.164014E+02 | 0.150203E+02 |
| 0.174100E+04 | 0.157445E+02 | 0.164414E+02 | 0.150475E+02 |
| 0.174200E+04 | 0.157701E+02 | 0.164725E+02 | 0.150677E+02 |
| 0.174300E+04 | 0.157796E+02 | 0.164836E+02 | 0.150755E+02 |
| 0.174400E+04 | 0.157848E+02 | 0.164894E+02 | 0.150802E+02 |
| 0.174500E+04 | 0.157889E+02 | 0.164937E+02 | 0.150841E+02 |
| 0.174600E+04 | 0.157926E+02 | 0.164975E+02 | 0.150877E+02 |
| 0.174700E+04 | 0.157961E+02 | 0.165011E+02 | 0.150911E+02 |
| 0.174800E+04 | 0.158034E+02 | 0.165091E+02 | 0.150977E+02 |
| 0.174900E+04 | 0.158135E+02 | 0.165204E+02 | 0.151066E+02 |
| 0.175000E+04 | 0.158207E+02 | 0.165282E+02 | 0.151131E+02 |
| 0.175100E+04 | 0.158239E+02 | 0.165313E+02 | 0.151164E+02 |
| 0.175200E+04 | 0.158247E+02 | 0.165316E+02 | 0.151178E+02 |
| 0.175300E+04 | 0.158257E+02 | 0.165321E+02 | 0.151192E+02 |
| 0.175400E+04 | 0.158261E+02 | 0.165320E+02 | 0.151202E+02 |
| 0.175500E+04 | 0.158260E+02 | 0.165314E+02 | 0.151205E+02 |
| 0.175600E+04 | 0.158265E+02 | 0.165316E+02 | 0.151215E+02 |
| 0.175700E+04 | 0.158289E+02 | 0.165339E+02 | 0.151239E+02 |
| 0.175800E+04 | 0.158332E+02 | 0.165386E+02 | 0.151279E+02 |
| 0.175900E+04 | 0.158383E+02 | 0.165441E+02 | 0.151325E+02 |
| 0.176000E+04 | 0.158424E+02 | 0.165484E+02 | 0.151364E+02 |
| 0.176100E+04 | 0.158468E+02 | 0.165530E+02 | 0.151406E+02 |
| 0.176200E+04 | 0.158506E+02 | 0.165569E+02 | 0.151443E+02 |
| 0.176300E+04 | 0.158516E+02 | 0.165575E+02 | 0.151458E+02 |
| 0.176400E+04 | 0.158518E+02 | 0.165571E+02 | 0.151465E+02 |
| 0.176500E+04 | 0.158508E+02 | 0.165554E+02 | 0.151463E+02 |
| 0.176600E+04 | 0.158489E+02 | 0.165526E+02 | 0.151451E+02 |
| 0.176700E+04 | 0.158497E+02 | 0.165531E+02 | 0.151463E+02 |
| 0.176800E+04 | 0.158555E+02 | 0.165594E+02 | 0.151515E+02 |
| 0.176900E+04 | 0.158640E+02 | 0.165690E+02 | 0.151590E+02 |
| 0.177000E+04 | 0.158719E+02 | 0.165777E+02 | 0.151661E+02 |
| 0.177100E+04 | 0.158765E+02 | 0.165825E+02 | 0.151706E+02 |
| 0.177200E+04 | 0.158784E+02 | 0.165839E+02 | 0.151728E+02 |
| 0.177300E+04 | 0.158775E+02 | 0.165822E+02 | 0.151728E+02 |
| 0.177400E+04 | 0.158751E+02 | 0.165786E+02 | 0.151715E+02 |
| 0.177500E+04 | 0.158724E+02 | 0.165749E+02 | 0.151699E+02 |
| 0.177600E+04 | 0.158705E+02 | 0.165722E+02 | 0.151688E+02 |
| 0.177700E+04 | 0.158718E+02 | 0.165733E+02 | 0.151703E+02 |
| 0.177800E+04 | 0.158770E+02 | 0.165791E+02 | 0.151750E+02 |
| 0.177900E+04 | 0.158837E+02 | 0.165865E+02 | 0.151810E+02 |
| 0.178000E+04 | 0.158873E+02 | 0.165902E+02 | 0.151844E+02 |
| 0.178100E+04 | 0.158893E+02 | 0.165920E+02 | 0.151867E+02 |
| 0.178200E+04 | 0.158898E+02 | 0.165920E+02 | 0.151877E+02 |
| 0.178300E+04 | 0.157674E+02 | 0.164578E+02 | 0.150771E+02 |
| 0.178400E+04 | 0.156736E+02 | 0.163494E+02 | 0.149979E+02 |
| 0.178500E+04 | 0.157525E+02 | 0.164338E+02 | 0.150712E+02 |
| 0.178600E+04 | 0.158018E+02 | 0.164870E+02 | 0.151165E+02 |
| 0.178700E+04 | 0.158276E+02 | 0.165178E+02 | 0.151374E+02 |
| 0.178800E+04 | 0.158417E+02 | 0.165396E+02 | 0.151437E+02 |
| 0.178900E+04 | 0.158061E+02 | 0.164991E+02 | 0.151131E+02 |
| 0.179000E+04 | 0.157640E+02 | 0.164491E+02 | 0.150790E+02 |

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| 0.179100E+04 | 0.157842E+02 | 0.164727E+02 | 0.150958E+02 |
| 0.179200E+04 | 0.158159E+02 | 0.165104E+02 | 0.151215E+02 |
| 0.179300E+04 | 0.158338E+02 | 0.165312E+02 | 0.151365E+02 |
| 0.179400E+04 | 0.158438E+02 | 0.165420E+02 | 0.151455E+02 |
| 0.179500E+04 | 0.158493E+02 | 0.165475E+02 | 0.151511E+02 |
| 0.179600E+04 | 0.158527E+02 | 0.165504E+02 | 0.151549E+02 |
| 0.179700E+04 | 0.158553E+02 | 0.165525E+02 | 0.151581E+02 |
| 0.179800E+04 | 0.158573E+02 | 0.165539E+02 | 0.151607E+02 |
| 0.179900E+04 | 0.158594E+02 | 0.165555E+02 | 0.151633E+02 |
| 0.180000E+04 | 0.158620E+02 | 0.165577E+02 | 0.151663E+02 |
| 0.180100E+04 | 0.158675E+02 | 0.165635E+02 | 0.151716E+02 |
| 0.180200E+04 | 0.158735E+02 | 0.165698E+02 | 0.151772E+02 |
| 0.180300E+04 | 0.158755E+02 | 0.165714E+02 | 0.151796E+02 |
| 0.180400E+04 | 0.158761E+02 | 0.165713E+02 | 0.151808E+02 |
| 0.180500E+04 | 0.158774E+02 | 0.165722E+02 | 0.151826E+02 |
| 0.180600E+04 | 0.158782E+02 | 0.165726E+02 | 0.151839E+02 |
| 0.180700E+04 | 0.158777E+02 | 0.165715E+02 | 0.151840E+02 |
| 0.180800E+04 | 0.158770E+02 | 0.165702E+02 | 0.151839E+02 |
| 0.180900E+04 | 0.156798E+02 | 0.163468E+02 | 0.150127E+02 |
| 0.181000E+04 | 0.154564E+02 | 0.160852E+02 | 0.148275E+02 |
| 0.181100E+04 | 0.156659E+02 | 0.161791E+02 | 0.151526E+02 |
| 0.181200E+04 | 0.157948E+02 | 0.162886E+02 | 0.153010E+02 |
| 0.181300E+04 | 0.158754E+02 | 0.163699E+02 | 0.153808E+02 |
| 0.181400E+04 | 0.159140E+02 | 0.164091E+02 | 0.154189E+02 |
| 0.181500E+04 | 0.157300E+02 | 0.161986E+02 | 0.152614E+02 |
| 0.181600E+04 | 0.155212E+02 | 0.159664E+02 | 0.150761E+02 |
| 0.181700E+04 | 0.155826E+02 | 0.160408E+02 | 0.151244E+02 |
| 0.181800E+04 | 0.157036E+02 | 0.161897E+02 | 0.152175E+02 |
| 0.181900E+04 | 0.157867E+02 | 0.162909E+02 | 0.152825E+02 |
| 0.182000E+04 | 0.158368E+02 | 0.163573E+02 | 0.153163E+02 |
| 0.182100E+04 | 0.158673E+02 | 0.163991E+02 | 0.153355E+02 |
| 0.182200E+04 | 0.158894E+02 | 0.164301E+02 | 0.153488E+02 |
| 0.182300E+04 | 0.159068E+02 | 0.164546E+02 | 0.153589E+02 |
| 0.182400E+04 | 0.159208E+02 | 0.164745E+02 | 0.153671E+02 |
| 0.182500E+04 | 0.159330E+02 | 0.164917E+02 | 0.153743E+02 |
| 0.182600E+04 | 0.159445E+02 | 0.165076E+02 | 0.153815E+02 |
| 0.182700E+04 | 0.159556E+02 | 0.165226E+02 | 0.153887E+02 |
| 0.182800E+04 | 0.159835E+02 | 0.165794E+02 | 0.153876E+02 |
| 0.182900E+04 | 0.159796E+02 | 0.165850E+02 | 0.153742E+02 |
| 0.183000E+04 | 0.159308E+02 | 0.165369E+02 | 0.153247E+02 |
| 0.183100E+04 | 0.157253E+02 | 0.163108E+02 | 0.151398E+02 |
| 0.183200E+04 | 0.155665E+02 | 0.161065E+02 | 0.150265E+02 |
| 0.183300E+04 | 0.156822E+02 | 0.162418E+02 | 0.151225E+02 |
| 0.183400E+04 | 0.157923E+02 | 0.163618E+02 | 0.152227E+02 |
| 0.183500E+04 | 0.157373E+02 | 0.162921E+02 | 0.151825E+02 |
| 0.183600E+04 | 0.156656E+02 | 0.162096E+02 | 0.151216E+02 |
| 0.183700E+04 | 0.157342E+02 | 0.162890E+02 | 0.151795E+02 |
| 0.183800E+04 | 0.158139E+02 | 0.163773E+02 | 0.152504E+02 |
| 0.183900E+04 | 0.158577E+02 | 0.164278E+02 | 0.152875E+02 |
| 0.184000E+04 | 0.158444E+02 | 0.164102E+02 | 0.152786E+02 |
| 0.184100E+04 | 0.158182E+02 | 0.163764E+02 | 0.152600E+02 |
| 0.184200E+04 | 0.158421E+02 | 0.164056E+02 | 0.152785E+02 |
| 0.184300E+04 | 0.158322E+02 | 0.164176E+02 | 0.152469E+02 |
| 0.184400E+04 | 0.157952E+02 | 0.163962E+02 | 0.151943E+02 |

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| 0.184500E+04 | 0.158073E+02 | 0.164286E+02 | 0.151861E+02 |
| 0.184600E+04 | 0.157933E+02 | 0.164781E+02 | 0.151086E+02 |
| 0.184700E+04 | 0.157942E+02 | 0.165049E+02 | 0.150835E+02 |
| 0.184800E+04 | 0.157977E+02 | 0.165265E+02 | 0.150689E+02 |
| 0.184900E+04 | 0.158031E+02 | 0.165438E+02 | 0.150625E+02 |
| 0.185000E+04 | 0.158053E+02 | 0.165537E+02 | 0.150568E+02 |
| 0.185100E+04 | 0.158066E+02 | 0.165582E+02 | 0.150550E+02 |
| 0.185200E+04 | 0.158093E+02 | 0.165609E+02 | 0.150577E+02 |
| 0.185300E+04 | 0.158118E+02 | 0.165612E+02 | 0.150624E+02 |
| 0.185400E+04 | 0.157823E+02 | 0.165229E+02 | 0.150417E+02 |
| 0.185500E+04 | 0.157610E+02 | 0.164933E+02 | 0.150287E+02 |
| 0.185600E+04 | 0.157816E+02 | 0.165128E+02 | 0.150503E+02 |
| 0.185700E+04 | 0.157998E+02 | 0.165298E+02 | 0.150697E+02 |
| 0.185800E+04 | 0.158138E+02 | 0.165416E+02 | 0.150861E+02 |
| 0.185900E+04 | 0.158289E+02 | 0.165545E+02 | 0.151034E+02 |
| 0.186000E+04 | 0.158439E+02 | 0.165673E+02 | 0.151205E+02 |
| 0.186100E+04 | 0.158556E+02 | 0.165764E+02 | 0.151347E+02 |
| 0.186200E+04 | 0.158631E+02 | 0.165810E+02 | 0.151452E+02 |
| 0.186300E+04 | 0.158682E+02 | 0.165830E+02 | 0.151534E+02 |
| 0.186400E+04 | 0.158725E+02 | 0.165845E+02 | 0.151606E+02 |
| 0.186500E+04 | 0.158762E+02 | 0.165856E+02 | 0.151667E+02 |
| 0.186600E+04 | 0.158791E+02 | 0.165863E+02 | 0.151719E+02 |
| 0.186700E+04 | 0.158818E+02 | 0.165871E+02 | 0.151765E+02 |
| 0.186800E+04 | 0.158864E+02 | 0.165905E+02 | 0.151823E+02 |
| 0.186900E+04 | 0.158952E+02 | 0.165990E+02 | 0.151913E+02 |
| 0.187000E+04 | 0.159074E+02 | 0.166118E+02 | 0.152030E+02 |
| 0.187100E+04 | 0.159188E+02 | 0.166238E+02 | 0.152138E+02 |
| 0.187200E+04 | 0.159268E+02 | 0.166319E+02 | 0.152217E+02 |
| 0.187300E+04 | 0.159311E+02 | 0.166358E+02 | 0.152265E+02 |
| 0.187400E+04 | 0.159325E+02 | 0.166363E+02 | 0.152286E+02 |
| 0.187500E+04 | 0.159323E+02 | 0.166353E+02 | 0.152293E+02 |
| 0.187600E+04 | 0.159319E+02 | 0.166342E+02 | 0.152296E+02 |
| 0.187700E+04 | 0.159327E+02 | 0.166346E+02 | 0.152307E+02 |
| 0.187800E+04 | 0.159343E+02 | 0.166363E+02 | 0.152323E+02 |
| 0.187900E+04 | 0.159366E+02 | 0.166388E+02 | 0.152344E+02 |
| 0.188000E+04 | 0.159410E+02 | 0.166438E+02 | 0.152381E+02 |
| 0.188100E+04 | 0.159479E+02 | 0.166518E+02 | 0.152439E+02 |
| 0.188200E+04 | 0.159551E+02 | 0.166604E+02 | 0.152499E+02 |
| 0.188300E+04 | 0.158264E+02 | 0.165173E+02 | 0.151354E+02 |
| 0.188400E+04 | 0.156808E+02 | 0.163508E+02 | 0.150108E+02 |
| 0.188500E+04 | 0.157272E+02 | 0.164051E+02 | 0.150493E+02 |
| 0.188600E+04 | 0.158103E+02 | 0.165011E+02 | 0.151194E+02 |
| 0.188700E+04 | 0.158508E+02 | 0.165480E+02 | 0.151536E+02 |
| 0.188800E+04 | 0.158733E+02 | 0.165744E+02 | 0.151723E+02 |
| 0.188900E+04 | 0.158892E+02 | 0.165927E+02 | 0.151857E+02 |
| 0.189000E+04 | 0.159020E+02 | 0.166073E+02 | 0.151966E+02 |
| 0.189100E+04 | 0.159153E+02 | 0.166225E+02 | 0.152080E+02 |
| 0.189200E+04 | 0.159293E+02 | 0.166385E+02 | 0.152201E+02 |
| 0.189300E+04 | 0.159425E+02 | 0.166535E+02 | 0.152315E+02 |
| 0.189400E+04 | 0.159549E+02 | 0.166675E+02 | 0.152423E+02 |
| 0.189500E+04 | 0.159644E+02 | 0.166781E+02 | 0.152507E+02 |
| 0.189600E+04 | 0.159692E+02 | 0.166831E+02 | 0.152552E+02 |
| 0.189700E+04 | 0.159704E+02 | 0.166840E+02 | 0.152568E+02 |
| 0.189800E+04 | 0.159709E+02 | 0.166841E+02 | 0.152576E+02 |

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| 0.189900E+04 | 0.159717E+02 | 0.166847E+02 | 0.152587E+02 |
| 0.190000E+04 | 0.159725E+02 | 0.166854E+02 | 0.152596E+02 |
| 0.190100E+04 | 0.159725E+02 | 0.166852E+02 | 0.152598E+02 |
| 0.190200E+04 | 0.158415E+02 | 0.165400E+02 | 0.151430E+02 |
| 0.190300E+04 | 0.156991E+02 | 0.163760E+02 | 0.150222E+02 |
| 0.190400E+04 | 0.157540E+02 | 0.164351E+02 | 0.150729E+02 |
| 0.190500E+04 | 0.158402E+02 | 0.165316E+02 | 0.151487E+02 |
| 0.190600E+04 | 0.158818E+02 | 0.165821E+02 | 0.151816E+02 |
| 0.190700E+04 | 0.158887E+02 | 0.165947E+02 | 0.151827E+02 |
| 0.190800E+04 | 0.158943E+02 | 0.166020E+02 | 0.151867E+02 |
| 0.190900E+04 | 0.159148E+02 | 0.166258E+02 | 0.152038E+02 |
| 0.191000E+04 | 0.159284E+02 | 0.166413E+02 | 0.152154E+02 |
| 0.191100E+04 | 0.159357E+02 | 0.166492E+02 | 0.152223E+02 |
| 0.191200E+04 | 0.158605E+02 | 0.165604E+02 | 0.151605E+02 |
| 0.191300E+04 | 0.157888E+02 | 0.164854E+02 | 0.150922E+02 |
| 0.191400E+04 | 0.157889E+02 | 0.164851E+02 | 0.150926E+02 |
| 0.191500E+04 | 0.158260E+02 | 0.165284E+02 | 0.151236E+02 |
| 0.191600E+04 | 0.158772E+02 | 0.165840E+02 | 0.151705E+02 |
| 0.191700E+04 | 0.159147E+02 | 0.166263E+02 | 0.152031E+02 |
| 0.191800E+04 | 0.159396E+02 | 0.166543E+02 | 0.152249E+02 |
| 0.191900E+04 | 0.159552E+02 | 0.166711E+02 | 0.152392E+02 |
| 0.192000E+04 | 0.159636E+02 | 0.166795E+02 | 0.152478E+02 |
| 0.192100E+04 | 0.159692E+02 | 0.166845E+02 | 0.152539E+02 |
| 0.192200E+04 | 0.159736E+02 | 0.166882E+02 | 0.152590E+02 |
| 0.192300E+04 | 0.159783E+02 | 0.166925E+02 | 0.152641E+02 |
| 0.192400E+04 | 0.159334E+02 | 0.166389E+02 | 0.152279E+02 |
| 0.192500E+04 | 0.158851E+02 | 0.165812E+02 | 0.151889E+02 |
| 0.192600E+04 | 0.159101E+02 | 0.166100E+02 | 0.152102E+02 |
| 0.192700E+04 | 0.159539E+02 | 0.166622E+02 | 0.152457E+02 |
| 0.192800E+04 | 0.159806E+02 | 0.166935E+02 | 0.152677E+02 |
| 0.192900E+04 | 0.159955E+02 | 0.167102E+02 | 0.152808E+02 |
| 0.193000E+04 | 0.160062E+02 | 0.167216E+02 | 0.152908E+02 |
| 0.193100E+04 | 0.160147E+02 | 0.167303E+02 | 0.152991E+02 |
| 0.193200E+04 | 0.160213E+02 | 0.167368E+02 | 0.153059E+02 |
| 0.193300E+04 | 0.160268E+02 | 0.167419E+02 | 0.153116E+02 |
| 0.193400E+04 | 0.160333E+02 | 0.167484E+02 | 0.153182E+02 |
| 0.193500E+04 | 0.160439E+02 | 0.167597E+02 | 0.153281E+02 |
| 0.193600E+04 | 0.160580E+02 | 0.167751E+02 | 0.153410E+02 |
| 0.193700E+04 | 0.160697E+02 | 0.167876E+02 | 0.153517E+02 |
| 0.193800E+04 | 0.160751E+02 | 0.167927E+02 | 0.153574E+02 |
| 0.193900E+04 | 0.160770E+02 | 0.167937E+02 | 0.153602E+02 |
| 0.194000E+04 | 0.160769E+02 | 0.167924E+02 | 0.153614E+02 |
| 0.194100E+04 | 0.160769E+02 | 0.167913E+02 | 0.153624E+02 |
| 0.194200E+04 | 0.160785E+02 | 0.167923E+02 | 0.153647E+02 |
| 0.194300E+04 | 0.160805E+02 | 0.167938E+02 | 0.153672E+02 |
| 0.194400E+04 | 0.160837E+02 | 0.167967E+02 | 0.153706E+02 |
| 0.194500E+04 | 0.160920E+02 | 0.168057E+02 | 0.153782E+02 |
| 0.194600E+04 | 0.161022E+02 | 0.168170E+02 | 0.153874E+02 |
| 0.194700E+04 | 0.161108E+02 | 0.168263E+02 | 0.153953E+02 |
| 0.194800E+04 | 0.161189E+02 | 0.168349E+02 | 0.154029E+02 |
| 0.194900E+04 | 0.161238E+02 | 0.168398E+02 | 0.154078E+02 |
| 0.195000E+04 | 0.161229E+02 | 0.168378E+02 | 0.154080E+02 |
| 0.195100E+04 | 0.161180E+02 | 0.168311E+02 | 0.154048E+02 |
| 0.195200E+04 | 0.161140E+02 | 0.168257E+02 | 0.154023E+02 |

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| 0.195300E+04 | 0.161118E+02 | 0.168225E+02 | 0.154010E+02 |
| 0.195400E+04 | 0.161107E+02 | 0.168208E+02 | 0.154007E+02 |
| 0.195500E+04 | 0.161130E+02 | 0.168231E+02 | 0.154030E+02 |
| 0.195600E+04 | 0.161058E+02 | 0.168143E+02 | 0.153973E+02 |
| 0.195700E+04 | 0.161086E+02 | 0.168173E+02 | 0.154000E+02 |
| 0.195800E+04 | 0.161306E+02 | 0.168428E+02 | 0.154185E+02 |
| 0.195900E+04 | 0.161432E+02 | 0.168573E+02 | 0.154291E+02 |
| 0.196000E+04 | 0.161478E+02 | 0.168621E+02 | 0.154336E+02 |
| 0.196100E+04 | 0.161340E+02 | 0.168452E+02 | 0.154228E+02 |
| 0.196200E+04 | 0.161102E+02 | 0.168165E+02 | 0.154038E+02 |
| 0.196300E+04 | 0.160845E+02 | 0.167860E+02 | 0.153829E+02 |
| 0.196400E+04 | 0.160362E+02 | 0.167254E+02 | 0.153470E+02 |
| 0.196500E+04 | 0.160004E+02 | 0.166951E+02 | 0.153057E+02 |
| 0.196600E+04 | 0.159936E+02 | 0.166874E+02 | 0.152999E+02 |
| 0.196700E+04 | 0.160167E+02 | 0.167148E+02 | 0.153186E+02 |
| 0.196800E+04 | 0.160272E+02 | 0.167277E+02 | 0.153266E+02 |
| 0.196900E+04 | 0.160098E+02 | 0.167080E+02 | 0.153116E+02 |
| 0.197000E+04 | 0.160114E+02 | 0.167100E+02 | 0.153129E+02 |
| 0.197100E+04 | 0.160408E+02 | 0.167411E+02 | 0.153406E+02 |
| 0.197200E+04 | 0.160739E+02 | 0.167794E+02 | 0.153683E+02 |
| 0.197300E+04 | 0.160961E+02 | 0.168053E+02 | 0.153870E+02 |
| 0.197400E+04 | 0.161001E+02 | 0.168092E+02 | 0.153910E+02 |
| 0.197500E+04 | 0.160780E+02 | 0.167823E+02 | 0.153737E+02 |
| 0.197600E+04 | 0.160646E+02 | 0.167656E+02 | 0.153636E+02 |
| 0.197700E+04 | 0.160929E+02 | 0.167982E+02 | 0.153875E+02 |
| 0.197800E+04 | 0.161319E+02 | 0.168438E+02 | 0.154199E+02 |
| 0.197900E+04 | 0.161637E+02 | 0.168806E+02 | 0.154469E+02 |
| 0.198000E+04 | 0.161950E+02 | 0.169161E+02 | 0.154739E+02 |
| 0.198100E+04 | 0.162291E+02 | 0.169546E+02 | 0.155036E+02 |
| 0.198200E+04 | 0.161686E+02 | 0.168823E+02 | 0.154548E+02 |
| 0.198300E+04 | 0.160240E+02 | 0.167153E+02 | 0.153327E+02 |
| 0.198400E+04 | 0.159701E+02 | 0.166542E+02 | 0.152859E+02 |
| 0.198500E+04 | 0.160376E+02 | 0.167323E+02 | 0.153428E+02 |
| 0.198600E+04 | 0.161041E+02 | 0.168078E+02 | 0.154005E+02 |
| 0.198700E+04 | 0.161489E+02 | 0.168612E+02 | 0.154366E+02 |
| 0.198800E+04 | 0.161941E+02 | 0.169136E+02 | 0.154746E+02 |
| 0.198900E+04 | 0.162427E+02 | 0.169686E+02 | 0.155168E+02 |
| 0.199000E+04 | 0.162786E+02 | 0.170029E+02 | 0.155543E+02 |
| 0.199100E+04 | 0.162497E+02 | 0.169631E+02 | 0.155364E+02 |
| 0.199200E+04 | 0.160918E+02 | 0.167727E+02 | 0.154109E+02 |
| 0.199300E+04 | 0.160097E+02 | 0.166791E+02 | 0.153402E+02 |
| 0.199400E+04 | 0.160749E+02 | 0.167535E+02 | 0.153963E+02 |
| 0.199500E+04 | 0.161539E+02 | 0.168396E+02 | 0.154683E+02 |
| 0.199600E+04 | 0.162162E+02 | 0.169094E+02 | 0.155229E+02 |
| 0.199700E+04 | 0.162681E+02 | 0.169693E+02 | 0.155668E+02 |
| 0.199800E+04 | 0.163186E+02 | 0.170273E+02 | 0.156099E+02 |

```
#          91
# IPCC AR4 Millenium Runs output (vary solar forcing)
# ++++++
#
# Model: Bern2.5CC version with active C-cycle
# -----
# Prescribed forcing timeseries as described in file
```

```
# readme_doRuns_IPCC_Chap6_millennium_21jan06.txt
# provided by F. Joos, University of Bern.
#
# Contact:
# -----
# Gian-Kasper Plattner
# Climate and Environmental Physics
# Physics Institute, University of Bern
# Sidlerstrasse 5, CH-3012 Bern, Switzerland
# plattner@climate.unibe.ch
# http://www.climate.unibe.ch/~plattner/
# tel: ++41 (0)31 631-44-67
# fax: ++41 (0)31 631-87-42
#
# Some model setup informations:
# -----
# All runs with horizontal/vertical diffusion
#
# Run with standard ocean parameters
#   as used in Plattner et al. 2001/2002
#   with Kv (diffusivity)  $4 \cdot 10^{-5}$  m2/s
#
# Climate sens. set to ~ 3.2 degrees C
# parameterized see Knutti et al. (Clim. Dyn. 2003)
#
# Model version is annual mean.
#
# No radiation code, CO2 radiative forcing calculated
# for as  $RF=5.35 \cdot \ln(CO2/CO2\_preind)$ ,
# Non-co2 radiative forcing prescribed according to
# Joos et al. GBC 2001 with updates for solar forcing
#
# More model description:
# -----
# Zonally averaged dynamical ocean with 3 basins and
# Southern Ocean, zonally averaged one layer energy
# and moisture balance atmosphere, thermodynamic
# sea ice (Stocker et al., J. Climate 1992).
#
# Carbon cycle components: Ocean/Atm/Terr.biosphere;
# Ocean carbon cycle is a description of the cycles
# of organic carbon and CaCO3 (Marchal et al., Tellus
# Tellus B), based on Redfield approach using PO4 as
# biolimiting nutrient.
#
# Land Biota: Lund-Jena-Postdam Dynamical Global
# Vegetation Model (LPJ-DGVM)
# at GCM resolution (Gerber et al. 2003, Climate
# Dynamics; Sitch et al. 2003, Global Change Biology)
#
# LPJ forced by Cramer/Leemans annual mean
# climatology plus interannual climate variability
# from Hadley simulation (30-recycled climate) plus
# changes in the fields of surface temperature,
```

```

# precipitation, and cloudcover as simulated with the
# Impulse-EOF version of ECHAM-3/LSG in response to
# projected radiative forcing changes.
#
# Land use changes are not explicitly considered.
#
# Impact of climate change on terrestrial C-storage
# included
#
# References:
# -----
# Carbon cycle Ocean: Marchal et al., Tellus 1998
# Carbon cycle Terr. Bio: Sitch et al., GCB 2003
#           Gerber et al., Clim. Dyn. 2003
# Ccycle-climate feedbacks and global warming:
#           Plattner et al., Tellus 2001
#           Plattner et al., GBC 2002
# Non-CO2 forcing: Joos et al., GBC 2001
# Climate model: Stocker et al., J. Climate 1992
# Sea level: Knutti et al., J. Climate 2000
# Global warming Physics: Knutti et al., Nature 2002
#           Knutti et al., Cl. Dyn. 2003
#           and refs therein.
#
# Output columns:
# -----
# Time (yr AD)
# Global mean air temperature (deg C)
# NH-averaged air temperature (deg C)
# SH-averaged air temperature (deg C)
0.100100E+04 0.159155E+02 0.165835E+02 0.152475E+02
0.100200E+04 0.159209E+02 0.165892E+02 0.152525E+02
0.100300E+04 0.159252E+02 0.165938E+02 0.152567E+02
0.100400E+04 0.158977E+02 0.165611E+02 0.152344E+02
0.100500E+04 0.158655E+02 0.165220E+02 0.152089E+02
0.100600E+04 0.158774E+02 0.165361E+02 0.152187E+02
0.100700E+04 0.158992E+02 0.165626E+02 0.152358E+02
0.100800E+04 0.159109E+02 0.165768E+02 0.152449E+02
0.100900E+04 0.159171E+02 0.165843E+02 0.152500E+02
0.101000E+04 0.159213E+02 0.165891E+02 0.152535E+02
0.101100E+04 0.159242E+02 0.165924E+02 0.152560E+02
0.101200E+04 0.159263E+02 0.165946E+02 0.152579E+02
0.101300E+04 0.159279E+02 0.165964E+02 0.152593E+02
0.101400E+04 0.159292E+02 0.165979E+02 0.152606E+02
0.101500E+04 0.158213E+02 0.164710E+02 0.151715E+02
0.101600E+04 0.157214E+02 0.163645E+02 0.150782E+02
0.101700E+04 0.157650E+02 0.164064E+02 0.151236E+02
0.101800E+04 0.158283E+02 0.164797E+02 0.151770E+02
0.101900E+04 0.158570E+02 0.165118E+02 0.152022E+02
0.102000E+04 0.158701E+02 0.165312E+02 0.152089E+02
0.102100E+04 0.158780E+02 0.165447E+02 0.152113E+02
0.102200E+04 0.158856E+02 0.165546E+02 0.152167E+02
0.102300E+04 0.158920E+02 0.165619E+02 0.152220E+02
0.102400E+04 0.158971E+02 0.165676E+02 0.152267E+02

```

| | | | |
|--------------|--------------|--------------|--------------|
| 0.102500E+04 | 0.159014E+02 | 0.165720E+02 | 0.152307E+02 |
| 0.102600E+04 | 0.157770E+02 | 0.164254E+02 | 0.151285E+02 |
| 0.102700E+04 | 0.156600E+02 | 0.162963E+02 | 0.150237E+02 |
| 0.102800E+04 | 0.157085E+02 | 0.163461E+02 | 0.150709E+02 |
| 0.102900E+04 | 0.157839E+02 | 0.164324E+02 | 0.151353E+02 |
| 0.103000E+04 | 0.158211E+02 | 0.164751E+02 | 0.151670E+02 |
| 0.103100E+04 | 0.158403E+02 | 0.164997E+02 | 0.151808E+02 |
| 0.103200E+04 | 0.158500E+02 | 0.165164E+02 | 0.151835E+02 |
| 0.103300E+04 | 0.158594E+02 | 0.165285E+02 | 0.151903E+02 |
| 0.103400E+04 | 0.158673E+02 | 0.165375E+02 | 0.151971E+02 |
| 0.103500E+04 | 0.158737E+02 | 0.165443E+02 | 0.152032E+02 |
| 0.103600E+04 | 0.158791E+02 | 0.165496E+02 | 0.152085E+02 |
| 0.103700E+04 | 0.158835E+02 | 0.165539E+02 | 0.152131E+02 |
| 0.103800E+04 | 0.158873E+02 | 0.165574E+02 | 0.152171E+02 |
| 0.103900E+04 | 0.158904E+02 | 0.165603E+02 | 0.152206E+02 |
| 0.104000E+04 | 0.158931E+02 | 0.165627E+02 | 0.152235E+02 |
| 0.104100E+04 | 0.158954E+02 | 0.165646E+02 | 0.152261E+02 |
| 0.104200E+04 | 0.158973E+02 | 0.165663E+02 | 0.152284E+02 |
| 0.104300E+04 | 0.158990E+02 | 0.165676E+02 | 0.152303E+02 |
| 0.104400E+04 | 0.159004E+02 | 0.165687E+02 | 0.152320E+02 |
| 0.104500E+04 | 0.159016E+02 | 0.165697E+02 | 0.152335E+02 |
| 0.104600E+04 | 0.159027E+02 | 0.165706E+02 | 0.152348E+02 |
| 0.104700E+04 | 0.159038E+02 | 0.165715E+02 | 0.152361E+02 |
| 0.104800E+04 | 0.159047E+02 | 0.165722E+02 | 0.152372E+02 |
| 0.104900E+04 | 0.159055E+02 | 0.165729E+02 | 0.152382E+02 |
| 0.105000E+04 | 0.159063E+02 | 0.165735E+02 | 0.152392E+02 |
| 0.105100E+04 | 0.159070E+02 | 0.165740E+02 | 0.152400E+02 |
| 0.105200E+04 | 0.159077E+02 | 0.165745E+02 | 0.152409E+02 |
| 0.105300E+04 | 0.159083E+02 | 0.165750E+02 | 0.152416E+02 |
| 0.105400E+04 | 0.159089E+02 | 0.165754E+02 | 0.152423E+02 |
| 0.105500E+04 | 0.159095E+02 | 0.165759E+02 | 0.152431E+02 |
| 0.105600E+04 | 0.159101E+02 | 0.165764E+02 | 0.152438E+02 |
| 0.105700E+04 | 0.159107E+02 | 0.165769E+02 | 0.152445E+02 |
| 0.105800E+04 | 0.157526E+02 | 0.163976E+02 | 0.151075E+02 |
| 0.105900E+04 | 0.155681E+02 | 0.161824E+02 | 0.149539E+02 |
| 0.106000E+04 | 0.157024E+02 | 0.162482E+02 | 0.151566E+02 |
| 0.106100E+04 | 0.158714E+02 | 0.163711E+02 | 0.153716E+02 |
| 0.106200E+04 | 0.159064E+02 | 0.163799E+02 | 0.154328E+02 |
| 0.106300E+04 | 0.158912E+02 | 0.163588E+02 | 0.154235E+02 |
| 0.106400E+04 | 0.159282E+02 | 0.164062E+02 | 0.154501E+02 |
| 0.106500E+04 | 0.159701E+02 | 0.164636E+02 | 0.154766E+02 |
| 0.106600E+04 | 0.159940E+02 | 0.164998E+02 | 0.154882E+02 |
| 0.106700E+04 | 0.160082E+02 | 0.165240E+02 | 0.154924E+02 |
| 0.106800E+04 | 0.160205E+02 | 0.165424E+02 | 0.154986E+02 |
| 0.106900E+04 | 0.160272E+02 | 0.165572E+02 | 0.154971E+02 |
| 0.107000E+04 | 0.160326E+02 | 0.165692E+02 | 0.154960E+02 |
| 0.107100E+04 | 0.160368E+02 | 0.165792E+02 | 0.154944E+02 |
| 0.107200E+04 | 0.160401E+02 | 0.165874E+02 | 0.154927E+02 |
| 0.107300E+04 | 0.160427E+02 | 0.165944E+02 | 0.154910E+02 |
| 0.107400E+04 | 0.160449E+02 | 0.166004E+02 | 0.154894E+02 |
| 0.107500E+04 | 0.160467E+02 | 0.166055E+02 | 0.154880E+02 |
| 0.107600E+04 | 0.160483E+02 | 0.166098E+02 | 0.154867E+02 |
| 0.107700E+04 | 0.160495E+02 | 0.166134E+02 | 0.154855E+02 |
| 0.107800E+04 | 0.160504E+02 | 0.166164E+02 | 0.154844E+02 |

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| 0.107900E+04 | 0.160513E+02 | 0.166191E+02 | 0.154835E+02 |
| 0.108000E+04 | 0.160218E+02 | 0.165851E+02 | 0.154584E+02 |
| 0.108100E+04 | 0.159894E+02 | 0.165469E+02 | 0.154319E+02 |
| 0.108200E+04 | 0.160000E+02 | 0.165607E+02 | 0.154393E+02 |
| 0.108300E+04 | 0.160202E+02 | 0.165871E+02 | 0.154533E+02 |
| 0.108400E+04 | 0.160222E+02 | 0.166017E+02 | 0.154427E+02 |
| 0.108500E+04 | 0.160174E+02 | 0.166096E+02 | 0.154252E+02 |
| 0.108600E+04 | 0.160121E+02 | 0.166147E+02 | 0.154095E+02 |
| 0.108700E+04 | 0.159660E+02 | 0.166182E+02 | 0.153138E+02 |
| 0.108800E+04 | 0.159316E+02 | 0.166197E+02 | 0.152435E+02 |
| 0.108900E+04 | 0.159111E+02 | 0.166198E+02 | 0.152025E+02 |
| 0.109000E+04 | 0.158969E+02 | 0.166188E+02 | 0.151750E+02 |
| 0.109100E+04 | 0.158874E+02 | 0.166175E+02 | 0.151574E+02 |
| 0.109200E+04 | 0.158810E+02 | 0.166159E+02 | 0.151460E+02 |
| 0.109300E+04 | 0.158765E+02 | 0.166143E+02 | 0.151387E+02 |
| 0.109400E+04 | 0.158754E+02 | 0.166129E+02 | 0.151380E+02 |
| 0.109500E+04 | 0.158763E+02 | 0.166119E+02 | 0.151407E+02 |
| 0.109600E+04 | 0.158786E+02 | 0.166114E+02 | 0.151459E+02 |
| 0.109700E+04 | 0.158099E+02 | 0.165273E+02 | 0.150926E+02 |
| 0.109800E+04 | 0.157483E+02 | 0.164610E+02 | 0.150355E+02 |
| 0.109900E+04 | 0.157746E+02 | 0.164875E+02 | 0.150618E+02 |
| 0.110000E+04 | 0.158230E+02 | 0.165395E+02 | 0.151065E+02 |
| 0.110100E+04 | 0.158454E+02 | 0.165578E+02 | 0.151331E+02 |
| 0.110200E+04 | 0.158613E+02 | 0.165715E+02 | 0.151512E+02 |
| 0.110300E+04 | 0.158743E+02 | 0.165824E+02 | 0.151662E+02 |
| 0.110400E+04 | 0.158852E+02 | 0.165912E+02 | 0.151792E+02 |
| 0.110500E+04 | 0.158948E+02 | 0.165988E+02 | 0.151909E+02 |
| 0.110600E+04 | 0.159034E+02 | 0.166054E+02 | 0.152014E+02 |
| 0.110700E+04 | 0.159111E+02 | 0.166114E+02 | 0.152108E+02 |
| 0.110800E+04 | 0.159182E+02 | 0.166169E+02 | 0.152195E+02 |
| 0.110900E+04 | 0.159249E+02 | 0.166223E+02 | 0.152274E+02 |
| 0.111000E+04 | 0.159314E+02 | 0.166278E+02 | 0.152350E+02 |
| 0.111100E+04 | 0.159377E+02 | 0.166333E+02 | 0.152421E+02 |
| 0.111200E+04 | 0.159437E+02 | 0.166387E+02 | 0.152487E+02 |
| 0.111300E+04 | 0.159495E+02 | 0.166441E+02 | 0.152550E+02 |
| 0.111400E+04 | 0.159551E+02 | 0.166494E+02 | 0.152609E+02 |
| 0.111500E+04 | 0.159603E+02 | 0.166543E+02 | 0.152663E+02 |
| 0.111600E+04 | 0.159649E+02 | 0.166588E+02 | 0.152710E+02 |
| 0.111700E+04 | 0.159691E+02 | 0.166628E+02 | 0.152753E+02 |
| 0.111800E+04 | 0.159729E+02 | 0.166667E+02 | 0.152792E+02 |
| 0.111900E+04 | 0.159765E+02 | 0.166703E+02 | 0.152828E+02 |
| 0.112000E+04 | 0.159799E+02 | 0.166737E+02 | 0.152861E+02 |
| 0.112100E+04 | 0.159831E+02 | 0.166770E+02 | 0.152891E+02 |
| 0.112200E+04 | 0.159861E+02 | 0.166802E+02 | 0.152919E+02 |
| 0.112300E+04 | 0.159889E+02 | 0.166833E+02 | 0.152946E+02 |
| 0.112400E+04 | 0.159916E+02 | 0.166862E+02 | 0.152970E+02 |
| 0.112500E+04 | 0.159942E+02 | 0.166891E+02 | 0.152993E+02 |
| 0.112600E+04 | 0.159967E+02 | 0.166919E+02 | 0.153015E+02 |
| 0.112700E+04 | 0.159991E+02 | 0.166946E+02 | 0.153036E+02 |
| 0.112800E+04 | 0.160014E+02 | 0.166972E+02 | 0.153055E+02 |
| 0.112900E+04 | 0.160036E+02 | 0.166997E+02 | 0.153074E+02 |
| 0.113000E+04 | 0.160057E+02 | 0.167022E+02 | 0.153092E+02 |
| 0.113100E+04 | 0.160078E+02 | 0.167046E+02 | 0.153109E+02 |
| 0.113200E+04 | 0.160097E+02 | 0.167069E+02 | 0.153126E+02 |

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| 0.113300E+04 | 0.160118E+02 | 0.167093E+02 | 0.153142E+02 |
| 0.113400E+04 | 0.160140E+02 | 0.167119E+02 | 0.153160E+02 |
| 0.113500E+04 | 0.160161E+02 | 0.167144E+02 | 0.153178E+02 |
| 0.113600E+04 | 0.160182E+02 | 0.167169E+02 | 0.153195E+02 |
| 0.113700E+04 | 0.160202E+02 | 0.167192E+02 | 0.153212E+02 |
| 0.113800E+04 | 0.160222E+02 | 0.167215E+02 | 0.153228E+02 |
| 0.113900E+04 | 0.160240E+02 | 0.167236E+02 | 0.153244E+02 |
| 0.114000E+04 | 0.160256E+02 | 0.167255E+02 | 0.153258E+02 |
| 0.114100E+04 | 0.160271E+02 | 0.167272E+02 | 0.153270E+02 |
| 0.114200E+04 | 0.160285E+02 | 0.167288E+02 | 0.153282E+02 |
| 0.114300E+04 | 0.160298E+02 | 0.167303E+02 | 0.153294E+02 |
| 0.114400E+04 | 0.160311E+02 | 0.167317E+02 | 0.153304E+02 |
| 0.114500E+04 | 0.160322E+02 | 0.167330E+02 | 0.153315E+02 |
| 0.114600E+04 | 0.160333E+02 | 0.167342E+02 | 0.153325E+02 |
| 0.114700E+04 | 0.160343E+02 | 0.167353E+02 | 0.153333E+02 |
| 0.114800E+04 | 0.160351E+02 | 0.167361E+02 | 0.153341E+02 |
| 0.114900E+04 | 0.160358E+02 | 0.167368E+02 | 0.153347E+02 |
| 0.115000E+04 | 0.160363E+02 | 0.167373E+02 | 0.153353E+02 |
| 0.115100E+04 | 0.160368E+02 | 0.167377E+02 | 0.153358E+02 |
| 0.115200E+04 | 0.160372E+02 | 0.167381E+02 | 0.153363E+02 |
| 0.115300E+04 | 0.160375E+02 | 0.167383E+02 | 0.153366E+02 |
| 0.115400E+04 | 0.160377E+02 | 0.167385E+02 | 0.153370E+02 |
| 0.115500E+04 | 0.160379E+02 | 0.167386E+02 | 0.153373E+02 |
| 0.115600E+04 | 0.160380E+02 | 0.167385E+02 | 0.153375E+02 |
| 0.115700E+04 | 0.160379E+02 | 0.167382E+02 | 0.153376E+02 |
| 0.115800E+04 | 0.160376E+02 | 0.167377E+02 | 0.153375E+02 |
| 0.115900E+04 | 0.160372E+02 | 0.167371E+02 | 0.153373E+02 |
| 0.116000E+04 | 0.160367E+02 | 0.167363E+02 | 0.153371E+02 |
| 0.116100E+04 | 0.160362E+02 | 0.167355E+02 | 0.153368E+02 |
| 0.116200E+04 | 0.160358E+02 | 0.167349E+02 | 0.153367E+02 |
| 0.116300E+04 | 0.160357E+02 | 0.167346E+02 | 0.153368E+02 |
| 0.116400E+04 | 0.160358E+02 | 0.167345E+02 | 0.153370E+02 |
| 0.116500E+04 | 0.160359E+02 | 0.167345E+02 | 0.153374E+02 |
| 0.116600E+04 | 0.159930E+02 | 0.166837E+02 | 0.153022E+02 |
| 0.116700E+04 | 0.159464E+02 | 0.166292E+02 | 0.152636E+02 |
| 0.116800E+04 | 0.159600E+02 | 0.166457E+02 | 0.152742E+02 |
| 0.116900E+04 | 0.159866E+02 | 0.166781E+02 | 0.152951E+02 |
| 0.117000E+04 | 0.160004E+02 | 0.166949E+02 | 0.153060E+02 |
| 0.117100E+04 | 0.160074E+02 | 0.167030E+02 | 0.153119E+02 |
| 0.117200E+04 | 0.160119E+02 | 0.167079E+02 | 0.153159E+02 |
| 0.117300E+04 | 0.160150E+02 | 0.167111E+02 | 0.153189E+02 |
| 0.117400E+04 | 0.160172E+02 | 0.167132E+02 | 0.153212E+02 |
| 0.117500E+04 | 0.157641E+02 | 0.164281E+02 | 0.151002E+02 |
| 0.117600E+04 | 0.156187E+02 | 0.161368E+02 | 0.151005E+02 |
| 0.117700E+04 | 0.157262E+02 | 0.162059E+02 | 0.152466E+02 |
| 0.117800E+04 | 0.158665E+02 | 0.163478E+02 | 0.153853E+02 |
| 0.117900E+04 | 0.159583E+02 | 0.164437E+02 | 0.154728E+02 |
| 0.118000E+04 | 0.160115E+02 | 0.165076E+02 | 0.155154E+02 |
| 0.118100E+04 | 0.160423E+02 | 0.165479E+02 | 0.155367E+02 |
| 0.118200E+04 | 0.160629E+02 | 0.165776E+02 | 0.155483E+02 |
| 0.118300E+04 | 0.160774E+02 | 0.166006E+02 | 0.155543E+02 |
| 0.118400E+04 | 0.160901E+02 | 0.166188E+02 | 0.155613E+02 |
| 0.118500E+04 | 0.160976E+02 | 0.166333E+02 | 0.155619E+02 |
| 0.118600E+04 | 0.161031E+02 | 0.166448E+02 | 0.155615E+02 |

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| 0.118700E+04 | 0.161071E+02 | 0.166539E+02 | 0.155602E+02 |
| 0.118800E+04 | 0.161099E+02 | 0.166612E+02 | 0.155586E+02 |
| 0.118900E+04 | 0.161334E+02 | 0.167069E+02 | 0.155598E+02 |
| 0.119000E+04 | 0.161327E+02 | 0.167065E+02 | 0.155588E+02 |
| 0.119100E+04 | 0.161322E+02 | 0.167074E+02 | 0.155571E+02 |
| 0.119200E+04 | 0.161319E+02 | 0.167084E+02 | 0.155553E+02 |
| 0.119300E+04 | 0.161240E+02 | 0.167094E+02 | 0.155386E+02 |
| 0.119400E+04 | 0.160214E+02 | 0.166002E+02 | 0.154426E+02 |
| 0.119500E+04 | 0.159266E+02 | 0.165069E+02 | 0.153463E+02 |
| 0.119600E+04 | 0.159470E+02 | 0.165403E+02 | 0.153538E+02 |
| 0.119700E+04 | 0.159938E+02 | 0.166044E+02 | 0.153832E+02 |
| 0.119800E+04 | 0.159733E+02 | 0.166301E+02 | 0.153165E+02 |
| 0.119900E+04 | 0.159482E+02 | 0.166444E+02 | 0.152520E+02 |
| 0.120000E+04 | 0.159354E+02 | 0.166526E+02 | 0.152182E+02 |
| 0.120100E+04 | 0.159268E+02 | 0.166569E+02 | 0.151968E+02 |
| 0.120200E+04 | 0.159216E+02 | 0.166589E+02 | 0.151843E+02 |
| 0.120300E+04 | 0.159191E+02 | 0.166595E+02 | 0.151786E+02 |
| 0.120400E+04 | 0.159166E+02 | 0.166591E+02 | 0.151741E+02 |
| 0.120500E+04 | 0.158664E+02 | 0.165991E+02 | 0.151337E+02 |
| 0.120600E+04 | 0.158113E+02 | 0.165312E+02 | 0.150915E+02 |
| 0.120700E+04 | 0.158427E+02 | 0.165735E+02 | 0.151118E+02 |
| 0.120800E+04 | 0.158776E+02 | 0.166097E+02 | 0.151455E+02 |
| 0.120900E+04 | 0.158981E+02 | 0.166288E+02 | 0.151674E+02 |
| 0.121000E+04 | 0.159110E+02 | 0.166387E+02 | 0.151834E+02 |
| 0.121100E+04 | 0.159211E+02 | 0.166451E+02 | 0.151971E+02 |
| 0.121200E+04 | 0.159295E+02 | 0.166497E+02 | 0.152093E+02 |
| 0.121300E+04 | 0.159368E+02 | 0.166532E+02 | 0.152203E+02 |
| 0.121400E+04 | 0.159412E+02 | 0.166523E+02 | 0.152302E+02 |
| 0.121500E+04 | 0.159417E+02 | 0.166451E+02 | 0.152383E+02 |
| 0.121600E+04 | 0.159454E+02 | 0.166451E+02 | 0.152458E+02 |
| 0.121700E+04 | 0.159497E+02 | 0.166466E+02 | 0.152527E+02 |
| 0.121800E+04 | 0.159538E+02 | 0.166485E+02 | 0.152591E+02 |
| 0.121900E+04 | 0.159578E+02 | 0.166506E+02 | 0.152649E+02 |
| 0.122000E+04 | 0.159615E+02 | 0.166528E+02 | 0.152702E+02 |
| 0.122100E+04 | 0.159650E+02 | 0.166550E+02 | 0.152751E+02 |
| 0.122200E+04 | 0.159684E+02 | 0.166573E+02 | 0.152794E+02 |
| 0.122300E+04 | 0.159715E+02 | 0.166595E+02 | 0.152834E+02 |
| 0.122400E+04 | 0.159744E+02 | 0.166618E+02 | 0.152871E+02 |
| 0.122500E+04 | 0.159772E+02 | 0.166640E+02 | 0.152904E+02 |
| 0.122600E+04 | 0.159798E+02 | 0.166662E+02 | 0.152934E+02 |
| 0.122700E+04 | 0.159512E+02 | 0.166321E+02 | 0.152703E+02 |
| 0.122800E+04 | 0.159185E+02 | 0.165928E+02 | 0.152442E+02 |
| 0.122900E+04 | 0.157488E+02 | 0.164060E+02 | 0.150916E+02 |
| 0.123000E+04 | 0.155640E+02 | 0.161868E+02 | 0.149411E+02 |
| 0.123100E+04 | 0.157756E+02 | 0.162876E+02 | 0.152636E+02 |
| 0.123200E+04 | 0.159056E+02 | 0.163980E+02 | 0.154133E+02 |
| 0.123300E+04 | 0.159822E+02 | 0.164718E+02 | 0.154927E+02 |
| 0.123400E+04 | 0.160216E+02 | 0.165128E+02 | 0.155304E+02 |
| 0.123500E+04 | 0.160469E+02 | 0.165448E+02 | 0.155489E+02 |
| 0.123600E+04 | 0.160638E+02 | 0.165696E+02 | 0.155579E+02 |
| 0.123700E+04 | 0.160753E+02 | 0.165892E+02 | 0.155615E+02 |
| 0.123800E+04 | 0.160834E+02 | 0.166051E+02 | 0.155618E+02 |
| 0.123900E+04 | 0.160918E+02 | 0.166181E+02 | 0.155654E+02 |
| 0.124000E+04 | 0.160957E+02 | 0.166290E+02 | 0.155623E+02 |

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| 0.124100E+04 | 0.160987E+02 | 0.166380E+02 | 0.155594E+02 |
| 0.124200E+04 | 0.161230E+02 | 0.166886E+02 | 0.155574E+02 |
| 0.124300E+04 | 0.161230E+02 | 0.166895E+02 | 0.155566E+02 |
| 0.124400E+04 | 0.161223E+02 | 0.166907E+02 | 0.155539E+02 |
| 0.124500E+04 | 0.161217E+02 | 0.166926E+02 | 0.155509E+02 |
| 0.124600E+04 | 0.161211E+02 | 0.166942E+02 | 0.155479E+02 |
| 0.124700E+04 | 0.161202E+02 | 0.166954E+02 | 0.155449E+02 |
| 0.124800E+04 | 0.161119E+02 | 0.166962E+02 | 0.155277E+02 |
| 0.124900E+04 | 0.161002E+02 | 0.166964E+02 | 0.155039E+02 |
| 0.125000E+04 | 0.160896E+02 | 0.166961E+02 | 0.154831E+02 |
| 0.125100E+04 | 0.160803E+02 | 0.166953E+02 | 0.154653E+02 |
| 0.125200E+04 | 0.160221E+02 | 0.166938E+02 | 0.153503E+02 |
| 0.125300E+04 | 0.159889E+02 | 0.166911E+02 | 0.152866E+02 |
| 0.125400E+04 | 0.159666E+02 | 0.166870E+02 | 0.152462E+02 |
| 0.125500E+04 | 0.159499E+02 | 0.166818E+02 | 0.152181E+02 |
| 0.125600E+04 | 0.159376E+02 | 0.166759E+02 | 0.151992E+02 |
| 0.125700E+04 | 0.159286E+02 | 0.166699E+02 | 0.151873E+02 |
| 0.125800E+04 | 0.159206E+02 | 0.166642E+02 | 0.151771E+02 |
| 0.125900E+04 | 0.154807E+02 | 0.161494E+02 | 0.148121E+02 |
| 0.126000E+04 | 0.152572E+02 | 0.160683E+02 | 0.144461E+02 |
| 0.126100E+04 | 0.154588E+02 | 0.163821E+02 | 0.145355E+02 |
| 0.126200E+04 | 0.156558E+02 | 0.165738E+02 | 0.147379E+02 |
| 0.126300E+04 | 0.157923E+02 | 0.166953E+02 | 0.148893E+02 |
| 0.126400E+04 | 0.158568E+02 | 0.167406E+02 | 0.149730E+02 |
| 0.126500E+04 | 0.158900E+02 | 0.167545E+02 | 0.150255E+02 |
| 0.126600E+04 | 0.159115E+02 | 0.167589E+02 | 0.150640E+02 |
| 0.126700E+04 | 0.159227E+02 | 0.167575E+02 | 0.150880E+02 |
| 0.126800E+04 | 0.159307E+02 | 0.167536E+02 | 0.151077E+02 |
| 0.126900E+04 | 0.159367E+02 | 0.167486E+02 | 0.151247E+02 |
| 0.127000E+04 | 0.159415E+02 | 0.167434E+02 | 0.151395E+02 |
| 0.127100E+04 | 0.159453E+02 | 0.167383E+02 | 0.151523E+02 |
| 0.127200E+04 | 0.159485E+02 | 0.167336E+02 | 0.151634E+02 |
| 0.127300E+04 | 0.159510E+02 | 0.167292E+02 | 0.151729E+02 |
| 0.127400E+04 | 0.159530E+02 | 0.167251E+02 | 0.151809E+02 |
| 0.127500E+04 | 0.158544E+02 | 0.166053E+02 | 0.151035E+02 |
| 0.127600E+04 | 0.157431E+02 | 0.164816E+02 | 0.150046E+02 |
| 0.127700E+04 | 0.157788E+02 | 0.165214E+02 | 0.150362E+02 |
| 0.127800E+04 | 0.158412E+02 | 0.165867E+02 | 0.150956E+02 |
| 0.127900E+04 | 0.158710E+02 | 0.166153E+02 | 0.151268E+02 |
| 0.128000E+04 | 0.158755E+02 | 0.166076E+02 | 0.151434E+02 |
| 0.128100E+04 | 0.158771E+02 | 0.165999E+02 | 0.151544E+02 |
| 0.128200E+04 | 0.158361E+02 | 0.165112E+02 | 0.151610E+02 |
| 0.128300E+04 | 0.158180E+02 | 0.164730E+02 | 0.151631E+02 |
| 0.128400E+04 | 0.158099E+02 | 0.164557E+02 | 0.151641E+02 |
| 0.128500E+04 | 0.156680E+02 | 0.162921E+02 | 0.150440E+02 |
| 0.128600E+04 | 0.155212E+02 | 0.161200E+02 | 0.149223E+02 |
| 0.128700E+04 | 0.156037E+02 | 0.162401E+02 | 0.149673E+02 |
| 0.128800E+04 | 0.158194E+02 | 0.163373E+02 | 0.153015E+02 |
| 0.128900E+04 | 0.158806E+02 | 0.163862E+02 | 0.153750E+02 |
| 0.129000E+04 | 0.159281E+02 | 0.164155E+02 | 0.154407E+02 |
| 0.129100E+04 | 0.159485E+02 | 0.164389E+02 | 0.154580E+02 |
| 0.129200E+04 | 0.159630E+02 | 0.164605E+02 | 0.154654E+02 |
| 0.129300E+04 | 0.159730E+02 | 0.164790E+02 | 0.154671E+02 |
| 0.129400E+04 | 0.159836E+02 | 0.164952E+02 | 0.154720E+02 |

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| 0.129500E+04 | 0.158516E+02 | 0.163463E+02 | 0.153569E+02 |
| 0.129600E+04 | 0.157273E+02 | 0.162135E+02 | 0.152410E+02 |
| 0.129700E+04 | 0.157762E+02 | 0.162788E+02 | 0.152737E+02 |
| 0.129800E+04 | 0.158534E+02 | 0.163750E+02 | 0.153318E+02 |
| 0.129900E+04 | 0.158826E+02 | 0.164088E+02 | 0.153563E+02 |
| 0.130000E+04 | 0.159022E+02 | 0.164376E+02 | 0.153668E+02 |
| 0.130100E+04 | 0.159166E+02 | 0.164602E+02 | 0.153729E+02 |
| 0.130200E+04 | 0.159277E+02 | 0.164786E+02 | 0.153768E+02 |
| 0.130300E+04 | 0.159365E+02 | 0.164937E+02 | 0.153794E+02 |
| 0.130400E+04 | 0.159437E+02 | 0.165063E+02 | 0.153812E+02 |
| 0.130500E+04 | 0.159498E+02 | 0.165171E+02 | 0.153825E+02 |
| 0.130600E+04 | 0.159550E+02 | 0.165265E+02 | 0.153835E+02 |
| 0.130700E+04 | 0.159595E+02 | 0.165348E+02 | 0.153843E+02 |
| 0.130800E+04 | 0.159634E+02 | 0.165420E+02 | 0.153849E+02 |
| 0.130900E+04 | 0.159669E+02 | 0.165483E+02 | 0.153854E+02 |
| 0.131000E+04 | 0.159925E+02 | 0.165984E+02 | 0.153867E+02 |
| 0.131100E+04 | 0.159960E+02 | 0.166017E+02 | 0.153903E+02 |
| 0.131200E+04 | 0.159968E+02 | 0.166020E+02 | 0.153916E+02 |
| 0.131300E+04 | 0.159915E+02 | 0.166036E+02 | 0.153793E+02 |
| 0.131400E+04 | 0.159816E+02 | 0.166054E+02 | 0.153578E+02 |
| 0.131500E+04 | 0.159730E+02 | 0.166070E+02 | 0.153391E+02 |
| 0.131600E+04 | 0.159365E+02 | 0.166080E+02 | 0.152649E+02 |
| 0.131700E+04 | 0.158919E+02 | 0.166079E+02 | 0.151758E+02 |
| 0.131800E+04 | 0.158676E+02 | 0.166061E+02 | 0.151291E+02 |
| 0.131900E+04 | 0.158506E+02 | 0.166035E+02 | 0.150976E+02 |
| 0.132000E+04 | 0.158387E+02 | 0.166006E+02 | 0.150767E+02 |
| 0.132100E+04 | 0.158288E+02 | 0.165975E+02 | 0.150601E+02 |
| 0.132200E+04 | 0.158235E+02 | 0.165944E+02 | 0.150526E+02 |
| 0.132300E+04 | 0.158210E+02 | 0.165916E+02 | 0.150504E+02 |
| 0.132400E+04 | 0.158207E+02 | 0.165893E+02 | 0.150520E+02 |
| 0.132500E+04 | 0.158220E+02 | 0.165876E+02 | 0.150563E+02 |
| 0.132600E+04 | 0.158244E+02 | 0.165863E+02 | 0.150624E+02 |
| 0.132700E+04 | 0.158276E+02 | 0.165856E+02 | 0.150696E+02 |
| 0.132800E+04 | 0.158312E+02 | 0.165849E+02 | 0.150774E+02 |
| 0.132900E+04 | 0.157124E+02 | 0.164412E+02 | 0.149837E+02 |
| 0.133000E+04 | 0.156000E+02 | 0.163193E+02 | 0.148807E+02 |
| 0.133100E+04 | 0.156443E+02 | 0.163665E+02 | 0.149222E+02 |
| 0.133200E+04 | 0.157169E+02 | 0.164435E+02 | 0.149904E+02 |
| 0.133300E+04 | 0.157558E+02 | 0.164836E+02 | 0.150280E+02 |
| 0.133400E+04 | 0.157793E+02 | 0.165071E+02 | 0.150516E+02 |
| 0.133500E+04 | 0.157970E+02 | 0.165239E+02 | 0.150701E+02 |
| 0.133600E+04 | 0.158117E+02 | 0.165374E+02 | 0.150860E+02 |
| 0.133700E+04 | 0.158245E+02 | 0.165490E+02 | 0.151000E+02 |
| 0.133800E+04 | 0.158357E+02 | 0.165590E+02 | 0.151124E+02 |
| 0.133900E+04 | 0.158458E+02 | 0.165679E+02 | 0.151236E+02 |
| 0.134000E+04 | 0.158546E+02 | 0.165757E+02 | 0.151335E+02 |
| 0.134100E+04 | 0.158623E+02 | 0.165823E+02 | 0.151422E+02 |
| 0.134200E+04 | 0.158690E+02 | 0.165880E+02 | 0.151499E+02 |
| 0.134300E+04 | 0.158749E+02 | 0.165931E+02 | 0.151568E+02 |
| 0.134400E+04 | 0.158803E+02 | 0.165976E+02 | 0.151630E+02 |
| 0.134500E+04 | 0.157810E+02 | 0.164796E+02 | 0.150824E+02 |
| 0.134600E+04 | 0.156860E+02 | 0.163782E+02 | 0.149939E+02 |
| 0.134700E+04 | 0.157263E+02 | 0.164183E+02 | 0.150343E+02 |
| 0.134800E+04 | 0.157903E+02 | 0.164873E+02 | 0.150932E+02 |

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| 0.134900E+04 | 0.158240E+02 | 0.165247E+02 | 0.151232E+02 |
| 0.135000E+04 | 0.158390E+02 | 0.165469E+02 | 0.151311E+02 |
| 0.135100E+04 | 0.158509E+02 | 0.165625E+02 | 0.151392E+02 |
| 0.135200E+04 | 0.158613E+02 | 0.165743E+02 | 0.151483E+02 |
| 0.135300E+04 | 0.158701E+02 | 0.165835E+02 | 0.151567E+02 |
| 0.135400E+04 | 0.158775E+02 | 0.165910E+02 | 0.151640E+02 |
| 0.135500E+04 | 0.158838E+02 | 0.165971E+02 | 0.151705E+02 |
| 0.135600E+04 | 0.158893E+02 | 0.166023E+02 | 0.151763E+02 |
| 0.135700E+04 | 0.158942E+02 | 0.166068E+02 | 0.151815E+02 |
| 0.135800E+04 | 0.158986E+02 | 0.166109E+02 | 0.151863E+02 |
| 0.135900E+04 | 0.159026E+02 | 0.166145E+02 | 0.151906E+02 |
| 0.136000E+04 | 0.159062E+02 | 0.166178E+02 | 0.151946E+02 |
| 0.136100E+04 | 0.159095E+02 | 0.166209E+02 | 0.151982E+02 |
| 0.136200E+04 | 0.159126E+02 | 0.166236E+02 | 0.152016E+02 |
| 0.136300E+04 | 0.159155E+02 | 0.166262E+02 | 0.152047E+02 |
| 0.136400E+04 | 0.159181E+02 | 0.166286E+02 | 0.152076E+02 |
| 0.136500E+04 | 0.159206E+02 | 0.166308E+02 | 0.152104E+02 |
| 0.136600E+04 | 0.159229E+02 | 0.166328E+02 | 0.152130E+02 |
| 0.136700E+04 | 0.159251E+02 | 0.166348E+02 | 0.152155E+02 |
| 0.136800E+04 | 0.159272E+02 | 0.166366E+02 | 0.152178E+02 |
| 0.136900E+04 | 0.159292E+02 | 0.166383E+02 | 0.152201E+02 |
| 0.137000E+04 | 0.159311E+02 | 0.166400E+02 | 0.152223E+02 |
| 0.137100E+04 | 0.159330E+02 | 0.166415E+02 | 0.152244E+02 |
| 0.137200E+04 | 0.159347E+02 | 0.166430E+02 | 0.152265E+02 |
| 0.137300E+04 | 0.159364E+02 | 0.166444E+02 | 0.152285E+02 |
| 0.137400E+04 | 0.159380E+02 | 0.166457E+02 | 0.152303E+02 |
| 0.137500E+04 | 0.158966E+02 | 0.165967E+02 | 0.151964E+02 |
| 0.137600E+04 | 0.158502E+02 | 0.165415E+02 | 0.151589E+02 |
| 0.137700E+04 | 0.158650E+02 | 0.165592E+02 | 0.151707E+02 |
| 0.137800E+04 | 0.158934E+02 | 0.165937E+02 | 0.151931E+02 |
| 0.137900E+04 | 0.159088E+02 | 0.166121E+02 | 0.152055E+02 |
| 0.138000E+04 | 0.159172E+02 | 0.166217E+02 | 0.152128E+02 |
| 0.138100E+04 | 0.159231E+02 | 0.166279E+02 | 0.152182E+02 |
| 0.138200E+04 | 0.159275E+02 | 0.166324E+02 | 0.152227E+02 |
| 0.138300E+04 | 0.159311E+02 | 0.166357E+02 | 0.152264E+02 |
| 0.138400E+04 | 0.159339E+02 | 0.166382E+02 | 0.152295E+02 |
| 0.138500E+04 | 0.159361E+02 | 0.166400E+02 | 0.152322E+02 |
| 0.138600E+04 | 0.159378E+02 | 0.166412E+02 | 0.152344E+02 |
| 0.138700E+04 | 0.158964E+02 | 0.165921E+02 | 0.152007E+02 |
| 0.138800E+04 | 0.158500E+02 | 0.165366E+02 | 0.151633E+02 |
| 0.138900E+04 | 0.158645E+02 | 0.165539E+02 | 0.151751E+02 |
| 0.139000E+04 | 0.158926E+02 | 0.165878E+02 | 0.151973E+02 |
| 0.139100E+04 | 0.159075E+02 | 0.166056E+02 | 0.152094E+02 |
| 0.139200E+04 | 0.159154E+02 | 0.166145E+02 | 0.152164E+02 |
| 0.139300E+04 | 0.159206E+02 | 0.166199E+02 | 0.152213E+02 |
| 0.139400E+04 | 0.159242E+02 | 0.166233E+02 | 0.152250E+02 |
| 0.139500E+04 | 0.159268E+02 | 0.166256E+02 | 0.152280E+02 |
| 0.139600E+04 | 0.159285E+02 | 0.166268E+02 | 0.152302E+02 |
| 0.139700E+04 | 0.159293E+02 | 0.166270E+02 | 0.152317E+02 |
| 0.139800E+04 | 0.159296E+02 | 0.166266E+02 | 0.152327E+02 |
| 0.139900E+04 | 0.159295E+02 | 0.166257E+02 | 0.152332E+02 |
| 0.140000E+04 | 0.159290E+02 | 0.166246E+02 | 0.152335E+02 |
| 0.140100E+04 | 0.159285E+02 | 0.166234E+02 | 0.152336E+02 |
| 0.140200E+04 | 0.159281E+02 | 0.166224E+02 | 0.152338E+02 |

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| 0.140300E+04 | 0.159278E+02 | 0.166215E+02 | 0.152341E+02 |
| 0.140400E+04 | 0.159274E+02 | 0.166206E+02 | 0.152342E+02 |
| 0.140500E+04 | 0.159270E+02 | 0.166197E+02 | 0.152344E+02 |
| 0.140600E+04 | 0.159266E+02 | 0.166188E+02 | 0.152345E+02 |
| 0.140700E+04 | 0.159262E+02 | 0.166178E+02 | 0.152345E+02 |
| 0.140800E+04 | 0.158868E+02 | 0.165715E+02 | 0.152022E+02 |
| 0.140900E+04 | 0.158420E+02 | 0.165183E+02 | 0.151656E+02 |
| 0.141000E+04 | 0.158664E+02 | 0.165566E+02 | 0.151763E+02 |
| 0.141100E+04 | 0.158905E+02 | 0.165843E+02 | 0.151967E+02 |
| 0.141200E+04 | 0.159030E+02 | 0.165986E+02 | 0.152075E+02 |
| 0.141300E+04 | 0.159094E+02 | 0.166055E+02 | 0.152134E+02 |
| 0.141400E+04 | 0.159135E+02 | 0.166096E+02 | 0.152175E+02 |
| 0.141500E+04 | 0.159161E+02 | 0.166118E+02 | 0.152204E+02 |
| 0.141600E+04 | 0.159174E+02 | 0.166124E+02 | 0.152223E+02 |
| 0.141700E+04 | 0.159178E+02 | 0.166121E+02 | 0.152234E+02 |
| 0.141800E+04 | 0.159176E+02 | 0.166111E+02 | 0.152240E+02 |
| 0.141900E+04 | 0.159169E+02 | 0.166096E+02 | 0.152241E+02 |
| 0.142000E+04 | 0.159159E+02 | 0.166079E+02 | 0.152239E+02 |
| 0.142100E+04 | 0.159149E+02 | 0.166062E+02 | 0.152236E+02 |
| 0.142200E+04 | 0.159140E+02 | 0.166047E+02 | 0.152234E+02 |
| 0.142300E+04 | 0.159132E+02 | 0.166033E+02 | 0.152231E+02 |
| 0.142400E+04 | 0.159124E+02 | 0.166020E+02 | 0.152228E+02 |
| 0.142500E+04 | 0.159116E+02 | 0.166007E+02 | 0.152225E+02 |
| 0.142600E+04 | 0.159108E+02 | 0.165994E+02 | 0.152222E+02 |
| 0.142700E+04 | 0.159100E+02 | 0.165981E+02 | 0.152218E+02 |
| 0.142800E+04 | 0.159089E+02 | 0.165966E+02 | 0.152212E+02 |
| 0.142900E+04 | 0.159073E+02 | 0.165945E+02 | 0.152201E+02 |
| 0.143000E+04 | 0.159054E+02 | 0.165921E+02 | 0.152188E+02 |
| 0.143100E+04 | 0.159033E+02 | 0.165894E+02 | 0.152172E+02 |
| 0.143200E+04 | 0.159011E+02 | 0.165867E+02 | 0.152155E+02 |
| 0.143300E+04 | 0.158987E+02 | 0.165838E+02 | 0.152137E+02 |
| 0.143400E+04 | 0.158695E+02 | 0.165499E+02 | 0.151891E+02 |
| 0.143500E+04 | 0.158398E+02 | 0.165154E+02 | 0.151643E+02 |
| 0.143600E+04 | 0.158475E+02 | 0.165247E+02 | 0.151703E+02 |
| 0.143700E+04 | 0.158607E+02 | 0.165407E+02 | 0.151806E+02 |
| 0.143800E+04 | 0.158662E+02 | 0.165476E+02 | 0.151849E+02 |
| 0.143900E+04 | 0.158682E+02 | 0.165500E+02 | 0.151863E+02 |
| 0.144000E+04 | 0.158687E+02 | 0.165506E+02 | 0.151867E+02 |
| 0.144100E+04 | 0.158684E+02 | 0.165502E+02 | 0.151865E+02 |
| 0.144200E+04 | 0.158678E+02 | 0.165494E+02 | 0.151862E+02 |
| 0.144300E+04 | 0.158674E+02 | 0.165489E+02 | 0.151860E+02 |
| 0.144400E+04 | 0.158671E+02 | 0.165484E+02 | 0.151859E+02 |
| 0.144500E+04 | 0.158668E+02 | 0.165480E+02 | 0.151857E+02 |
| 0.144600E+04 | 0.158666E+02 | 0.165475E+02 | 0.151856E+02 |
| 0.144700E+04 | 0.158663E+02 | 0.165471E+02 | 0.151855E+02 |
| 0.144800E+04 | 0.158660E+02 | 0.165467E+02 | 0.151853E+02 |
| 0.144900E+04 | 0.158657E+02 | 0.165463E+02 | 0.151852E+02 |
| 0.145000E+04 | 0.158655E+02 | 0.165459E+02 | 0.151850E+02 |
| 0.145100E+04 | 0.158652E+02 | 0.165456E+02 | 0.151849E+02 |
| 0.145200E+04 | 0.158649E+02 | 0.165451E+02 | 0.151847E+02 |
| 0.145300E+04 | 0.156986E+02 | 0.163494E+02 | 0.150478E+02 |
| 0.145400E+04 | 0.155209E+02 | 0.161407E+02 | 0.149010E+02 |
| 0.145500E+04 | 0.157223E+02 | 0.162074E+02 | 0.152372E+02 |
| 0.145600E+04 | 0.158517E+02 | 0.163244E+02 | 0.153790E+02 |

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| 0.145700E+04 | 0.159077E+02 | 0.163858E+02 | 0.154296E+02 |
| 0.145800E+04 | 0.159363E+02 | 0.164214E+02 | 0.154512E+02 |
| 0.145900E+04 | 0.157969E+02 | 0.162550E+02 | 0.153387E+02 |
| 0.146000E+04 | 0.155383E+02 | 0.159649E+02 | 0.151117E+02 |
| 0.146100E+04 | 0.154816E+02 | 0.159199E+02 | 0.150434E+02 |
| 0.146200E+04 | 0.156008E+02 | 0.160635E+02 | 0.151380E+02 |
| 0.146300E+04 | 0.157161E+02 | 0.161978E+02 | 0.152344E+02 |
| 0.146400E+04 | 0.157779E+02 | 0.162756E+02 | 0.152803E+02 |
| 0.146500E+04 | 0.158130E+02 | 0.163221E+02 | 0.153038E+02 |
| 0.146600E+04 | 0.157942E+02 | 0.163031E+02 | 0.152853E+02 |
| 0.146700E+04 | 0.157648E+02 | 0.162694E+02 | 0.152601E+02 |
| 0.146800E+04 | 0.157932E+02 | 0.163077E+02 | 0.152787E+02 |
| 0.146900E+04 | 0.158317E+02 | 0.163593E+02 | 0.153042E+02 |
| 0.147000E+04 | 0.158545E+02 | 0.163903E+02 | 0.153187E+02 |
| 0.147100E+04 | 0.158691E+02 | 0.164102E+02 | 0.153280E+02 |
| 0.147200E+04 | 0.158802E+02 | 0.164253E+02 | 0.153350E+02 |
| 0.147300E+04 | 0.159115E+02 | 0.164797E+02 | 0.153434E+02 |
| 0.147400E+04 | 0.159169E+02 | 0.164839E+02 | 0.153498E+02 |
| 0.147500E+04 | 0.159220E+02 | 0.164894E+02 | 0.153546E+02 |
| 0.147600E+04 | 0.159266E+02 | 0.164947E+02 | 0.153586E+02 |
| 0.147700E+04 | 0.159309E+02 | 0.164996E+02 | 0.153622E+02 |
| 0.147800E+04 | 0.159350E+02 | 0.165043E+02 | 0.153657E+02 |
| 0.147900E+04 | 0.159388E+02 | 0.165085E+02 | 0.153690E+02 |
| 0.148000E+04 | 0.159372E+02 | 0.165125E+02 | 0.153620E+02 |
| 0.148100E+04 | 0.158914E+02 | 0.164707E+02 | 0.153122E+02 |
| 0.148200E+04 | 0.158569E+02 | 0.164401E+02 | 0.152738E+02 |
| 0.148300E+04 | 0.158195E+02 | 0.164051E+02 | 0.152338E+02 |
| 0.148400E+04 | 0.158050E+02 | 0.164048E+02 | 0.152052E+02 |
| 0.148500E+04 | 0.158148E+02 | 0.164485E+02 | 0.151811E+02 |
| 0.148600E+04 | 0.157900E+02 | 0.164782E+02 | 0.151018E+02 |
| 0.148700E+04 | 0.157779E+02 | 0.164903E+02 | 0.150655E+02 |
| 0.148800E+04 | 0.157811E+02 | 0.164964E+02 | 0.150659E+02 |
| 0.148900E+04 | 0.157809E+02 | 0.164998E+02 | 0.150620E+02 |
| 0.149000E+04 | 0.157796E+02 | 0.165015E+02 | 0.150577E+02 |
| 0.149100E+04 | 0.157791E+02 | 0.165020E+02 | 0.150562E+02 |
| 0.149200E+04 | 0.157799E+02 | 0.165020E+02 | 0.150578E+02 |
| 0.149300E+04 | 0.157818E+02 | 0.165018E+02 | 0.150619E+02 |
| 0.149400E+04 | 0.157847E+02 | 0.165016E+02 | 0.150678E+02 |
| 0.149500E+04 | 0.157421E+02 | 0.164482E+02 | 0.150361E+02 |
| 0.149600E+04 | 0.156978E+02 | 0.163923E+02 | 0.150032E+02 |
| 0.149700E+04 | 0.157180E+02 | 0.164116E+02 | 0.150243E+02 |
| 0.149800E+04 | 0.157474E+02 | 0.164401E+02 | 0.150546E+02 |
| 0.149900E+04 | 0.157617E+02 | 0.164504E+02 | 0.150730E+02 |
| 0.150000E+04 | 0.157724E+02 | 0.164588E+02 | 0.150861E+02 |
| 0.150100E+04 | 0.157814E+02 | 0.164655E+02 | 0.150973E+02 |
| 0.150200E+04 | 0.157892E+02 | 0.164712E+02 | 0.151072E+02 |
| 0.150300E+04 | 0.157961E+02 | 0.164760E+02 | 0.151162E+02 |
| 0.150400E+04 | 0.157634E+02 | 0.164350E+02 | 0.150919E+02 |
| 0.150500E+04 | 0.157249E+02 | 0.163868E+02 | 0.150630E+02 |
| 0.150600E+04 | 0.157419E+02 | 0.164051E+02 | 0.150788E+02 |
| 0.150700E+04 | 0.157725E+02 | 0.164402E+02 | 0.151047E+02 |
| 0.150800E+04 | 0.157905E+02 | 0.164605E+02 | 0.151205E+02 |
| 0.150900E+04 | 0.158017E+02 | 0.164724E+02 | 0.151309E+02 |
| 0.151000E+04 | 0.158103E+02 | 0.164812E+02 | 0.151393E+02 |

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| 0.151100E+04 | 0.158174E+02 | 0.164884E+02 | 0.151463E+02 |
| 0.151200E+04 | 0.158233E+02 | 0.164944E+02 | 0.151523E+02 |
| 0.151300E+04 | 0.158283E+02 | 0.164993E+02 | 0.151573E+02 |
| 0.151400E+04 | 0.158325E+02 | 0.165034E+02 | 0.151616E+02 |
| 0.151500E+04 | 0.158360E+02 | 0.165068E+02 | 0.151652E+02 |
| 0.151600E+04 | 0.158391E+02 | 0.165099E+02 | 0.151684E+02 |
| 0.151700E+04 | 0.158418E+02 | 0.165126E+02 | 0.151711E+02 |
| 0.151800E+04 | 0.158442E+02 | 0.165151E+02 | 0.151734E+02 |
| 0.151900E+04 | 0.158464E+02 | 0.165174E+02 | 0.151754E+02 |
| 0.152000E+04 | 0.158485E+02 | 0.165197E+02 | 0.151773E+02 |
| 0.152100E+04 | 0.158505E+02 | 0.165220E+02 | 0.151790E+02 |
| 0.152200E+04 | 0.158523E+02 | 0.165241E+02 | 0.151805E+02 |
| 0.152300E+04 | 0.158541E+02 | 0.165262E+02 | 0.151819E+02 |
| 0.152400E+04 | 0.158556E+02 | 0.165280E+02 | 0.151832E+02 |
| 0.152500E+04 | 0.158570E+02 | 0.165298E+02 | 0.151843E+02 |
| 0.152600E+04 | 0.158584E+02 | 0.165314E+02 | 0.151853E+02 |
| 0.152700E+04 | 0.157598E+02 | 0.164234E+02 | 0.150961E+02 |
| 0.152800E+04 | 0.156533E+02 | 0.162994E+02 | 0.150073E+02 |
| 0.152900E+04 | 0.156942E+02 | 0.163413E+02 | 0.150471E+02 |
| 0.153000E+04 | 0.157584E+02 | 0.164157E+02 | 0.151010E+02 |
| 0.153100E+04 | 0.157864E+02 | 0.164467E+02 | 0.151262E+02 |
| 0.153200E+04 | 0.157988E+02 | 0.164660E+02 | 0.151317E+02 |
| 0.153300E+04 | 0.158070E+02 | 0.164795E+02 | 0.151346E+02 |
| 0.153400E+04 | 0.158147E+02 | 0.164895E+02 | 0.151400E+02 |
| 0.153500E+04 | 0.158212E+02 | 0.164971E+02 | 0.151452E+02 |
| 0.153600E+04 | 0.158266E+02 | 0.165033E+02 | 0.151499E+02 |
| 0.153700E+04 | 0.158312E+02 | 0.165085E+02 | 0.151540E+02 |
| 0.153800E+04 | 0.158351E+02 | 0.165127E+02 | 0.151575E+02 |
| 0.153900E+04 | 0.158383E+02 | 0.165161E+02 | 0.151606E+02 |
| 0.154000E+04 | 0.158410E+02 | 0.165189E+02 | 0.151632E+02 |
| 0.154100E+04 | 0.158435E+02 | 0.165214E+02 | 0.151656E+02 |
| 0.154200E+04 | 0.158457E+02 | 0.165237E+02 | 0.151677E+02 |
| 0.154300E+04 | 0.158477E+02 | 0.165258E+02 | 0.151696E+02 |
| 0.154400E+04 | 0.158495E+02 | 0.165277E+02 | 0.151714E+02 |
| 0.154500E+04 | 0.158512E+02 | 0.165294E+02 | 0.151730E+02 |
| 0.154600E+04 | 0.158527E+02 | 0.165310E+02 | 0.151744E+02 |
| 0.154700E+04 | 0.158541E+02 | 0.165324E+02 | 0.151757E+02 |
| 0.154800E+04 | 0.158553E+02 | 0.165337E+02 | 0.151769E+02 |
| 0.154900E+04 | 0.158564E+02 | 0.165349E+02 | 0.151780E+02 |
| 0.155000E+04 | 0.158574E+02 | 0.165359E+02 | 0.151790E+02 |
| 0.155100E+04 | 0.158583E+02 | 0.165368E+02 | 0.151798E+02 |
| 0.155200E+04 | 0.158591E+02 | 0.165375E+02 | 0.151806E+02 |
| 0.155300E+04 | 0.158597E+02 | 0.165381E+02 | 0.151813E+02 |
| 0.155400E+04 | 0.158603E+02 | 0.165387E+02 | 0.151819E+02 |
| 0.155500E+04 | 0.158608E+02 | 0.165391E+02 | 0.151825E+02 |
| 0.155600E+04 | 0.158612E+02 | 0.165395E+02 | 0.151830E+02 |
| 0.155700E+04 | 0.158616E+02 | 0.165398E+02 | 0.151834E+02 |
| 0.155800E+04 | 0.158620E+02 | 0.165401E+02 | 0.151839E+02 |
| 0.155900E+04 | 0.158623E+02 | 0.165402E+02 | 0.151843E+02 |
| 0.156000E+04 | 0.158626E+02 | 0.165405E+02 | 0.151848E+02 |
| 0.156100E+04 | 0.158631E+02 | 0.165409E+02 | 0.151853E+02 |
| 0.156200E+04 | 0.158636E+02 | 0.165413E+02 | 0.151860E+02 |
| 0.156300E+04 | 0.158642E+02 | 0.165418E+02 | 0.151866E+02 |
| 0.156400E+04 | 0.157724E+02 | 0.164418E+02 | 0.151029E+02 |

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| 0.156500E+04 | 0.156692E+02 | 0.163206E+02 | 0.150179E+02 |
| 0.156600E+04 | 0.157063E+02 | 0.163577E+02 | 0.150550E+02 |
| 0.156700E+04 | 0.157687E+02 | 0.164302E+02 | 0.151072E+02 |
| 0.156800E+04 | 0.157953E+02 | 0.164588E+02 | 0.151319E+02 |
| 0.156900E+04 | 0.158074E+02 | 0.164769E+02 | 0.151379E+02 |
| 0.157000E+04 | 0.157880E+02 | 0.164580E+02 | 0.151180E+02 |
| 0.157100E+04 | 0.157674E+02 | 0.164338E+02 | 0.151011E+02 |
| 0.157200E+04 | 0.157842E+02 | 0.164534E+02 | 0.151150E+02 |
| 0.157300E+04 | 0.158059E+02 | 0.164792E+02 | 0.151326E+02 |
| 0.157400E+04 | 0.158187E+02 | 0.164940E+02 | 0.151434E+02 |
| 0.157500E+04 | 0.158270E+02 | 0.165031E+02 | 0.151509E+02 |
| 0.157600E+04 | 0.158334E+02 | 0.165097E+02 | 0.151570E+02 |
| 0.157700E+04 | 0.158386E+02 | 0.165150E+02 | 0.151622E+02 |
| 0.157800E+04 | 0.158433E+02 | 0.165196E+02 | 0.151670E+02 |
| 0.157900E+04 | 0.158475E+02 | 0.165236E+02 | 0.151713E+02 |
| 0.158000E+04 | 0.158512E+02 | 0.165272E+02 | 0.151753E+02 |
| 0.158100E+04 | 0.158547E+02 | 0.165305E+02 | 0.151789E+02 |
| 0.158200E+04 | 0.158579E+02 | 0.165334E+02 | 0.151823E+02 |
| 0.158300E+04 | 0.158609E+02 | 0.165362E+02 | 0.151855E+02 |
| 0.158400E+04 | 0.158637E+02 | 0.165389E+02 | 0.151885E+02 |
| 0.158500E+04 | 0.158663E+02 | 0.165412E+02 | 0.151913E+02 |
| 0.158600E+04 | 0.158686E+02 | 0.165433E+02 | 0.151938E+02 |
| 0.158700E+04 | 0.157132E+02 | 0.163666E+02 | 0.150598E+02 |
| 0.158800E+04 | 0.154744E+02 | 0.160886E+02 | 0.148602E+02 |
| 0.158900E+04 | 0.156158E+02 | 0.161042E+02 | 0.151275E+02 |
| 0.159000E+04 | 0.157461E+02 | 0.162183E+02 | 0.152740E+02 |
| 0.159100E+04 | 0.158410E+02 | 0.163176E+02 | 0.153645E+02 |
| 0.159200E+04 | 0.158870E+02 | 0.163685E+02 | 0.154054E+02 |
| 0.159300E+04 | 0.159142E+02 | 0.164041E+02 | 0.154242E+02 |
| 0.159400E+04 | 0.159325E+02 | 0.164313E+02 | 0.154336E+02 |
| 0.159500E+04 | 0.159455E+02 | 0.164532E+02 | 0.154379E+02 |
| 0.159600E+04 | 0.159551E+02 | 0.164710E+02 | 0.154392E+02 |
| 0.159700E+04 | 0.159644E+02 | 0.164859E+02 | 0.154430E+02 |
| 0.159800E+04 | 0.159701E+02 | 0.164982E+02 | 0.154419E+02 |
| 0.159900E+04 | 0.159745E+02 | 0.165086E+02 | 0.154403E+02 |
| 0.160000E+04 | 0.160005E+02 | 0.165604E+02 | 0.154407E+02 |
| 0.160100E+04 | 0.158122E+02 | 0.163475E+02 | 0.152769E+02 |
| 0.160200E+04 | 0.155901E+02 | 0.160843E+02 | 0.150960E+02 |
| 0.160300E+04 | 0.156584E+02 | 0.161822E+02 | 0.151345E+02 |
| 0.160400E+04 | 0.157541E+02 | 0.162916E+02 | 0.152167E+02 |
| 0.160500E+04 | 0.158219E+02 | 0.163701E+02 | 0.152738E+02 |
| 0.160600E+04 | 0.158609E+02 | 0.164181E+02 | 0.153037E+02 |
| 0.160700E+04 | 0.158846E+02 | 0.164478E+02 | 0.153213E+02 |
| 0.160800E+04 | 0.159013E+02 | 0.164688E+02 | 0.153337E+02 |
| 0.160900E+04 | 0.159137E+02 | 0.164844E+02 | 0.153430E+02 |
| 0.161000E+04 | 0.159234E+02 | 0.164964E+02 | 0.153503E+02 |
| 0.161100E+04 | 0.159310E+02 | 0.165057E+02 | 0.153562E+02 |
| 0.161200E+04 | 0.159371E+02 | 0.165130E+02 | 0.153611E+02 |
| 0.161300E+04 | 0.158360E+02 | 0.163919E+02 | 0.152802E+02 |
| 0.161400E+04 | 0.157406E+02 | 0.162851E+02 | 0.151960E+02 |
| 0.161500E+04 | 0.157794E+02 | 0.163302E+02 | 0.152285E+02 |
| 0.161600E+04 | 0.158420E+02 | 0.164039E+02 | 0.152801E+02 |
| 0.161700E+04 | 0.158742E+02 | 0.164425E+02 | 0.153060E+02 |
| 0.161800E+04 | 0.158916E+02 | 0.164651E+02 | 0.153181E+02 |

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| 0.161900E+04 | 0.158932E+02 | 0.164807E+02 | 0.153057E+02 |
| 0.162000E+04 | 0.158925E+02 | 0.164920E+02 | 0.152929E+02 |
| 0.162100E+04 | 0.158471E+02 | 0.165003E+02 | 0.151940E+02 |
| 0.162200E+04 | 0.156965E+02 | 0.163644E+02 | 0.150287E+02 |
| 0.162300E+04 | 0.155671E+02 | 0.162420E+02 | 0.148922E+02 |
| 0.162400E+04 | 0.156052E+02 | 0.162909E+02 | 0.149196E+02 |
| 0.162500E+04 | 0.156690E+02 | 0.163711E+02 | 0.149668E+02 |
| 0.162600E+04 | 0.156993E+02 | 0.164112E+02 | 0.149874E+02 |
| 0.162700E+04 | 0.157160E+02 | 0.164336E+02 | 0.149985E+02 |
| 0.162800E+04 | 0.157286E+02 | 0.164485E+02 | 0.150087E+02 |
| 0.162900E+04 | 0.157391E+02 | 0.164591E+02 | 0.150191E+02 |
| 0.163000E+04 | 0.157487E+02 | 0.164672E+02 | 0.150301E+02 |
| 0.163100E+04 | 0.157575E+02 | 0.164737E+02 | 0.150414E+02 |
| 0.163200E+04 | 0.157656E+02 | 0.164787E+02 | 0.150526E+02 |
| 0.163300E+04 | 0.157729E+02 | 0.164825E+02 | 0.150632E+02 |
| 0.163400E+04 | 0.157794E+02 | 0.164855E+02 | 0.150733E+02 |
| 0.163500E+04 | 0.157854E+02 | 0.164880E+02 | 0.150828E+02 |
| 0.163600E+04 | 0.157909E+02 | 0.164902E+02 | 0.150916E+02 |
| 0.163700E+04 | 0.157960E+02 | 0.164921E+02 | 0.150998E+02 |
| 0.163800E+04 | 0.158006E+02 | 0.164938E+02 | 0.151074E+02 |
| 0.163900E+04 | 0.158049E+02 | 0.164955E+02 | 0.151143E+02 |
| 0.164000E+04 | 0.158090E+02 | 0.164973E+02 | 0.151207E+02 |
| 0.164100E+04 | 0.156164E+02 | 0.162750E+02 | 0.149578E+02 |
| 0.164200E+04 | 0.153906E+02 | 0.160159E+02 | 0.147653E+02 |
| 0.164300E+04 | 0.154635E+02 | 0.161100E+02 | 0.148171E+02 |
| 0.164400E+04 | 0.155646E+02 | 0.162184E+02 | 0.149108E+02 |
| 0.164500E+04 | 0.156245E+02 | 0.162818E+02 | 0.149673E+02 |
| 0.164600E+04 | 0.156531E+02 | 0.163139E+02 | 0.149922E+02 |
| 0.164700E+04 | 0.156822E+02 | 0.163524E+02 | 0.150120E+02 |
| 0.164800E+04 | 0.157101E+02 | 0.163854E+02 | 0.150349E+02 |
| 0.164900E+04 | 0.157302E+02 | 0.164077E+02 | 0.150528E+02 |
| 0.165000E+04 | 0.157455E+02 | 0.164238E+02 | 0.150671E+02 |
| 0.165100E+04 | 0.157577E+02 | 0.164364E+02 | 0.150791E+02 |
| 0.165200E+04 | 0.157678E+02 | 0.164464E+02 | 0.150892E+02 |
| 0.165300E+04 | 0.157760E+02 | 0.164544E+02 | 0.150976E+02 |
| 0.165400E+04 | 0.157828E+02 | 0.164609E+02 | 0.151047E+02 |
| 0.165500E+04 | 0.157884E+02 | 0.164662E+02 | 0.151106E+02 |
| 0.165600E+04 | 0.157930E+02 | 0.164705E+02 | 0.151154E+02 |
| 0.165700E+04 | 0.157968E+02 | 0.164741E+02 | 0.151195E+02 |
| 0.165800E+04 | 0.157999E+02 | 0.164770E+02 | 0.151227E+02 |
| 0.165900E+04 | 0.158024E+02 | 0.164794E+02 | 0.151254E+02 |
| 0.166000E+04 | 0.158046E+02 | 0.164816E+02 | 0.151277E+02 |
| 0.166100E+04 | 0.158068E+02 | 0.164838E+02 | 0.151298E+02 |
| 0.166200E+04 | 0.158089E+02 | 0.164860E+02 | 0.151317E+02 |
| 0.166300E+04 | 0.158107E+02 | 0.164879E+02 | 0.151334E+02 |
| 0.166400E+04 | 0.158124E+02 | 0.164898E+02 | 0.151350E+02 |
| 0.166500E+04 | 0.158139E+02 | 0.164916E+02 | 0.151363E+02 |
| 0.166600E+04 | 0.158154E+02 | 0.164932E+02 | 0.151375E+02 |
| 0.166700E+04 | 0.157219E+02 | 0.163835E+02 | 0.150603E+02 |
| 0.166800E+04 | 0.156393E+02 | 0.162928E+02 | 0.149858E+02 |
| 0.166900E+04 | 0.156740E+02 | 0.163283E+02 | 0.150197E+02 |
| 0.167000E+04 | 0.157309E+02 | 0.163948E+02 | 0.150671E+02 |
| 0.167100E+04 | 0.157546E+02 | 0.164201E+02 | 0.150892E+02 |
| 0.167200E+04 | 0.157647E+02 | 0.164366E+02 | 0.150927E+02 |

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| 0.167300E+04 | 0.157717E+02 | 0.164485E+02 | 0.150949E+02 |
| 0.167400E+04 | 0.156464E+02 | 0.163025E+02 | 0.149903E+02 |
| 0.167500E+04 | 0.155271E+02 | 0.161725E+02 | 0.148816E+02 |
| 0.167600E+04 | 0.155809E+02 | 0.162270E+02 | 0.149348E+02 |
| 0.167700E+04 | 0.156587E+02 | 0.163162E+02 | 0.150011E+02 |
| 0.167800E+04 | 0.156979E+02 | 0.163612E+02 | 0.150346E+02 |
| 0.167900E+04 | 0.157186E+02 | 0.163878E+02 | 0.150495E+02 |
| 0.168000E+04 | 0.157301E+02 | 0.164064E+02 | 0.150538E+02 |
| 0.168100E+04 | 0.156317E+02 | 0.162918E+02 | 0.149716E+02 |
| 0.168200E+04 | 0.155379E+02 | 0.161918E+02 | 0.148840E+02 |
| 0.168300E+04 | 0.155854E+02 | 0.162408E+02 | 0.149301E+02 |
| 0.168400E+04 | 0.156550E+02 | 0.163166E+02 | 0.149935E+02 |
| 0.168500E+04 | 0.156910E+02 | 0.163567E+02 | 0.150252E+02 |
| 0.168600E+04 | 0.157096E+02 | 0.163811E+02 | 0.150380E+02 |
| 0.168700E+04 | 0.157209E+02 | 0.163983E+02 | 0.150434E+02 |
| 0.168800E+04 | 0.157316E+02 | 0.164110E+02 | 0.150521E+02 |
| 0.168900E+04 | 0.157115E+02 | 0.163869E+02 | 0.150362E+02 |
| 0.169000E+04 | 0.156984E+02 | 0.163702E+02 | 0.150266E+02 |
| 0.169100E+04 | 0.157224E+02 | 0.163974E+02 | 0.150473E+02 |
| 0.169200E+04 | 0.157407E+02 | 0.164185E+02 | 0.150628E+02 |
| 0.169300E+04 | 0.157510E+02 | 0.164299E+02 | 0.150722E+02 |
| 0.169400E+04 | 0.157587E+02 | 0.164379E+02 | 0.150795E+02 |
| 0.169500E+04 | 0.156370E+02 | 0.163016E+02 | 0.149724E+02 |
| 0.169600E+04 | 0.155058E+02 | 0.161448E+02 | 0.148669E+02 |
| 0.169700E+04 | 0.155564E+02 | 0.161998E+02 | 0.149130E+02 |
| 0.169800E+04 | 0.157199E+02 | 0.162960E+02 | 0.151438E+02 |
| 0.169900E+04 | 0.158122E+02 | 0.163454E+02 | 0.152790E+02 |
| 0.170000E+04 | 0.158754E+02 | 0.163766E+02 | 0.153742E+02 |
| 0.170100E+04 | 0.158976E+02 | 0.164003E+02 | 0.153949E+02 |
| 0.170200E+04 | 0.159116E+02 | 0.164191E+02 | 0.154040E+02 |
| 0.170300E+04 | 0.159205E+02 | 0.164345E+02 | 0.154065E+02 |
| 0.170400E+04 | 0.159261E+02 | 0.164471E+02 | 0.154052E+02 |
| 0.170500E+04 | 0.159296E+02 | 0.164575E+02 | 0.154016E+02 |
| 0.170600E+04 | 0.159315E+02 | 0.164661E+02 | 0.153970E+02 |
| 0.170700E+04 | 0.159355E+02 | 0.164732E+02 | 0.153978E+02 |
| 0.170800E+04 | 0.159350E+02 | 0.164792E+02 | 0.153908E+02 |
| 0.170900E+04 | 0.159350E+02 | 0.164842E+02 | 0.153859E+02 |
| 0.171000E+04 | 0.159347E+02 | 0.164882E+02 | 0.153812E+02 |
| 0.171100E+04 | 0.159342E+02 | 0.164915E+02 | 0.153769E+02 |
| 0.171200E+04 | 0.159335E+02 | 0.164942E+02 | 0.153728E+02 |
| 0.171300E+04 | 0.159328E+02 | 0.164964E+02 | 0.153692E+02 |
| 0.171400E+04 | 0.159322E+02 | 0.164984E+02 | 0.153660E+02 |
| 0.171500E+04 | 0.159316E+02 | 0.165001E+02 | 0.153631E+02 |
| 0.171600E+04 | 0.159311E+02 | 0.165015E+02 | 0.153606E+02 |
| 0.171700E+04 | 0.159307E+02 | 0.165030E+02 | 0.153585E+02 |
| 0.171800E+04 | 0.159305E+02 | 0.165043E+02 | 0.153567E+02 |
| 0.171900E+04 | 0.159303E+02 | 0.165055E+02 | 0.153551E+02 |
| 0.172000E+04 | 0.159302E+02 | 0.165066E+02 | 0.153538E+02 |
| 0.172100E+04 | 0.159253E+02 | 0.165081E+02 | 0.153424E+02 |
| 0.172200E+04 | 0.159150E+02 | 0.165100E+02 | 0.153200E+02 |
| 0.172300E+04 | 0.159065E+02 | 0.165122E+02 | 0.153009E+02 |
| 0.172400E+04 | 0.158735E+02 | 0.165143E+02 | 0.152328E+02 |
| 0.172500E+04 | 0.158276E+02 | 0.165158E+02 | 0.151394E+02 |
| 0.172600E+04 | 0.158046E+02 | 0.165163E+02 | 0.150928E+02 |

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| 0.172700E+04 | 0.157892E+02 | 0.165163E+02 | 0.150621E+02 |
| 0.172800E+04 | 0.157790E+02 | 0.165157E+02 | 0.150422E+02 |
| 0.172900E+04 | 0.156549E+02 | 0.163865E+02 | 0.149232E+02 |
| 0.173000E+04 | 0.155742E+02 | 0.162819E+02 | 0.148665E+02 |
| 0.173100E+04 | 0.156523E+02 | 0.163685E+02 | 0.149361E+02 |
| 0.173200E+04 | 0.156991E+02 | 0.164177E+02 | 0.149806E+02 |
| 0.173300E+04 | 0.157211E+02 | 0.164410E+02 | 0.150013E+02 |
| 0.173400E+04 | 0.157368E+02 | 0.164564E+02 | 0.150171E+02 |
| 0.173500E+04 | 0.157494E+02 | 0.164676E+02 | 0.150312E+02 |
| 0.173600E+04 | 0.157606E+02 | 0.164766E+02 | 0.150446E+02 |
| 0.173700E+04 | 0.157712E+02 | 0.164847E+02 | 0.150577E+02 |
| 0.173800E+04 | 0.157812E+02 | 0.164920E+02 | 0.150703E+02 |
| 0.173900E+04 | 0.157401E+02 | 0.164398E+02 | 0.150405E+02 |
| 0.174000E+04 | 0.157122E+02 | 0.164027E+02 | 0.150218E+02 |
| 0.174100E+04 | 0.157494E+02 | 0.164427E+02 | 0.150561E+02 |
| 0.174200E+04 | 0.157783E+02 | 0.164745E+02 | 0.150821E+02 |
| 0.174300E+04 | 0.157942E+02 | 0.164905E+02 | 0.150979E+02 |
| 0.174400E+04 | 0.158061E+02 | 0.165016E+02 | 0.151105E+02 |
| 0.174500E+04 | 0.158159E+02 | 0.165104E+02 | 0.151214E+02 |
| 0.174600E+04 | 0.158238E+02 | 0.165178E+02 | 0.151299E+02 |
| 0.174700E+04 | 0.158280E+02 | 0.165242E+02 | 0.151318E+02 |
| 0.174800E+04 | 0.158320E+02 | 0.165297E+02 | 0.151343E+02 |
| 0.174900E+04 | 0.158362E+02 | 0.165347E+02 | 0.151376E+02 |
| 0.175000E+04 | 0.158403E+02 | 0.165393E+02 | 0.151414E+02 |
| 0.175100E+04 | 0.158445E+02 | 0.165437E+02 | 0.151454E+02 |
| 0.175200E+04 | 0.158486E+02 | 0.165478E+02 | 0.151495E+02 |
| 0.175300E+04 | 0.158526E+02 | 0.165517E+02 | 0.151536E+02 |
| 0.175400E+04 | 0.158565E+02 | 0.165555E+02 | 0.151576E+02 |
| 0.175500E+04 | 0.158603E+02 | 0.165592E+02 | 0.151615E+02 |
| 0.175600E+04 | 0.158640E+02 | 0.165628E+02 | 0.151652E+02 |
| 0.175700E+04 | 0.158675E+02 | 0.165663E+02 | 0.151686E+02 |
| 0.175800E+04 | 0.158708E+02 | 0.165697E+02 | 0.151720E+02 |
| 0.175900E+04 | 0.158740E+02 | 0.165729E+02 | 0.151751E+02 |
| 0.176000E+04 | 0.158771E+02 | 0.165761E+02 | 0.151781E+02 |
| 0.176100E+04 | 0.158801E+02 | 0.165793E+02 | 0.151809E+02 |
| 0.176200E+04 | 0.158830E+02 | 0.165824E+02 | 0.151836E+02 |
| 0.176300E+04 | 0.158858E+02 | 0.165854E+02 | 0.151862E+02 |
| 0.176400E+04 | 0.158884E+02 | 0.165883E+02 | 0.151886E+02 |
| 0.176500E+04 | 0.158908E+02 | 0.165908E+02 | 0.151907E+02 |
| 0.176600E+04 | 0.158928E+02 | 0.165931E+02 | 0.151925E+02 |
| 0.176700E+04 | 0.158947E+02 | 0.165952E+02 | 0.151942E+02 |
| 0.176800E+04 | 0.158964E+02 | 0.165971E+02 | 0.151957E+02 |
| 0.176900E+04 | 0.158980E+02 | 0.165989E+02 | 0.151970E+02 |
| 0.177000E+04 | 0.158994E+02 | 0.166006E+02 | 0.151983E+02 |
| 0.177100E+04 | 0.159008E+02 | 0.166022E+02 | 0.151994E+02 |
| 0.177200E+04 | 0.159019E+02 | 0.166035E+02 | 0.152003E+02 |
| 0.177300E+04 | 0.159026E+02 | 0.166043E+02 | 0.152009E+02 |
| 0.177400E+04 | 0.159029E+02 | 0.166047E+02 | 0.152011E+02 |
| 0.177500E+04 | 0.159031E+02 | 0.166049E+02 | 0.152012E+02 |
| 0.177600E+04 | 0.159031E+02 | 0.166050E+02 | 0.152012E+02 |
| 0.177700E+04 | 0.159029E+02 | 0.166049E+02 | 0.152010E+02 |
| 0.177800E+04 | 0.159026E+02 | 0.166046E+02 | 0.152007E+02 |
| 0.177900E+04 | 0.159022E+02 | 0.166041E+02 | 0.152003E+02 |
| 0.178000E+04 | 0.159019E+02 | 0.166037E+02 | 0.152000E+02 |

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| 0.178100E+04 | 0.159018E+02 | 0.166037E+02 | 0.151998E+02 |
| 0.178200E+04 | 0.159019E+02 | 0.166039E+02 | 0.151999E+02 |
| 0.178300E+04 | 0.157862E+02 | 0.164735E+02 | 0.150989E+02 |
| 0.178400E+04 | 0.156921E+02 | 0.163697E+02 | 0.150145E+02 |
| 0.178500E+04 | 0.157734E+02 | 0.164570E+02 | 0.150898E+02 |
| 0.178600E+04 | 0.158192E+02 | 0.165063E+02 | 0.151322E+02 |
| 0.178700E+04 | 0.158388E+02 | 0.165295E+02 | 0.151481E+02 |
| 0.178800E+04 | 0.158466E+02 | 0.165446E+02 | 0.151487E+02 |
| 0.178900E+04 | 0.158077E+02 | 0.165008E+02 | 0.151146E+02 |
| 0.179000E+04 | 0.157643E+02 | 0.164500E+02 | 0.150787E+02 |
| 0.179100E+04 | 0.157857E+02 | 0.164755E+02 | 0.150959E+02 |
| 0.179200E+04 | 0.158189E+02 | 0.165152E+02 | 0.151226E+02 |
| 0.179300E+04 | 0.158376E+02 | 0.165373E+02 | 0.151380E+02 |
| 0.179400E+04 | 0.158488E+02 | 0.165500E+02 | 0.151477E+02 |
| 0.179500E+04 | 0.158570E+02 | 0.165588E+02 | 0.151552E+02 |
| 0.179600E+04 | 0.158633E+02 | 0.165655E+02 | 0.151612E+02 |
| 0.179700E+04 | 0.158684E+02 | 0.165706E+02 | 0.151662E+02 |
| 0.179800E+04 | 0.158726E+02 | 0.165747E+02 | 0.151704E+02 |
| 0.179900E+04 | 0.158760E+02 | 0.165780E+02 | 0.151740E+02 |
| 0.180000E+04 | 0.158789E+02 | 0.165806E+02 | 0.151771E+02 |
| 0.180100E+04 | 0.158813E+02 | 0.165828E+02 | 0.151798E+02 |
| 0.180200E+04 | 0.158833E+02 | 0.165845E+02 | 0.151821E+02 |
| 0.180300E+04 | 0.158850E+02 | 0.165859E+02 | 0.151841E+02 |
| 0.180400E+04 | 0.158864E+02 | 0.165871E+02 | 0.151858E+02 |
| 0.180500E+04 | 0.158877E+02 | 0.165880E+02 | 0.151874E+02 |
| 0.180600E+04 | 0.158887E+02 | 0.165887E+02 | 0.151887E+02 |
| 0.180700E+04 | 0.158896E+02 | 0.165892E+02 | 0.151899E+02 |
| 0.180800E+04 | 0.158903E+02 | 0.165896E+02 | 0.151910E+02 |
| 0.180900E+04 | 0.156937E+02 | 0.163667E+02 | 0.150207E+02 |
| 0.181000E+04 | 0.154708E+02 | 0.161056E+02 | 0.148360E+02 |
| 0.181100E+04 | 0.156805E+02 | 0.161989E+02 | 0.151620E+02 |
| 0.181200E+04 | 0.158080E+02 | 0.163068E+02 | 0.153092E+02 |
| 0.181300E+04 | 0.158876E+02 | 0.163885E+02 | 0.153867E+02 |
| 0.181400E+04 | 0.159217E+02 | 0.164210E+02 | 0.154224E+02 |
| 0.181500E+04 | 0.157344E+02 | 0.162065E+02 | 0.152623E+02 |
| 0.181600E+04 | 0.155212E+02 | 0.159690E+02 | 0.150735E+02 |
| 0.181700E+04 | 0.155783E+02 | 0.160388E+02 | 0.151177E+02 |
| 0.181800E+04 | 0.156985E+02 | 0.161879E+02 | 0.152091E+02 |
| 0.181900E+04 | 0.157783E+02 | 0.162838E+02 | 0.152728E+02 |
| 0.182000E+04 | 0.158270E+02 | 0.163485E+02 | 0.153055E+02 |
| 0.182100E+04 | 0.158566E+02 | 0.163893E+02 | 0.153239E+02 |
| 0.182200E+04 | 0.158775E+02 | 0.164188E+02 | 0.153361E+02 |
| 0.182300E+04 | 0.158929E+02 | 0.164411E+02 | 0.153446E+02 |
| 0.182400E+04 | 0.159045E+02 | 0.164581E+02 | 0.153508E+02 |
| 0.182500E+04 | 0.159131E+02 | 0.164711E+02 | 0.153552E+02 |
| 0.182600E+04 | 0.159197E+02 | 0.164810E+02 | 0.153583E+02 |
| 0.182700E+04 | 0.159471E+02 | 0.165312E+02 | 0.153629E+02 |
| 0.182800E+04 | 0.159487E+02 | 0.165311E+02 | 0.153663E+02 |
| 0.182900E+04 | 0.159501E+02 | 0.165323E+02 | 0.153679E+02 |
| 0.183000E+04 | 0.159060E+02 | 0.164794E+02 | 0.153326E+02 |
| 0.183100E+04 | 0.157065E+02 | 0.162509E+02 | 0.151621E+02 |
| 0.183200E+04 | 0.155492E+02 | 0.160746E+02 | 0.150238E+02 |
| 0.183300E+04 | 0.156413E+02 | 0.161879E+02 | 0.150946E+02 |
| 0.183400E+04 | 0.157492E+02 | 0.163092E+02 | 0.151892E+02 |

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| 0.183500E+04 | 0.156914E+02 | 0.162375E+02 | 0.151453E+02 |
| 0.183600E+04 | 0.156109E+02 | 0.161462E+02 | 0.150756E+02 |
| 0.183700E+04 | 0.156696E+02 | 0.162156E+02 | 0.151236E+02 |
| 0.183800E+04 | 0.157467E+02 | 0.163038E+02 | 0.151895E+02 |
| 0.183900E+04 | 0.157854E+02 | 0.163458E+02 | 0.152249E+02 |
| 0.184000E+04 | 0.157723E+02 | 0.163285E+02 | 0.152161E+02 |
| 0.184100E+04 | 0.157485E+02 | 0.162980E+02 | 0.151990E+02 |
| 0.184200E+04 | 0.157766E+02 | 0.163315E+02 | 0.152216E+02 |
| 0.184300E+04 | 0.157749E+02 | 0.163440E+02 | 0.152058E+02 |
| 0.184400E+04 | 0.157535E+02 | 0.163283E+02 | 0.151787E+02 |
| 0.184500E+04 | 0.157843E+02 | 0.163603E+02 | 0.152082E+02 |
| 0.184600E+04 | 0.158264E+02 | 0.164106E+02 | 0.152422E+02 |
| 0.184700E+04 | 0.158368E+02 | 0.164324E+02 | 0.152412E+02 |
| 0.184800E+04 | 0.158123E+02 | 0.164494E+02 | 0.151752E+02 |
| 0.184900E+04 | 0.157795E+02 | 0.164617E+02 | 0.150972E+02 |
| 0.185000E+04 | 0.157642E+02 | 0.164700E+02 | 0.150584E+02 |
| 0.185100E+04 | 0.157541E+02 | 0.164755E+02 | 0.150328E+02 |
| 0.185200E+04 | 0.157478E+02 | 0.164792E+02 | 0.150165E+02 |
| 0.185300E+04 | 0.157431E+02 | 0.164816E+02 | 0.150045E+02 |
| 0.185400E+04 | 0.157100E+02 | 0.164468E+02 | 0.149732E+02 |
| 0.185500E+04 | 0.156879E+02 | 0.164214E+02 | 0.149544E+02 |
| 0.185600E+04 | 0.157083E+02 | 0.164442E+02 | 0.149725E+02 |
| 0.185700E+04 | 0.157253E+02 | 0.164618E+02 | 0.149887E+02 |
| 0.185800E+04 | 0.157355E+02 | 0.164701E+02 | 0.150009E+02 |
| 0.185900E+04 | 0.157441E+02 | 0.164757E+02 | 0.150125E+02 |
| 0.186000E+04 | 0.157520E+02 | 0.164801E+02 | 0.150238E+02 |
| 0.186100E+04 | 0.157595E+02 | 0.164841E+02 | 0.150350E+02 |
| 0.186200E+04 | 0.157668E+02 | 0.164878E+02 | 0.150458E+02 |
| 0.186300E+04 | 0.157738E+02 | 0.164914E+02 | 0.150562E+02 |
| 0.186400E+04 | 0.157806E+02 | 0.164951E+02 | 0.150661E+02 |
| 0.186500E+04 | 0.157871E+02 | 0.164987E+02 | 0.150755E+02 |
| 0.186600E+04 | 0.157933E+02 | 0.165024E+02 | 0.150843E+02 |
| 0.186700E+04 | 0.157992E+02 | 0.165059E+02 | 0.150924E+02 |
| 0.186800E+04 | 0.158047E+02 | 0.165094E+02 | 0.150999E+02 |
| 0.186900E+04 | 0.158099E+02 | 0.165129E+02 | 0.151069E+02 |
| 0.187000E+04 | 0.158148E+02 | 0.165164E+02 | 0.151133E+02 |
| 0.187100E+04 | 0.158195E+02 | 0.165198E+02 | 0.151192E+02 |
| 0.187200E+04 | 0.158239E+02 | 0.165232E+02 | 0.151246E+02 |
| 0.187300E+04 | 0.158281E+02 | 0.165266E+02 | 0.151296E+02 |
| 0.187400E+04 | 0.158321E+02 | 0.165300E+02 | 0.151342E+02 |
| 0.187500E+04 | 0.158359E+02 | 0.165334E+02 | 0.151385E+02 |
| 0.187600E+04 | 0.158395E+02 | 0.165367E+02 | 0.151424E+02 |
| 0.187700E+04 | 0.158430E+02 | 0.165400E+02 | 0.151460E+02 |
| 0.187800E+04 | 0.158462E+02 | 0.165432E+02 | 0.151493E+02 |
| 0.187900E+04 | 0.158494E+02 | 0.165464E+02 | 0.151523E+02 |
| 0.188000E+04 | 0.158523E+02 | 0.165495E+02 | 0.151551E+02 |
| 0.188100E+04 | 0.158550E+02 | 0.165524E+02 | 0.151577E+02 |
| 0.188200E+04 | 0.158576E+02 | 0.165552E+02 | 0.151601E+02 |
| 0.188300E+04 | 0.157276E+02 | 0.164127E+02 | 0.150425E+02 |
| 0.188400E+04 | 0.155762E+02 | 0.162387E+02 | 0.149137E+02 |
| 0.188500E+04 | 0.156302E+02 | 0.162938E+02 | 0.149667E+02 |
| 0.188600E+04 | 0.157171E+02 | 0.163944E+02 | 0.150398E+02 |
| 0.188700E+04 | 0.157599E+02 | 0.164432E+02 | 0.150767E+02 |
| 0.188800E+04 | 0.157801E+02 | 0.164716E+02 | 0.150885E+02 |

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|--------------|--------------|--------------|--------------|
| 0.188900E+04 | 0.157942E+02 | 0.164914E+02 | 0.150970E+02 |
| 0.189000E+04 | 0.158065E+02 | 0.165063E+02 | 0.151066E+02 |
| 0.189100E+04 | 0.158166E+02 | 0.165180E+02 | 0.151151E+02 |
| 0.189200E+04 | 0.158250E+02 | 0.165274E+02 | 0.151225E+02 |
| 0.189300E+04 | 0.158321E+02 | 0.165352E+02 | 0.151289E+02 |
| 0.189400E+04 | 0.158382E+02 | 0.165418E+02 | 0.151346E+02 |
| 0.189500E+04 | 0.158436E+02 | 0.165476E+02 | 0.151395E+02 |
| 0.189600E+04 | 0.158483E+02 | 0.165527E+02 | 0.151439E+02 |
| 0.189700E+04 | 0.158525E+02 | 0.165572E+02 | 0.151477E+02 |
| 0.189800E+04 | 0.158561E+02 | 0.165611E+02 | 0.151511E+02 |
| 0.189900E+04 | 0.158594E+02 | 0.165647E+02 | 0.151541E+02 |
| 0.190000E+04 | 0.158623E+02 | 0.165678E+02 | 0.151568E+02 |
| 0.190100E+04 | 0.158650E+02 | 0.165708E+02 | 0.151593E+02 |
| 0.190200E+04 | 0.157377E+02 | 0.164306E+02 | 0.150448E+02 |
| 0.190300E+04 | 0.155938E+02 | 0.162649E+02 | 0.149226E+02 |
| 0.190400E+04 | 0.156473E+02 | 0.163193E+02 | 0.149752E+02 |
| 0.190500E+04 | 0.157298E+02 | 0.164144E+02 | 0.150453E+02 |
| 0.190600E+04 | 0.157707E+02 | 0.164608E+02 | 0.150805E+02 |
| 0.190700E+04 | 0.157759E+02 | 0.164711E+02 | 0.150807E+02 |
| 0.190800E+04 | 0.157786E+02 | 0.164776E+02 | 0.150795E+02 |
| 0.190900E+04 | 0.157986E+02 | 0.165017E+02 | 0.150956E+02 |
| 0.191000E+04 | 0.158145E+02 | 0.165202E+02 | 0.151089E+02 |
| 0.191100E+04 | 0.158253E+02 | 0.165321E+02 | 0.151185E+02 |
| 0.191200E+04 | 0.157526E+02 | 0.164464E+02 | 0.150588E+02 |
| 0.191300E+04 | 0.156824E+02 | 0.163731E+02 | 0.149917E+02 |
| 0.191400E+04 | 0.156819E+02 | 0.163721E+02 | 0.149918E+02 |
| 0.191500E+04 | 0.157149E+02 | 0.164106E+02 | 0.150193E+02 |
| 0.191600E+04 | 0.157597E+02 | 0.164597E+02 | 0.150598E+02 |
| 0.191700E+04 | 0.157870E+02 | 0.164891E+02 | 0.150850E+02 |
| 0.191800E+04 | 0.158047E+02 | 0.165085E+02 | 0.151009E+02 |
| 0.191900E+04 | 0.158181E+02 | 0.165229E+02 | 0.151132E+02 |
| 0.192000E+04 | 0.158288E+02 | 0.165343E+02 | 0.151234E+02 |
| 0.192100E+04 | 0.158377E+02 | 0.165434E+02 | 0.151320E+02 |
| 0.192200E+04 | 0.158453E+02 | 0.165511E+02 | 0.151395E+02 |
| 0.192300E+04 | 0.158519E+02 | 0.165576E+02 | 0.151462E+02 |
| 0.192400E+04 | 0.158066E+02 | 0.165036E+02 | 0.151095E+02 |
| 0.192500E+04 | 0.157542E+02 | 0.164409E+02 | 0.150675E+02 |
| 0.192600E+04 | 0.157741E+02 | 0.164642E+02 | 0.150841E+02 |
| 0.192700E+04 | 0.158113E+02 | 0.165087E+02 | 0.151140E+02 |
| 0.192800E+04 | 0.158326E+02 | 0.165338E+02 | 0.151314E+02 |
| 0.192900E+04 | 0.158452E+02 | 0.165480E+02 | 0.151424E+02 |
| 0.193000E+04 | 0.158545E+02 | 0.165580E+02 | 0.151510E+02 |
| 0.193100E+04 | 0.158618E+02 | 0.165656E+02 | 0.151580E+02 |
| 0.193200E+04 | 0.158678E+02 | 0.165716E+02 | 0.151640E+02 |
| 0.193300E+04 | 0.158729E+02 | 0.165766E+02 | 0.151693E+02 |
| 0.193400E+04 | 0.158774E+02 | 0.165808E+02 | 0.151739E+02 |
| 0.193500E+04 | 0.158812E+02 | 0.165844E+02 | 0.151781E+02 |
| 0.193600E+04 | 0.158847E+02 | 0.165875E+02 | 0.151820E+02 |
| 0.193700E+04 | 0.158879E+02 | 0.165903E+02 | 0.151855E+02 |
| 0.193800E+04 | 0.158908E+02 | 0.165929E+02 | 0.151888E+02 |
| 0.193900E+04 | 0.158935E+02 | 0.165952E+02 | 0.151918E+02 |
| 0.194000E+04 | 0.158961E+02 | 0.165974E+02 | 0.151947E+02 |
| 0.194100E+04 | 0.158985E+02 | 0.165995E+02 | 0.151974E+02 |
| 0.194200E+04 | 0.159007E+02 | 0.166014E+02 | 0.152000E+02 |

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| 0.194300E+04 | 0.159029E+02 | 0.166033E+02 | 0.152025E+02 |
| 0.194400E+04 | 0.159050E+02 | 0.166051E+02 | 0.152049E+02 |
| 0.194500E+04 | 0.159069E+02 | 0.166066E+02 | 0.152071E+02 |
| 0.194600E+04 | 0.159084E+02 | 0.166078E+02 | 0.152090E+02 |
| 0.194700E+04 | 0.159098E+02 | 0.166088E+02 | 0.152107E+02 |
| 0.194800E+04 | 0.159109E+02 | 0.166096E+02 | 0.152122E+02 |
| 0.194900E+04 | 0.159120E+02 | 0.166103E+02 | 0.152137E+02 |
| 0.195000E+04 | 0.159129E+02 | 0.166109E+02 | 0.152150E+02 |
| 0.195100E+04 | 0.159139E+02 | 0.166116E+02 | 0.152162E+02 |
| 0.195200E+04 | 0.159148E+02 | 0.166122E+02 | 0.152174E+02 |
| 0.195300E+04 | 0.159157E+02 | 0.166128E+02 | 0.152186E+02 |
| 0.195400E+04 | 0.159166E+02 | 0.166135E+02 | 0.152197E+02 |
| 0.195500E+04 | 0.159174E+02 | 0.166141E+02 | 0.152208E+02 |
| 0.195600E+04 | 0.159023E+02 | 0.165960E+02 | 0.152086E+02 |
| 0.195700E+04 | 0.158914E+02 | 0.165829E+02 | 0.151999E+02 |
| 0.195800E+04 | 0.159014E+02 | 0.165945E+02 | 0.152083E+02 |
| 0.195900E+04 | 0.159089E+02 | 0.166034E+02 | 0.152144E+02 |
| 0.196000E+04 | 0.159125E+02 | 0.166074E+02 | 0.152176E+02 |
| 0.196100E+04 | 0.158994E+02 | 0.165919E+02 | 0.152070E+02 |
| 0.196200E+04 | 0.158791E+02 | 0.165678E+02 | 0.151904E+02 |
| 0.196300E+04 | 0.158569E+02 | 0.165418E+02 | 0.151720E+02 |
| 0.196400E+04 | 0.158226E+02 | 0.165066E+02 | 0.151385E+02 |
| 0.196500E+04 | 0.157740E+02 | 0.164525E+02 | 0.150955E+02 |
| 0.196600E+04 | 0.157622E+02 | 0.164396E+02 | 0.150848E+02 |
| 0.196700E+04 | 0.157755E+02 | 0.164560E+02 | 0.150951E+02 |
| 0.196800E+04 | 0.157741E+02 | 0.164552E+02 | 0.150931E+02 |
| 0.196900E+04 | 0.157458E+02 | 0.164231E+02 | 0.150685E+02 |
| 0.197000E+04 | 0.157379E+02 | 0.164147E+02 | 0.150612E+02 |
| 0.197100E+04 | 0.157665E+02 | 0.164483E+02 | 0.150846E+02 |
| 0.197200E+04 | 0.157956E+02 | 0.164806E+02 | 0.151105E+02 |
| 0.197300E+04 | 0.158099E+02 | 0.164957E+02 | 0.151240E+02 |
| 0.197400E+04 | 0.158094E+02 | 0.164945E+02 | 0.151243E+02 |
| 0.197500E+04 | 0.157834E+02 | 0.164634E+02 | 0.151034E+02 |
| 0.197600E+04 | 0.157646E+02 | 0.164406E+02 | 0.150886E+02 |
| 0.197700E+04 | 0.157840E+02 | 0.164633E+02 | 0.151046E+02 |
| 0.197800E+04 | 0.158060E+02 | 0.164895E+02 | 0.151225E+02 |
| 0.197900E+04 | 0.158140E+02 | 0.164987E+02 | 0.151293E+02 |
| 0.198000E+04 | 0.158210E+02 | 0.165063E+02 | 0.151356E+02 |
| 0.198100E+04 | 0.158310E+02 | 0.165176E+02 | 0.151445E+02 |
| 0.198200E+04 | 0.157507E+02 | 0.164233E+02 | 0.150781E+02 |
| 0.198300E+04 | 0.155963E+02 | 0.162523E+02 | 0.149404E+02 |
| 0.198400E+04 | 0.155380E+02 | 0.161790E+02 | 0.148970E+02 |
| 0.198500E+04 | 0.157278E+02 | 0.162573E+02 | 0.151983E+02 |
| 0.198600E+04 | 0.158208E+02 | 0.163373E+02 | 0.153042E+02 |
| 0.198700E+04 | 0.158797E+02 | 0.163737E+02 | 0.153857E+02 |
| 0.198800E+04 | 0.159143E+02 | 0.164111E+02 | 0.154175E+02 |
| 0.198900E+04 | 0.159428E+02 | 0.164470E+02 | 0.154385E+02 |
| 0.199000E+04 | 0.159622E+02 | 0.164749E+02 | 0.154496E+02 |
| 0.199100E+04 | 0.159228E+02 | 0.164296E+02 | 0.154161E+02 |
| 0.199200E+04 | 0.157679E+02 | 0.162596E+02 | 0.152763E+02 |
| 0.199300E+04 | 0.156643E+02 | 0.161434E+02 | 0.151852E+02 |
| 0.199400E+04 | 0.157223E+02 | 0.162167E+02 | 0.152280E+02 |
| 0.199500E+04 | 0.157960E+02 | 0.163096E+02 | 0.152823E+02 |
| 0.199600E+04 | 0.158453E+02 | 0.163735E+02 | 0.153170E+02 |

0.199700E+04 0.158789E+02 0.164193E+02 0.153384E+02
0.199800E+04 0.159043E+02 0.164543E+02 0.153543E+02

```
#          91
# IPCC AR4 Millenium Runs output (vary solar forcing)
# ++++++
#
# Model: Bern2.5CC version with active C-cycle
# -----
# Prescribed forcing timeseries as described in file
# readme_doRuns_IPCC_Chap6_millennium_21jan06.txt
# provided by F. Joos, University of Bern.
#
# Contact:
# -----
# Gian-Kasper Plattner
# Climate and Environmental Physics
# Physics Institute, University of Bern
# Sidlerstrasse 5, CH-3012 Bern, Switzerland
# plattner@climate.unibe.ch
# http://www.climate.unibe.ch/~plattner/
# tel: ++41 (0)31 631-44-67
# fax: ++41 (0)31 631-87-42
#
# Some model setup informations:
# -----
# All runs with horizontal/vertical diffusion
#
# Run with standard ocean parameters
#   as used in Plattner et al. 2001/2002
#   with Kv (diffusivity) 4*10^-5 m2/s
#
# Climate sens. set to ~ 3.2 degrees C
# parameterized see Knutti et al. (Clim. Dyn. 2003)
#
# Model version is annual mean.
#
# No radiation code, CO2 radiative forcing calculated
# for as RF=5.35*ln(CO2/CO2_preind),
# Non-co2 radiative forcing prescribed according to
# Joos et al. GBC 2001 with updates for solar forcing
#
# More model description:
# -----
# Zonally averaged dynamical ocean with 3 basins and
# Southern Ocean, zonally averaged one layer energy
# and moisture balance atmosphere, thermodynamic
# sea ice (Stocker et al., J. Climate 1992).
#
# Carbon cycle components: Ocean/Atm/Terr.biosphere;
# Ocean carbon cycle is a description of the cycles
# of organic carbon and CaCO3 (Marchal et al., Tellus
# Tellus B), based on Redfield approach using PO4 as
# biolimiting nutrient.
```

```

#
# Land Biota: Lund-Jena-Postdam Dynamical Global
#           Vegetation Model (LPJ-DGVM)
# at GCM resolution (Gerber et al. 2003, Climate
# Dynamics; Sitch et al. 2003, Global Change Biology)
#
# LPJ forced by Cramer/Leemans annual mean
# climatology plus interannual climate variability
# from Hadley simulation (30-recycled climate) plus
# changes in the fields of surface temperature,
# precipitation, and cloudcover as simulated with the
# Impulse-EOF version of ECHAM-3/LSG in response to
# projected radiative forcing changes.
#
# Land use changes are not explicitly considered.
#
# Impact of climate change on terrestrial C-storage
# included
#
# References:
# -----
# Carbon cycle Ocean: Marchal et al., Tellus 1998
# Carbon cycle Terr. Bio: Sitch et al., GCB 2003
#           Gerber et al., Clim. Dyn. 2003
# Ccycle-climate feedbacks and global warming:
#           Plattner et al., Tellus 2001
#           Plattner et al., GCB 2002
# Non-CO2 forcing: Joos et al., GCB 2001
# Climate model: Stocker et al., J. Climate 1992
# Sea level: Knutti et al., J. Climate 2000
# Global warming Physics: Knutti et al., Nature 2002
#           Knutti et al., Cl. Dyn. 2003
#           and refs therein.
#
# Output columns:
# -----
# Time (yr AD)
# Global mean air temperature (deg C)
# NH-averaged air temperature (deg C)
# SH-averaged air temperature (deg C)
0.100100E+04 0.159155E+02 0.165835E+02 0.152475E+02
0.100200E+04 0.159209E+02 0.165892E+02 0.152525E+02
0.100300E+04 0.159252E+02 0.165938E+02 0.152567E+02
0.100400E+04 0.158977E+02 0.165611E+02 0.152344E+02
0.100500E+04 0.158655E+02 0.165220E+02 0.152089E+02
0.100600E+04 0.158774E+02 0.165361E+02 0.152187E+02
0.100700E+04 0.158992E+02 0.165626E+02 0.152358E+02
0.100800E+04 0.159109E+02 0.165768E+02 0.152449E+02
0.100900E+04 0.159171E+02 0.165843E+02 0.152500E+02
0.101000E+04 0.159213E+02 0.165891E+02 0.152535E+02
0.101100E+04 0.159242E+02 0.165924E+02 0.152560E+02
0.101200E+04 0.159263E+02 0.165946E+02 0.152579E+02
0.101300E+04 0.159279E+02 0.165964E+02 0.152593E+02
0.101400E+04 0.159292E+02 0.165979E+02 0.152606E+02

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| 0.101500E+04 | 0.158213E+02 | 0.164710E+02 | 0.151715E+02 |
| 0.101600E+04 | 0.157214E+02 | 0.163645E+02 | 0.150782E+02 |
| 0.101700E+04 | 0.157650E+02 | 0.164064E+02 | 0.151236E+02 |
| 0.101800E+04 | 0.158283E+02 | 0.164797E+02 | 0.151770E+02 |
| 0.101900E+04 | 0.158570E+02 | 0.165118E+02 | 0.152022E+02 |
| 0.102000E+04 | 0.158701E+02 | 0.165312E+02 | 0.152089E+02 |
| 0.102100E+04 | 0.158780E+02 | 0.165447E+02 | 0.152113E+02 |
| 0.102200E+04 | 0.158856E+02 | 0.165546E+02 | 0.152167E+02 |
| 0.102300E+04 | 0.158920E+02 | 0.165619E+02 | 0.152220E+02 |
| 0.102400E+04 | 0.158971E+02 | 0.165676E+02 | 0.152267E+02 |
| 0.102500E+04 | 0.159014E+02 | 0.165720E+02 | 0.152307E+02 |
| 0.102600E+04 | 0.157770E+02 | 0.164254E+02 | 0.151285E+02 |
| 0.102700E+04 | 0.156600E+02 | 0.162963E+02 | 0.150237E+02 |
| 0.102800E+04 | 0.157085E+02 | 0.163461E+02 | 0.150709E+02 |
| 0.102900E+04 | 0.157839E+02 | 0.164324E+02 | 0.151353E+02 |
| 0.103000E+04 | 0.158211E+02 | 0.164751E+02 | 0.151670E+02 |
| 0.103100E+04 | 0.158403E+02 | 0.164997E+02 | 0.151808E+02 |
| 0.103200E+04 | 0.158500E+02 | 0.165164E+02 | 0.151835E+02 |
| 0.103300E+04 | 0.158594E+02 | 0.165285E+02 | 0.151903E+02 |
| 0.103400E+04 | 0.158673E+02 | 0.165375E+02 | 0.151971E+02 |
| 0.103500E+04 | 0.158737E+02 | 0.165443E+02 | 0.152032E+02 |
| 0.103600E+04 | 0.158791E+02 | 0.165496E+02 | 0.152085E+02 |
| 0.103700E+04 | 0.158835E+02 | 0.165539E+02 | 0.152131E+02 |
| 0.103800E+04 | 0.158873E+02 | 0.165574E+02 | 0.152171E+02 |
| 0.103900E+04 | 0.158904E+02 | 0.165603E+02 | 0.152206E+02 |
| 0.104000E+04 | 0.158931E+02 | 0.165627E+02 | 0.152235E+02 |
| 0.104100E+04 | 0.158954E+02 | 0.165646E+02 | 0.152261E+02 |
| 0.104200E+04 | 0.158973E+02 | 0.165663E+02 | 0.152284E+02 |
| 0.104300E+04 | 0.158990E+02 | 0.165676E+02 | 0.152303E+02 |
| 0.104400E+04 | 0.159004E+02 | 0.165687E+02 | 0.152320E+02 |
| 0.104500E+04 | 0.159016E+02 | 0.165697E+02 | 0.152335E+02 |
| 0.104600E+04 | 0.159027E+02 | 0.165706E+02 | 0.152348E+02 |
| 0.104700E+04 | 0.159038E+02 | 0.165715E+02 | 0.152361E+02 |
| 0.104800E+04 | 0.159047E+02 | 0.165722E+02 | 0.152372E+02 |
| 0.104900E+04 | 0.159055E+02 | 0.165729E+02 | 0.152382E+02 |
| 0.105000E+04 | 0.159063E+02 | 0.165735E+02 | 0.152392E+02 |
| 0.105100E+04 | 0.159070E+02 | 0.165740E+02 | 0.152400E+02 |
| 0.105200E+04 | 0.159077E+02 | 0.165745E+02 | 0.152409E+02 |
| 0.105300E+04 | 0.159083E+02 | 0.165750E+02 | 0.152416E+02 |
| 0.105400E+04 | 0.159089E+02 | 0.165754E+02 | 0.152423E+02 |
| 0.105500E+04 | 0.159095E+02 | 0.165759E+02 | 0.152431E+02 |
| 0.105600E+04 | 0.159101E+02 | 0.165764E+02 | 0.152438E+02 |
| 0.105700E+04 | 0.159107E+02 | 0.165769E+02 | 0.152445E+02 |
| 0.105800E+04 | 0.157526E+02 | 0.163976E+02 | 0.151075E+02 |
| 0.105900E+04 | 0.155681E+02 | 0.161824E+02 | 0.149539E+02 |
| 0.106000E+04 | 0.157024E+02 | 0.162482E+02 | 0.151566E+02 |
| 0.106100E+04 | 0.158714E+02 | 0.163711E+02 | 0.153716E+02 |
| 0.106200E+04 | 0.159064E+02 | 0.163799E+02 | 0.154328E+02 |
| 0.106300E+04 | 0.158912E+02 | 0.163588E+02 | 0.154235E+02 |
| 0.106400E+04 | 0.159282E+02 | 0.164062E+02 | 0.154501E+02 |
| 0.106500E+04 | 0.159701E+02 | 0.164636E+02 | 0.154766E+02 |
| 0.106600E+04 | 0.159940E+02 | 0.164998E+02 | 0.154882E+02 |
| 0.106700E+04 | 0.160082E+02 | 0.165240E+02 | 0.154924E+02 |
| 0.106800E+04 | 0.160205E+02 | 0.165424E+02 | 0.154986E+02 |

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| 0.106900E+04 | 0.160272E+02 | 0.165572E+02 | 0.154971E+02 |
| 0.107000E+04 | 0.160326E+02 | 0.165692E+02 | 0.154960E+02 |
| 0.107100E+04 | 0.160368E+02 | 0.165792E+02 | 0.154944E+02 |
| 0.107200E+04 | 0.160401E+02 | 0.165874E+02 | 0.154927E+02 |
| 0.107300E+04 | 0.160427E+02 | 0.165944E+02 | 0.154910E+02 |
| 0.107400E+04 | 0.160449E+02 | 0.166004E+02 | 0.154894E+02 |
| 0.107500E+04 | 0.160467E+02 | 0.166055E+02 | 0.154880E+02 |
| 0.107600E+04 | 0.160483E+02 | 0.166098E+02 | 0.154867E+02 |
| 0.107700E+04 | 0.160495E+02 | 0.166134E+02 | 0.154855E+02 |
| 0.107800E+04 | 0.160504E+02 | 0.166164E+02 | 0.154844E+02 |
| 0.107900E+04 | 0.160513E+02 | 0.166191E+02 | 0.154835E+02 |
| 0.108000E+04 | 0.160218E+02 | 0.165851E+02 | 0.154584E+02 |
| 0.108100E+04 | 0.159894E+02 | 0.165469E+02 | 0.154319E+02 |
| 0.108200E+04 | 0.160000E+02 | 0.165607E+02 | 0.154393E+02 |
| 0.108300E+04 | 0.160202E+02 | 0.165871E+02 | 0.154533E+02 |
| 0.108400E+04 | 0.160222E+02 | 0.166017E+02 | 0.154427E+02 |
| 0.108500E+04 | 0.160174E+02 | 0.166096E+02 | 0.154252E+02 |
| 0.108600E+04 | 0.160121E+02 | 0.166147E+02 | 0.154095E+02 |
| 0.108700E+04 | 0.159660E+02 | 0.166182E+02 | 0.153138E+02 |
| 0.108800E+04 | 0.159316E+02 | 0.166197E+02 | 0.152435E+02 |
| 0.108900E+04 | 0.159111E+02 | 0.166198E+02 | 0.152025E+02 |
| 0.109000E+04 | 0.158969E+02 | 0.166188E+02 | 0.151750E+02 |
| 0.109100E+04 | 0.158874E+02 | 0.166175E+02 | 0.151574E+02 |
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| 0.109300E+04 | 0.158765E+02 | 0.166143E+02 | 0.151387E+02 |
| 0.109400E+04 | 0.158754E+02 | 0.166129E+02 | 0.151380E+02 |
| 0.109500E+04 | 0.158763E+02 | 0.166119E+02 | 0.151407E+02 |
| 0.109600E+04 | 0.158786E+02 | 0.166114E+02 | 0.151459E+02 |
| 0.109700E+04 | 0.158099E+02 | 0.165273E+02 | 0.150926E+02 |
| 0.109800E+04 | 0.157483E+02 | 0.164610E+02 | 0.150355E+02 |
| 0.109900E+04 | 0.157746E+02 | 0.164875E+02 | 0.150618E+02 |
| 0.110000E+04 | 0.158230E+02 | 0.165395E+02 | 0.151065E+02 |
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| 0.110300E+04 | 0.158743E+02 | 0.165824E+02 | 0.151662E+02 |
| 0.110400E+04 | 0.158852E+02 | 0.165912E+02 | 0.151792E+02 |
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| 0.110800E+04 | 0.159182E+02 | 0.166169E+02 | 0.152195E+02 |
| 0.110900E+04 | 0.159249E+02 | 0.166223E+02 | 0.152274E+02 |
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| 0.111100E+04 | 0.159377E+02 | 0.166333E+02 | 0.152421E+02 |
| 0.111200E+04 | 0.159437E+02 | 0.166387E+02 | 0.152487E+02 |
| 0.111300E+04 | 0.159495E+02 | 0.166441E+02 | 0.152550E+02 |
| 0.111400E+04 | 0.159551E+02 | 0.166494E+02 | 0.152609E+02 |
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| 0.111600E+04 | 0.159649E+02 | 0.166588E+02 | 0.152710E+02 |
| 0.111700E+04 | 0.159691E+02 | 0.166628E+02 | 0.152753E+02 |
| 0.111800E+04 | 0.159729E+02 | 0.166667E+02 | 0.152792E+02 |
| 0.111900E+04 | 0.159765E+02 | 0.166703E+02 | 0.152828E+02 |
| 0.112000E+04 | 0.159799E+02 | 0.166737E+02 | 0.152861E+02 |
| 0.112100E+04 | 0.159831E+02 | 0.166770E+02 | 0.152891E+02 |
| 0.112200E+04 | 0.159861E+02 | 0.166802E+02 | 0.152919E+02 |

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| 0.112300E+04 | 0.159889E+02 | 0.166833E+02 | 0.152946E+02 |
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| 0.112500E+04 | 0.159942E+02 | 0.166891E+02 | 0.152993E+02 |
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| 0.112900E+04 | 0.160036E+02 | 0.166997E+02 | 0.153074E+02 |
| 0.113000E+04 | 0.160057E+02 | 0.167022E+02 | 0.153092E+02 |
| 0.113100E+04 | 0.160078E+02 | 0.167046E+02 | 0.153109E+02 |
| 0.113200E+04 | 0.160097E+02 | 0.167069E+02 | 0.153126E+02 |
| 0.113300E+04 | 0.160118E+02 | 0.167093E+02 | 0.153142E+02 |
| 0.113400E+04 | 0.160140E+02 | 0.167119E+02 | 0.153160E+02 |
| 0.113500E+04 | 0.160161E+02 | 0.167144E+02 | 0.153178E+02 |
| 0.113600E+04 | 0.160182E+02 | 0.167169E+02 | 0.153195E+02 |
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| 0.113800E+04 | 0.160222E+02 | 0.167215E+02 | 0.153228E+02 |
| 0.113900E+04 | 0.160240E+02 | 0.167236E+02 | 0.153244E+02 |
| 0.114000E+04 | 0.160256E+02 | 0.167255E+02 | 0.153258E+02 |
| 0.114100E+04 | 0.160271E+02 | 0.167272E+02 | 0.153270E+02 |
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| 0.114300E+04 | 0.160298E+02 | 0.167303E+02 | 0.153294E+02 |
| 0.114400E+04 | 0.160311E+02 | 0.167317E+02 | 0.153304E+02 |
| 0.114500E+04 | 0.160322E+02 | 0.167330E+02 | 0.153315E+02 |
| 0.114600E+04 | 0.160333E+02 | 0.167342E+02 | 0.153325E+02 |
| 0.114700E+04 | 0.160343E+02 | 0.167353E+02 | 0.153333E+02 |
| 0.114800E+04 | 0.160351E+02 | 0.167361E+02 | 0.153341E+02 |
| 0.114900E+04 | 0.160358E+02 | 0.167368E+02 | 0.153347E+02 |
| 0.115000E+04 | 0.160363E+02 | 0.167373E+02 | 0.153353E+02 |
| 0.115100E+04 | 0.160368E+02 | 0.167377E+02 | 0.153358E+02 |
| 0.115200E+04 | 0.160372E+02 | 0.167381E+02 | 0.153363E+02 |
| 0.115300E+04 | 0.160375E+02 | 0.167383E+02 | 0.153366E+02 |
| 0.115400E+04 | 0.160377E+02 | 0.167385E+02 | 0.153370E+02 |
| 0.115500E+04 | 0.160379E+02 | 0.167386E+02 | 0.153373E+02 |
| 0.115600E+04 | 0.160380E+02 | 0.167385E+02 | 0.153375E+02 |
| 0.115700E+04 | 0.160379E+02 | 0.167382E+02 | 0.153376E+02 |
| 0.115800E+04 | 0.160376E+02 | 0.167377E+02 | 0.153375E+02 |
| 0.115900E+04 | 0.160372E+02 | 0.167371E+02 | 0.153373E+02 |
| 0.116000E+04 | 0.160367E+02 | 0.167363E+02 | 0.153371E+02 |
| 0.116100E+04 | 0.160362E+02 | 0.167355E+02 | 0.153368E+02 |
| 0.116200E+04 | 0.160358E+02 | 0.167349E+02 | 0.153367E+02 |
| 0.116300E+04 | 0.160357E+02 | 0.167346E+02 | 0.153368E+02 |
| 0.116400E+04 | 0.160358E+02 | 0.167345E+02 | 0.153370E+02 |
| 0.116500E+04 | 0.160359E+02 | 0.167345E+02 | 0.153374E+02 |
| 0.116600E+04 | 0.159930E+02 | 0.166837E+02 | 0.153022E+02 |
| 0.116700E+04 | 0.159464E+02 | 0.166292E+02 | 0.152636E+02 |
| 0.116800E+04 | 0.159600E+02 | 0.166457E+02 | 0.152742E+02 |
| 0.116900E+04 | 0.159866E+02 | 0.166781E+02 | 0.152951E+02 |
| 0.117000E+04 | 0.160004E+02 | 0.166949E+02 | 0.153060E+02 |
| 0.117100E+04 | 0.160074E+02 | 0.167030E+02 | 0.153119E+02 |
| 0.117200E+04 | 0.160119E+02 | 0.167079E+02 | 0.153159E+02 |
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| 0.117900E+04 | 0.159583E+02 | 0.164437E+02 | 0.154728E+02 |
| 0.118000E+04 | 0.160115E+02 | 0.165076E+02 | 0.155154E+02 |
| 0.118100E+04 | 0.160423E+02 | 0.165479E+02 | 0.155367E+02 |
| 0.118200E+04 | 0.160629E+02 | 0.165776E+02 | 0.155483E+02 |
| 0.118300E+04 | 0.160774E+02 | 0.166006E+02 | 0.155543E+02 |
| 0.118400E+04 | 0.160901E+02 | 0.166188E+02 | 0.155613E+02 |
| 0.118500E+04 | 0.160976E+02 | 0.166333E+02 | 0.155619E+02 |
| 0.118600E+04 | 0.161031E+02 | 0.166448E+02 | 0.155615E+02 |
| 0.118700E+04 | 0.161071E+02 | 0.166539E+02 | 0.155602E+02 |
| 0.118800E+04 | 0.161099E+02 | 0.166612E+02 | 0.155586E+02 |
| 0.118900E+04 | 0.161334E+02 | 0.167069E+02 | 0.155598E+02 |
| 0.119000E+04 | 0.161327E+02 | 0.167065E+02 | 0.155588E+02 |
| 0.119100E+04 | 0.161322E+02 | 0.167074E+02 | 0.155571E+02 |
| 0.119200E+04 | 0.161319E+02 | 0.167084E+02 | 0.155553E+02 |
| 0.119300E+04 | 0.161240E+02 | 0.167094E+02 | 0.155386E+02 |
| 0.119400E+04 | 0.160214E+02 | 0.166002E+02 | 0.154426E+02 |
| 0.119500E+04 | 0.159266E+02 | 0.165069E+02 | 0.153463E+02 |
| 0.119600E+04 | 0.159470E+02 | 0.165403E+02 | 0.153538E+02 |
| 0.119700E+04 | 0.159938E+02 | 0.166044E+02 | 0.153832E+02 |
| 0.119800E+04 | 0.159733E+02 | 0.166301E+02 | 0.153165E+02 |
| 0.119900E+04 | 0.159482E+02 | 0.166444E+02 | 0.152520E+02 |
| 0.120000E+04 | 0.159354E+02 | 0.166526E+02 | 0.152182E+02 |
| 0.120100E+04 | 0.159268E+02 | 0.166569E+02 | 0.151968E+02 |
| 0.120200E+04 | 0.159216E+02 | 0.166589E+02 | 0.151843E+02 |
| 0.120300E+04 | 0.159191E+02 | 0.166595E+02 | 0.151786E+02 |
| 0.120400E+04 | 0.159166E+02 | 0.166591E+02 | 0.151741E+02 |
| 0.120500E+04 | 0.158664E+02 | 0.165991E+02 | 0.151337E+02 |
| 0.120600E+04 | 0.158113E+02 | 0.165312E+02 | 0.150915E+02 |
| 0.120700E+04 | 0.158427E+02 | 0.165735E+02 | 0.151118E+02 |
| 0.120800E+04 | 0.158776E+02 | 0.166097E+02 | 0.151455E+02 |
| 0.120900E+04 | 0.158981E+02 | 0.166288E+02 | 0.151674E+02 |
| 0.121000E+04 | 0.159110E+02 | 0.166387E+02 | 0.151834E+02 |
| 0.121100E+04 | 0.159211E+02 | 0.166451E+02 | 0.151971E+02 |
| 0.121200E+04 | 0.159295E+02 | 0.166497E+02 | 0.152093E+02 |
| 0.121300E+04 | 0.159368E+02 | 0.166532E+02 | 0.152203E+02 |
| 0.121400E+04 | 0.159412E+02 | 0.166523E+02 | 0.152302E+02 |
| 0.121500E+04 | 0.159417E+02 | 0.166451E+02 | 0.152383E+02 |
| 0.121600E+04 | 0.159454E+02 | 0.166451E+02 | 0.152458E+02 |
| 0.121700E+04 | 0.159497E+02 | 0.166466E+02 | 0.152527E+02 |
| 0.121800E+04 | 0.159538E+02 | 0.166485E+02 | 0.152591E+02 |
| 0.121900E+04 | 0.159578E+02 | 0.166506E+02 | 0.152649E+02 |
| 0.122000E+04 | 0.159615E+02 | 0.166528E+02 | 0.152702E+02 |
| 0.122100E+04 | 0.159650E+02 | 0.166550E+02 | 0.152751E+02 |
| 0.122200E+04 | 0.159684E+02 | 0.166573E+02 | 0.152794E+02 |
| 0.122300E+04 | 0.159715E+02 | 0.166595E+02 | 0.152834E+02 |
| 0.122400E+04 | 0.159744E+02 | 0.166618E+02 | 0.152871E+02 |
| 0.122500E+04 | 0.159772E+02 | 0.166640E+02 | 0.152904E+02 |
| 0.122600E+04 | 0.159798E+02 | 0.166662E+02 | 0.152934E+02 |
| 0.122700E+04 | 0.159512E+02 | 0.166321E+02 | 0.152703E+02 |
| 0.122800E+04 | 0.159185E+02 | 0.165928E+02 | 0.152442E+02 |
| 0.122900E+04 | 0.157488E+02 | 0.164060E+02 | 0.150916E+02 |
| 0.123000E+04 | 0.155640E+02 | 0.161868E+02 | 0.149411E+02 |

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| 0.123100E+04 | 0.157756E+02 | 0.162876E+02 | 0.152636E+02 |
| 0.123200E+04 | 0.159056E+02 | 0.163980E+02 | 0.154133E+02 |
| 0.123300E+04 | 0.159822E+02 | 0.164718E+02 | 0.154927E+02 |
| 0.123400E+04 | 0.160216E+02 | 0.165128E+02 | 0.155304E+02 |
| 0.123500E+04 | 0.160469E+02 | 0.165448E+02 | 0.155489E+02 |
| 0.123600E+04 | 0.160638E+02 | 0.165696E+02 | 0.155579E+02 |
| 0.123700E+04 | 0.160753E+02 | 0.165892E+02 | 0.155615E+02 |
| 0.123800E+04 | 0.160834E+02 | 0.166051E+02 | 0.155618E+02 |
| 0.123900E+04 | 0.160918E+02 | 0.166181E+02 | 0.155654E+02 |
| 0.124000E+04 | 0.160957E+02 | 0.166290E+02 | 0.155623E+02 |
| 0.124100E+04 | 0.160987E+02 | 0.166380E+02 | 0.155594E+02 |
| 0.124200E+04 | 0.161230E+02 | 0.166886E+02 | 0.155574E+02 |
| 0.124300E+04 | 0.161230E+02 | 0.166895E+02 | 0.155566E+02 |
| 0.124400E+04 | 0.161223E+02 | 0.166907E+02 | 0.155539E+02 |
| 0.124500E+04 | 0.161217E+02 | 0.166926E+02 | 0.155509E+02 |
| 0.124600E+04 | 0.161211E+02 | 0.166942E+02 | 0.155479E+02 |
| 0.124700E+04 | 0.161202E+02 | 0.166954E+02 | 0.155449E+02 |
| 0.124800E+04 | 0.161119E+02 | 0.166962E+02 | 0.155277E+02 |
| 0.124900E+04 | 0.161002E+02 | 0.166964E+02 | 0.155039E+02 |
| 0.125000E+04 | 0.160896E+02 | 0.166961E+02 | 0.154831E+02 |
| 0.125100E+04 | 0.160803E+02 | 0.166953E+02 | 0.154653E+02 |
| 0.125200E+04 | 0.160221E+02 | 0.166938E+02 | 0.153503E+02 |
| 0.125300E+04 | 0.159889E+02 | 0.166911E+02 | 0.152866E+02 |
| 0.125400E+04 | 0.159666E+02 | 0.166870E+02 | 0.152462E+02 |
| 0.125500E+04 | 0.159499E+02 | 0.166818E+02 | 0.152181E+02 |
| 0.125600E+04 | 0.159376E+02 | 0.166759E+02 | 0.151992E+02 |
| 0.125700E+04 | 0.159286E+02 | 0.166699E+02 | 0.151873E+02 |
| 0.125800E+04 | 0.159206E+02 | 0.166642E+02 | 0.151771E+02 |
| 0.125900E+04 | 0.154807E+02 | 0.161494E+02 | 0.148121E+02 |
| 0.126000E+04 | 0.152572E+02 | 0.160683E+02 | 0.144461E+02 |
| 0.126100E+04 | 0.154588E+02 | 0.163821E+02 | 0.145355E+02 |
| 0.126200E+04 | 0.156558E+02 | 0.165738E+02 | 0.147379E+02 |
| 0.126300E+04 | 0.157923E+02 | 0.166953E+02 | 0.148893E+02 |
| 0.126400E+04 | 0.158568E+02 | 0.167406E+02 | 0.149730E+02 |
| 0.126500E+04 | 0.158900E+02 | 0.167545E+02 | 0.150255E+02 |
| 0.126600E+04 | 0.159115E+02 | 0.167589E+02 | 0.150640E+02 |
| 0.126700E+04 | 0.159227E+02 | 0.167575E+02 | 0.150880E+02 |
| 0.126800E+04 | 0.159307E+02 | 0.167536E+02 | 0.151077E+02 |
| 0.126900E+04 | 0.159367E+02 | 0.167486E+02 | 0.151247E+02 |
| 0.127000E+04 | 0.159415E+02 | 0.167434E+02 | 0.151395E+02 |
| 0.127100E+04 | 0.159453E+02 | 0.167383E+02 | 0.151523E+02 |
| 0.127200E+04 | 0.159485E+02 | 0.167336E+02 | 0.151634E+02 |
| 0.127300E+04 | 0.159510E+02 | 0.167292E+02 | 0.151729E+02 |
| 0.127400E+04 | 0.159530E+02 | 0.167251E+02 | 0.151809E+02 |
| 0.127500E+04 | 0.158544E+02 | 0.166053E+02 | 0.151035E+02 |
| 0.127600E+04 | 0.157431E+02 | 0.164816E+02 | 0.150046E+02 |
| 0.127700E+04 | 0.157788E+02 | 0.165214E+02 | 0.150362E+02 |
| 0.127800E+04 | 0.158412E+02 | 0.165867E+02 | 0.150956E+02 |
| 0.127900E+04 | 0.158710E+02 | 0.166153E+02 | 0.151268E+02 |
| 0.128000E+04 | 0.158755E+02 | 0.166076E+02 | 0.151434E+02 |
| 0.128100E+04 | 0.158771E+02 | 0.165999E+02 | 0.151544E+02 |
| 0.128200E+04 | 0.158361E+02 | 0.165112E+02 | 0.151610E+02 |
| 0.128300E+04 | 0.158180E+02 | 0.164730E+02 | 0.151631E+02 |
| 0.128400E+04 | 0.158099E+02 | 0.164557E+02 | 0.151641E+02 |

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| 0.128500E+04 | 0.156680E+02 | 0.162921E+02 | 0.150440E+02 |
| 0.128600E+04 | 0.155212E+02 | 0.161200E+02 | 0.149223E+02 |
| 0.128700E+04 | 0.156037E+02 | 0.162401E+02 | 0.149673E+02 |
| 0.128800E+04 | 0.158194E+02 | 0.163373E+02 | 0.153015E+02 |
| 0.128900E+04 | 0.158806E+02 | 0.163862E+02 | 0.153750E+02 |
| 0.129000E+04 | 0.159281E+02 | 0.164155E+02 | 0.154407E+02 |
| 0.129100E+04 | 0.159485E+02 | 0.164389E+02 | 0.154580E+02 |
| 0.129200E+04 | 0.159630E+02 | 0.164605E+02 | 0.154654E+02 |
| 0.129300E+04 | 0.159730E+02 | 0.164790E+02 | 0.154671E+02 |
| 0.129400E+04 | 0.159836E+02 | 0.164952E+02 | 0.154720E+02 |
| 0.129500E+04 | 0.158516E+02 | 0.163463E+02 | 0.153569E+02 |
| 0.129600E+04 | 0.157273E+02 | 0.162135E+02 | 0.152410E+02 |
| 0.129700E+04 | 0.157762E+02 | 0.162788E+02 | 0.152737E+02 |
| 0.129800E+04 | 0.158534E+02 | 0.163750E+02 | 0.153318E+02 |
| 0.129900E+04 | 0.158826E+02 | 0.164088E+02 | 0.153563E+02 |
| 0.130000E+04 | 0.159022E+02 | 0.164376E+02 | 0.153668E+02 |
| 0.130100E+04 | 0.159166E+02 | 0.164602E+02 | 0.153729E+02 |
| 0.130200E+04 | 0.159277E+02 | 0.164786E+02 | 0.153768E+02 |
| 0.130300E+04 | 0.159365E+02 | 0.164937E+02 | 0.153794E+02 |
| 0.130400E+04 | 0.159437E+02 | 0.165063E+02 | 0.153812E+02 |
| 0.130500E+04 | 0.159498E+02 | 0.165171E+02 | 0.153825E+02 |
| 0.130600E+04 | 0.159550E+02 | 0.165265E+02 | 0.153835E+02 |
| 0.130700E+04 | 0.159595E+02 | 0.165348E+02 | 0.153843E+02 |
| 0.130800E+04 | 0.159634E+02 | 0.165420E+02 | 0.153849E+02 |
| 0.130900E+04 | 0.159669E+02 | 0.165483E+02 | 0.153854E+02 |
| 0.131000E+04 | 0.159925E+02 | 0.165984E+02 | 0.153867E+02 |
| 0.131100E+04 | 0.159960E+02 | 0.166017E+02 | 0.153903E+02 |
| 0.131200E+04 | 0.159968E+02 | 0.166020E+02 | 0.153916E+02 |
| 0.131300E+04 | 0.159915E+02 | 0.166036E+02 | 0.153793E+02 |
| 0.131400E+04 | 0.159816E+02 | 0.166054E+02 | 0.153578E+02 |
| 0.131500E+04 | 0.159730E+02 | 0.166070E+02 | 0.153391E+02 |
| 0.131600E+04 | 0.159365E+02 | 0.166080E+02 | 0.152649E+02 |
| 0.131700E+04 | 0.158919E+02 | 0.166079E+02 | 0.151758E+02 |
| 0.131800E+04 | 0.158676E+02 | 0.166061E+02 | 0.151291E+02 |
| 0.131900E+04 | 0.158506E+02 | 0.166035E+02 | 0.150976E+02 |
| 0.132000E+04 | 0.158387E+02 | 0.166006E+02 | 0.150767E+02 |
| 0.132100E+04 | 0.158288E+02 | 0.165975E+02 | 0.150601E+02 |
| 0.132200E+04 | 0.158235E+02 | 0.165944E+02 | 0.150526E+02 |
| 0.132300E+04 | 0.158210E+02 | 0.165916E+02 | 0.150504E+02 |
| 0.132400E+04 | 0.158207E+02 | 0.165893E+02 | 0.150520E+02 |
| 0.132500E+04 | 0.158220E+02 | 0.165876E+02 | 0.150563E+02 |
| 0.132600E+04 | 0.158244E+02 | 0.165863E+02 | 0.150624E+02 |
| 0.132700E+04 | 0.158276E+02 | 0.165856E+02 | 0.150696E+02 |
| 0.132800E+04 | 0.158312E+02 | 0.165849E+02 | 0.150774E+02 |
| 0.132900E+04 | 0.157124E+02 | 0.164412E+02 | 0.149837E+02 |
| 0.133000E+04 | 0.156000E+02 | 0.163193E+02 | 0.148807E+02 |
| 0.133100E+04 | 0.156443E+02 | 0.163665E+02 | 0.149222E+02 |
| 0.133200E+04 | 0.157169E+02 | 0.164435E+02 | 0.149904E+02 |
| 0.133300E+04 | 0.157558E+02 | 0.164836E+02 | 0.150280E+02 |
| 0.133400E+04 | 0.157793E+02 | 0.165071E+02 | 0.150516E+02 |
| 0.133500E+04 | 0.157970E+02 | 0.165239E+02 | 0.150701E+02 |
| 0.133600E+04 | 0.158117E+02 | 0.165374E+02 | 0.150860E+02 |
| 0.133700E+04 | 0.158245E+02 | 0.165490E+02 | 0.151000E+02 |
| 0.133800E+04 | 0.158357E+02 | 0.165590E+02 | 0.151124E+02 |

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| 0.133900E+04 | 0.158458E+02 | 0.165679E+02 | 0.151236E+02 |
| 0.134000E+04 | 0.158546E+02 | 0.165757E+02 | 0.151335E+02 |
| 0.134100E+04 | 0.158623E+02 | 0.165823E+02 | 0.151422E+02 |
| 0.134200E+04 | 0.158690E+02 | 0.165880E+02 | 0.151499E+02 |
| 0.134300E+04 | 0.158749E+02 | 0.165931E+02 | 0.151568E+02 |
| 0.134400E+04 | 0.158803E+02 | 0.165976E+02 | 0.151630E+02 |
| 0.134500E+04 | 0.157810E+02 | 0.164796E+02 | 0.150824E+02 |
| 0.134600E+04 | 0.156860E+02 | 0.163782E+02 | 0.149939E+02 |
| 0.134700E+04 | 0.157263E+02 | 0.164183E+02 | 0.150343E+02 |
| 0.134800E+04 | 0.157903E+02 | 0.164873E+02 | 0.150932E+02 |
| 0.134900E+04 | 0.158240E+02 | 0.165247E+02 | 0.151232E+02 |
| 0.135000E+04 | 0.158390E+02 | 0.165469E+02 | 0.151311E+02 |
| 0.135100E+04 | 0.158509E+02 | 0.165625E+02 | 0.151392E+02 |
| 0.135200E+04 | 0.158613E+02 | 0.165743E+02 | 0.151483E+02 |
| 0.135300E+04 | 0.158701E+02 | 0.165835E+02 | 0.151567E+02 |
| 0.135400E+04 | 0.158775E+02 | 0.165910E+02 | 0.151640E+02 |
| 0.135500E+04 | 0.158838E+02 | 0.165971E+02 | 0.151705E+02 |
| 0.135600E+04 | 0.158893E+02 | 0.166023E+02 | 0.151763E+02 |
| 0.135700E+04 | 0.158942E+02 | 0.166068E+02 | 0.151815E+02 |
| 0.135800E+04 | 0.158986E+02 | 0.166109E+02 | 0.151863E+02 |
| 0.135900E+04 | 0.159026E+02 | 0.166145E+02 | 0.151906E+02 |
| 0.136000E+04 | 0.159062E+02 | 0.166178E+02 | 0.151946E+02 |
| 0.136100E+04 | 0.159095E+02 | 0.166209E+02 | 0.151982E+02 |
| 0.136200E+04 | 0.159126E+02 | 0.166236E+02 | 0.152016E+02 |
| 0.136300E+04 | 0.159155E+02 | 0.166262E+02 | 0.152047E+02 |
| 0.136400E+04 | 0.159181E+02 | 0.166286E+02 | 0.152076E+02 |
| 0.136500E+04 | 0.159206E+02 | 0.166308E+02 | 0.152104E+02 |
| 0.136600E+04 | 0.159229E+02 | 0.166328E+02 | 0.152130E+02 |
| 0.136700E+04 | 0.159251E+02 | 0.166348E+02 | 0.152155E+02 |
| 0.136800E+04 | 0.159272E+02 | 0.166366E+02 | 0.152178E+02 |
| 0.136900E+04 | 0.159292E+02 | 0.166383E+02 | 0.152201E+02 |
| 0.137000E+04 | 0.159311E+02 | 0.166400E+02 | 0.152223E+02 |
| 0.137100E+04 | 0.159330E+02 | 0.166415E+02 | 0.152244E+02 |
| 0.137200E+04 | 0.159347E+02 | 0.166430E+02 | 0.152265E+02 |
| 0.137300E+04 | 0.159364E+02 | 0.166444E+02 | 0.152285E+02 |
| 0.137400E+04 | 0.159380E+02 | 0.166457E+02 | 0.152303E+02 |
| 0.137500E+04 | 0.158966E+02 | 0.165967E+02 | 0.151964E+02 |
| 0.137600E+04 | 0.158502E+02 | 0.165415E+02 | 0.151589E+02 |
| 0.137700E+04 | 0.158650E+02 | 0.165592E+02 | 0.151707E+02 |
| 0.137800E+04 | 0.158934E+02 | 0.165937E+02 | 0.151931E+02 |
| 0.137900E+04 | 0.159088E+02 | 0.166121E+02 | 0.152055E+02 |
| 0.138000E+04 | 0.159172E+02 | 0.166217E+02 | 0.152128E+02 |
| 0.138100E+04 | 0.159231E+02 | 0.166279E+02 | 0.152182E+02 |
| 0.138200E+04 | 0.159275E+02 | 0.166324E+02 | 0.152227E+02 |
| 0.138300E+04 | 0.159311E+02 | 0.166357E+02 | 0.152264E+02 |
| 0.138400E+04 | 0.159339E+02 | 0.166382E+02 | 0.152295E+02 |
| 0.138500E+04 | 0.159361E+02 | 0.166400E+02 | 0.152322E+02 |
| 0.138600E+04 | 0.159378E+02 | 0.166412E+02 | 0.152344E+02 |
| 0.138700E+04 | 0.158964E+02 | 0.165921E+02 | 0.152007E+02 |
| 0.138800E+04 | 0.158500E+02 | 0.165366E+02 | 0.151633E+02 |
| 0.138900E+04 | 0.158645E+02 | 0.165539E+02 | 0.151751E+02 |
| 0.139000E+04 | 0.158926E+02 | 0.165878E+02 | 0.151973E+02 |
| 0.139100E+04 | 0.159075E+02 | 0.166056E+02 | 0.152094E+02 |
| 0.139200E+04 | 0.159154E+02 | 0.166145E+02 | 0.152164E+02 |

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| 0.139300E+04 | 0.159206E+02 | 0.166199E+02 | 0.152213E+02 |
| 0.139400E+04 | 0.159242E+02 | 0.166233E+02 | 0.152250E+02 |
| 0.139500E+04 | 0.159268E+02 | 0.166256E+02 | 0.152280E+02 |
| 0.139600E+04 | 0.159285E+02 | 0.166268E+02 | 0.152302E+02 |
| 0.139700E+04 | 0.159293E+02 | 0.166270E+02 | 0.152317E+02 |
| 0.139800E+04 | 0.159296E+02 | 0.166266E+02 | 0.152327E+02 |
| 0.139900E+04 | 0.159295E+02 | 0.166257E+02 | 0.152332E+02 |
| 0.140000E+04 | 0.159290E+02 | 0.166246E+02 | 0.152335E+02 |
| 0.140100E+04 | 0.159285E+02 | 0.166234E+02 | 0.152336E+02 |
| 0.140200E+04 | 0.159281E+02 | 0.166224E+02 | 0.152338E+02 |
| 0.140300E+04 | 0.159278E+02 | 0.166215E+02 | 0.152341E+02 |
| 0.140400E+04 | 0.159274E+02 | 0.166206E+02 | 0.152342E+02 |
| 0.140500E+04 | 0.159270E+02 | 0.166197E+02 | 0.152344E+02 |
| 0.140600E+04 | 0.159266E+02 | 0.166188E+02 | 0.152345E+02 |
| 0.140700E+04 | 0.159262E+02 | 0.166178E+02 | 0.152345E+02 |
| 0.140800E+04 | 0.158868E+02 | 0.165715E+02 | 0.152022E+02 |
| 0.140900E+04 | 0.158420E+02 | 0.165183E+02 | 0.151656E+02 |
| 0.141000E+04 | 0.158664E+02 | 0.165566E+02 | 0.151763E+02 |
| 0.141100E+04 | 0.158905E+02 | 0.165843E+02 | 0.151967E+02 |
| 0.141200E+04 | 0.159030E+02 | 0.165986E+02 | 0.152075E+02 |
| 0.141300E+04 | 0.159094E+02 | 0.166055E+02 | 0.152134E+02 |
| 0.141400E+04 | 0.159135E+02 | 0.166096E+02 | 0.152175E+02 |
| 0.141500E+04 | 0.159161E+02 | 0.166118E+02 | 0.152204E+02 |
| 0.141600E+04 | 0.159174E+02 | 0.166124E+02 | 0.152223E+02 |
| 0.141700E+04 | 0.159178E+02 | 0.166121E+02 | 0.152234E+02 |
| 0.141800E+04 | 0.159176E+02 | 0.166111E+02 | 0.152240E+02 |
| 0.141900E+04 | 0.159169E+02 | 0.166096E+02 | 0.152241E+02 |
| 0.142000E+04 | 0.159159E+02 | 0.166079E+02 | 0.152239E+02 |
| 0.142100E+04 | 0.159149E+02 | 0.166062E+02 | 0.152236E+02 |
| 0.142200E+04 | 0.159140E+02 | 0.166047E+02 | 0.152234E+02 |
| 0.142300E+04 | 0.159132E+02 | 0.166033E+02 | 0.152231E+02 |
| 0.142400E+04 | 0.159124E+02 | 0.166020E+02 | 0.152228E+02 |
| 0.142500E+04 | 0.159116E+02 | 0.166007E+02 | 0.152225E+02 |
| 0.142600E+04 | 0.159108E+02 | 0.165994E+02 | 0.152222E+02 |
| 0.142700E+04 | 0.159100E+02 | 0.165981E+02 | 0.152218E+02 |
| 0.142800E+04 | 0.159089E+02 | 0.165966E+02 | 0.152212E+02 |
| 0.142900E+04 | 0.159073E+02 | 0.165945E+02 | 0.152201E+02 |
| 0.143000E+04 | 0.159054E+02 | 0.165921E+02 | 0.152188E+02 |
| 0.143100E+04 | 0.159033E+02 | 0.165894E+02 | 0.152172E+02 |
| 0.143200E+04 | 0.159011E+02 | 0.165867E+02 | 0.152155E+02 |
| 0.143300E+04 | 0.158987E+02 | 0.165838E+02 | 0.152137E+02 |
| 0.143400E+04 | 0.158695E+02 | 0.165499E+02 | 0.151891E+02 |
| 0.143500E+04 | 0.158398E+02 | 0.165154E+02 | 0.151643E+02 |
| 0.143600E+04 | 0.158475E+02 | 0.165247E+02 | 0.151703E+02 |
| 0.143700E+04 | 0.158607E+02 | 0.165407E+02 | 0.151806E+02 |
| 0.143800E+04 | 0.158662E+02 | 0.165476E+02 | 0.151849E+02 |
| 0.143900E+04 | 0.158682E+02 | 0.165500E+02 | 0.151863E+02 |
| 0.144000E+04 | 0.158687E+02 | 0.165506E+02 | 0.151867E+02 |
| 0.144100E+04 | 0.158684E+02 | 0.165502E+02 | 0.151865E+02 |
| 0.144200E+04 | 0.158678E+02 | 0.165494E+02 | 0.151862E+02 |
| 0.144300E+04 | 0.158674E+02 | 0.165489E+02 | 0.151860E+02 |
| 0.144400E+04 | 0.158671E+02 | 0.165484E+02 | 0.151859E+02 |
| 0.144500E+04 | 0.158668E+02 | 0.165480E+02 | 0.151857E+02 |
| 0.144600E+04 | 0.158666E+02 | 0.165475E+02 | 0.151856E+02 |

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| 0.144700E+04 | 0.158663E+02 | 0.165471E+02 | 0.151855E+02 |
| 0.144800E+04 | 0.158660E+02 | 0.165467E+02 | 0.151853E+02 |
| 0.144900E+04 | 0.158657E+02 | 0.165463E+02 | 0.151852E+02 |
| 0.145000E+04 | 0.158655E+02 | 0.165459E+02 | 0.151850E+02 |
| 0.145100E+04 | 0.158652E+02 | 0.165456E+02 | 0.151849E+02 |
| 0.145200E+04 | 0.158649E+02 | 0.165451E+02 | 0.151847E+02 |
| 0.145300E+04 | 0.156986E+02 | 0.163494E+02 | 0.150478E+02 |
| 0.145400E+04 | 0.155209E+02 | 0.161407E+02 | 0.149010E+02 |
| 0.145500E+04 | 0.157223E+02 | 0.162074E+02 | 0.152372E+02 |
| 0.145600E+04 | 0.158517E+02 | 0.163244E+02 | 0.153790E+02 |
| 0.145700E+04 | 0.159077E+02 | 0.163858E+02 | 0.154296E+02 |
| 0.145800E+04 | 0.159363E+02 | 0.164214E+02 | 0.154512E+02 |
| 0.145900E+04 | 0.157969E+02 | 0.162550E+02 | 0.153387E+02 |
| 0.146000E+04 | 0.155383E+02 | 0.159649E+02 | 0.151117E+02 |
| 0.146100E+04 | 0.154816E+02 | 0.159199E+02 | 0.150434E+02 |
| 0.146200E+04 | 0.156008E+02 | 0.160635E+02 | 0.151380E+02 |
| 0.146300E+04 | 0.157161E+02 | 0.161978E+02 | 0.152344E+02 |
| 0.146400E+04 | 0.157779E+02 | 0.162756E+02 | 0.152803E+02 |
| 0.146500E+04 | 0.158130E+02 | 0.163221E+02 | 0.153038E+02 |
| 0.146600E+04 | 0.157942E+02 | 0.163031E+02 | 0.152853E+02 |
| 0.146700E+04 | 0.157648E+02 | 0.162694E+02 | 0.152601E+02 |
| 0.146800E+04 | 0.157932E+02 | 0.163077E+02 | 0.152787E+02 |
| 0.146900E+04 | 0.158317E+02 | 0.163593E+02 | 0.153042E+02 |
| 0.147000E+04 | 0.158545E+02 | 0.163903E+02 | 0.153187E+02 |
| 0.147100E+04 | 0.158691E+02 | 0.164102E+02 | 0.153280E+02 |
| 0.147200E+04 | 0.158802E+02 | 0.164253E+02 | 0.153350E+02 |
| 0.147300E+04 | 0.159115E+02 | 0.164797E+02 | 0.153434E+02 |
| 0.147400E+04 | 0.159169E+02 | 0.164839E+02 | 0.153498E+02 |
| 0.147500E+04 | 0.159220E+02 | 0.164894E+02 | 0.153546E+02 |
| 0.147600E+04 | 0.159266E+02 | 0.164947E+02 | 0.153586E+02 |
| 0.147700E+04 | 0.159309E+02 | 0.164996E+02 | 0.153622E+02 |
| 0.147800E+04 | 0.159350E+02 | 0.165043E+02 | 0.153657E+02 |
| 0.147900E+04 | 0.159388E+02 | 0.165085E+02 | 0.153690E+02 |
| 0.148000E+04 | 0.159372E+02 | 0.165125E+02 | 0.153620E+02 |
| 0.148100E+04 | 0.158914E+02 | 0.164707E+02 | 0.153122E+02 |
| 0.148200E+04 | 0.158569E+02 | 0.164401E+02 | 0.152738E+02 |
| 0.148300E+04 | 0.158195E+02 | 0.164051E+02 | 0.152338E+02 |
| 0.148400E+04 | 0.158050E+02 | 0.164048E+02 | 0.152052E+02 |
| 0.148500E+04 | 0.158148E+02 | 0.164485E+02 | 0.151811E+02 |
| 0.148600E+04 | 0.157900E+02 | 0.164782E+02 | 0.151018E+02 |
| 0.148700E+04 | 0.157779E+02 | 0.164903E+02 | 0.150655E+02 |
| 0.148800E+04 | 0.157811E+02 | 0.164964E+02 | 0.150659E+02 |
| 0.148900E+04 | 0.157809E+02 | 0.164998E+02 | 0.150620E+02 |
| 0.149000E+04 | 0.157796E+02 | 0.165015E+02 | 0.150577E+02 |
| 0.149100E+04 | 0.157791E+02 | 0.165020E+02 | 0.150562E+02 |
| 0.149200E+04 | 0.157799E+02 | 0.165020E+02 | 0.150578E+02 |
| 0.149300E+04 | 0.157818E+02 | 0.165018E+02 | 0.150619E+02 |
| 0.149400E+04 | 0.157847E+02 | 0.165016E+02 | 0.150678E+02 |
| 0.149500E+04 | 0.157421E+02 | 0.164482E+02 | 0.150361E+02 |
| 0.149600E+04 | 0.156978E+02 | 0.163923E+02 | 0.150032E+02 |
| 0.149700E+04 | 0.157180E+02 | 0.164116E+02 | 0.150243E+02 |
| 0.149800E+04 | 0.157474E+02 | 0.164401E+02 | 0.150546E+02 |
| 0.149900E+04 | 0.157617E+02 | 0.164504E+02 | 0.150730E+02 |
| 0.150000E+04 | 0.157724E+02 | 0.164588E+02 | 0.150861E+02 |

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| 0.150100E+04 | 0.157814E+02 | 0.164655E+02 | 0.150973E+02 |
| 0.150200E+04 | 0.157892E+02 | 0.164712E+02 | 0.151072E+02 |
| 0.150300E+04 | 0.157961E+02 | 0.164760E+02 | 0.151162E+02 |
| 0.150400E+04 | 0.157634E+02 | 0.164350E+02 | 0.150919E+02 |
| 0.150500E+04 | 0.157249E+02 | 0.163868E+02 | 0.150630E+02 |
| 0.150600E+04 | 0.157419E+02 | 0.164051E+02 | 0.150788E+02 |
| 0.150700E+04 | 0.157725E+02 | 0.164402E+02 | 0.151047E+02 |
| 0.150800E+04 | 0.157905E+02 | 0.164605E+02 | 0.151205E+02 |
| 0.150900E+04 | 0.158017E+02 | 0.164724E+02 | 0.151309E+02 |
| 0.151000E+04 | 0.158103E+02 | 0.164812E+02 | 0.151393E+02 |
| 0.151100E+04 | 0.158174E+02 | 0.164884E+02 | 0.151463E+02 |
| 0.151200E+04 | 0.158233E+02 | 0.164944E+02 | 0.151523E+02 |
| 0.151300E+04 | 0.158283E+02 | 0.164993E+02 | 0.151573E+02 |
| 0.151400E+04 | 0.158325E+02 | 0.165034E+02 | 0.151616E+02 |
| 0.151500E+04 | 0.158360E+02 | 0.165068E+02 | 0.151652E+02 |
| 0.151600E+04 | 0.158391E+02 | 0.165099E+02 | 0.151684E+02 |
| 0.151700E+04 | 0.158418E+02 | 0.165126E+02 | 0.151711E+02 |
| 0.151800E+04 | 0.158442E+02 | 0.165151E+02 | 0.151734E+02 |
| 0.151900E+04 | 0.158464E+02 | 0.165174E+02 | 0.151754E+02 |
| 0.152000E+04 | 0.158485E+02 | 0.165197E+02 | 0.151773E+02 |
| 0.152100E+04 | 0.158505E+02 | 0.165220E+02 | 0.151790E+02 |
| 0.152200E+04 | 0.158523E+02 | 0.165241E+02 | 0.151805E+02 |
| 0.152300E+04 | 0.158541E+02 | 0.165262E+02 | 0.151819E+02 |
| 0.152400E+04 | 0.158556E+02 | 0.165280E+02 | 0.151832E+02 |
| 0.152500E+04 | 0.158570E+02 | 0.165298E+02 | 0.151843E+02 |
| 0.152600E+04 | 0.158584E+02 | 0.165314E+02 | 0.151853E+02 |
| 0.152700E+04 | 0.157598E+02 | 0.164234E+02 | 0.150961E+02 |
| 0.152800E+04 | 0.156533E+02 | 0.162994E+02 | 0.150073E+02 |
| 0.152900E+04 | 0.156942E+02 | 0.163413E+02 | 0.150471E+02 |
| 0.153000E+04 | 0.157584E+02 | 0.164157E+02 | 0.151010E+02 |
| 0.153100E+04 | 0.157864E+02 | 0.164467E+02 | 0.151262E+02 |
| 0.153200E+04 | 0.157988E+02 | 0.164660E+02 | 0.151317E+02 |
| 0.153300E+04 | 0.158070E+02 | 0.164795E+02 | 0.151346E+02 |
| 0.153400E+04 | 0.158147E+02 | 0.164895E+02 | 0.151400E+02 |
| 0.153500E+04 | 0.158212E+02 | 0.164971E+02 | 0.151452E+02 |
| 0.153600E+04 | 0.158266E+02 | 0.165033E+02 | 0.151499E+02 |
| 0.153700E+04 | 0.158312E+02 | 0.165085E+02 | 0.151540E+02 |
| 0.153800E+04 | 0.158351E+02 | 0.165127E+02 | 0.151575E+02 |
| 0.153900E+04 | 0.158383E+02 | 0.165161E+02 | 0.151606E+02 |
| 0.154000E+04 | 0.158410E+02 | 0.165189E+02 | 0.151632E+02 |
| 0.154100E+04 | 0.158435E+02 | 0.165214E+02 | 0.151656E+02 |
| 0.154200E+04 | 0.158457E+02 | 0.165237E+02 | 0.151677E+02 |
| 0.154300E+04 | 0.158477E+02 | 0.165258E+02 | 0.151696E+02 |
| 0.154400E+04 | 0.158495E+02 | 0.165277E+02 | 0.151714E+02 |
| 0.154500E+04 | 0.158512E+02 | 0.165294E+02 | 0.151730E+02 |
| 0.154600E+04 | 0.158527E+02 | 0.165310E+02 | 0.151744E+02 |
| 0.154700E+04 | 0.158541E+02 | 0.165324E+02 | 0.151757E+02 |
| 0.154800E+04 | 0.158553E+02 | 0.165337E+02 | 0.151769E+02 |
| 0.154900E+04 | 0.158564E+02 | 0.165349E+02 | 0.151780E+02 |
| 0.155000E+04 | 0.158574E+02 | 0.165359E+02 | 0.151790E+02 |
| 0.155100E+04 | 0.158583E+02 | 0.165368E+02 | 0.151798E+02 |
| 0.155200E+04 | 0.158591E+02 | 0.165375E+02 | 0.151806E+02 |
| 0.155300E+04 | 0.158597E+02 | 0.165381E+02 | 0.151813E+02 |
| 0.155400E+04 | 0.158603E+02 | 0.165387E+02 | 0.151819E+02 |

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| 0.155500E+04 | 0.158608E+02 | 0.165391E+02 | 0.151825E+02 |
| 0.155600E+04 | 0.158612E+02 | 0.165395E+02 | 0.151830E+02 |
| 0.155700E+04 | 0.158616E+02 | 0.165398E+02 | 0.151834E+02 |
| 0.155800E+04 | 0.158620E+02 | 0.165401E+02 | 0.151839E+02 |
| 0.155900E+04 | 0.158623E+02 | 0.165402E+02 | 0.151843E+02 |
| 0.156000E+04 | 0.158626E+02 | 0.165405E+02 | 0.151848E+02 |
| 0.156100E+04 | 0.158631E+02 | 0.165409E+02 | 0.151853E+02 |
| 0.156200E+04 | 0.158636E+02 | 0.165413E+02 | 0.151860E+02 |
| 0.156300E+04 | 0.158642E+02 | 0.165418E+02 | 0.151866E+02 |
| 0.156400E+04 | 0.157724E+02 | 0.164418E+02 | 0.151029E+02 |
| 0.156500E+04 | 0.156692E+02 | 0.163206E+02 | 0.150179E+02 |
| 0.156600E+04 | 0.157063E+02 | 0.163577E+02 | 0.150550E+02 |
| 0.156700E+04 | 0.157687E+02 | 0.164302E+02 | 0.151072E+02 |
| 0.156800E+04 | 0.157953E+02 | 0.164588E+02 | 0.151319E+02 |
| 0.156900E+04 | 0.158074E+02 | 0.164769E+02 | 0.151379E+02 |
| 0.157000E+04 | 0.157880E+02 | 0.164580E+02 | 0.151180E+02 |
| 0.157100E+04 | 0.157674E+02 | 0.164338E+02 | 0.151011E+02 |
| 0.157200E+04 | 0.157842E+02 | 0.164534E+02 | 0.151150E+02 |
| 0.157300E+04 | 0.158059E+02 | 0.164792E+02 | 0.151326E+02 |
| 0.157400E+04 | 0.158187E+02 | 0.164940E+02 | 0.151434E+02 |
| 0.157500E+04 | 0.158270E+02 | 0.165031E+02 | 0.151509E+02 |
| 0.157600E+04 | 0.158334E+02 | 0.165097E+02 | 0.151570E+02 |
| 0.157700E+04 | 0.158386E+02 | 0.165150E+02 | 0.151622E+02 |
| 0.157800E+04 | 0.158433E+02 | 0.165196E+02 | 0.151670E+02 |
| 0.157900E+04 | 0.158475E+02 | 0.165236E+02 | 0.151713E+02 |
| 0.158000E+04 | 0.158512E+02 | 0.165272E+02 | 0.151753E+02 |
| 0.158100E+04 | 0.158547E+02 | 0.165305E+02 | 0.151789E+02 |
| 0.158200E+04 | 0.158579E+02 | 0.165334E+02 | 0.151823E+02 |
| 0.158300E+04 | 0.158609E+02 | 0.165362E+02 | 0.151855E+02 |
| 0.158400E+04 | 0.158637E+02 | 0.165389E+02 | 0.151885E+02 |
| 0.158500E+04 | 0.158663E+02 | 0.165412E+02 | 0.151913E+02 |
| 0.158600E+04 | 0.158686E+02 | 0.165433E+02 | 0.151938E+02 |
| 0.158700E+04 | 0.157132E+02 | 0.163666E+02 | 0.150598E+02 |
| 0.158800E+04 | 0.154744E+02 | 0.160886E+02 | 0.148602E+02 |
| 0.158900E+04 | 0.156158E+02 | 0.161042E+02 | 0.151275E+02 |
| 0.159000E+04 | 0.157461E+02 | 0.162183E+02 | 0.152740E+02 |
| 0.159100E+04 | 0.158410E+02 | 0.163176E+02 | 0.153645E+02 |
| 0.159200E+04 | 0.158870E+02 | 0.163685E+02 | 0.154054E+02 |
| 0.159300E+04 | 0.159142E+02 | 0.164041E+02 | 0.154242E+02 |
| 0.159400E+04 | 0.159325E+02 | 0.164313E+02 | 0.154336E+02 |
| 0.159500E+04 | 0.159455E+02 | 0.164532E+02 | 0.154379E+02 |
| 0.159600E+04 | 0.159551E+02 | 0.164710E+02 | 0.154392E+02 |
| 0.159700E+04 | 0.159644E+02 | 0.164859E+02 | 0.154430E+02 |
| 0.159800E+04 | 0.159701E+02 | 0.164982E+02 | 0.154419E+02 |
| 0.159900E+04 | 0.159745E+02 | 0.165086E+02 | 0.154403E+02 |
| 0.160000E+04 | 0.160005E+02 | 0.165604E+02 | 0.154407E+02 |
| 0.160100E+04 | 0.158122E+02 | 0.163475E+02 | 0.152769E+02 |
| 0.160200E+04 | 0.155901E+02 | 0.160843E+02 | 0.150960E+02 |
| 0.160300E+04 | 0.156584E+02 | 0.161822E+02 | 0.151345E+02 |
| 0.160400E+04 | 0.157541E+02 | 0.162916E+02 | 0.152167E+02 |
| 0.160500E+04 | 0.158219E+02 | 0.163701E+02 | 0.152738E+02 |
| 0.160600E+04 | 0.158609E+02 | 0.164181E+02 | 0.153037E+02 |
| 0.160700E+04 | 0.158846E+02 | 0.164478E+02 | 0.153213E+02 |
| 0.160800E+04 | 0.159013E+02 | 0.164688E+02 | 0.153337E+02 |

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| 0.160900E+04 | 0.159137E+02 | 0.164844E+02 | 0.153430E+02 |
| 0.161000E+04 | 0.159234E+02 | 0.164964E+02 | 0.153503E+02 |
| 0.161100E+04 | 0.159310E+02 | 0.165057E+02 | 0.153562E+02 |
| 0.161200E+04 | 0.159371E+02 | 0.165130E+02 | 0.153611E+02 |
| 0.161300E+04 | 0.158360E+02 | 0.163919E+02 | 0.152802E+02 |
| 0.161400E+04 | 0.157406E+02 | 0.162851E+02 | 0.151960E+02 |
| 0.161500E+04 | 0.157794E+02 | 0.163302E+02 | 0.152285E+02 |
| 0.161600E+04 | 0.158420E+02 | 0.164039E+02 | 0.152801E+02 |
| 0.161700E+04 | 0.158742E+02 | 0.164425E+02 | 0.153060E+02 |
| 0.161800E+04 | 0.158916E+02 | 0.164651E+02 | 0.153181E+02 |
| 0.161900E+04 | 0.158932E+02 | 0.164807E+02 | 0.153057E+02 |
| 0.162000E+04 | 0.158925E+02 | 0.164920E+02 | 0.152929E+02 |
| 0.162100E+04 | 0.158471E+02 | 0.165003E+02 | 0.151940E+02 |
| 0.162200E+04 | 0.156965E+02 | 0.163644E+02 | 0.150287E+02 |
| 0.162300E+04 | 0.155671E+02 | 0.162420E+02 | 0.148922E+02 |
| 0.162400E+04 | 0.156052E+02 | 0.162909E+02 | 0.149196E+02 |
| 0.162500E+04 | 0.156690E+02 | 0.163711E+02 | 0.149668E+02 |
| 0.162600E+04 | 0.156993E+02 | 0.164112E+02 | 0.149874E+02 |
| 0.162700E+04 | 0.157160E+02 | 0.164336E+02 | 0.149985E+02 |
| 0.162800E+04 | 0.157286E+02 | 0.164485E+02 | 0.150087E+02 |
| 0.162900E+04 | 0.157391E+02 | 0.164591E+02 | 0.150191E+02 |
| 0.163000E+04 | 0.157487E+02 | 0.164672E+02 | 0.150301E+02 |
| 0.163100E+04 | 0.157575E+02 | 0.164737E+02 | 0.150414E+02 |
| 0.163200E+04 | 0.157656E+02 | 0.164787E+02 | 0.150526E+02 |
| 0.163300E+04 | 0.157729E+02 | 0.164825E+02 | 0.150632E+02 |
| 0.163400E+04 | 0.157794E+02 | 0.164855E+02 | 0.150733E+02 |
| 0.163500E+04 | 0.157854E+02 | 0.164880E+02 | 0.150828E+02 |
| 0.163600E+04 | 0.157909E+02 | 0.164902E+02 | 0.150916E+02 |
| 0.163700E+04 | 0.157960E+02 | 0.164921E+02 | 0.150998E+02 |
| 0.163800E+04 | 0.158006E+02 | 0.164938E+02 | 0.151074E+02 |
| 0.163900E+04 | 0.158049E+02 | 0.164955E+02 | 0.151143E+02 |
| 0.164000E+04 | 0.158090E+02 | 0.164973E+02 | 0.151207E+02 |
| 0.164100E+04 | 0.156164E+02 | 0.162750E+02 | 0.149578E+02 |
| 0.164200E+04 | 0.153906E+02 | 0.160159E+02 | 0.147653E+02 |
| 0.164300E+04 | 0.154635E+02 | 0.161100E+02 | 0.148171E+02 |
| 0.164400E+04 | 0.155646E+02 | 0.162184E+02 | 0.149108E+02 |
| 0.164500E+04 | 0.156245E+02 | 0.162818E+02 | 0.149673E+02 |
| 0.164600E+04 | 0.156531E+02 | 0.163139E+02 | 0.149922E+02 |
| 0.164700E+04 | 0.156822E+02 | 0.163524E+02 | 0.150120E+02 |
| 0.164800E+04 | 0.157101E+02 | 0.163854E+02 | 0.150349E+02 |
| 0.164900E+04 | 0.157302E+02 | 0.164077E+02 | 0.150528E+02 |
| 0.165000E+04 | 0.157455E+02 | 0.164238E+02 | 0.150671E+02 |
| 0.165100E+04 | 0.157577E+02 | 0.164364E+02 | 0.150791E+02 |
| 0.165200E+04 | 0.157678E+02 | 0.164464E+02 | 0.150892E+02 |
| 0.165300E+04 | 0.157760E+02 | 0.164544E+02 | 0.150976E+02 |
| 0.165400E+04 | 0.157828E+02 | 0.164609E+02 | 0.151047E+02 |
| 0.165500E+04 | 0.157884E+02 | 0.164662E+02 | 0.151106E+02 |
| 0.165600E+04 | 0.157930E+02 | 0.164705E+02 | 0.151154E+02 |
| 0.165700E+04 | 0.157968E+02 | 0.164741E+02 | 0.151195E+02 |
| 0.165800E+04 | 0.157999E+02 | 0.164770E+02 | 0.151227E+02 |
| 0.165900E+04 | 0.158024E+02 | 0.164794E+02 | 0.151254E+02 |
| 0.166000E+04 | 0.158046E+02 | 0.164816E+02 | 0.151277E+02 |
| 0.166100E+04 | 0.158068E+02 | 0.164838E+02 | 0.151298E+02 |
| 0.166200E+04 | 0.158089E+02 | 0.164860E+02 | 0.151317E+02 |

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| 0.166300E+04 | 0.158107E+02 | 0.164879E+02 | 0.151334E+02 |
| 0.166400E+04 | 0.158124E+02 | 0.164898E+02 | 0.151350E+02 |
| 0.166500E+04 | 0.158139E+02 | 0.164916E+02 | 0.151363E+02 |
| 0.166600E+04 | 0.158154E+02 | 0.164932E+02 | 0.151375E+02 |
| 0.166700E+04 | 0.157219E+02 | 0.163835E+02 | 0.150603E+02 |
| 0.166800E+04 | 0.156393E+02 | 0.162928E+02 | 0.149858E+02 |
| 0.166900E+04 | 0.156740E+02 | 0.163283E+02 | 0.150197E+02 |
| 0.167000E+04 | 0.157309E+02 | 0.163948E+02 | 0.150671E+02 |
| 0.167100E+04 | 0.157546E+02 | 0.164201E+02 | 0.150892E+02 |
| 0.167200E+04 | 0.157647E+02 | 0.164366E+02 | 0.150927E+02 |
| 0.167300E+04 | 0.157717E+02 | 0.164485E+02 | 0.150949E+02 |
| 0.167400E+04 | 0.156464E+02 | 0.163025E+02 | 0.149903E+02 |
| 0.167500E+04 | 0.155271E+02 | 0.161725E+02 | 0.148816E+02 |
| 0.167600E+04 | 0.155809E+02 | 0.162270E+02 | 0.149348E+02 |
| 0.167700E+04 | 0.156587E+02 | 0.163162E+02 | 0.150011E+02 |
| 0.167800E+04 | 0.156979E+02 | 0.163612E+02 | 0.150346E+02 |
| 0.167900E+04 | 0.157186E+02 | 0.163878E+02 | 0.150495E+02 |
| 0.168000E+04 | 0.157301E+02 | 0.164064E+02 | 0.150538E+02 |
| 0.168100E+04 | 0.156317E+02 | 0.162918E+02 | 0.149716E+02 |
| 0.168200E+04 | 0.155379E+02 | 0.161918E+02 | 0.148840E+02 |
| 0.168300E+04 | 0.155854E+02 | 0.162408E+02 | 0.149301E+02 |
| 0.168400E+04 | 0.156550E+02 | 0.163166E+02 | 0.149935E+02 |
| 0.168500E+04 | 0.156910E+02 | 0.163567E+02 | 0.150252E+02 |
| 0.168600E+04 | 0.157096E+02 | 0.163811E+02 | 0.150380E+02 |
| 0.168700E+04 | 0.157209E+02 | 0.163983E+02 | 0.150434E+02 |
| 0.168800E+04 | 0.157316E+02 | 0.164110E+02 | 0.150521E+02 |
| 0.168900E+04 | 0.157115E+02 | 0.163869E+02 | 0.150362E+02 |
| 0.169000E+04 | 0.156984E+02 | 0.163702E+02 | 0.150266E+02 |
| 0.169100E+04 | 0.157224E+02 | 0.163974E+02 | 0.150473E+02 |
| 0.169200E+04 | 0.157407E+02 | 0.164185E+02 | 0.150628E+02 |
| 0.169300E+04 | 0.157510E+02 | 0.164299E+02 | 0.150722E+02 |
| 0.169400E+04 | 0.157587E+02 | 0.164379E+02 | 0.150795E+02 |
| 0.169500E+04 | 0.156370E+02 | 0.163016E+02 | 0.149724E+02 |
| 0.169600E+04 | 0.155058E+02 | 0.161448E+02 | 0.148669E+02 |
| 0.169700E+04 | 0.155564E+02 | 0.161998E+02 | 0.149130E+02 |
| 0.169800E+04 | 0.157199E+02 | 0.162960E+02 | 0.151438E+02 |
| 0.169900E+04 | 0.158122E+02 | 0.163454E+02 | 0.152790E+02 |
| 0.170000E+04 | 0.158754E+02 | 0.163766E+02 | 0.153742E+02 |
| 0.170100E+04 | 0.158976E+02 | 0.164003E+02 | 0.153949E+02 |
| 0.170200E+04 | 0.159116E+02 | 0.164191E+02 | 0.154040E+02 |
| 0.170300E+04 | 0.159205E+02 | 0.164345E+02 | 0.154065E+02 |
| 0.170400E+04 | 0.159261E+02 | 0.164471E+02 | 0.154052E+02 |
| 0.170500E+04 | 0.159296E+02 | 0.164575E+02 | 0.154016E+02 |
| 0.170600E+04 | 0.159315E+02 | 0.164661E+02 | 0.153970E+02 |
| 0.170700E+04 | 0.159355E+02 | 0.164732E+02 | 0.153978E+02 |
| 0.170800E+04 | 0.159350E+02 | 0.164792E+02 | 0.153908E+02 |
| 0.170900E+04 | 0.159350E+02 | 0.164842E+02 | 0.153859E+02 |
| 0.171000E+04 | 0.159347E+02 | 0.164882E+02 | 0.153812E+02 |
| 0.171100E+04 | 0.159342E+02 | 0.164915E+02 | 0.153769E+02 |
| 0.171200E+04 | 0.159335E+02 | 0.164942E+02 | 0.153728E+02 |
| 0.171300E+04 | 0.159328E+02 | 0.164964E+02 | 0.153692E+02 |
| 0.171400E+04 | 0.159322E+02 | 0.164984E+02 | 0.153660E+02 |
| 0.171500E+04 | 0.159316E+02 | 0.165001E+02 | 0.153631E+02 |
| 0.171600E+04 | 0.159311E+02 | 0.165015E+02 | 0.153606E+02 |

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| 0.171700E+04 | 0.159307E+02 | 0.165030E+02 | 0.153585E+02 |
| 0.171800E+04 | 0.159305E+02 | 0.165043E+02 | 0.153567E+02 |
| 0.171900E+04 | 0.159303E+02 | 0.165055E+02 | 0.153551E+02 |
| 0.172000E+04 | 0.159302E+02 | 0.165066E+02 | 0.153538E+02 |
| 0.172100E+04 | 0.159253E+02 | 0.165081E+02 | 0.153424E+02 |
| 0.172200E+04 | 0.159150E+02 | 0.165100E+02 | 0.153200E+02 |
| 0.172300E+04 | 0.159065E+02 | 0.165122E+02 | 0.153009E+02 |
| 0.172400E+04 | 0.158735E+02 | 0.165143E+02 | 0.152328E+02 |
| 0.172500E+04 | 0.158276E+02 | 0.165158E+02 | 0.151394E+02 |
| 0.172600E+04 | 0.158046E+02 | 0.165163E+02 | 0.150928E+02 |
| 0.172700E+04 | 0.157892E+02 | 0.165163E+02 | 0.150621E+02 |
| 0.172800E+04 | 0.157790E+02 | 0.165157E+02 | 0.150422E+02 |
| 0.172900E+04 | 0.156549E+02 | 0.163865E+02 | 0.149232E+02 |
| 0.173000E+04 | 0.155742E+02 | 0.162819E+02 | 0.148665E+02 |
| 0.173100E+04 | 0.156523E+02 | 0.163685E+02 | 0.149361E+02 |
| 0.173200E+04 | 0.156991E+02 | 0.164177E+02 | 0.149806E+02 |
| 0.173300E+04 | 0.157211E+02 | 0.164410E+02 | 0.150013E+02 |
| 0.173400E+04 | 0.157368E+02 | 0.164564E+02 | 0.150171E+02 |
| 0.173500E+04 | 0.157494E+02 | 0.164676E+02 | 0.150312E+02 |
| 0.173600E+04 | 0.157606E+02 | 0.164766E+02 | 0.150446E+02 |
| 0.173700E+04 | 0.157712E+02 | 0.164847E+02 | 0.150577E+02 |
| 0.173800E+04 | 0.157812E+02 | 0.164920E+02 | 0.150703E+02 |
| 0.173900E+04 | 0.157401E+02 | 0.164398E+02 | 0.150405E+02 |
| 0.174000E+04 | 0.157122E+02 | 0.164027E+02 | 0.150218E+02 |
| 0.174100E+04 | 0.157494E+02 | 0.164427E+02 | 0.150561E+02 |
| 0.174200E+04 | 0.157783E+02 | 0.164745E+02 | 0.150821E+02 |
| 0.174300E+04 | 0.157942E+02 | 0.164905E+02 | 0.150979E+02 |
| 0.174400E+04 | 0.158061E+02 | 0.165016E+02 | 0.151105E+02 |
| 0.174500E+04 | 0.158159E+02 | 0.165104E+02 | 0.151214E+02 |
| 0.174600E+04 | 0.158238E+02 | 0.165178E+02 | 0.151299E+02 |
| 0.174700E+04 | 0.158280E+02 | 0.165242E+02 | 0.151318E+02 |
| 0.174800E+04 | 0.158320E+02 | 0.165297E+02 | 0.151343E+02 |
| 0.174900E+04 | 0.158362E+02 | 0.165347E+02 | 0.151376E+02 |
| 0.175000E+04 | 0.158403E+02 | 0.165393E+02 | 0.151414E+02 |
| 0.175100E+04 | 0.158445E+02 | 0.165437E+02 | 0.151454E+02 |
| 0.175200E+04 | 0.158486E+02 | 0.165478E+02 | 0.151495E+02 |
| 0.175300E+04 | 0.158526E+02 | 0.165517E+02 | 0.151536E+02 |
| 0.175400E+04 | 0.158565E+02 | 0.165555E+02 | 0.151576E+02 |
| 0.175500E+04 | 0.158603E+02 | 0.165592E+02 | 0.151615E+02 |
| 0.175600E+04 | 0.158640E+02 | 0.165628E+02 | 0.151652E+02 |
| 0.175700E+04 | 0.158675E+02 | 0.165663E+02 | 0.151686E+02 |
| 0.175800E+04 | 0.158708E+02 | 0.165697E+02 | 0.151720E+02 |
| 0.175900E+04 | 0.158740E+02 | 0.165729E+02 | 0.151751E+02 |
| 0.176000E+04 | 0.158771E+02 | 0.165761E+02 | 0.151781E+02 |
| 0.176100E+04 | 0.158801E+02 | 0.165793E+02 | 0.151809E+02 |
| 0.176200E+04 | 0.158830E+02 | 0.165824E+02 | 0.151836E+02 |
| 0.176300E+04 | 0.158858E+02 | 0.165854E+02 | 0.151862E+02 |
| 0.176400E+04 | 0.158884E+02 | 0.165883E+02 | 0.151886E+02 |
| 0.176500E+04 | 0.158907E+02 | 0.165908E+02 | 0.151906E+02 |
| 0.176600E+04 | 0.158928E+02 | 0.165931E+02 | 0.151925E+02 |
| 0.176700E+04 | 0.158948E+02 | 0.165953E+02 | 0.151943E+02 |
| 0.176800E+04 | 0.158968E+02 | 0.165975E+02 | 0.151960E+02 |
| 0.176900E+04 | 0.158986E+02 | 0.165997E+02 | 0.151976E+02 |
| 0.177000E+04 | 0.159004E+02 | 0.166018E+02 | 0.151991E+02 |

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| 0.177100E+04 | 0.159022E+02 | 0.166038E+02 | 0.152006E+02 |
| 0.177200E+04 | 0.159037E+02 | 0.166055E+02 | 0.152018E+02 |
| 0.177300E+04 | 0.159048E+02 | 0.166068E+02 | 0.152027E+02 |
| 0.177400E+04 | 0.159056E+02 | 0.166078E+02 | 0.152034E+02 |
| 0.177500E+04 | 0.159062E+02 | 0.166085E+02 | 0.152039E+02 |
| 0.177600E+04 | 0.159067E+02 | 0.166091E+02 | 0.152043E+02 |
| 0.177700E+04 | 0.159071E+02 | 0.166096E+02 | 0.152046E+02 |
| 0.177800E+04 | 0.159073E+02 | 0.166099E+02 | 0.152048E+02 |
| 0.177900E+04 | 0.159075E+02 | 0.166101E+02 | 0.152049E+02 |
| 0.178000E+04 | 0.159077E+02 | 0.166105E+02 | 0.152050E+02 |
| 0.178100E+04 | 0.159083E+02 | 0.166111E+02 | 0.152054E+02 |
| 0.178200E+04 | 0.159090E+02 | 0.166120E+02 | 0.152060E+02 |
| 0.178300E+04 | 0.157935E+02 | 0.164815E+02 | 0.151055E+02 |
| 0.178400E+04 | 0.157001E+02 | 0.163788E+02 | 0.150214E+02 |
| 0.178500E+04 | 0.157815E+02 | 0.164667E+02 | 0.150963E+02 |
| 0.178600E+04 | 0.158285E+02 | 0.165167E+02 | 0.151403E+02 |
| 0.178700E+04 | 0.158482E+02 | 0.165407E+02 | 0.151558E+02 |
| 0.178800E+04 | 0.158572E+02 | 0.165566E+02 | 0.151577E+02 |
| 0.178900E+04 | 0.158192E+02 | 0.165136E+02 | 0.151247E+02 |
| 0.179000E+04 | 0.157765E+02 | 0.164634E+02 | 0.150896E+02 |
| 0.179100E+04 | 0.157986E+02 | 0.164896E+02 | 0.151075E+02 |
| 0.179200E+04 | 0.158325E+02 | 0.165302E+02 | 0.151348E+02 |
| 0.179300E+04 | 0.158520E+02 | 0.165532E+02 | 0.151509E+02 |
| 0.179400E+04 | 0.158640E+02 | 0.165667E+02 | 0.151613E+02 |
| 0.179500E+04 | 0.158729E+02 | 0.165764E+02 | 0.151694E+02 |
| 0.179600E+04 | 0.158800E+02 | 0.165840E+02 | 0.151761E+02 |
| 0.179700E+04 | 0.158859E+02 | 0.165900E+02 | 0.151818E+02 |
| 0.179800E+04 | 0.158909E+02 | 0.165950E+02 | 0.151868E+02 |
| 0.179900E+04 | 0.158952E+02 | 0.165992E+02 | 0.151911E+02 |
| 0.180000E+04 | 0.158989E+02 | 0.166028E+02 | 0.151949E+02 |
| 0.180100E+04 | 0.159021E+02 | 0.166058E+02 | 0.151984E+02 |
| 0.180200E+04 | 0.159050E+02 | 0.166085E+02 | 0.152015E+02 |
| 0.180300E+04 | 0.159076E+02 | 0.166109E+02 | 0.152043E+02 |
| 0.180400E+04 | 0.159099E+02 | 0.166130E+02 | 0.152068E+02 |
| 0.180500E+04 | 0.159121E+02 | 0.166149E+02 | 0.152092E+02 |
| 0.180600E+04 | 0.159140E+02 | 0.166166E+02 | 0.152114E+02 |
| 0.180700E+04 | 0.159159E+02 | 0.166182E+02 | 0.152135E+02 |
| 0.180800E+04 | 0.159176E+02 | 0.166197E+02 | 0.152155E+02 |
| 0.180900E+04 | 0.157218E+02 | 0.163974E+02 | 0.150462E+02 |
| 0.181000E+04 | 0.154993E+02 | 0.161375E+02 | 0.148611E+02 |
| 0.181100E+04 | 0.157017E+02 | 0.162308E+02 | 0.151726E+02 |
| 0.181200E+04 | 0.158325E+02 | 0.163398E+02 | 0.153252E+02 |
| 0.181300E+04 | 0.159143E+02 | 0.164185E+02 | 0.154101E+02 |
| 0.181400E+04 | 0.159529E+02 | 0.164565E+02 | 0.154494E+02 |
| 0.181500E+04 | 0.157646E+02 | 0.162434E+02 | 0.152857E+02 |
| 0.181600E+04 | 0.155564E+02 | 0.160077E+02 | 0.151051E+02 |
| 0.181700E+04 | 0.156148E+02 | 0.160786E+02 | 0.151510E+02 |
| 0.181800E+04 | 0.157353E+02 | 0.162270E+02 | 0.152435E+02 |
| 0.181900E+04 | 0.158176E+02 | 0.163268E+02 | 0.153083E+02 |
| 0.182000E+04 | 0.158677E+02 | 0.163933E+02 | 0.153421E+02 |
| 0.182100E+04 | 0.158986E+02 | 0.164357E+02 | 0.153615E+02 |
| 0.182200E+04 | 0.159207E+02 | 0.164668E+02 | 0.153746E+02 |
| 0.182300E+04 | 0.159374E+02 | 0.164906E+02 | 0.153842E+02 |
| 0.182400E+04 | 0.159502E+02 | 0.165091E+02 | 0.153913E+02 |

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| 0.182500E+04 | 0.159601E+02 | 0.165235E+02 | 0.153967E+02 |
| 0.182600E+04 | 0.159679E+02 | 0.165349E+02 | 0.154009E+02 |
| 0.182700E+04 | 0.159741E+02 | 0.165440E+02 | 0.154042E+02 |
| 0.182800E+04 | 0.159999E+02 | 0.165907E+02 | 0.154092E+02 |
| 0.182900E+04 | 0.159912E+02 | 0.165920E+02 | 0.153905E+02 |
| 0.183000E+04 | 0.159379E+02 | 0.165399E+02 | 0.153359E+02 |
| 0.183100E+04 | 0.157279E+02 | 0.163124E+02 | 0.151434E+02 |
| 0.183200E+04 | 0.155800E+02 | 0.161112E+02 | 0.150487E+02 |
| 0.183300E+04 | 0.156912E+02 | 0.162503E+02 | 0.151321E+02 |
| 0.183400E+04 | 0.158029E+02 | 0.163730E+02 | 0.152328E+02 |
| 0.183500E+04 | 0.157465E+02 | 0.163030E+02 | 0.151900E+02 |
| 0.183600E+04 | 0.156679E+02 | 0.162129E+02 | 0.151230E+02 |
| 0.183700E+04 | 0.157287E+02 | 0.162837E+02 | 0.151737E+02 |
| 0.183800E+04 | 0.158057E+02 | 0.163697E+02 | 0.152417E+02 |
| 0.183900E+04 | 0.158486E+02 | 0.164182E+02 | 0.152791E+02 |
| 0.184000E+04 | 0.158378E+02 | 0.164036E+02 | 0.152719E+02 |
| 0.184100E+04 | 0.158158E+02 | 0.163752E+02 | 0.152564E+02 |
| 0.184200E+04 | 0.158456E+02 | 0.164108E+02 | 0.152804E+02 |
| 0.184300E+04 | 0.158193E+02 | 0.163896E+02 | 0.152489E+02 |
| 0.184400E+04 | 0.158042E+02 | 0.164076E+02 | 0.152007E+02 |
| 0.184500E+04 | 0.158174E+02 | 0.164451E+02 | 0.151897E+02 |
| 0.184600E+04 | 0.158041E+02 | 0.164926E+02 | 0.151157E+02 |
| 0.184700E+04 | 0.158029E+02 | 0.165163E+02 | 0.150895E+02 |
| 0.184800E+04 | 0.158024E+02 | 0.165330E+02 | 0.150719E+02 |
| 0.184900E+04 | 0.158036E+02 | 0.165450E+02 | 0.150622E+02 |
| 0.185000E+04 | 0.158055E+02 | 0.165542E+02 | 0.150568E+02 |
| 0.185100E+04 | 0.158085E+02 | 0.165609E+02 | 0.150560E+02 |
| 0.185200E+04 | 0.158131E+02 | 0.165659E+02 | 0.150604E+02 |
| 0.185300E+04 | 0.158188E+02 | 0.165698E+02 | 0.150677E+02 |
| 0.185400E+04 | 0.157939E+02 | 0.165368E+02 | 0.150510E+02 |
| 0.185500E+04 | 0.157779E+02 | 0.165132E+02 | 0.150425E+02 |
| 0.185600E+04 | 0.158030E+02 | 0.165378E+02 | 0.150681E+02 |
| 0.185700E+04 | 0.158234E+02 | 0.165572E+02 | 0.150896E+02 |
| 0.185800E+04 | 0.158362E+02 | 0.165672E+02 | 0.151053E+02 |
| 0.185900E+04 | 0.158468E+02 | 0.165744E+02 | 0.151191E+02 |
| 0.186000E+04 | 0.158561E+02 | 0.165804E+02 | 0.151318E+02 |
| 0.186100E+04 | 0.158647E+02 | 0.165858E+02 | 0.151436E+02 |
| 0.186200E+04 | 0.158729E+02 | 0.165911E+02 | 0.151547E+02 |
| 0.186300E+04 | 0.158808E+02 | 0.165964E+02 | 0.151652E+02 |
| 0.186400E+04 | 0.158883E+02 | 0.166016E+02 | 0.151750E+02 |
| 0.186500E+04 | 0.158954E+02 | 0.166066E+02 | 0.151841E+02 |
| 0.186600E+04 | 0.159021E+02 | 0.166116E+02 | 0.151926E+02 |
| 0.186700E+04 | 0.159085E+02 | 0.166165E+02 | 0.152004E+02 |
| 0.186800E+04 | 0.159146E+02 | 0.166214E+02 | 0.152078E+02 |
| 0.186900E+04 | 0.159205E+02 | 0.166263E+02 | 0.152146E+02 |
| 0.187000E+04 | 0.159262E+02 | 0.166313E+02 | 0.152211E+02 |
| 0.187100E+04 | 0.159316E+02 | 0.166361E+02 | 0.152271E+02 |
| 0.187200E+04 | 0.159367E+02 | 0.166409E+02 | 0.152326E+02 |
| 0.187300E+04 | 0.159416E+02 | 0.166455E+02 | 0.152378E+02 |
| 0.187400E+04 | 0.159463E+02 | 0.166500E+02 | 0.152426E+02 |
| 0.187500E+04 | 0.159508E+02 | 0.166545E+02 | 0.152471E+02 |
| 0.187600E+04 | 0.159553E+02 | 0.166591E+02 | 0.152515E+02 |
| 0.187700E+04 | 0.159597E+02 | 0.166637E+02 | 0.152557E+02 |
| 0.187800E+04 | 0.159640E+02 | 0.166683E+02 | 0.152597E+02 |

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| 0.187900E+04 | 0.159683E+02 | 0.166730E+02 | 0.152636E+02 |
| 0.188000E+04 | 0.159725E+02 | 0.166776E+02 | 0.152673E+02 |
| 0.188100E+04 | 0.159765E+02 | 0.166821E+02 | 0.152709E+02 |
| 0.188200E+04 | 0.159802E+02 | 0.166863E+02 | 0.152742E+02 |
| 0.188300E+04 | 0.158486E+02 | 0.165397E+02 | 0.151574E+02 |
| 0.188400E+04 | 0.157016E+02 | 0.163715E+02 | 0.150317E+02 |
| 0.188500E+04 | 0.157490E+02 | 0.164271E+02 | 0.150709E+02 |
| 0.188600E+04 | 0.158348E+02 | 0.165263E+02 | 0.151433E+02 |
| 0.188700E+04 | 0.158795E+02 | 0.165783E+02 | 0.151808E+02 |
| 0.188800E+04 | 0.159051E+02 | 0.166083E+02 | 0.152019E+02 |
| 0.188900E+04 | 0.159234E+02 | 0.166294E+02 | 0.152174E+02 |
| 0.189000E+04 | 0.159376E+02 | 0.166455E+02 | 0.152296E+02 |
| 0.189100E+04 | 0.159488E+02 | 0.166582E+02 | 0.152395E+02 |
| 0.189200E+04 | 0.159579E+02 | 0.166682E+02 | 0.152476E+02 |
| 0.189300E+04 | 0.159653E+02 | 0.166763E+02 | 0.152544E+02 |
| 0.189400E+04 | 0.159717E+02 | 0.166832E+02 | 0.152601E+02 |
| 0.189500E+04 | 0.159772E+02 | 0.166892E+02 | 0.152651E+02 |
| 0.189600E+04 | 0.159820E+02 | 0.166944E+02 | 0.152695E+02 |
| 0.189700E+04 | 0.159862E+02 | 0.166990E+02 | 0.152734E+02 |
| 0.189800E+04 | 0.159900E+02 | 0.167032E+02 | 0.152768E+02 |
| 0.189900E+04 | 0.159934E+02 | 0.167069E+02 | 0.152799E+02 |
| 0.190000E+04 | 0.159966E+02 | 0.167103E+02 | 0.152828E+02 |
| 0.190100E+04 | 0.159993E+02 | 0.167133E+02 | 0.152852E+02 |
| 0.190200E+04 | 0.158608E+02 | 0.165511E+02 | 0.151705E+02 |
| 0.190300E+04 | 0.157279E+02 | 0.164063E+02 | 0.150494E+02 |
| 0.190400E+04 | 0.157739E+02 | 0.164598E+02 | 0.150879E+02 |
| 0.190500E+04 | 0.158541E+02 | 0.165522E+02 | 0.151561E+02 |
| 0.190600E+04 | 0.158950E+02 | 0.165994E+02 | 0.151905E+02 |
| 0.190700E+04 | 0.159034E+02 | 0.166092E+02 | 0.151977E+02 |
| 0.190800E+04 | 0.159088E+02 | 0.166150E+02 | 0.152027E+02 |
| 0.190900E+04 | 0.159292E+02 | 0.166383E+02 | 0.152201E+02 |
| 0.191000E+04 | 0.159447E+02 | 0.166561E+02 | 0.152333E+02 |
| 0.191100E+04 | 0.159555E+02 | 0.166681E+02 | 0.152429E+02 |
| 0.191200E+04 | 0.158836E+02 | 0.165834E+02 | 0.151838E+02 |
| 0.191300E+04 | 0.158140E+02 | 0.165108E+02 | 0.151173E+02 |
| 0.191400E+04 | 0.158155E+02 | 0.165123E+02 | 0.151187E+02 |
| 0.191500E+04 | 0.158501E+02 | 0.165527E+02 | 0.151476E+02 |
| 0.191600E+04 | 0.158950E+02 | 0.166010E+02 | 0.151891E+02 |
| 0.191700E+04 | 0.159249E+02 | 0.166345E+02 | 0.152153E+02 |
| 0.191800E+04 | 0.159439E+02 | 0.166558E+02 | 0.152321E+02 |
| 0.191900E+04 | 0.159585E+02 | 0.166716E+02 | 0.152453E+02 |
| 0.192000E+04 | 0.159702E+02 | 0.166841E+02 | 0.152563E+02 |
| 0.192100E+04 | 0.159805E+02 | 0.166950E+02 | 0.152661E+02 |
| 0.192200E+04 | 0.159903E+02 | 0.167052E+02 | 0.152754E+02 |
| 0.192300E+04 | 0.159995E+02 | 0.167148E+02 | 0.152842E+02 |
| 0.192400E+04 | 0.159574E+02 | 0.166645E+02 | 0.152503E+02 |
| 0.192500E+04 | 0.159090E+02 | 0.166066E+02 | 0.152114E+02 |
| 0.192600E+04 | 0.159314E+02 | 0.166321E+02 | 0.152306E+02 |
| 0.192700E+04 | 0.159712E+02 | 0.166795E+02 | 0.152628E+02 |
| 0.192800E+04 | 0.159950E+02 | 0.167074E+02 | 0.152825E+02 |
| 0.192900E+04 | 0.160100E+02 | 0.167243E+02 | 0.152957E+02 |
| 0.193000E+04 | 0.160216E+02 | 0.167368E+02 | 0.153063E+02 |
| 0.193100E+04 | 0.160319E+02 | 0.167478E+02 | 0.153161E+02 |
| 0.193200E+04 | 0.160423E+02 | 0.167587E+02 | 0.153259E+02 |

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| 0.193300E+04 | 0.160526E+02 | 0.167695E+02 | 0.153356E+02 |
| 0.193400E+04 | 0.160627E+02 | 0.167801E+02 | 0.153453E+02 |
| 0.193500E+04 | 0.160726E+02 | 0.167905E+02 | 0.153547E+02 |
| 0.193600E+04 | 0.160806E+02 | 0.167985E+02 | 0.153626E+02 |
| 0.193700E+04 | 0.160852E+02 | 0.168028E+02 | 0.153677E+02 |
| 0.193800E+04 | 0.160877E+02 | 0.168045E+02 | 0.153710E+02 |
| 0.193900E+04 | 0.160889E+02 | 0.168047E+02 | 0.153731E+02 |
| 0.194000E+04 | 0.160891E+02 | 0.168038E+02 | 0.153743E+02 |
| 0.194100E+04 | 0.160906E+02 | 0.168047E+02 | 0.153765E+02 |
| 0.194200E+04 | 0.160951E+02 | 0.168091E+02 | 0.153811E+02 |
| 0.194300E+04 | 0.161016E+02 | 0.168160E+02 | 0.153873E+02 |
| 0.194400E+04 | 0.161094E+02 | 0.168243E+02 | 0.153945E+02 |
| 0.194500E+04 | 0.161178E+02 | 0.168334E+02 | 0.154023E+02 |
| 0.194600E+04 | 0.161244E+02 | 0.168402E+02 | 0.154085E+02 |
| 0.194700E+04 | 0.161273E+02 | 0.168428E+02 | 0.154119E+02 |
| 0.194800E+04 | 0.161281E+02 | 0.168428E+02 | 0.154134E+02 |
| 0.194900E+04 | 0.161276E+02 | 0.168414E+02 | 0.154138E+02 |
| 0.195000E+04 | 0.161263E+02 | 0.168391E+02 | 0.154135E+02 |
| 0.195100E+04 | 0.161257E+02 | 0.168377E+02 | 0.154136E+02 |
| 0.195200E+04 | 0.161266E+02 | 0.168383E+02 | 0.154150E+02 |
| 0.195300E+04 | 0.161287E+02 | 0.168402E+02 | 0.154172E+02 |
| 0.195400E+04 | 0.161314E+02 | 0.168429E+02 | 0.154200E+02 |
| 0.195500E+04 | 0.161348E+02 | 0.168464E+02 | 0.154233E+02 |
| 0.195600E+04 | 0.161219E+02 | 0.168308E+02 | 0.154130E+02 |
| 0.195700E+04 | 0.161122E+02 | 0.168189E+02 | 0.154054E+02 |
| 0.195800E+04 | 0.161226E+02 | 0.168309E+02 | 0.154143E+02 |
| 0.195900E+04 | 0.161304E+02 | 0.168401E+02 | 0.154208E+02 |
| 0.196000E+04 | 0.161354E+02 | 0.168455E+02 | 0.154252E+02 |
| 0.196100E+04 | 0.161256E+02 | 0.168338E+02 | 0.154174E+02 |
| 0.196200E+04 | 0.161092E+02 | 0.168142E+02 | 0.154043E+02 |
| 0.196300E+04 | 0.160903E+02 | 0.167918E+02 | 0.153888E+02 |
| 0.196400E+04 | 0.160471E+02 | 0.167367E+02 | 0.153574E+02 |
| 0.196500E+04 | 0.160007E+02 | 0.166845E+02 | 0.153168E+02 |
| 0.196600E+04 | 0.160069E+02 | 0.167019E+02 | 0.153118E+02 |
| 0.196700E+04 | 0.160263E+02 | 0.167247E+02 | 0.153280E+02 |
| 0.196800E+04 | 0.160322E+02 | 0.167322E+02 | 0.153323E+02 |
| 0.196900E+04 | 0.160108E+02 | 0.167077E+02 | 0.153139E+02 |
| 0.197000E+04 | 0.160088E+02 | 0.167053E+02 | 0.153123E+02 |
| 0.197100E+04 | 0.160394E+02 | 0.167384E+02 | 0.153403E+02 |
| 0.197200E+04 | 0.160742E+02 | 0.167784E+02 | 0.153699E+02 |
| 0.197300E+04 | 0.160989E+02 | 0.168073E+02 | 0.153906E+02 |
| 0.197400E+04 | 0.161076E+02 | 0.168168E+02 | 0.153985E+02 |
| 0.197500E+04 | 0.160913E+02 | 0.167967E+02 | 0.153859E+02 |
| 0.197600E+04 | 0.160825E+02 | 0.167854E+02 | 0.153797E+02 |
| 0.197700E+04 | 0.161111E+02 | 0.168181E+02 | 0.154040E+02 |
| 0.197800E+04 | 0.161441E+02 | 0.168565E+02 | 0.154316E+02 |
| 0.197900E+04 | 0.161642E+02 | 0.168794E+02 | 0.154491E+02 |
| 0.198000E+04 | 0.161841E+02 | 0.169015E+02 | 0.154667E+02 |
| 0.198100E+04 | 0.162102E+02 | 0.169308E+02 | 0.154897E+02 |
| 0.198200E+04 | 0.161477E+02 | 0.168565E+02 | 0.154390E+02 |
| 0.198300E+04 | 0.160077E+02 | 0.166964E+02 | 0.153191E+02 |
| 0.198400E+04 | 0.159574E+02 | 0.166407E+02 | 0.152742E+02 |
| 0.198500E+04 | 0.160346E+02 | 0.167284E+02 | 0.153408E+02 |
| 0.198600E+04 | 0.161027E+02 | 0.168017E+02 | 0.154037E+02 |

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| 0.198700E+04 | 0.161461E+02 | 0.168496E+02 | 0.154425E+02 |
| 0.198800E+04 | 0.161877E+02 | 0.168970E+02 | 0.154783E+02 |
| 0.198900E+04 | 0.162273E+02 | 0.169422E+02 | 0.155124E+02 |
| 0.199000E+04 | 0.162600E+02 | 0.169788E+02 | 0.155411E+02 |
| 0.199100E+04 | 0.162304E+02 | 0.169413E+02 | 0.155195E+02 |
| 0.199200E+04 | 0.160971E+02 | 0.167983E+02 | 0.153959E+02 |
| 0.199300E+04 | 0.160231E+02 | 0.167158E+02 | 0.153304E+02 |
| 0.199400E+04 | 0.160947E+02 | 0.167964E+02 | 0.153929E+02 |
| 0.199500E+04 | 0.161794E+02 | 0.168877E+02 | 0.154710E+02 |
| 0.199600E+04 | 0.162493E+02 | 0.169667E+02 | 0.155319E+02 |
| 0.199700E+04 | 0.162987E+02 | 0.170180E+02 | 0.155794E+02 |
| 0.199800E+04 | 0.163396E+02 | 0.170587E+02 | 0.156205E+02 |

```
#          91
# IPCC AR4 Millenium Runs output (vary solar forcing)
# ++++++
#
# Model: Bern2.5CC version with active C-cycle
# -----
# Prescribed forcing timeseries as described in file
# readme_doRuns_IPCC_Chap6_millennium_21jan06.txt
# provided by F. Joos, University of Bern.
#
# Contact:
# -----
# Gian-Kasper Plattner
# Climate and Environmental Physics
# Physics Institute, University of Bern
# Sidlerstrasse 5, CH-3012 Bern, Switzerland
# plattner@climate.unibe.ch
# http://www.climate.unibe.ch/~plattner/
# tel: ++41 (0)31 631-44-67
# fax: ++41 (0)31 631-87-42
#
# Some model setup informations:
# -----
# All runs with horizontal/vertical diffusion
#
# Run with standard ocean parameters
#   as used in Plattner et al. 2001/2002
#   with Kv (diffusivity) 4*10^-5 m2/s
#
# Climate sens. set to ~ 3.2 degrees C
# parameterized see Knutti et al. (Clim. Dyn. 2003)
#
# Model version is annual mean.
#
# No radiation code, CO2 radiative forcing calculated
# for as RF=5.35*ln(CO2/CO2_preind),
# Non-co2 radiative forcing prescribed according to
# Joos et al. GBC 2001 with updates for solar forcing
#
# More model description:
# -----
```

```

# Zonally averaged dynamical ocean with 3 basins and
# Southern Ocean, zonally averaged one layer energy
# and moisture balance atmosphere, thermodynamic
# sea ice (Stocker et al., J. Climate 1992).
#
# Carbon cycle components: Ocean/Atm/Terr.biosphere;
# Ocean carbon cycle is a description of the cycles
# of organic carbon and CaCO3 (Marchal et al., Tellus
# Tellus B), based on Redfield approach using PO4 as
# biolimiting nutrient.
#
# Land Biota: Lund-Jena-Postdam Dynamical Global
#           Vegetation Model (LPJ-DGVM)
# at GCM resolution (Gerber et al. 2003, Climate
# Dynamics; Sitch et al. 2003, Global Change Biology)
#
# LPJ forced by Cramer/Leemans annual mean
# climatology plus interannual climate variability
# from Hadley simulation (30-recycled climate) plus
# changes in the fields of surface temperature,
# precipitation, and cloudcover as simulated with the
# Impulse-EOF version of ECHAM-3/LSG in response to
# projected radiative forcing changes.
#
# Land use changes are not explicitly considered.
#
# Impact of climate change on terrestrial C-storage
# included
#
# References:
# -----
# Carbon cycle Ocean: Marchal et al., Tellus 1998
# Carbon cycle Terr. Bio: Sitch et al., GCB 2003
#           Gerber et al., Clim. Dyn. 2003
# Ccycle-climate feedbacks and global warming:
#           Plattner et al., Tellus 2001
#           Plattner et al., GBC 2002
# Non-CO2 forcing: Joos et al., GBC 2001
# Climate model: Stocker et al., J. Climate 1992
# Sea level: Knutti et al., J. Climate 2000
# Global warming Physics: Knutti et al., Nature 2002
#           Knutti et al., Cl. Dyn. 2003
#           and refs therein.
#
# Output columns:
# -----
# Time (yr AD)
# Global mean air temperature (deg C)
# NH-averaged air temperature (deg C)
# SH-averaged air temperature (deg C)
0.100100E+04  0.158198E+02  0.164832E+02  0.151565E+02
0.100200E+04  0.158243E+02  0.164878E+02  0.151609E+02
0.100300E+04  0.158278E+02  0.164912E+02  0.151644E+02
0.100400E+04  0.157993E+02  0.164574E+02  0.151413E+02

```

| | | | |
|--------------|--------------|--------------|--------------|
| 0.100500E+04 | 0.157659E+02 | 0.164170E+02 | 0.151147E+02 |
| 0.100600E+04 | 0.157759E+02 | 0.164288E+02 | 0.151229E+02 |
| 0.100700E+04 | 0.157951E+02 | 0.164521E+02 | 0.151380E+02 |
| 0.100800E+04 | 0.158040E+02 | 0.164631E+02 | 0.151448E+02 |
| 0.100900E+04 | 0.158072E+02 | 0.164670E+02 | 0.151474E+02 |
| 0.101000E+04 | 0.158081E+02 | 0.164681E+02 | 0.151481E+02 |
| 0.101100E+04 | 0.158077E+02 | 0.164676E+02 | 0.151478E+02 |
| 0.101200E+04 | 0.158057E+02 | 0.164648E+02 | 0.151465E+02 |
| 0.101300E+04 | 0.158043E+02 | 0.164635E+02 | 0.151451E+02 |
| 0.101400E+04 | 0.158024E+02 | 0.164611E+02 | 0.151437E+02 |
| 0.101500E+04 | 0.157049E+02 | 0.163571E+02 | 0.150527E+02 |
| 0.101600E+04 | 0.155972E+02 | 0.162248E+02 | 0.149697E+02 |
| 0.101700E+04 | 0.156922E+02 | 0.162643E+02 | 0.151202E+02 |
| 0.101800E+04 | 0.158353E+02 | 0.163405E+02 | 0.153301E+02 |
| 0.101900E+04 | 0.158968E+02 | 0.163819E+02 | 0.154116E+02 |
| 0.102000E+04 | 0.159173E+02 | 0.164048E+02 | 0.154297E+02 |
| 0.102100E+04 | 0.159278E+02 | 0.164205E+02 | 0.154351E+02 |
| 0.102200E+04 | 0.159328E+02 | 0.164321E+02 | 0.154335E+02 |
| 0.102300E+04 | 0.159343E+02 | 0.164407E+02 | 0.154280E+02 |
| 0.102400E+04 | 0.159336E+02 | 0.164470E+02 | 0.154202E+02 |
| 0.102500E+04 | 0.159334E+02 | 0.164515E+02 | 0.154153E+02 |
| 0.102600E+04 | 0.158073E+02 | 0.163080E+02 | 0.153065E+02 |
| 0.102700E+04 | 0.156707E+02 | 0.161542E+02 | 0.151872E+02 |
| 0.102800E+04 | 0.157090E+02 | 0.162032E+02 | 0.152148E+02 |
| 0.102900E+04 | 0.157763E+02 | 0.162882E+02 | 0.152644E+02 |
| 0.103000E+04 | 0.158056E+02 | 0.163276E+02 | 0.152836E+02 |
| 0.103100E+04 | 0.158192E+02 | 0.163487E+02 | 0.152896E+02 |
| 0.103200E+04 | 0.158266E+02 | 0.163619E+02 | 0.152913E+02 |
| 0.103300E+04 | 0.158306E+02 | 0.163703E+02 | 0.152909E+02 |
| 0.103400E+04 | 0.158323E+02 | 0.163755E+02 | 0.152891E+02 |
| 0.103500E+04 | 0.158325E+02 | 0.163784E+02 | 0.152866E+02 |
| 0.103600E+04 | 0.158318E+02 | 0.163799E+02 | 0.152837E+02 |
| 0.103700E+04 | 0.158306E+02 | 0.163806E+02 | 0.152807E+02 |
| 0.103800E+04 | 0.158291E+02 | 0.163805E+02 | 0.152777E+02 |
| 0.103900E+04 | 0.158272E+02 | 0.163798E+02 | 0.152746E+02 |
| 0.104000E+04 | 0.158251E+02 | 0.163787E+02 | 0.152715E+02 |
| 0.104100E+04 | 0.158228E+02 | 0.163772E+02 | 0.152684E+02 |
| 0.104200E+04 | 0.158204E+02 | 0.163754E+02 | 0.152653E+02 |
| 0.104300E+04 | 0.158178E+02 | 0.163734E+02 | 0.152623E+02 |
| 0.104400E+04 | 0.158152E+02 | 0.163712E+02 | 0.152592E+02 |
| 0.104500E+04 | 0.158126E+02 | 0.163690E+02 | 0.152563E+02 |
| 0.104600E+04 | 0.158103E+02 | 0.163669E+02 | 0.152536E+02 |
| 0.104700E+04 | 0.158080E+02 | 0.163649E+02 | 0.152510E+02 |
| 0.104800E+04 | 0.158058E+02 | 0.163630E+02 | 0.152486E+02 |
| 0.104900E+04 | 0.158037E+02 | 0.163611E+02 | 0.152462E+02 |
| 0.105000E+04 | 0.158016E+02 | 0.163592E+02 | 0.152439E+02 |
| 0.105100E+04 | 0.157995E+02 | 0.163574E+02 | 0.152417E+02 |
| 0.105200E+04 | 0.157976E+02 | 0.163555E+02 | 0.152396E+02 |
| 0.105300E+04 | 0.157956E+02 | 0.163537E+02 | 0.152375E+02 |
| 0.105400E+04 | 0.157937E+02 | 0.163520E+02 | 0.152355E+02 |
| 0.105500E+04 | 0.157920E+02 | 0.163503E+02 | 0.152336E+02 |
| 0.105600E+04 | 0.157904E+02 | 0.163489E+02 | 0.152320E+02 |
| 0.105700E+04 | 0.157890E+02 | 0.163477E+02 | 0.152304E+02 |
| 0.105800E+04 | 0.156301E+02 | 0.161679E+02 | 0.150922E+02 |

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| 0.105900E+04 | 0.154447E+02 | 0.159496E+02 | 0.149398E+02 |
| 0.106000E+04 | 0.155357E+02 | 0.160581E+02 | 0.150132E+02 |
| 0.106100E+04 | 0.156325E+02 | 0.161633E+02 | 0.151018E+02 |
| 0.106200E+04 | 0.156342E+02 | 0.161646E+02 | 0.151038E+02 |
| 0.106300E+04 | 0.156119E+02 | 0.161382E+02 | 0.150856E+02 |
| 0.106400E+04 | 0.156460E+02 | 0.161794E+02 | 0.151126E+02 |
| 0.106500E+04 | 0.156822E+02 | 0.162208E+02 | 0.151436E+02 |
| 0.106600E+04 | 0.157069E+02 | 0.162513E+02 | 0.151626E+02 |
| 0.106700E+04 | 0.157247E+02 | 0.162733E+02 | 0.151762E+02 |
| 0.106800E+04 | 0.157394E+02 | 0.162911E+02 | 0.151878E+02 |
| 0.106900E+04 | 0.157523E+02 | 0.163065E+02 | 0.151981E+02 |
| 0.107000E+04 | 0.157640E+02 | 0.163203E+02 | 0.152077E+02 |
| 0.107100E+04 | 0.157749E+02 | 0.163330E+02 | 0.152168E+02 |
| 0.107200E+04 | 0.157851E+02 | 0.163448E+02 | 0.152254E+02 |
| 0.107300E+04 | 0.157949E+02 | 0.163560E+02 | 0.152337E+02 |
| 0.107400E+04 | 0.158043E+02 | 0.163667E+02 | 0.152418E+02 |
| 0.107500E+04 | 0.158134E+02 | 0.163771E+02 | 0.152497E+02 |
| 0.107600E+04 | 0.158220E+02 | 0.163869E+02 | 0.152571E+02 |
| 0.107700E+04 | 0.158298E+02 | 0.163957E+02 | 0.152639E+02 |
| 0.107800E+04 | 0.158370E+02 | 0.164038E+02 | 0.152702E+02 |
| 0.107900E+04 | 0.158400E+02 | 0.164116E+02 | 0.152683E+02 |
| 0.108000E+04 | 0.158048E+02 | 0.163826E+02 | 0.152269E+02 |
| 0.108100E+04 | 0.157708E+02 | 0.163494E+02 | 0.151922E+02 |
| 0.108200E+04 | 0.157538E+02 | 0.163680E+02 | 0.151397E+02 |
| 0.108300E+04 | 0.157311E+02 | 0.163982E+02 | 0.150640E+02 |
| 0.108400E+04 | 0.157226E+02 | 0.164154E+02 | 0.150299E+02 |
| 0.108500E+04 | 0.157160E+02 | 0.164249E+02 | 0.150071E+02 |
| 0.108600E+04 | 0.157110E+02 | 0.164313E+02 | 0.149906E+02 |
| 0.108700E+04 | 0.157089E+02 | 0.164360E+02 | 0.149817E+02 |
| 0.108800E+04 | 0.157096E+02 | 0.164399E+02 | 0.149794E+02 |
| 0.108900E+04 | 0.157124E+02 | 0.164433E+02 | 0.149816E+02 |
| 0.109000E+04 | 0.157168E+02 | 0.164467E+02 | 0.149869E+02 |
| 0.109100E+04 | 0.157223E+02 | 0.164502E+02 | 0.149944E+02 |
| 0.109200E+04 | 0.157287E+02 | 0.164539E+02 | 0.150035E+02 |
| 0.109300E+04 | 0.157358E+02 | 0.164580E+02 | 0.150136E+02 |
| 0.109400E+04 | 0.157432E+02 | 0.164622E+02 | 0.150242E+02 |
| 0.109500E+04 | 0.157509E+02 | 0.164668E+02 | 0.150350E+02 |
| 0.109600E+04 | 0.157588E+02 | 0.164717E+02 | 0.150458E+02 |
| 0.109700E+04 | 0.156945E+02 | 0.163925E+02 | 0.149966E+02 |
| 0.109800E+04 | 0.156362E+02 | 0.163297E+02 | 0.149426E+02 |
| 0.109900E+04 | 0.156661E+02 | 0.163605E+02 | 0.149717E+02 |
| 0.110000E+04 | 0.157151E+02 | 0.164127E+02 | 0.150175E+02 |
| 0.110100E+04 | 0.157415E+02 | 0.164386E+02 | 0.150443E+02 |
| 0.110200E+04 | 0.157598E+02 | 0.164572E+02 | 0.150623E+02 |
| 0.110300E+04 | 0.157741E+02 | 0.164720E+02 | 0.150762E+02 |
| 0.110400E+04 | 0.157834E+02 | 0.164844E+02 | 0.150824E+02 |
| 0.110500E+04 | 0.157919E+02 | 0.164952E+02 | 0.150887E+02 |
| 0.110600E+04 | 0.158000E+02 | 0.165047E+02 | 0.150953E+02 |
| 0.110700E+04 | 0.158079E+02 | 0.165135E+02 | 0.151023E+02 |
| 0.110800E+04 | 0.158155E+02 | 0.165217E+02 | 0.151093E+02 |
| 0.110900E+04 | 0.158233E+02 | 0.165301E+02 | 0.151166E+02 |
| 0.111000E+04 | 0.158318E+02 | 0.165391E+02 | 0.151245E+02 |
| 0.111100E+04 | 0.158407E+02 | 0.165486E+02 | 0.151327E+02 |
| 0.111200E+04 | 0.158496E+02 | 0.165582E+02 | 0.151411E+02 |

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| 0.111300E+04 | 0.158587E+02 | 0.165679E+02 | 0.151494E+02 |
| 0.111400E+04 | 0.158678E+02 | 0.165777E+02 | 0.151578E+02 |
| 0.111500E+04 | 0.158761E+02 | 0.165868E+02 | 0.151655E+02 |
| 0.111600E+04 | 0.158832E+02 | 0.165944E+02 | 0.151721E+02 |
| 0.111700E+04 | 0.158895E+02 | 0.166011E+02 | 0.151780E+02 |
| 0.111800E+04 | 0.158952E+02 | 0.166071E+02 | 0.151833E+02 |
| 0.111900E+04 | 0.159005E+02 | 0.166128E+02 | 0.151883E+02 |
| 0.112000E+04 | 0.159055E+02 | 0.166181E+02 | 0.151929E+02 |
| 0.112100E+04 | 0.159101E+02 | 0.166231E+02 | 0.151972E+02 |
| 0.112200E+04 | 0.159145E+02 | 0.166279E+02 | 0.152012E+02 |
| 0.112300E+04 | 0.159187E+02 | 0.166324E+02 | 0.152050E+02 |
| 0.112400E+04 | 0.159227E+02 | 0.166368E+02 | 0.152087E+02 |
| 0.112500E+04 | 0.159265E+02 | 0.166410E+02 | 0.152121E+02 |
| 0.112600E+04 | 0.159303E+02 | 0.166450E+02 | 0.152155E+02 |
| 0.112700E+04 | 0.159339E+02 | 0.166490E+02 | 0.152187E+02 |
| 0.112800E+04 | 0.159373E+02 | 0.166528E+02 | 0.152219E+02 |
| 0.112900E+04 | 0.159407E+02 | 0.166566E+02 | 0.152249E+02 |
| 0.113000E+04 | 0.159440E+02 | 0.166602E+02 | 0.152279E+02 |
| 0.113100E+04 | 0.159472E+02 | 0.166637E+02 | 0.152308E+02 |
| 0.113200E+04 | 0.159504E+02 | 0.166671E+02 | 0.152336E+02 |
| 0.113300E+04 | 0.159538E+02 | 0.166708E+02 | 0.152367E+02 |
| 0.113400E+04 | 0.159577E+02 | 0.166752E+02 | 0.152403E+02 |
| 0.113500E+04 | 0.159618E+02 | 0.166797E+02 | 0.152439E+02 |
| 0.113600E+04 | 0.159657E+02 | 0.166839E+02 | 0.152474E+02 |
| 0.113700E+04 | 0.159694E+02 | 0.166880E+02 | 0.152509E+02 |
| 0.113800E+04 | 0.159731E+02 | 0.166919E+02 | 0.152543E+02 |
| 0.113900E+04 | 0.159765E+02 | 0.166955E+02 | 0.152575E+02 |
| 0.114000E+04 | 0.159795E+02 | 0.166986E+02 | 0.152604E+02 |
| 0.114100E+04 | 0.159821E+02 | 0.167012E+02 | 0.152629E+02 |
| 0.114200E+04 | 0.159844E+02 | 0.167036E+02 | 0.152653E+02 |
| 0.114300E+04 | 0.159866E+02 | 0.167057E+02 | 0.152676E+02 |
| 0.114400E+04 | 0.159887E+02 | 0.167077E+02 | 0.152697E+02 |
| 0.114500E+04 | 0.159906E+02 | 0.167095E+02 | 0.152718E+02 |
| 0.114600E+04 | 0.159925E+02 | 0.167112E+02 | 0.152737E+02 |
| 0.114700E+04 | 0.159940E+02 | 0.167125E+02 | 0.152754E+02 |
| 0.114800E+04 | 0.159950E+02 | 0.167133E+02 | 0.152767E+02 |
| 0.114900E+04 | 0.159957E+02 | 0.167137E+02 | 0.152778E+02 |
| 0.115000E+04 | 0.159962E+02 | 0.167138E+02 | 0.152786E+02 |
| 0.115100E+04 | 0.159965E+02 | 0.167137E+02 | 0.152793E+02 |
| 0.115200E+04 | 0.159966E+02 | 0.167134E+02 | 0.152798E+02 |
| 0.115300E+04 | 0.159966E+02 | 0.167130E+02 | 0.152803E+02 |
| 0.115400E+04 | 0.159965E+02 | 0.167124E+02 | 0.152806E+02 |
| 0.115500E+04 | 0.159962E+02 | 0.167117E+02 | 0.152808E+02 |
| 0.115600E+04 | 0.159956E+02 | 0.167106E+02 | 0.152807E+02 |
| 0.115700E+04 | 0.159945E+02 | 0.167088E+02 | 0.152802E+02 |
| 0.115800E+04 | 0.159930E+02 | 0.167066E+02 | 0.152793E+02 |
| 0.115900E+04 | 0.159911E+02 | 0.167041E+02 | 0.152782E+02 |
| 0.116000E+04 | 0.159891E+02 | 0.167013E+02 | 0.152769E+02 |
| 0.116100E+04 | 0.159869E+02 | 0.166984E+02 | 0.152754E+02 |
| 0.116200E+04 | 0.159852E+02 | 0.166961E+02 | 0.152744E+02 |
| 0.116300E+04 | 0.159846E+02 | 0.166950E+02 | 0.152742E+02 |
| 0.116400E+04 | 0.159845E+02 | 0.166946E+02 | 0.152745E+02 |
| 0.116500E+04 | 0.159849E+02 | 0.166947E+02 | 0.152751E+02 |
| 0.116600E+04 | 0.159428E+02 | 0.166451E+02 | 0.152404E+02 |

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| 0.116700E+04 | 0.158959E+02 | 0.165897E+02 | 0.152021E+02 |
| 0.116800E+04 | 0.159091E+02 | 0.166057E+02 | 0.152125E+02 |
| 0.116900E+04 | 0.159352E+02 | 0.166373E+02 | 0.152331E+02 |
| 0.117000E+04 | 0.159482E+02 | 0.166529E+02 | 0.152434E+02 |
| 0.117100E+04 | 0.159542E+02 | 0.166597E+02 | 0.152486E+02 |
| 0.117200E+04 | 0.159575E+02 | 0.166631E+02 | 0.152518E+02 |
| 0.117300E+04 | 0.159593E+02 | 0.166647E+02 | 0.152539E+02 |
| 0.117400E+04 | 0.159602E+02 | 0.166651E+02 | 0.152553E+02 |
| 0.117500E+04 | 0.157061E+02 | 0.163789E+02 | 0.150332E+02 |
| 0.117600E+04 | 0.155551E+02 | 0.160890E+02 | 0.150211E+02 |
| 0.117700E+04 | 0.156680E+02 | 0.161564E+02 | 0.151796E+02 |
| 0.117800E+04 | 0.158097E+02 | 0.162988E+02 | 0.153207E+02 |
| 0.117900E+04 | 0.159020E+02 | 0.163952E+02 | 0.154089E+02 |
| 0.118000E+04 | 0.159557E+02 | 0.164593E+02 | 0.154521E+02 |
| 0.118100E+04 | 0.159869E+02 | 0.164999E+02 | 0.154740E+02 |
| 0.118200E+04 | 0.160078E+02 | 0.165298E+02 | 0.154859E+02 |
| 0.118300E+04 | 0.160225E+02 | 0.165528E+02 | 0.154922E+02 |
| 0.118400E+04 | 0.160353E+02 | 0.165710E+02 | 0.154997E+02 |
| 0.118500E+04 | 0.160428E+02 | 0.165854E+02 | 0.155002E+02 |
| 0.118600E+04 | 0.160485E+02 | 0.165969E+02 | 0.155001E+02 |
| 0.118700E+04 | 0.160526E+02 | 0.166060E+02 | 0.154991E+02 |
| 0.118800E+04 | 0.160766E+02 | 0.166547E+02 | 0.154985E+02 |
| 0.118900E+04 | 0.160785E+02 | 0.166574E+02 | 0.154996E+02 |
| 0.119000E+04 | 0.160782E+02 | 0.166579E+02 | 0.154985E+02 |
| 0.119100E+04 | 0.160780E+02 | 0.166591E+02 | 0.154970E+02 |
| 0.119200E+04 | 0.160753E+02 | 0.166606E+02 | 0.154901E+02 |
| 0.119300E+04 | 0.160652E+02 | 0.166627E+02 | 0.154678E+02 |
| 0.119400E+04 | 0.159646E+02 | 0.165549E+02 | 0.153743E+02 |
| 0.119500E+04 | 0.158724E+02 | 0.164638E+02 | 0.152809E+02 |
| 0.119600E+04 | 0.158950E+02 | 0.164984E+02 | 0.152916E+02 |
| 0.119700E+04 | 0.159428E+02 | 0.165637E+02 | 0.153219E+02 |
| 0.119800E+04 | 0.159032E+02 | 0.165916E+02 | 0.152148E+02 |
| 0.119900E+04 | 0.158916E+02 | 0.166071E+02 | 0.151761E+02 |
| 0.120000E+04 | 0.158844E+02 | 0.166165E+02 | 0.151523E+02 |
| 0.120100E+04 | 0.158801E+02 | 0.166220E+02 | 0.151382E+02 |
| 0.120200E+04 | 0.158785E+02 | 0.166253E+02 | 0.151316E+02 |
| 0.120300E+04 | 0.158775E+02 | 0.166273E+02 | 0.151277E+02 |
| 0.120400E+04 | 0.158785E+02 | 0.166281E+02 | 0.151289E+02 |
| 0.120500E+04 | 0.158302E+02 | 0.165689E+02 | 0.150915E+02 |
| 0.120600E+04 | 0.157769E+02 | 0.165023E+02 | 0.150514E+02 |
| 0.120700E+04 | 0.158087E+02 | 0.165443E+02 | 0.150730E+02 |
| 0.120800E+04 | 0.158443E+02 | 0.165808E+02 | 0.151079E+02 |
| 0.120900E+04 | 0.158653E+02 | 0.166001E+02 | 0.151305E+02 |
| 0.121000E+04 | 0.158786E+02 | 0.166102E+02 | 0.151469E+02 |
| 0.121100E+04 | 0.158889E+02 | 0.166169E+02 | 0.151609E+02 |
| 0.121200E+04 | 0.158974E+02 | 0.166217E+02 | 0.151732E+02 |
| 0.121300E+04 | 0.159048E+02 | 0.166253E+02 | 0.151842E+02 |
| 0.121400E+04 | 0.159113E+02 | 0.166284E+02 | 0.151942E+02 |
| 0.121500E+04 | 0.159171E+02 | 0.166310E+02 | 0.152032E+02 |
| 0.121600E+04 | 0.159224E+02 | 0.166335E+02 | 0.152113E+02 |
| 0.121700E+04 | 0.159272E+02 | 0.166357E+02 | 0.152186E+02 |
| 0.121800E+04 | 0.159263E+02 | 0.166278E+02 | 0.152248E+02 |
| 0.121900E+04 | 0.159273E+02 | 0.166248E+02 | 0.152299E+02 |
| 0.122000E+04 | 0.159301E+02 | 0.166254E+02 | 0.152348E+02 |

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| 0.122100E+04 | 0.159331E+02 | 0.166269E+02 | 0.152393E+02 |
| 0.122200E+04 | 0.159361E+02 | 0.166288E+02 | 0.152434E+02 |
| 0.122300E+04 | 0.159390E+02 | 0.166308E+02 | 0.152472E+02 |
| 0.122400E+04 | 0.159418E+02 | 0.166328E+02 | 0.152507E+02 |
| 0.122500E+04 | 0.159444E+02 | 0.166350E+02 | 0.152539E+02 |
| 0.122600E+04 | 0.159470E+02 | 0.166371E+02 | 0.152568E+02 |
| 0.122700E+04 | 0.159183E+02 | 0.166029E+02 | 0.152336E+02 |
| 0.122800E+04 | 0.158857E+02 | 0.165640E+02 | 0.152075E+02 |
| 0.122900E+04 | 0.157160E+02 | 0.163768E+02 | 0.150552E+02 |
| 0.123000E+04 | 0.155305E+02 | 0.161578E+02 | 0.149031E+02 |
| 0.123100E+04 | 0.156023E+02 | 0.162576E+02 | 0.149471E+02 |
| 0.123200E+04 | 0.158348E+02 | 0.163672E+02 | 0.153024E+02 |
| 0.123300E+04 | 0.159388E+02 | 0.164432E+02 | 0.154343E+02 |
| 0.123400E+04 | 0.159826E+02 | 0.164817E+02 | 0.154834E+02 |
| 0.123500E+04 | 0.160090E+02 | 0.165129E+02 | 0.155051E+02 |
| 0.123600E+04 | 0.160305E+02 | 0.165364E+02 | 0.155247E+02 |
| 0.123700E+04 | 0.160418E+02 | 0.165544E+02 | 0.155293E+02 |
| 0.123800E+04 | 0.160491E+02 | 0.165688E+02 | 0.155293E+02 |
| 0.123900E+04 | 0.160539E+02 | 0.165807E+02 | 0.155271E+02 |
| 0.124000E+04 | 0.160592E+02 | 0.165906E+02 | 0.155278E+02 |
| 0.124100E+04 | 0.160611E+02 | 0.165986E+02 | 0.155237E+02 |
| 0.124200E+04 | 0.160843E+02 | 0.166467E+02 | 0.155218E+02 |
| 0.124300E+04 | 0.160824E+02 | 0.166463E+02 | 0.155184E+02 |
| 0.124400E+04 | 0.160807E+02 | 0.166474E+02 | 0.155139E+02 |
| 0.124500E+04 | 0.160789E+02 | 0.166485E+02 | 0.155093E+02 |
| 0.124600E+04 | 0.160769E+02 | 0.166490E+02 | 0.155047E+02 |
| 0.124700E+04 | 0.160746E+02 | 0.166490E+02 | 0.155002E+02 |
| 0.124800E+04 | 0.160721E+02 | 0.166484E+02 | 0.154957E+02 |
| 0.124900E+04 | 0.160626E+02 | 0.166474E+02 | 0.154779E+02 |
| 0.125000E+04 | 0.160491E+02 | 0.166458E+02 | 0.154524E+02 |
| 0.125100E+04 | 0.160367E+02 | 0.166437E+02 | 0.154298E+02 |
| 0.125200E+04 | 0.160257E+02 | 0.166410E+02 | 0.154104E+02 |
| 0.125300E+04 | 0.159714E+02 | 0.166378E+02 | 0.153049E+02 |
| 0.125400E+04 | 0.159320E+02 | 0.166324E+02 | 0.152317E+02 |
| 0.125500E+04 | 0.159047E+02 | 0.166241E+02 | 0.151854E+02 |
| 0.125600E+04 | 0.158828E+02 | 0.166139E+02 | 0.151518E+02 |
| 0.125700E+04 | 0.158651E+02 | 0.166026E+02 | 0.151276E+02 |
| 0.125800E+04 | 0.158516E+02 | 0.165919E+02 | 0.151112E+02 |
| 0.125900E+04 | 0.154066E+02 | 0.160746E+02 | 0.147385E+02 |
| 0.126000E+04 | 0.151789E+02 | 0.159923E+02 | 0.143655E+02 |
| 0.126100E+04 | 0.153780E+02 | 0.163057E+02 | 0.144502E+02 |
| 0.126200E+04 | 0.155707E+02 | 0.164931E+02 | 0.146483E+02 |
| 0.126300E+04 | 0.157044E+02 | 0.166117E+02 | 0.147971E+02 |
| 0.126400E+04 | 0.157668E+02 | 0.166547E+02 | 0.148790E+02 |
| 0.126500E+04 | 0.157988E+02 | 0.166676E+02 | 0.149300E+02 |
| 0.126600E+04 | 0.158190E+02 | 0.166698E+02 | 0.149681E+02 |
| 0.126700E+04 | 0.158328E+02 | 0.166672E+02 | 0.149984E+02 |
| 0.126800E+04 | 0.158402E+02 | 0.166622E+02 | 0.150182E+02 |
| 0.126900E+04 | 0.158443E+02 | 0.166562E+02 | 0.150324E+02 |
| 0.127000E+04 | 0.158475E+02 | 0.166500E+02 | 0.150451E+02 |
| 0.127100E+04 | 0.158502E+02 | 0.166441E+02 | 0.150563E+02 |
| 0.127200E+04 | 0.158528E+02 | 0.166392E+02 | 0.150663E+02 |
| 0.127300E+04 | 0.158547E+02 | 0.166344E+02 | 0.150751E+02 |
| 0.127400E+04 | 0.158561E+02 | 0.166298E+02 | 0.150824E+02 |

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| 0.127500E+04 | 0.157561E+02 | 0.165083E+02 | 0.150038E+02 |
| 0.127600E+04 | 0.156436E+02 | 0.163829E+02 | 0.149043E+02 |
| 0.127700E+04 | 0.156838E+02 | 0.164215E+02 | 0.149462E+02 |
| 0.127800E+04 | 0.157466E+02 | 0.164854E+02 | 0.150078E+02 |
| 0.127900E+04 | 0.157763E+02 | 0.165146E+02 | 0.150380E+02 |
| 0.128000E+04 | 0.157896E+02 | 0.165289E+02 | 0.150502E+02 |
| 0.128100E+04 | 0.157950E+02 | 0.165382E+02 | 0.150519E+02 |
| 0.128200E+04 | 0.158011E+02 | 0.165447E+02 | 0.150575E+02 |
| 0.128300E+04 | 0.158063E+02 | 0.165496E+02 | 0.150630E+02 |
| 0.128400E+04 | 0.158105E+02 | 0.165533E+02 | 0.150677E+02 |
| 0.128500E+04 | 0.156789E+02 | 0.164073E+02 | 0.149505E+02 |
| 0.128600E+04 | 0.155441E+02 | 0.162566E+02 | 0.148316E+02 |
| 0.128700E+04 | 0.156781E+02 | 0.163160E+02 | 0.150403E+02 |
| 0.128800E+04 | 0.158254E+02 | 0.164030E+02 | 0.152478E+02 |
| 0.128900E+04 | 0.158797E+02 | 0.164310E+02 | 0.153284E+02 |
| 0.129000E+04 | 0.158992E+02 | 0.164390E+02 | 0.153593E+02 |
| 0.129100E+04 | 0.158601E+02 | 0.163505E+02 | 0.153698E+02 |
| 0.129200E+04 | 0.158519E+02 | 0.163342E+02 | 0.153696E+02 |
| 0.129300E+04 | 0.158478E+02 | 0.163252E+02 | 0.153704E+02 |
| 0.129400E+04 | 0.158432E+02 | 0.163233E+02 | 0.153632E+02 |
| 0.129500E+04 | 0.157342E+02 | 0.162231E+02 | 0.152453E+02 |
| 0.129600E+04 | 0.155999E+02 | 0.160736E+02 | 0.151262E+02 |
| 0.129700E+04 | 0.156462E+02 | 0.161356E+02 | 0.151568E+02 |
| 0.129800E+04 | 0.157221E+02 | 0.162319E+02 | 0.152122E+02 |
| 0.129900E+04 | 0.157562E+02 | 0.162769E+02 | 0.152354E+02 |
| 0.130000E+04 | 0.157744E+02 | 0.163043E+02 | 0.152444E+02 |
| 0.130100E+04 | 0.157867E+02 | 0.163246E+02 | 0.152488E+02 |
| 0.130200E+04 | 0.157957E+02 | 0.163406E+02 | 0.152509E+02 |
| 0.130300E+04 | 0.158027E+02 | 0.163537E+02 | 0.152517E+02 |
| 0.130400E+04 | 0.158081E+02 | 0.163645E+02 | 0.152517E+02 |
| 0.130500E+04 | 0.158125E+02 | 0.163737E+02 | 0.152513E+02 |
| 0.130600E+04 | 0.158160E+02 | 0.163814E+02 | 0.152505E+02 |
| 0.130700E+04 | 0.158188E+02 | 0.163879E+02 | 0.152496E+02 |
| 0.130800E+04 | 0.158210E+02 | 0.163935E+02 | 0.152485E+02 |
| 0.130900E+04 | 0.158229E+02 | 0.163984E+02 | 0.152473E+02 |
| 0.131000E+04 | 0.158244E+02 | 0.164028E+02 | 0.152461E+02 |
| 0.131100E+04 | 0.158258E+02 | 0.164066E+02 | 0.152449E+02 |
| 0.131200E+04 | 0.158269E+02 | 0.164101E+02 | 0.152437E+02 |
| 0.131300E+04 | 0.158278E+02 | 0.164131E+02 | 0.152426E+02 |
| 0.131400E+04 | 0.158286E+02 | 0.164157E+02 | 0.152415E+02 |
| 0.131500E+04 | 0.158292E+02 | 0.164180E+02 | 0.152404E+02 |
| 0.131600E+04 | 0.158297E+02 | 0.164201E+02 | 0.152394E+02 |
| 0.131700E+04 | 0.158301E+02 | 0.164218E+02 | 0.152384E+02 |
| 0.131800E+04 | 0.158304E+02 | 0.164233E+02 | 0.152374E+02 |
| 0.131900E+04 | 0.158305E+02 | 0.164245E+02 | 0.152365E+02 |
| 0.132000E+04 | 0.158306E+02 | 0.164256E+02 | 0.152356E+02 |
| 0.132100E+04 | 0.158306E+02 | 0.164264E+02 | 0.152347E+02 |
| 0.132200E+04 | 0.158305E+02 | 0.164271E+02 | 0.152339E+02 |
| 0.132300E+04 | 0.158305E+02 | 0.164278E+02 | 0.152332E+02 |
| 0.132400E+04 | 0.158307E+02 | 0.164287E+02 | 0.152327E+02 |
| 0.132500E+04 | 0.158312E+02 | 0.164299E+02 | 0.152324E+02 |
| 0.132600E+04 | 0.158315E+02 | 0.164308E+02 | 0.152322E+02 |
| 0.132700E+04 | 0.158319E+02 | 0.164318E+02 | 0.152321E+02 |
| 0.132800E+04 | 0.158324E+02 | 0.164327E+02 | 0.152320E+02 |

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| 0.132900E+04 | 0.157141E+02 | 0.162918E+02 | 0.151363E+02 |
| 0.133000E+04 | 0.156046E+02 | 0.161702E+02 | 0.150389E+02 |
| 0.133100E+04 | 0.156442E+02 | 0.162173E+02 | 0.150711E+02 |
| 0.133200E+04 | 0.157106E+02 | 0.162948E+02 | 0.151263E+02 |
| 0.133300E+04 | 0.157420E+02 | 0.163337E+02 | 0.151504E+02 |
| 0.133400E+04 | 0.157600E+02 | 0.163560E+02 | 0.151639E+02 |
| 0.133500E+04 | 0.157732E+02 | 0.163725E+02 | 0.151740E+02 |
| 0.133600E+04 | 0.157850E+02 | 0.163868E+02 | 0.151833E+02 |
| 0.133700E+04 | 0.157958E+02 | 0.163998E+02 | 0.151919E+02 |
| 0.133800E+04 | 0.158060E+02 | 0.164118E+02 | 0.152001E+02 |
| 0.133900E+04 | 0.158156E+02 | 0.164232E+02 | 0.152080E+02 |
| 0.134000E+04 | 0.158241E+02 | 0.164330E+02 | 0.152151E+02 |
| 0.134100E+04 | 0.158309E+02 | 0.164408E+02 | 0.152210E+02 |
| 0.134200E+04 | 0.158366E+02 | 0.164472E+02 | 0.152260E+02 |
| 0.134300E+04 | 0.158416E+02 | 0.164526E+02 | 0.152305E+02 |
| 0.134400E+04 | 0.158414E+02 | 0.164574E+02 | 0.152255E+02 |
| 0.134500E+04 | 0.157354E+02 | 0.163394E+02 | 0.151314E+02 |
| 0.134600E+04 | 0.156320E+02 | 0.162377E+02 | 0.150263E+02 |
| 0.134700E+04 | 0.156879E+02 | 0.162778E+02 | 0.150981E+02 |
| 0.134800E+04 | 0.157488E+02 | 0.163464E+02 | 0.151512E+02 |
| 0.134900E+04 | 0.157809E+02 | 0.163830E+02 | 0.151788E+02 |
| 0.135000E+04 | 0.157996E+02 | 0.164046E+02 | 0.151947E+02 |
| 0.135100E+04 | 0.158130E+02 | 0.164199E+02 | 0.152062E+02 |
| 0.135200E+04 | 0.158234E+02 | 0.164316E+02 | 0.152152E+02 |
| 0.135300E+04 | 0.158316E+02 | 0.164408E+02 | 0.152224E+02 |
| 0.135400E+04 | 0.158384E+02 | 0.164484E+02 | 0.152285E+02 |
| 0.135500E+04 | 0.158404E+02 | 0.164547E+02 | 0.152261E+02 |
| 0.135600E+04 | 0.158336E+02 | 0.164599E+02 | 0.152073E+02 |
| 0.135700E+04 | 0.158276E+02 | 0.164644E+02 | 0.151908E+02 |
| 0.135800E+04 | 0.157768E+02 | 0.164683E+02 | 0.150853E+02 |
| 0.135900E+04 | 0.157462E+02 | 0.164709E+02 | 0.150215E+02 |
| 0.136000E+04 | 0.157277E+02 | 0.164723E+02 | 0.149831E+02 |
| 0.136100E+04 | 0.157152E+02 | 0.164728E+02 | 0.149575E+02 |
| 0.136200E+04 | 0.157056E+02 | 0.164729E+02 | 0.149383E+02 |
| 0.136300E+04 | 0.157006E+02 | 0.164726E+02 | 0.149286E+02 |
| 0.136400E+04 | 0.156988E+02 | 0.164725E+02 | 0.149252E+02 |
| 0.136500E+04 | 0.156995E+02 | 0.164725E+02 | 0.149264E+02 |
| 0.136600E+04 | 0.157020E+02 | 0.164730E+02 | 0.149309E+02 |
| 0.136700E+04 | 0.157058E+02 | 0.164739E+02 | 0.149378E+02 |
| 0.136800E+04 | 0.157107E+02 | 0.164752E+02 | 0.149461E+02 |
| 0.136900E+04 | 0.157162E+02 | 0.164769E+02 | 0.149555E+02 |
| 0.137000E+04 | 0.157223E+02 | 0.164791E+02 | 0.149655E+02 |
| 0.137100E+04 | 0.157287E+02 | 0.164817E+02 | 0.149756E+02 |
| 0.137200E+04 | 0.157352E+02 | 0.164846E+02 | 0.149859E+02 |
| 0.137300E+04 | 0.157418E+02 | 0.164878E+02 | 0.149959E+02 |
| 0.137400E+04 | 0.157482E+02 | 0.164909E+02 | 0.150055E+02 |
| 0.137500E+04 | 0.157110E+02 | 0.164436E+02 | 0.149784E+02 |
| 0.137600E+04 | 0.156687E+02 | 0.163899E+02 | 0.149474E+02 |
| 0.137700E+04 | 0.156873E+02 | 0.164093E+02 | 0.149653E+02 |
| 0.137800E+04 | 0.157195E+02 | 0.164457E+02 | 0.149934E+02 |
| 0.137900E+04 | 0.157384E+02 | 0.164659E+02 | 0.150109E+02 |
| 0.138000E+04 | 0.157501E+02 | 0.164773E+02 | 0.150229E+02 |
| 0.138100E+04 | 0.157590E+02 | 0.164854E+02 | 0.150326E+02 |
| 0.138200E+04 | 0.157664E+02 | 0.164918E+02 | 0.150409E+02 |

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| 0.138300E+04 | 0.157726E+02 | 0.164971E+02 | 0.150481E+02 |
| 0.138400E+04 | 0.157778E+02 | 0.165012E+02 | 0.150543E+02 |
| 0.138500E+04 | 0.157819E+02 | 0.165043E+02 | 0.150595E+02 |
| 0.138600E+04 | 0.157852E+02 | 0.165066E+02 | 0.150638E+02 |
| 0.138700E+04 | 0.157449E+02 | 0.164583E+02 | 0.150316E+02 |
| 0.138800E+04 | 0.156993E+02 | 0.164031E+02 | 0.149955E+02 |
| 0.138900E+04 | 0.157147E+02 | 0.164210E+02 | 0.150084E+02 |
| 0.139000E+04 | 0.157435E+02 | 0.164554E+02 | 0.150316E+02 |
| 0.139100E+04 | 0.157590E+02 | 0.164736E+02 | 0.150443E+02 |
| 0.139200E+04 | 0.157671E+02 | 0.164825E+02 | 0.150516E+02 |
| 0.139300E+04 | 0.157720E+02 | 0.164876E+02 | 0.150564E+02 |
| 0.139400E+04 | 0.157751E+02 | 0.164904E+02 | 0.150597E+02 |
| 0.139500E+04 | 0.157769E+02 | 0.164918E+02 | 0.150620E+02 |
| 0.139600E+04 | 0.157773E+02 | 0.164916E+02 | 0.150631E+02 |
| 0.139700E+04 | 0.157762E+02 | 0.164896E+02 | 0.150628E+02 |
| 0.139800E+04 | 0.157739E+02 | 0.164864E+02 | 0.150615E+02 |
| 0.139900E+04 | 0.157709E+02 | 0.164823E+02 | 0.150594E+02 |
| 0.140000E+04 | 0.157671E+02 | 0.164775E+02 | 0.150567E+02 |
| 0.140100E+04 | 0.157635E+02 | 0.164729E+02 | 0.150541E+02 |
| 0.140200E+04 | 0.157606E+02 | 0.164693E+02 | 0.150520E+02 |
| 0.140300E+04 | 0.157582E+02 | 0.164663E+02 | 0.150501E+02 |
| 0.140400E+04 | 0.157559E+02 | 0.164635E+02 | 0.150484E+02 |
| 0.140500E+04 | 0.157537E+02 | 0.164607E+02 | 0.150467E+02 |
| 0.140600E+04 | 0.157516E+02 | 0.164581E+02 | 0.150451E+02 |
| 0.140700E+04 | 0.157494E+02 | 0.164554E+02 | 0.150435E+02 |
| 0.140800E+04 | 0.157081E+02 | 0.164067E+02 | 0.150094E+02 |
| 0.140900E+04 | 0.156755E+02 | 0.163786E+02 | 0.149725E+02 |
| 0.141000E+04 | 0.156913E+02 | 0.163894E+02 | 0.149932E+02 |
| 0.141100E+04 | 0.157153E+02 | 0.164161E+02 | 0.150146E+02 |
| 0.141200E+04 | 0.157268E+02 | 0.164294E+02 | 0.150242E+02 |
| 0.141300E+04 | 0.157318E+02 | 0.164352E+02 | 0.150284E+02 |
| 0.141400E+04 | 0.157296E+02 | 0.164381E+02 | 0.150210E+02 |
| 0.141500E+04 | 0.157280E+02 | 0.164384E+02 | 0.150176E+02 |
| 0.141600E+04 | 0.157259E+02 | 0.164361E+02 | 0.150158E+02 |
| 0.141700E+04 | 0.157228E+02 | 0.164319E+02 | 0.150137E+02 |
| 0.141800E+04 | 0.157188E+02 | 0.164266E+02 | 0.150111E+02 |
| 0.141900E+04 | 0.157141E+02 | 0.164204E+02 | 0.150079E+02 |
| 0.142000E+04 | 0.157089E+02 | 0.164136E+02 | 0.150042E+02 |
| 0.142100E+04 | 0.157039E+02 | 0.164070E+02 | 0.150007E+02 |
| 0.142200E+04 | 0.156996E+02 | 0.164015E+02 | 0.149977E+02 |
| 0.142300E+04 | 0.156958E+02 | 0.163965E+02 | 0.149950E+02 |
| 0.142400E+04 | 0.156922E+02 | 0.163918E+02 | 0.149925E+02 |
| 0.142500E+04 | 0.156888E+02 | 0.163876E+02 | 0.149901E+02 |
| 0.142600E+04 | 0.156855E+02 | 0.163834E+02 | 0.149876E+02 |
| 0.142700E+04 | 0.156822E+02 | 0.163792E+02 | 0.149853E+02 |
| 0.142800E+04 | 0.156782E+02 | 0.163742E+02 | 0.149822E+02 |
| 0.142900E+04 | 0.156789E+02 | 0.163675E+02 | 0.149903E+02 |
| 0.143000E+04 | 0.156736E+02 | 0.163600E+02 | 0.149871E+02 |
| 0.143100E+04 | 0.156667E+02 | 0.163519E+02 | 0.149814E+02 |
| 0.143200E+04 | 0.156592E+02 | 0.163436E+02 | 0.149748E+02 |
| 0.143300E+04 | 0.156511E+02 | 0.163347E+02 | 0.149676E+02 |
| 0.143400E+04 | 0.156158E+02 | 0.162944E+02 | 0.149373E+02 |
| 0.143500E+04 | 0.155802E+02 | 0.162536E+02 | 0.149068E+02 |
| 0.143600E+04 | 0.155822E+02 | 0.162570E+02 | 0.149073E+02 |

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| 0.143700E+04 | 0.155892E+02 | 0.162663E+02 | 0.149121E+02 |
| 0.143800E+04 | 0.155886E+02 | 0.162665E+02 | 0.149107E+02 |
| 0.143900E+04 | 0.155844E+02 | 0.162622E+02 | 0.149066E+02 |
| 0.144000E+04 | 0.155787E+02 | 0.162559E+02 | 0.149014E+02 |
| 0.144100E+04 | 0.155718E+02 | 0.162480E+02 | 0.148956E+02 |
| 0.144200E+04 | 0.155658E+02 | 0.162415E+02 | 0.148902E+02 |
| 0.144300E+04 | 0.155610E+02 | 0.162361E+02 | 0.148859E+02 |
| 0.144400E+04 | 0.155568E+02 | 0.162312E+02 | 0.148823E+02 |
| 0.144500E+04 | 0.155533E+02 | 0.162274E+02 | 0.148791E+02 |
| 0.144600E+04 | 0.155500E+02 | 0.162238E+02 | 0.148763E+02 |
| 0.144700E+04 | 0.155471E+02 | 0.162203E+02 | 0.148738E+02 |
| 0.144800E+04 | 0.155443E+02 | 0.162171E+02 | 0.148714E+02 |
| 0.144900E+04 | 0.155417E+02 | 0.162140E+02 | 0.148693E+02 |
| 0.145000E+04 | 0.155392E+02 | 0.162111E+02 | 0.148673E+02 |
| 0.145100E+04 | 0.155369E+02 | 0.162083E+02 | 0.148655E+02 |
| 0.145200E+04 | 0.155344E+02 | 0.162054E+02 | 0.148635E+02 |
| 0.145300E+04 | 0.153648E+02 | 0.160061E+02 | 0.147235E+02 |
| 0.145400E+04 | 0.153195E+02 | 0.157941E+02 | 0.148450E+02 |
| 0.145500E+04 | 0.154361E+02 | 0.158967E+02 | 0.149755E+02 |
| 0.145600E+04 | 0.155373E+02 | 0.160004E+02 | 0.150742E+02 |
| 0.145700E+04 | 0.155806E+02 | 0.160455E+02 | 0.151157E+02 |
| 0.145800E+04 | 0.156014E+02 | 0.160718E+02 | 0.151311E+02 |
| 0.145900E+04 | 0.154650E+02 | 0.159178E+02 | 0.150122E+02 |
| 0.146000E+04 | 0.152136E+02 | 0.156044E+02 | 0.148229E+02 |
| 0.146100E+04 | 0.151641E+02 | 0.155570E+02 | 0.147711E+02 |
| 0.146200E+04 | 0.152892E+02 | 0.157025E+02 | 0.148759E+02 |
| 0.146300E+04 | 0.154100E+02 | 0.158396E+02 | 0.149805E+02 |
| 0.146400E+04 | 0.154736E+02 | 0.159152E+02 | 0.150320E+02 |
| 0.146500E+04 | 0.155104E+02 | 0.159613E+02 | 0.150596E+02 |
| 0.146600E+04 | 0.154927E+02 | 0.159418E+02 | 0.150437E+02 |
| 0.146700E+04 | 0.154644E+02 | 0.159083E+02 | 0.150206E+02 |
| 0.146800E+04 | 0.154940E+02 | 0.159471E+02 | 0.150409E+02 |
| 0.146900E+04 | 0.155300E+02 | 0.159986E+02 | 0.150615E+02 |
| 0.147000E+04 | 0.155468E+02 | 0.160300E+02 | 0.150637E+02 |
| 0.147100E+04 | 0.155793E+02 | 0.160950E+02 | 0.150636E+02 |
| 0.147200E+04 | 0.155849E+02 | 0.161063E+02 | 0.150635E+02 |
| 0.147300E+04 | 0.155898E+02 | 0.161174E+02 | 0.150622E+02 |
| 0.147400E+04 | 0.155946E+02 | 0.161279E+02 | 0.150612E+02 |
| 0.147500E+04 | 0.155993E+02 | 0.161376E+02 | 0.150610E+02 |
| 0.147600E+04 | 0.156039E+02 | 0.161464E+02 | 0.150615E+02 |
| 0.147700E+04 | 0.156090E+02 | 0.161549E+02 | 0.150631E+02 |
| 0.147800E+04 | 0.156148E+02 | 0.161637E+02 | 0.150659E+02 |
| 0.147900E+04 | 0.156212E+02 | 0.161726E+02 | 0.150698E+02 |
| 0.148000E+04 | 0.156280E+02 | 0.161817E+02 | 0.150744E+02 |
| 0.148100E+04 | 0.155971E+02 | 0.161451E+02 | 0.150490E+02 |
| 0.148200E+04 | 0.155858E+02 | 0.161388E+02 | 0.150329E+02 |
| 0.148300E+04 | 0.155628E+02 | 0.161126E+02 | 0.150129E+02 |
| 0.148400E+04 | 0.155432E+02 | 0.160886E+02 | 0.149978E+02 |
| 0.148500E+04 | 0.155828E+02 | 0.161357E+02 | 0.150299E+02 |
| 0.148600E+04 | 0.156093E+02 | 0.161681E+02 | 0.150505E+02 |
| 0.148700E+04 | 0.156222E+02 | 0.161836E+02 | 0.150608E+02 |
| 0.148800E+04 | 0.156306E+02 | 0.161932E+02 | 0.150679E+02 |
| 0.148900E+04 | 0.156365E+02 | 0.161998E+02 | 0.150733E+02 |
| 0.149000E+04 | 0.156410E+02 | 0.162044E+02 | 0.150776E+02 |

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| 0.149100E+04 | 0.156445E+02 | 0.162079E+02 | 0.150811E+02 |
| 0.149200E+04 | 0.156472E+02 | 0.162104E+02 | 0.150840E+02 |
| 0.149300E+04 | 0.156494E+02 | 0.162123E+02 | 0.150864E+02 |
| 0.149400E+04 | 0.156511E+02 | 0.162138E+02 | 0.150885E+02 |
| 0.149500E+04 | 0.156076E+02 | 0.161618E+02 | 0.150535E+02 |
| 0.149600E+04 | 0.155622E+02 | 0.161069E+02 | 0.150174E+02 |
| 0.149700E+04 | 0.155793E+02 | 0.161269E+02 | 0.150317E+02 |
| 0.149800E+04 | 0.156076E+02 | 0.161619E+02 | 0.150533E+02 |
| 0.149900E+04 | 0.156224E+02 | 0.161805E+02 | 0.150643E+02 |
| 0.150000E+04 | 0.156306E+02 | 0.161907E+02 | 0.150704E+02 |
| 0.150100E+04 | 0.156361E+02 | 0.161976E+02 | 0.150746E+02 |
| 0.150200E+04 | 0.156403E+02 | 0.162028E+02 | 0.150778E+02 |
| 0.150300E+04 | 0.156435E+02 | 0.162067E+02 | 0.150804E+02 |
| 0.150400E+04 | 0.156085E+02 | 0.161654E+02 | 0.150517E+02 |
| 0.150500E+04 | 0.155694E+02 | 0.161186E+02 | 0.150201E+02 |
| 0.150600E+04 | 0.155831E+02 | 0.161352E+02 | 0.150310E+02 |
| 0.150700E+04 | 0.156089E+02 | 0.161672E+02 | 0.150506E+02 |
| 0.150800E+04 | 0.156231E+02 | 0.161853E+02 | 0.150609E+02 |
| 0.150900E+04 | 0.156312E+02 | 0.161957E+02 | 0.150667E+02 |
| 0.151000E+04 | 0.156370E+02 | 0.162032E+02 | 0.150708E+02 |
| 0.151100E+04 | 0.156414E+02 | 0.162089E+02 | 0.150739E+02 |
| 0.151200E+04 | 0.156446E+02 | 0.162131E+02 | 0.150761E+02 |
| 0.151300E+04 | 0.156465E+02 | 0.162158E+02 | 0.150773E+02 |
| 0.151400E+04 | 0.156476E+02 | 0.162174E+02 | 0.150777E+02 |
| 0.151500E+04 | 0.156480E+02 | 0.162183E+02 | 0.150777E+02 |
| 0.151600E+04 | 0.156479E+02 | 0.162186E+02 | 0.150772E+02 |
| 0.151700E+04 | 0.156474E+02 | 0.162184E+02 | 0.150763E+02 |
| 0.151800E+04 | 0.156466E+02 | 0.162179E+02 | 0.150752E+02 |
| 0.151900E+04 | 0.156458E+02 | 0.162175E+02 | 0.150740E+02 |
| 0.152000E+04 | 0.156452E+02 | 0.162174E+02 | 0.150730E+02 |
| 0.152100E+04 | 0.156448E+02 | 0.162175E+02 | 0.150721E+02 |
| 0.152200E+04 | 0.156444E+02 | 0.162177E+02 | 0.150711E+02 |
| 0.152300E+04 | 0.156440E+02 | 0.162179E+02 | 0.150702E+02 |
| 0.152400E+04 | 0.156436E+02 | 0.162181E+02 | 0.150692E+02 |
| 0.152500E+04 | 0.156432E+02 | 0.162183E+02 | 0.150682E+02 |
| 0.152600E+04 | 0.156428E+02 | 0.162185E+02 | 0.150671E+02 |
| 0.152700E+04 | 0.155372E+02 | 0.160940E+02 | 0.149803E+02 |
| 0.152800E+04 | 0.154258E+02 | 0.159622E+02 | 0.148893E+02 |
| 0.152900E+04 | 0.154593E+02 | 0.160037E+02 | 0.149149E+02 |
| 0.153000E+04 | 0.155211E+02 | 0.160789E+02 | 0.149633E+02 |
| 0.153100E+04 | 0.155452E+02 | 0.161057E+02 | 0.149848E+02 |
| 0.153200E+04 | 0.155586E+02 | 0.161225E+02 | 0.149946E+02 |
| 0.153300E+04 | 0.155672E+02 | 0.161339E+02 | 0.150006E+02 |
| 0.153400E+04 | 0.155731E+02 | 0.161418E+02 | 0.150044E+02 |
| 0.153500E+04 | 0.155772E+02 | 0.161475E+02 | 0.150069E+02 |
| 0.153600E+04 | 0.155804E+02 | 0.161519E+02 | 0.150088E+02 |
| 0.153700E+04 | 0.155824E+02 | 0.161547E+02 | 0.150100E+02 |
| 0.153800E+04 | 0.155844E+02 | 0.161578E+02 | 0.150109E+02 |
| 0.153900E+04 | 0.155857E+02 | 0.161601E+02 | 0.150114E+02 |
| 0.154000E+04 | 0.155863E+02 | 0.161610E+02 | 0.150115E+02 |
| 0.154100E+04 | 0.155873E+02 | 0.161630E+02 | 0.150117E+02 |
| 0.154200E+04 | 0.155884E+02 | 0.161649E+02 | 0.150120E+02 |
| 0.154300E+04 | 0.155892E+02 | 0.161661E+02 | 0.150122E+02 |
| 0.154400E+04 | 0.155904E+02 | 0.161683E+02 | 0.150125E+02 |

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| 0.154500E+04 | 0.155915E+02 | 0.161702E+02 | 0.150128E+02 |
| 0.154600E+04 | 0.155925E+02 | 0.161719E+02 | 0.150131E+02 |
| 0.154700E+04 | 0.155934E+02 | 0.161735E+02 | 0.150133E+02 |
| 0.154800E+04 | 0.155943E+02 | 0.161750E+02 | 0.150135E+02 |
| 0.154900E+04 | 0.155951E+02 | 0.161765E+02 | 0.150137E+02 |
| 0.155000E+04 | 0.155958E+02 | 0.161778E+02 | 0.150137E+02 |
| 0.155100E+04 | 0.155962E+02 | 0.161788E+02 | 0.150136E+02 |
| 0.155200E+04 | 0.155966E+02 | 0.161797E+02 | 0.150134E+02 |
| 0.155300E+04 | 0.155968E+02 | 0.161804E+02 | 0.150132E+02 |
| 0.155400E+04 | 0.155954E+02 | 0.161809E+02 | 0.150098E+02 |
| 0.155500E+04 | 0.155951E+02 | 0.161813E+02 | 0.150088E+02 |
| 0.155600E+04 | 0.155949E+02 | 0.161817E+02 | 0.150080E+02 |
| 0.155700E+04 | 0.155947E+02 | 0.161820E+02 | 0.150074E+02 |
| 0.155800E+04 | 0.155946E+02 | 0.161823E+02 | 0.150069E+02 |
| 0.155900E+04 | 0.155945E+02 | 0.161826E+02 | 0.150063E+02 |
| 0.156000E+04 | 0.155947E+02 | 0.161833E+02 | 0.150061E+02 |
| 0.156100E+04 | 0.155953E+02 | 0.161844E+02 | 0.150062E+02 |
| 0.156200E+04 | 0.155963E+02 | 0.161860E+02 | 0.150066E+02 |
| 0.156300E+04 | 0.155974E+02 | 0.161877E+02 | 0.150071E+02 |
| 0.156400E+04 | 0.155110E+02 | 0.160943E+02 | 0.149278E+02 |
| 0.156500E+04 | 0.154094E+02 | 0.159699E+02 | 0.148489E+02 |
| 0.156600E+04 | 0.154425E+02 | 0.160088E+02 | 0.148763E+02 |
| 0.156700E+04 | 0.155046E+02 | 0.160843E+02 | 0.149249E+02 |
| 0.156800E+04 | 0.155374E+02 | 0.161253E+02 | 0.149495E+02 |
| 0.156900E+04 | 0.155543E+02 | 0.161476E+02 | 0.149610E+02 |
| 0.157000E+04 | 0.155396E+02 | 0.161316E+02 | 0.149477E+02 |
| 0.157100E+04 | 0.155226E+02 | 0.161116E+02 | 0.149337E+02 |
| 0.157200E+04 | 0.155401E+02 | 0.161324E+02 | 0.149479E+02 |
| 0.157300E+04 | 0.155613E+02 | 0.161582E+02 | 0.149643E+02 |
| 0.157400E+04 | 0.155742E+02 | 0.161740E+02 | 0.149743E+02 |
| 0.157500E+04 | 0.155830E+02 | 0.161848E+02 | 0.149812E+02 |
| 0.157600E+04 | 0.155901E+02 | 0.161932E+02 | 0.149869E+02 |
| 0.157700E+04 | 0.155964E+02 | 0.162007E+02 | 0.149921E+02 |
| 0.157800E+04 | 0.156027E+02 | 0.162081E+02 | 0.149973E+02 |
| 0.157900E+04 | 0.156088E+02 | 0.162152E+02 | 0.150024E+02 |
| 0.158000E+04 | 0.156148E+02 | 0.162221E+02 | 0.150075E+02 |
| 0.158100E+04 | 0.156206E+02 | 0.162288E+02 | 0.150124E+02 |
| 0.158200E+04 | 0.156264E+02 | 0.162354E+02 | 0.150174E+02 |
| 0.158300E+04 | 0.156321E+02 | 0.162419E+02 | 0.150223E+02 |
| 0.158400E+04 | 0.156377E+02 | 0.162482E+02 | 0.150271E+02 |
| 0.158500E+04 | 0.156429E+02 | 0.162542E+02 | 0.150317E+02 |
| 0.158600E+04 | 0.156477E+02 | 0.162596E+02 | 0.150359E+02 |
| 0.158700E+04 | 0.154871E+02 | 0.160651E+02 | 0.149092E+02 |
| 0.158800E+04 | 0.152509E+02 | 0.157880E+02 | 0.147137E+02 |
| 0.158900E+04 | 0.152486E+02 | 0.157976E+02 | 0.146996E+02 |
| 0.159000E+04 | 0.153508E+02 | 0.159154E+02 | 0.147863E+02 |
| 0.159100E+04 | 0.154407E+02 | 0.160158E+02 | 0.148657E+02 |
| 0.159200E+04 | 0.154879E+02 | 0.160683E+02 | 0.149075E+02 |
| 0.159300E+04 | 0.155184E+02 | 0.161039E+02 | 0.149329E+02 |
| 0.159400E+04 | 0.155419E+02 | 0.161315E+02 | 0.149523E+02 |
| 0.159500E+04 | 0.155616E+02 | 0.161546E+02 | 0.149686E+02 |
| 0.159600E+04 | 0.155788E+02 | 0.161747E+02 | 0.149829E+02 |
| 0.159700E+04 | 0.155940E+02 | 0.161924E+02 | 0.149957E+02 |
| 0.159800E+04 | 0.156078E+02 | 0.162083E+02 | 0.150074E+02 |

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| 0.159900E+04 | 0.156174E+02 | 0.162228E+02 | 0.150121E+02 |
| 0.160000E+04 | 0.156392E+02 | 0.162773E+02 | 0.150011E+02 |
| 0.160100E+04 | 0.154486E+02 | 0.160671E+02 | 0.148300E+02 |
| 0.160200E+04 | 0.152501E+02 | 0.158072E+02 | 0.146930E+02 |
| 0.160300E+04 | 0.153277E+02 | 0.159076E+02 | 0.147479E+02 |
| 0.160400E+04 | 0.154305E+02 | 0.160211E+02 | 0.148398E+02 |
| 0.160500E+04 | 0.155049E+02 | 0.161033E+02 | 0.149066E+02 |
| 0.160600E+04 | 0.155498E+02 | 0.161548E+02 | 0.149448E+02 |
| 0.160700E+04 | 0.155790E+02 | 0.161881E+02 | 0.149700E+02 |
| 0.160800E+04 | 0.156009E+02 | 0.162127E+02 | 0.149890E+02 |
| 0.160900E+04 | 0.156182E+02 | 0.162320E+02 | 0.150044E+02 |
| 0.161000E+04 | 0.156313E+02 | 0.162476E+02 | 0.150150E+02 |
| 0.161100E+04 | 0.156314E+02 | 0.162607E+02 | 0.150021E+02 |
| 0.161200E+04 | 0.155915E+02 | 0.162714E+02 | 0.149115E+02 |
| 0.161300E+04 | 0.154522E+02 | 0.161522E+02 | 0.147522E+02 |
| 0.161400E+04 | 0.153279E+02 | 0.160461E+02 | 0.146098E+02 |
| 0.161500E+04 | 0.153687E+02 | 0.160909E+02 | 0.146466E+02 |
| 0.161600E+04 | 0.154274E+02 | 0.161655E+02 | 0.146894E+02 |
| 0.161700E+04 | 0.154562E+02 | 0.162047E+02 | 0.147077E+02 |
| 0.161800E+04 | 0.154727E+02 | 0.162277E+02 | 0.147178E+02 |
| 0.161900E+04 | 0.154856E+02 | 0.162438E+02 | 0.147275E+02 |
| 0.162000E+04 | 0.154970E+02 | 0.162559E+02 | 0.147381E+02 |
| 0.162100E+04 | 0.155077E+02 | 0.162657E+02 | 0.147497E+02 |
| 0.162200E+04 | 0.153965E+02 | 0.161321E+02 | 0.146609E+02 |
| 0.162300E+04 | 0.152880E+02 | 0.160132E+02 | 0.145628E+02 |
| 0.162400E+04 | 0.153391E+02 | 0.160663E+02 | 0.146120E+02 |
| 0.162500E+04 | 0.154177E+02 | 0.161510E+02 | 0.146845E+02 |
| 0.162600E+04 | 0.154616E+02 | 0.161970E+02 | 0.147262E+02 |
| 0.162700E+04 | 0.154892E+02 | 0.162248E+02 | 0.147536E+02 |
| 0.162800E+04 | 0.155104E+02 | 0.162450E+02 | 0.147759E+02 |
| 0.162900E+04 | 0.155279E+02 | 0.162608E+02 | 0.147951E+02 |
| 0.163000E+04 | 0.155429E+02 | 0.162737E+02 | 0.148121E+02 |
| 0.163100E+04 | 0.155560E+02 | 0.162846E+02 | 0.148273E+02 |
| 0.163200E+04 | 0.155668E+02 | 0.162932E+02 | 0.148404E+02 |
| 0.163300E+04 | 0.155732E+02 | 0.162989E+02 | 0.148474E+02 |
| 0.163400E+04 | 0.155760E+02 | 0.163026E+02 | 0.148494E+02 |
| 0.163500E+04 | 0.155782E+02 | 0.163048E+02 | 0.148516E+02 |
| 0.163600E+04 | 0.155799E+02 | 0.163060E+02 | 0.148538E+02 |
| 0.163700E+04 | 0.155812E+02 | 0.163064E+02 | 0.148560E+02 |
| 0.163800E+04 | 0.155821E+02 | 0.163062E+02 | 0.148580E+02 |
| 0.163900E+04 | 0.155828E+02 | 0.163058E+02 | 0.148598E+02 |
| 0.164000E+04 | 0.155836E+02 | 0.163055E+02 | 0.148617E+02 |
| 0.164100E+04 | 0.153829E+02 | 0.160814E+02 | 0.146844E+02 |
| 0.164200E+04 | 0.151608E+02 | 0.158185E+02 | 0.145032E+02 |
| 0.164300E+04 | 0.153669E+02 | 0.159113E+02 | 0.148225E+02 |
| 0.164400E+04 | 0.155055E+02 | 0.160204E+02 | 0.149907E+02 |
| 0.164500E+04 | 0.155739E+02 | 0.160860E+02 | 0.150619E+02 |
| 0.164600E+04 | 0.156051E+02 | 0.161200E+02 | 0.150903E+02 |
| 0.164700E+04 | 0.156373E+02 | 0.161604E+02 | 0.151142E+02 |
| 0.164800E+04 | 0.156620E+02 | 0.161950E+02 | 0.151291E+02 |
| 0.164900E+04 | 0.156765E+02 | 0.162182E+02 | 0.151347E+02 |
| 0.165000E+04 | 0.156849E+02 | 0.162345E+02 | 0.151354E+02 |
| 0.165100E+04 | 0.156898E+02 | 0.162463E+02 | 0.151332E+02 |
| 0.165200E+04 | 0.156922E+02 | 0.162549E+02 | 0.151295E+02 |

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| 0.165300E+04 | 0.156926E+02 | 0.162607E+02 | 0.151245E+02 |
| 0.165400E+04 | 0.156912E+02 | 0.162638E+02 | 0.151187E+02 |
| 0.165500E+04 | 0.156885E+02 | 0.162648E+02 | 0.151122E+02 |
| 0.165600E+04 | 0.156849E+02 | 0.162643E+02 | 0.151055E+02 |
| 0.165700E+04 | 0.156806E+02 | 0.162626E+02 | 0.150986E+02 |
| 0.165800E+04 | 0.156756E+02 | 0.162597E+02 | 0.150916E+02 |
| 0.165900E+04 | 0.156703E+02 | 0.162560E+02 | 0.150845E+02 |
| 0.166000E+04 | 0.156652E+02 | 0.162524E+02 | 0.150780E+02 |
| 0.166100E+04 | 0.156610E+02 | 0.162496E+02 | 0.150725E+02 |
| 0.166200E+04 | 0.156574E+02 | 0.162472E+02 | 0.150676E+02 |
| 0.166300E+04 | 0.156541E+02 | 0.162450E+02 | 0.150631E+02 |
| 0.166400E+04 | 0.156509E+02 | 0.162428E+02 | 0.150590E+02 |
| 0.166500E+04 | 0.156480E+02 | 0.162407E+02 | 0.150553E+02 |
| 0.166600E+04 | 0.156451E+02 | 0.162385E+02 | 0.150518E+02 |
| 0.166700E+04 | 0.155654E+02 | 0.161508E+02 | 0.149799E+02 |
| 0.166800E+04 | 0.154658E+02 | 0.160323E+02 | 0.148994E+02 |
| 0.166900E+04 | 0.154930E+02 | 0.160649E+02 | 0.149212E+02 |
| 0.167000E+04 | 0.155456E+02 | 0.161297E+02 | 0.149615E+02 |
| 0.167100E+04 | 0.155711E+02 | 0.161624E+02 | 0.149799E+02 |
| 0.167200E+04 | 0.155829E+02 | 0.161777E+02 | 0.149881E+02 |
| 0.167300E+04 | 0.155895E+02 | 0.161862E+02 | 0.149927E+02 |
| 0.167400E+04 | 0.154658E+02 | 0.160401E+02 | 0.148914E+02 |
| 0.167500E+04 | 0.153303E+02 | 0.158814E+02 | 0.147792E+02 |
| 0.167600E+04 | 0.153742E+02 | 0.159317E+02 | 0.148167E+02 |
| 0.167700E+04 | 0.154485E+02 | 0.160218E+02 | 0.148752E+02 |
| 0.167800E+04 | 0.154803E+02 | 0.160579E+02 | 0.149028E+02 |
| 0.167900E+04 | 0.154982E+02 | 0.160794E+02 | 0.149169E+02 |
| 0.168000E+04 | 0.155103E+02 | 0.160942E+02 | 0.149264E+02 |
| 0.168100E+04 | 0.154120E+02 | 0.159768E+02 | 0.148473E+02 |
| 0.168200E+04 | 0.153189E+02 | 0.158737E+02 | 0.147641E+02 |
| 0.168300E+04 | 0.153591E+02 | 0.159208E+02 | 0.147975E+02 |
| 0.168400E+04 | 0.154256E+02 | 0.160015E+02 | 0.148498E+02 |
| 0.168500E+04 | 0.154535E+02 | 0.160315E+02 | 0.148754E+02 |
| 0.168600E+04 | 0.154697E+02 | 0.160504E+02 | 0.148890E+02 |
| 0.168700E+04 | 0.154808E+02 | 0.160634E+02 | 0.148982E+02 |
| 0.168800E+04 | 0.154887E+02 | 0.160725E+02 | 0.149050E+02 |
| 0.168900E+04 | 0.154664E+02 | 0.160452E+02 | 0.148876E+02 |
| 0.169000E+04 | 0.154520E+02 | 0.160268E+02 | 0.148772E+02 |
| 0.169100E+04 | 0.154744E+02 | 0.160532E+02 | 0.148956E+02 |
| 0.169200E+04 | 0.154909E+02 | 0.160734E+02 | 0.149084E+02 |
| 0.169300E+04 | 0.155000E+02 | 0.160840E+02 | 0.149160E+02 |
| 0.169400E+04 | 0.155066E+02 | 0.160915E+02 | 0.149218E+02 |
| 0.169500E+04 | 0.153911E+02 | 0.159625E+02 | 0.148198E+02 |
| 0.169600E+04 | 0.152530E+02 | 0.157988E+02 | 0.147072E+02 |
| 0.169700E+04 | 0.153029E+02 | 0.158552E+02 | 0.147505E+02 |
| 0.169800E+04 | 0.153865E+02 | 0.159574E+02 | 0.148156E+02 |
| 0.169900E+04 | 0.154243E+02 | 0.160005E+02 | 0.148481E+02 |
| 0.170000E+04 | 0.154470E+02 | 0.160275E+02 | 0.148664E+02 |
| 0.170100E+04 | 0.154634E+02 | 0.160470E+02 | 0.148798E+02 |
| 0.170200E+04 | 0.154761E+02 | 0.160618E+02 | 0.148905E+02 |
| 0.170300E+04 | 0.154864E+02 | 0.160735E+02 | 0.148994E+02 |
| 0.170400E+04 | 0.154950E+02 | 0.160830E+02 | 0.149071E+02 |
| 0.170500E+04 | 0.155024E+02 | 0.160910E+02 | 0.149138E+02 |
| 0.170600E+04 | 0.155090E+02 | 0.160980E+02 | 0.149200E+02 |

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| 0.170700E+04 | 0.155151E+02 | 0.161045E+02 | 0.149258E+02 |
| 0.170800E+04 | 0.155210E+02 | 0.161107E+02 | 0.149313E+02 |
| 0.170900E+04 | 0.155265E+02 | 0.161165E+02 | 0.149365E+02 |
| 0.171000E+04 | 0.155317E+02 | 0.161221E+02 | 0.149414E+02 |
| 0.171100E+04 | 0.155368E+02 | 0.161274E+02 | 0.149461E+02 |
| 0.171200E+04 | 0.155416E+02 | 0.161326E+02 | 0.149507E+02 |
| 0.171300E+04 | 0.155465E+02 | 0.161379E+02 | 0.149552E+02 |
| 0.171400E+04 | 0.155516E+02 | 0.161434E+02 | 0.149598E+02 |
| 0.171500E+04 | 0.155568E+02 | 0.161491E+02 | 0.149645E+02 |
| 0.171600E+04 | 0.155621E+02 | 0.161550E+02 | 0.149692E+02 |
| 0.171700E+04 | 0.155674E+02 | 0.161609E+02 | 0.149738E+02 |
| 0.171800E+04 | 0.155727E+02 | 0.161669E+02 | 0.149785E+02 |
| 0.171900E+04 | 0.155781E+02 | 0.161730E+02 | 0.149832E+02 |
| 0.172000E+04 | 0.155717E+02 | 0.161791E+02 | 0.149643E+02 |
| 0.172100E+04 | 0.155666E+02 | 0.161861E+02 | 0.149472E+02 |
| 0.172200E+04 | 0.155081E+02 | 0.161946E+02 | 0.148217E+02 |
| 0.172300E+04 | 0.154858E+02 | 0.162031E+02 | 0.147685E+02 |
| 0.172400E+04 | 0.154740E+02 | 0.162115E+02 | 0.147366E+02 |
| 0.172500E+04 | 0.154687E+02 | 0.162198E+02 | 0.147175E+02 |
| 0.172600E+04 | 0.154663E+02 | 0.162284E+02 | 0.147042E+02 |
| 0.172700E+04 | 0.154694E+02 | 0.162372E+02 | 0.147016E+02 |
| 0.172800E+04 | 0.154750E+02 | 0.162454E+02 | 0.147046E+02 |
| 0.172900E+04 | 0.153625E+02 | 0.161226E+02 | 0.146023E+02 |
| 0.173000E+04 | 0.152918E+02 | 0.160248E+02 | 0.145589E+02 |
| 0.173100E+04 | 0.153823E+02 | 0.161184E+02 | 0.146463E+02 |
| 0.173200E+04 | 0.154383E+02 | 0.161748E+02 | 0.147018E+02 |
| 0.173300E+04 | 0.154680E+02 | 0.162052E+02 | 0.147309E+02 |
| 0.173400E+04 | 0.154904E+02 | 0.162274E+02 | 0.147533E+02 |
| 0.173500E+04 | 0.155091E+02 | 0.162452E+02 | 0.147729E+02 |
| 0.173600E+04 | 0.155263E+02 | 0.162612E+02 | 0.147914E+02 |
| 0.173700E+04 | 0.155433E+02 | 0.162770E+02 | 0.148097E+02 |
| 0.173800E+04 | 0.155601E+02 | 0.162925E+02 | 0.148277E+02 |
| 0.173900E+04 | 0.155257E+02 | 0.162484E+02 | 0.148030E+02 |
| 0.174000E+04 | 0.155039E+02 | 0.162188E+02 | 0.147891E+02 |
| 0.174100E+04 | 0.155467E+02 | 0.162658E+02 | 0.148277E+02 |
| 0.174200E+04 | 0.155780E+02 | 0.163026E+02 | 0.148534E+02 |
| 0.174300E+04 | 0.155946E+02 | 0.163232E+02 | 0.148660E+02 |
| 0.174400E+04 | 0.156077E+02 | 0.163386E+02 | 0.148768E+02 |
| 0.174500E+04 | 0.156193E+02 | 0.163514E+02 | 0.148871E+02 |
| 0.174600E+04 | 0.156298E+02 | 0.163626E+02 | 0.148971E+02 |
| 0.174700E+04 | 0.156396E+02 | 0.163726E+02 | 0.149066E+02 |
| 0.174800E+04 | 0.156489E+02 | 0.163819E+02 | 0.149159E+02 |
| 0.174900E+04 | 0.156576E+02 | 0.163905E+02 | 0.149246E+02 |
| 0.175000E+04 | 0.156658E+02 | 0.163986E+02 | 0.149330E+02 |
| 0.175100E+04 | 0.156737E+02 | 0.164064E+02 | 0.149410E+02 |
| 0.175200E+04 | 0.156813E+02 | 0.164139E+02 | 0.149487E+02 |
| 0.175300E+04 | 0.156885E+02 | 0.164211E+02 | 0.149560E+02 |
| 0.175400E+04 | 0.156955E+02 | 0.164281E+02 | 0.149629E+02 |
| 0.175500E+04 | 0.157023E+02 | 0.164349E+02 | 0.149696E+02 |
| 0.175600E+04 | 0.157087E+02 | 0.164415E+02 | 0.149760E+02 |
| 0.175700E+04 | 0.157148E+02 | 0.164476E+02 | 0.149819E+02 |
| 0.175800E+04 | 0.157205E+02 | 0.164535E+02 | 0.149875E+02 |
| 0.175900E+04 | 0.157260E+02 | 0.164591E+02 | 0.149928E+02 |
| 0.176000E+04 | 0.157313E+02 | 0.164646E+02 | 0.149980E+02 |

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| 0.176100E+04 | 0.157364E+02 | 0.164699E+02 | 0.150029E+02 |
| 0.176200E+04 | 0.157414E+02 | 0.164751E+02 | 0.150076E+02 |
| 0.176300E+04 | 0.157462E+02 | 0.164802E+02 | 0.150122E+02 |
| 0.176400E+04 | 0.157506E+02 | 0.164847E+02 | 0.150164E+02 |
| 0.176500E+04 | 0.157542E+02 | 0.164884E+02 | 0.150199E+02 |
| 0.176600E+04 | 0.157572E+02 | 0.164914E+02 | 0.150229E+02 |
| 0.176700E+04 | 0.157597E+02 | 0.164939E+02 | 0.150255E+02 |
| 0.176800E+04 | 0.157620E+02 | 0.164961E+02 | 0.150279E+02 |
| 0.176900E+04 | 0.157640E+02 | 0.164980E+02 | 0.150299E+02 |
| 0.177000E+04 | 0.157657E+02 | 0.164997E+02 | 0.150318E+02 |
| 0.177100E+04 | 0.157673E+02 | 0.165011E+02 | 0.150334E+02 |
| 0.177200E+04 | 0.157681E+02 | 0.165017E+02 | 0.150344E+02 |
| 0.177300E+04 | 0.157676E+02 | 0.165008E+02 | 0.150344E+02 |
| 0.177400E+04 | 0.157663E+02 | 0.164990E+02 | 0.150336E+02 |
| 0.177500E+04 | 0.157644E+02 | 0.164965E+02 | 0.150324E+02 |
| 0.177600E+04 | 0.157621E+02 | 0.164935E+02 | 0.150307E+02 |
| 0.177700E+04 | 0.157594E+02 | 0.164901E+02 | 0.150286E+02 |
| 0.177800E+04 | 0.157563E+02 | 0.164864E+02 | 0.150263E+02 |
| 0.177900E+04 | 0.157530E+02 | 0.164824E+02 | 0.150237E+02 |
| 0.178000E+04 | 0.157501E+02 | 0.164788E+02 | 0.150213E+02 |
| 0.178100E+04 | 0.157479E+02 | 0.164762E+02 | 0.150197E+02 |
| 0.178200E+04 | 0.157463E+02 | 0.164742E+02 | 0.150184E+02 |
| 0.178300E+04 | 0.156261E+02 | 0.163438E+02 | 0.149083E+02 |
| 0.178400E+04 | 0.155333E+02 | 0.162369E+02 | 0.148298E+02 |
| 0.178500E+04 | 0.156131E+02 | 0.163227E+02 | 0.149036E+02 |
| 0.178600E+04 | 0.156603E+02 | 0.163735E+02 | 0.149471E+02 |
| 0.178700E+04 | 0.156767E+02 | 0.163922E+02 | 0.149613E+02 |
| 0.178800E+04 | 0.156833E+02 | 0.164052E+02 | 0.149614E+02 |
| 0.178900E+04 | 0.156432E+02 | 0.163596E+02 | 0.149268E+02 |
| 0.179000E+04 | 0.155990E+02 | 0.163077E+02 | 0.148903E+02 |
| 0.179100E+04 | 0.156196E+02 | 0.163320E+02 | 0.149071E+02 |
| 0.179200E+04 | 0.156518E+02 | 0.163703E+02 | 0.149334E+02 |
| 0.179300E+04 | 0.156697E+02 | 0.163911E+02 | 0.149483E+02 |
| 0.179400E+04 | 0.156801E+02 | 0.164025E+02 | 0.149576E+02 |
| 0.179500E+04 | 0.156874E+02 | 0.164101E+02 | 0.149647E+02 |
| 0.179600E+04 | 0.156930E+02 | 0.164156E+02 | 0.149703E+02 |
| 0.179700E+04 | 0.156973E+02 | 0.164197E+02 | 0.149750E+02 |
| 0.179800E+04 | 0.157008E+02 | 0.164226E+02 | 0.149789E+02 |
| 0.179900E+04 | 0.157035E+02 | 0.164248E+02 | 0.149821E+02 |
| 0.180000E+04 | 0.157056E+02 | 0.164262E+02 | 0.149849E+02 |
| 0.180100E+04 | 0.157072E+02 | 0.164272E+02 | 0.149872E+02 |
| 0.180200E+04 | 0.157085E+02 | 0.164278E+02 | 0.149892E+02 |
| 0.180300E+04 | 0.157095E+02 | 0.164280E+02 | 0.149909E+02 |
| 0.180400E+04 | 0.157101E+02 | 0.164279E+02 | 0.149923E+02 |
| 0.180500E+04 | 0.157106E+02 | 0.164276E+02 | 0.149935E+02 |
| 0.180600E+04 | 0.157108E+02 | 0.164271E+02 | 0.149945E+02 |
| 0.180700E+04 | 0.157109E+02 | 0.164264E+02 | 0.149953E+02 |
| 0.180800E+04 | 0.157108E+02 | 0.164256E+02 | 0.149960E+02 |
| 0.180900E+04 | 0.155137E+02 | 0.162025E+02 | 0.148248E+02 |
| 0.181000E+04 | 0.152895E+02 | 0.159392E+02 | 0.146398E+02 |
| 0.181100E+04 | 0.154880E+02 | 0.160312E+02 | 0.149448E+02 |
| 0.181200E+04 | 0.156221E+02 | 0.161370E+02 | 0.151071E+02 |
| 0.181300E+04 | 0.157050E+02 | 0.162175E+02 | 0.151925E+02 |
| 0.181400E+04 | 0.157493E+02 | 0.162665E+02 | 0.152321E+02 |

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| 0.181500E+04 | 0.155576E+02 | 0.160427E+02 | 0.150725E+02 |
| 0.181600E+04 | 0.153323E+02 | 0.157830E+02 | 0.148815E+02 |
| 0.181700E+04 | 0.154100E+02 | 0.158924E+02 | 0.149276E+02 |
| 0.181800E+04 | 0.155228E+02 | 0.160270E+02 | 0.150186E+02 |
| 0.181900E+04 | 0.155969E+02 | 0.161128E+02 | 0.150811E+02 |
| 0.182000E+04 | 0.156399E+02 | 0.161682E+02 | 0.151116E+02 |
| 0.182100E+04 | 0.156658E+02 | 0.162037E+02 | 0.151280E+02 |
| 0.182200E+04 | 0.156838E+02 | 0.162292E+02 | 0.151383E+02 |
| 0.182300E+04 | 0.156966E+02 | 0.162482E+02 | 0.151451E+02 |
| 0.182400E+04 | 0.157058E+02 | 0.162623E+02 | 0.151494E+02 |
| 0.182500E+04 | 0.157120E+02 | 0.162722E+02 | 0.151518E+02 |
| 0.182600E+04 | 0.157377E+02 | 0.163216E+02 | 0.151539E+02 |
| 0.182700E+04 | 0.157400E+02 | 0.163229E+02 | 0.151570E+02 |
| 0.182800E+04 | 0.157398E+02 | 0.163221E+02 | 0.151574E+02 |
| 0.182900E+04 | 0.157391E+02 | 0.163215E+02 | 0.151567E+02 |
| 0.183000E+04 | 0.156933E+02 | 0.162668E+02 | 0.151197E+02 |
| 0.183100E+04 | 0.154905E+02 | 0.160377E+02 | 0.149432E+02 |
| 0.183200E+04 | 0.153467E+02 | 0.158828E+02 | 0.148107E+02 |
| 0.183300E+04 | 0.154298E+02 | 0.159780E+02 | 0.148815E+02 |
| 0.183400E+04 | 0.155394E+02 | 0.161016E+02 | 0.149771E+02 |
| 0.183500E+04 | 0.154825E+02 | 0.160311E+02 | 0.149339E+02 |
| 0.183600E+04 | 0.154041E+02 | 0.159426E+02 | 0.148656E+02 |
| 0.183700E+04 | 0.154654E+02 | 0.160153E+02 | 0.149156E+02 |
| 0.183800E+04 | 0.155479E+02 | 0.161118E+02 | 0.149841E+02 |
| 0.183900E+04 | 0.155871E+02 | 0.161520E+02 | 0.150221E+02 |
| 0.184000E+04 | 0.155765E+02 | 0.161373E+02 | 0.150157E+02 |
| 0.184100E+04 | 0.155558E+02 | 0.161102E+02 | 0.150013E+02 |
| 0.184200E+04 | 0.155873E+02 | 0.161477E+02 | 0.150270E+02 |
| 0.184300E+04 | 0.155919E+02 | 0.161696E+02 | 0.150143E+02 |
| 0.184400E+04 | 0.155708E+02 | 0.161519E+02 | 0.149898E+02 |
| 0.184500E+04 | 0.156038E+02 | 0.161861E+02 | 0.150215E+02 |
| 0.184600E+04 | 0.156508E+02 | 0.162418E+02 | 0.150597E+02 |
| 0.184700E+04 | 0.156624E+02 | 0.162643E+02 | 0.150606E+02 |
| 0.184800E+04 | 0.156222E+02 | 0.162831E+02 | 0.149613E+02 |
| 0.184900E+04 | 0.156010E+02 | 0.162973E+02 | 0.149048E+02 |
| 0.185000E+04 | 0.155901E+02 | 0.163079E+02 | 0.148722E+02 |
| 0.185100E+04 | 0.155834E+02 | 0.163157E+02 | 0.148512E+02 |
| 0.185200E+04 | 0.155803E+02 | 0.163217E+02 | 0.148388E+02 |
| 0.185300E+04 | 0.155781E+02 | 0.163266E+02 | 0.148296E+02 |
| 0.185400E+04 | 0.155480E+02 | 0.162943E+02 | 0.148018E+02 |
| 0.185500E+04 | 0.155287E+02 | 0.162715E+02 | 0.147859E+02 |
| 0.185600E+04 | 0.155520E+02 | 0.162970E+02 | 0.148070E+02 |
| 0.185700E+04 | 0.155716E+02 | 0.163172E+02 | 0.148260E+02 |
| 0.185800E+04 | 0.155843E+02 | 0.163280E+02 | 0.148406E+02 |
| 0.185900E+04 | 0.155950E+02 | 0.163357E+02 | 0.148544E+02 |
| 0.186000E+04 | 0.156049E+02 | 0.163420E+02 | 0.148677E+02 |
| 0.186100E+04 | 0.156142E+02 | 0.163477E+02 | 0.148807E+02 |
| 0.186200E+04 | 0.156232E+02 | 0.163531E+02 | 0.148932E+02 |
| 0.186300E+04 | 0.156318E+02 | 0.163583E+02 | 0.149052E+02 |
| 0.186400E+04 | 0.156401E+02 | 0.163634E+02 | 0.149167E+02 |
| 0.186500E+04 | 0.156481E+02 | 0.163685E+02 | 0.149277E+02 |
| 0.186600E+04 | 0.156556E+02 | 0.163734E+02 | 0.149378E+02 |
| 0.186700E+04 | 0.156625E+02 | 0.163779E+02 | 0.149471E+02 |
| 0.186800E+04 | 0.156689E+02 | 0.163822E+02 | 0.149556E+02 |

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| 0.186900E+04 | 0.156749E+02 | 0.163864E+02 | 0.149635E+02 |
| 0.187000E+04 | 0.156806E+02 | 0.163905E+02 | 0.149707E+02 |
| 0.187100E+04 | 0.156859E+02 | 0.163945E+02 | 0.149773E+02 |
| 0.187200E+04 | 0.156909E+02 | 0.163984E+02 | 0.149834E+02 |
| 0.187300E+04 | 0.156956E+02 | 0.164023E+02 | 0.149889E+02 |
| 0.187400E+04 | 0.157001E+02 | 0.164061E+02 | 0.149941E+02 |
| 0.187500E+04 | 0.157043E+02 | 0.164098E+02 | 0.149988E+02 |
| 0.187600E+04 | 0.157084E+02 | 0.164135E+02 | 0.150032E+02 |
| 0.187700E+04 | 0.157122E+02 | 0.164172E+02 | 0.150072E+02 |
| 0.187800E+04 | 0.157158E+02 | 0.164207E+02 | 0.150109E+02 |
| 0.187900E+04 | 0.157192E+02 | 0.164242E+02 | 0.150143E+02 |
| 0.188000E+04 | 0.157224E+02 | 0.164274E+02 | 0.150173E+02 |
| 0.188100E+04 | 0.157253E+02 | 0.164305E+02 | 0.150201E+02 |
| 0.188200E+04 | 0.157280E+02 | 0.164335E+02 | 0.150225E+02 |
| 0.188300E+04 | 0.155987E+02 | 0.162925E+02 | 0.149049E+02 |
| 0.188400E+04 | 0.154475E+02 | 0.161172E+02 | 0.147778E+02 |
| 0.188500E+04 | 0.155007E+02 | 0.161725E+02 | 0.148289E+02 |
| 0.188600E+04 | 0.155887E+02 | 0.162750E+02 | 0.149023E+02 |
| 0.188700E+04 | 0.156298E+02 | 0.163209E+02 | 0.149386E+02 |
| 0.188800E+04 | 0.156481E+02 | 0.163477E+02 | 0.149486E+02 |
| 0.188900E+04 | 0.156615E+02 | 0.163662E+02 | 0.149568E+02 |
| 0.189000E+04 | 0.156726E+02 | 0.163797E+02 | 0.149656E+02 |
| 0.189100E+04 | 0.156816E+02 | 0.163900E+02 | 0.149731E+02 |
| 0.189200E+04 | 0.156887E+02 | 0.163980E+02 | 0.149794E+02 |
| 0.189300E+04 | 0.156947E+02 | 0.164046E+02 | 0.149847E+02 |
| 0.189400E+04 | 0.157000E+02 | 0.164104E+02 | 0.149895E+02 |
| 0.189500E+04 | 0.157047E+02 | 0.164155E+02 | 0.149938E+02 |
| 0.189600E+04 | 0.157088E+02 | 0.164200E+02 | 0.149975E+02 |
| 0.189700E+04 | 0.157124E+02 | 0.164240E+02 | 0.150008E+02 |
| 0.189800E+04 | 0.157153E+02 | 0.164270E+02 | 0.150036E+02 |
| 0.189900E+04 | 0.157179E+02 | 0.164298E+02 | 0.150060E+02 |
| 0.190000E+04 | 0.157204E+02 | 0.164325E+02 | 0.150083E+02 |
| 0.190100E+04 | 0.157228E+02 | 0.164351E+02 | 0.150105E+02 |
| 0.190200E+04 | 0.155967E+02 | 0.162975E+02 | 0.148958E+02 |
| 0.190300E+04 | 0.154530E+02 | 0.161298E+02 | 0.147763E+02 |
| 0.190400E+04 | 0.155055E+02 | 0.161848E+02 | 0.148262E+02 |
| 0.190500E+04 | 0.155896E+02 | 0.162827E+02 | 0.148965E+02 |
| 0.190600E+04 | 0.156290E+02 | 0.163265E+02 | 0.149316E+02 |
| 0.190700E+04 | 0.156345E+02 | 0.163360E+02 | 0.149329E+02 |
| 0.190800E+04 | 0.156364E+02 | 0.163424E+02 | 0.149303E+02 |
| 0.190900E+04 | 0.156563E+02 | 0.163665E+02 | 0.149461E+02 |
| 0.191000E+04 | 0.156723E+02 | 0.163852E+02 | 0.149594E+02 |
| 0.191100E+04 | 0.156832E+02 | 0.163974E+02 | 0.149690E+02 |
| 0.191200E+04 | 0.156106E+02 | 0.163119E+02 | 0.149093E+02 |
| 0.191300E+04 | 0.155412E+02 | 0.162397E+02 | 0.148427E+02 |
| 0.191400E+04 | 0.155432E+02 | 0.162397E+02 | 0.148468E+02 |
| 0.191500E+04 | 0.155825E+02 | 0.162795E+02 | 0.148855E+02 |
| 0.191600E+04 | 0.156308E+02 | 0.163332E+02 | 0.149283E+02 |
| 0.191700E+04 | 0.156565E+02 | 0.163617E+02 | 0.149513E+02 |
| 0.191800E+04 | 0.156706E+02 | 0.163823E+02 | 0.149589E+02 |
| 0.191900E+04 | 0.156842E+02 | 0.163986E+02 | 0.149698E+02 |
| 0.192000E+04 | 0.156962E+02 | 0.164119E+02 | 0.149806E+02 |
| 0.192100E+04 | 0.157069E+02 | 0.164232E+02 | 0.149906E+02 |
| 0.192200E+04 | 0.157166E+02 | 0.164332E+02 | 0.150000E+02 |

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| 0.192300E+04 | 0.157255E+02 | 0.164422E+02 | 0.150088E+02 |
| 0.192400E+04 | 0.156825E+02 | 0.163907E+02 | 0.149743E+02 |
| 0.192500E+04 | 0.156331E+02 | 0.163316E+02 | 0.149346E+02 |
| 0.192600E+04 | 0.156682E+02 | 0.163808E+02 | 0.149555E+02 |
| 0.192700E+04 | 0.157066E+02 | 0.164245E+02 | 0.149886E+02 |
| 0.192800E+04 | 0.157245E+02 | 0.164405E+02 | 0.150084E+02 |
| 0.192900E+04 | 0.157362E+02 | 0.164512E+02 | 0.150212E+02 |
| 0.193000E+04 | 0.157466E+02 | 0.164617E+02 | 0.150315E+02 |
| 0.193100E+04 | 0.157556E+02 | 0.164708E+02 | 0.150404E+02 |
| 0.193200E+04 | 0.157634E+02 | 0.164786E+02 | 0.150483E+02 |
| 0.193300E+04 | 0.157704E+02 | 0.164854E+02 | 0.150554E+02 |
| 0.193400E+04 | 0.157767E+02 | 0.164915E+02 | 0.150619E+02 |
| 0.193500E+04 | 0.157825E+02 | 0.164971E+02 | 0.150680E+02 |
| 0.193600E+04 | 0.157880E+02 | 0.165023E+02 | 0.150737E+02 |
| 0.193700E+04 | 0.157932E+02 | 0.165073E+02 | 0.150791E+02 |
| 0.193800E+04 | 0.157982E+02 | 0.165121E+02 | 0.150843E+02 |
| 0.193900E+04 | 0.158030E+02 | 0.165167E+02 | 0.150894E+02 |
| 0.194000E+04 | 0.158077E+02 | 0.165212E+02 | 0.150942E+02 |
| 0.194100E+04 | 0.158123E+02 | 0.165255E+02 | 0.150990E+02 |
| 0.194200E+04 | 0.158167E+02 | 0.165298E+02 | 0.151036E+02 |
| 0.194300E+04 | 0.158211E+02 | 0.165340E+02 | 0.151081E+02 |
| 0.194400E+04 | 0.158254E+02 | 0.165381E+02 | 0.151126E+02 |
| 0.194500E+04 | 0.158291E+02 | 0.165417E+02 | 0.151166E+02 |
| 0.194600E+04 | 0.158320E+02 | 0.165442E+02 | 0.151198E+02 |
| 0.194700E+04 | 0.158342E+02 | 0.165460E+02 | 0.151224E+02 |
| 0.194800E+04 | 0.158360E+02 | 0.165473E+02 | 0.151247E+02 |
| 0.194900E+04 | 0.158376E+02 | 0.165484E+02 | 0.151268E+02 |
| 0.195000E+04 | 0.158389E+02 | 0.165492E+02 | 0.151286E+02 |
| 0.195100E+04 | 0.158401E+02 | 0.165500E+02 | 0.151302E+02 |
| 0.195200E+04 | 0.158413E+02 | 0.165508E+02 | 0.151318E+02 |
| 0.195300E+04 | 0.158424E+02 | 0.165515E+02 | 0.151333E+02 |
| 0.195400E+04 | 0.158435E+02 | 0.165522E+02 | 0.151348E+02 |
| 0.195500E+04 | 0.158446E+02 | 0.165529E+02 | 0.151362E+02 |
| 0.195600E+04 | 0.158296E+02 | 0.165350E+02 | 0.151242E+02 |
| 0.195700E+04 | 0.158189E+02 | 0.165220E+02 | 0.151158E+02 |
| 0.195800E+04 | 0.158290E+02 | 0.165337E+02 | 0.151243E+02 |
| 0.195900E+04 | 0.158366E+02 | 0.165427E+02 | 0.151306E+02 |
| 0.196000E+04 | 0.158404E+02 | 0.165468E+02 | 0.151340E+02 |
| 0.196100E+04 | 0.158274E+02 | 0.165313E+02 | 0.151235E+02 |
| 0.196200E+04 | 0.158072E+02 | 0.165072E+02 | 0.151071E+02 |
| 0.196300E+04 | 0.157852E+02 | 0.164815E+02 | 0.150889E+02 |
| 0.196400E+04 | 0.157480E+02 | 0.164482E+02 | 0.150479E+02 |
| 0.196500E+04 | 0.157029E+02 | 0.163927E+02 | 0.150131E+02 |
| 0.196600E+04 | 0.156912E+02 | 0.163798E+02 | 0.150026E+02 |
| 0.196700E+04 | 0.157050E+02 | 0.163969E+02 | 0.150132E+02 |
| 0.196800E+04 | 0.157040E+02 | 0.163964E+02 | 0.150115E+02 |
| 0.196900E+04 | 0.156759E+02 | 0.163646E+02 | 0.149871E+02 |
| 0.197000E+04 | 0.156684E+02 | 0.163565E+02 | 0.149802E+02 |
| 0.197100E+04 | 0.156973E+02 | 0.163906E+02 | 0.150040E+02 |
| 0.197200E+04 | 0.157282E+02 | 0.164260E+02 | 0.150305E+02 |
| 0.197300E+04 | 0.157417E+02 | 0.164392E+02 | 0.150442E+02 |
| 0.197400E+04 | 0.157413E+02 | 0.164378E+02 | 0.150447E+02 |
| 0.197500E+04 | 0.157155E+02 | 0.164070E+02 | 0.150241E+02 |
| 0.197600E+04 | 0.156971E+02 | 0.163846E+02 | 0.150096E+02 |

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|--------------|--------------|--------------|--------------|
| 0.197700E+04 | 0.157168E+02 | 0.164076E+02 | 0.150260E+02 |
| 0.197800E+04 | 0.157391E+02 | 0.164340E+02 | 0.150442E+02 |
| 0.197900E+04 | 0.157474E+02 | 0.164436E+02 | 0.150513E+02 |
| 0.198000E+04 | 0.157548E+02 | 0.164516E+02 | 0.150579E+02 |
| 0.198100E+04 | 0.157652E+02 | 0.164633E+02 | 0.150672E+02 |
| 0.198200E+04 | 0.156852E+02 | 0.163693E+02 | 0.150011E+02 |
| 0.198300E+04 | 0.155303E+02 | 0.161987E+02 | 0.148620E+02 |
| 0.198400E+04 | 0.154723E+02 | 0.161257E+02 | 0.148188E+02 |
| 0.198500E+04 | 0.155419E+02 | 0.162041E+02 | 0.148797E+02 |
| 0.198600E+04 | 0.157389E+02 | 0.162850E+02 | 0.151928E+02 |
| 0.198700E+04 | 0.157905E+02 | 0.163191E+02 | 0.152618E+02 |
| 0.198800E+04 | 0.158442E+02 | 0.163553E+02 | 0.153331E+02 |
| 0.198900E+04 | 0.158770E+02 | 0.163913E+02 | 0.153627E+02 |
| 0.199000E+04 | 0.158991E+02 | 0.164196E+02 | 0.153785E+02 |
| 0.199100E+04 | 0.158582E+02 | 0.163750E+02 | 0.153414E+02 |
| 0.199200E+04 | 0.157078E+02 | 0.162064E+02 | 0.152093E+02 |
| 0.199300E+04 | 0.156069E+02 | 0.160907E+02 | 0.151230E+02 |
| 0.199400E+04 | 0.156640E+02 | 0.161649E+02 | 0.151631E+02 |
| 0.199500E+04 | 0.157382E+02 | 0.162594E+02 | 0.152170E+02 |
| 0.199600E+04 | 0.157874E+02 | 0.163231E+02 | 0.152517E+02 |
| 0.199700E+04 | 0.158211E+02 | 0.163693E+02 | 0.152729E+02 |
| 0.199800E+04 | 0.158468E+02 | 0.164049E+02 | 0.152886E+02 |

```
#          91
# IPCC AR4 Millenium Runs output (vary solar forcing)
# ++++++
#
# Model: Bern2.5CC version with active C-cycle
# -----
# Prescribed forcing timeseries as described in file
# readme_doRuns_IPCC_Chap6_millennium_21jan06.txt
# provided by F. Joos, University of Bern.
#
# Contact:
# -----
# Gian-Kasper Plattner
# Climate and Environmental Physics
# Physics Institute, University of Bern
# Sidlerstrasse 5, CH-3012 Bern, Switzerland
# plattner@climate.unibe.ch
# http://www.climate.unibe.ch/~plattner/
# tel: ++41 (0)31 631-44-67
# fax: ++41 (0)31 631-87-42
#
# Some model setup informations:
# -----
# All runs with horizontal/vertical diffusion
#
# Run with standard ocean parameters
#   as used in Plattner et al. 2001/2002
#   with Kv (diffusivity) 4*10^-5 m2/s
#
# Climate sens. set to ~ 3.2 degrees C
# parameterized see Knutti et al. (Clim. Dyn. 2003)
```

```
#
# Model version is annual mean.
#
# No radiation code, CO2 radiative forcing calculated
# for as  $RF=5.35*\ln(CO2/CO2\_preind)$ ,
# Non-co2 radiative forcing prescribed according to
# Joos et al. GBC 2001 with updates for solar forcing
#
# More model description:
# -----
# Zonally averaged dynamical ocean with 3 basins and
# Southern Ocean, zonally averaged one layer energy
# and moisture balance atmosphere, thermodynamic
# sea ice (Stocker et al., J. Climate 1992).
#
# Carbon cycle components: Ocean/Atm/Terr.biosphere;
# Ocean carbon cycle is a description of the cycles
# of organic carbon and CaCO3 (Marchal et al., Tellus
# Tellus B), based on Redfield approach using PO4 as
# biolimiting nutrient.
#
# Land Biota: Lund-Jena-Postdam Dynamical Global
#               Vegetation Model (LPJ-DGVM)
# at GCM resolution (Gerber et al. 2003, Climate
# Dynamics; Sitch et al. 2003, Global Change Biology)
#
# LPJ forced by Cramer/Leemans annual mean
# climatology plus interannual climate variability
# from Hadley simulation (30-recycled climate) plus
# changes in the fields of surface temperature,
# precipitation, and cloudcover as simulated with the
# Impulse-EOF version of ECHAM-3/LSG in response to
# projected radiative forcing changes.
#
# Land use changes are not explicitly considered.
#
# Impact of climate change on terrestrial C-storage
# included
#
# References:
# -----
# Carbon cycle Ocean: Marchal et al., Tellus 1998
# Carbon cycle Terr. Bio: Sitch et al., GCB 2003
#                       Gerber et al., Clim. Dyn. 2003
# Ccycle-climate feedbacks and global warming:
#       Plattner et al., Tellus 2001
#       Plattner et al., GBC 2002
# Non-CO2 forcing: Joos et al., GBC 2001
# Climate model: Stocker et al., J. Climate 1992
# Sea level: Knutti et al., J. Climate 2000
# Global warming Physics: Knutti et al., Nature 2002
#                       Knutti et al., Cl. Dyn. 2003
#                       and refs therein.
#
```

Output columns:

Time (yr AD)

Global mean air temperature (deg C)

NH-averaged air temperature (deg C)

SH-averaged air temperature (deg C)

| | | | |
|--------------|--------------|--------------|--------------|
| 0.100100E+04 | 0.158198E+02 | 0.164832E+02 | 0.151565E+02 |
| 0.100200E+04 | 0.158243E+02 | 0.164878E+02 | 0.151609E+02 |
| 0.100300E+04 | 0.158278E+02 | 0.164912E+02 | 0.151644E+02 |
| 0.100400E+04 | 0.157993E+02 | 0.164574E+02 | 0.151413E+02 |
| 0.100500E+04 | 0.157659E+02 | 0.164170E+02 | 0.151147E+02 |
| 0.100600E+04 | 0.157759E+02 | 0.164288E+02 | 0.151229E+02 |
| 0.100700E+04 | 0.157951E+02 | 0.164521E+02 | 0.151380E+02 |
| 0.100800E+04 | 0.158040E+02 | 0.164631E+02 | 0.151448E+02 |
| 0.100900E+04 | 0.158072E+02 | 0.164670E+02 | 0.151474E+02 |
| 0.101000E+04 | 0.158081E+02 | 0.164681E+02 | 0.151481E+02 |
| 0.101100E+04 | 0.158077E+02 | 0.164676E+02 | 0.151478E+02 |
| 0.101200E+04 | 0.158057E+02 | 0.164648E+02 | 0.151465E+02 |
| 0.101300E+04 | 0.158043E+02 | 0.164635E+02 | 0.151451E+02 |
| 0.101400E+04 | 0.158024E+02 | 0.164611E+02 | 0.151437E+02 |
| 0.101500E+04 | 0.157049E+02 | 0.163571E+02 | 0.150527E+02 |
| 0.101600E+04 | 0.155972E+02 | 0.162248E+02 | 0.149697E+02 |
| 0.101700E+04 | 0.156922E+02 | 0.162643E+02 | 0.151202E+02 |
| 0.101800E+04 | 0.158353E+02 | 0.163405E+02 | 0.153301E+02 |
| 0.101900E+04 | 0.158968E+02 | 0.163819E+02 | 0.154116E+02 |
| 0.102000E+04 | 0.159173E+02 | 0.164048E+02 | 0.154297E+02 |
| 0.102100E+04 | 0.159278E+02 | 0.164205E+02 | 0.154351E+02 |
| 0.102200E+04 | 0.159328E+02 | 0.164321E+02 | 0.154335E+02 |
| 0.102300E+04 | 0.159343E+02 | 0.164407E+02 | 0.154280E+02 |
| 0.102400E+04 | 0.159336E+02 | 0.164470E+02 | 0.154202E+02 |
| 0.102500E+04 | 0.159334E+02 | 0.164515E+02 | 0.154153E+02 |
| 0.102600E+04 | 0.158073E+02 | 0.163080E+02 | 0.153065E+02 |
| 0.102700E+04 | 0.156707E+02 | 0.161542E+02 | 0.151872E+02 |
| 0.102800E+04 | 0.157090E+02 | 0.162032E+02 | 0.152148E+02 |
| 0.102900E+04 | 0.157763E+02 | 0.162882E+02 | 0.152644E+02 |
| 0.103000E+04 | 0.158056E+02 | 0.163276E+02 | 0.152836E+02 |
| 0.103100E+04 | 0.158192E+02 | 0.163487E+02 | 0.152896E+02 |
| 0.103200E+04 | 0.158266E+02 | 0.163619E+02 | 0.152913E+02 |
| 0.103300E+04 | 0.158306E+02 | 0.163703E+02 | 0.152909E+02 |
| 0.103400E+04 | 0.158323E+02 | 0.163755E+02 | 0.152891E+02 |
| 0.103500E+04 | 0.158325E+02 | 0.163784E+02 | 0.152866E+02 |
| 0.103600E+04 | 0.158318E+02 | 0.163799E+02 | 0.152837E+02 |
| 0.103700E+04 | 0.158306E+02 | 0.163806E+02 | 0.152807E+02 |
| 0.103800E+04 | 0.158291E+02 | 0.163805E+02 | 0.152777E+02 |
| 0.103900E+04 | 0.158272E+02 | 0.163798E+02 | 0.152746E+02 |
| 0.104000E+04 | 0.158251E+02 | 0.163787E+02 | 0.152715E+02 |
| 0.104100E+04 | 0.158228E+02 | 0.163772E+02 | 0.152684E+02 |
| 0.104200E+04 | 0.158204E+02 | 0.163754E+02 | 0.152653E+02 |
| 0.104300E+04 | 0.158178E+02 | 0.163734E+02 | 0.152623E+02 |
| 0.104400E+04 | 0.158152E+02 | 0.163712E+02 | 0.152592E+02 |
| 0.104500E+04 | 0.158126E+02 | 0.163690E+02 | 0.152563E+02 |
| 0.104600E+04 | 0.158103E+02 | 0.163669E+02 | 0.152536E+02 |
| 0.104700E+04 | 0.158080E+02 | 0.163649E+02 | 0.152510E+02 |
| 0.104800E+04 | 0.158058E+02 | 0.163630E+02 | 0.152486E+02 |

| | | | |
|--------------|--------------|--------------|--------------|
| 0.104900E+04 | 0.158037E+02 | 0.163611E+02 | 0.152462E+02 |
| 0.105000E+04 | 0.158016E+02 | 0.163592E+02 | 0.152439E+02 |
| 0.105100E+04 | 0.157995E+02 | 0.163574E+02 | 0.152417E+02 |
| 0.105200E+04 | 0.157976E+02 | 0.163555E+02 | 0.152396E+02 |
| 0.105300E+04 | 0.157956E+02 | 0.163537E+02 | 0.152375E+02 |
| 0.105400E+04 | 0.157937E+02 | 0.163520E+02 | 0.152355E+02 |
| 0.105500E+04 | 0.157920E+02 | 0.163503E+02 | 0.152336E+02 |
| 0.105600E+04 | 0.157904E+02 | 0.163489E+02 | 0.152320E+02 |
| 0.105700E+04 | 0.157890E+02 | 0.163477E+02 | 0.152304E+02 |
| 0.105800E+04 | 0.156301E+02 | 0.161679E+02 | 0.150922E+02 |
| 0.105900E+04 | 0.154447E+02 | 0.159496E+02 | 0.149398E+02 |
| 0.106000E+04 | 0.155357E+02 | 0.160581E+02 | 0.150132E+02 |
| 0.106100E+04 | 0.156325E+02 | 0.161633E+02 | 0.151018E+02 |
| 0.106200E+04 | 0.156342E+02 | 0.161646E+02 | 0.151038E+02 |
| 0.106300E+04 | 0.156119E+02 | 0.161382E+02 | 0.150856E+02 |
| 0.106400E+04 | 0.156460E+02 | 0.161794E+02 | 0.151126E+02 |
| 0.106500E+04 | 0.156822E+02 | 0.162208E+02 | 0.151436E+02 |
| 0.106600E+04 | 0.157069E+02 | 0.162513E+02 | 0.151626E+02 |
| 0.106700E+04 | 0.157247E+02 | 0.162733E+02 | 0.151762E+02 |
| 0.106800E+04 | 0.157394E+02 | 0.162911E+02 | 0.151878E+02 |
| 0.106900E+04 | 0.157523E+02 | 0.163065E+02 | 0.151981E+02 |
| 0.107000E+04 | 0.157640E+02 | 0.163203E+02 | 0.152077E+02 |
| 0.107100E+04 | 0.157749E+02 | 0.163330E+02 | 0.152168E+02 |
| 0.107200E+04 | 0.157851E+02 | 0.163448E+02 | 0.152254E+02 |
| 0.107300E+04 | 0.157949E+02 | 0.163560E+02 | 0.152337E+02 |
| 0.107400E+04 | 0.158043E+02 | 0.163667E+02 | 0.152418E+02 |
| 0.107500E+04 | 0.158134E+02 | 0.163771E+02 | 0.152497E+02 |
| 0.107600E+04 | 0.158220E+02 | 0.163869E+02 | 0.152571E+02 |
| 0.107700E+04 | 0.158298E+02 | 0.163957E+02 | 0.152639E+02 |
| 0.107800E+04 | 0.158370E+02 | 0.164038E+02 | 0.152702E+02 |
| 0.107900E+04 | 0.158400E+02 | 0.164116E+02 | 0.152683E+02 |
| 0.108000E+04 | 0.158048E+02 | 0.163826E+02 | 0.152269E+02 |
| 0.108100E+04 | 0.157708E+02 | 0.163494E+02 | 0.151922E+02 |
| 0.108200E+04 | 0.157538E+02 | 0.163680E+02 | 0.151397E+02 |
| 0.108300E+04 | 0.157311E+02 | 0.163982E+02 | 0.150640E+02 |
| 0.108400E+04 | 0.157226E+02 | 0.164154E+02 | 0.150299E+02 |
| 0.108500E+04 | 0.157160E+02 | 0.164249E+02 | 0.150071E+02 |
| 0.108600E+04 | 0.157110E+02 | 0.164313E+02 | 0.149906E+02 |
| 0.108700E+04 | 0.157089E+02 | 0.164360E+02 | 0.149817E+02 |
| 0.108800E+04 | 0.157096E+02 | 0.164399E+02 | 0.149794E+02 |
| 0.108900E+04 | 0.157124E+02 | 0.164433E+02 | 0.149816E+02 |
| 0.109000E+04 | 0.157168E+02 | 0.164467E+02 | 0.149869E+02 |
| 0.109100E+04 | 0.157223E+02 | 0.164502E+02 | 0.149944E+02 |
| 0.109200E+04 | 0.157287E+02 | 0.164539E+02 | 0.150035E+02 |
| 0.109300E+04 | 0.157358E+02 | 0.164580E+02 | 0.150136E+02 |
| 0.109400E+04 | 0.157432E+02 | 0.164622E+02 | 0.150242E+02 |
| 0.109500E+04 | 0.157509E+02 | 0.164668E+02 | 0.150350E+02 |
| 0.109600E+04 | 0.157588E+02 | 0.164717E+02 | 0.150458E+02 |
| 0.109700E+04 | 0.156945E+02 | 0.163925E+02 | 0.149966E+02 |
| 0.109800E+04 | 0.156362E+02 | 0.163297E+02 | 0.149426E+02 |
| 0.109900E+04 | 0.156661E+02 | 0.163605E+02 | 0.149717E+02 |
| 0.110000E+04 | 0.157151E+02 | 0.164127E+02 | 0.150175E+02 |
| 0.110100E+04 | 0.157415E+02 | 0.164386E+02 | 0.150443E+02 |
| 0.110200E+04 | 0.157598E+02 | 0.164572E+02 | 0.150623E+02 |

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| 0.110300E+04 | 0.157741E+02 | 0.164720E+02 | 0.150762E+02 |
| 0.110400E+04 | 0.157834E+02 | 0.164844E+02 | 0.150824E+02 |
| 0.110500E+04 | 0.157919E+02 | 0.164952E+02 | 0.150887E+02 |
| 0.110600E+04 | 0.158000E+02 | 0.165047E+02 | 0.150953E+02 |
| 0.110700E+04 | 0.158079E+02 | 0.165135E+02 | 0.151023E+02 |
| 0.110800E+04 | 0.158155E+02 | 0.165217E+02 | 0.151093E+02 |
| 0.110900E+04 | 0.158233E+02 | 0.165301E+02 | 0.151166E+02 |
| 0.111000E+04 | 0.158318E+02 | 0.165391E+02 | 0.151245E+02 |
| 0.111100E+04 | 0.158407E+02 | 0.165486E+02 | 0.151327E+02 |
| 0.111200E+04 | 0.158496E+02 | 0.165582E+02 | 0.151411E+02 |
| 0.111300E+04 | 0.158587E+02 | 0.165679E+02 | 0.151494E+02 |
| 0.111400E+04 | 0.158678E+02 | 0.165777E+02 | 0.151578E+02 |
| 0.111500E+04 | 0.158761E+02 | 0.165868E+02 | 0.151655E+02 |
| 0.111600E+04 | 0.158832E+02 | 0.165944E+02 | 0.151721E+02 |
| 0.111700E+04 | 0.158895E+02 | 0.166011E+02 | 0.151780E+02 |
| 0.111800E+04 | 0.158952E+02 | 0.166071E+02 | 0.151833E+02 |
| 0.111900E+04 | 0.159005E+02 | 0.166128E+02 | 0.151883E+02 |
| 0.112000E+04 | 0.159055E+02 | 0.166181E+02 | 0.151929E+02 |
| 0.112100E+04 | 0.159101E+02 | 0.166231E+02 | 0.151972E+02 |
| 0.112200E+04 | 0.159145E+02 | 0.166279E+02 | 0.152012E+02 |
| 0.112300E+04 | 0.159187E+02 | 0.166324E+02 | 0.152050E+02 |
| 0.112400E+04 | 0.159227E+02 | 0.166368E+02 | 0.152087E+02 |
| 0.112500E+04 | 0.159265E+02 | 0.166410E+02 | 0.152121E+02 |
| 0.112600E+04 | 0.159303E+02 | 0.166450E+02 | 0.152155E+02 |
| 0.112700E+04 | 0.159339E+02 | 0.166490E+02 | 0.152187E+02 |
| 0.112800E+04 | 0.159373E+02 | 0.166528E+02 | 0.152219E+02 |
| 0.112900E+04 | 0.159407E+02 | 0.166566E+02 | 0.152249E+02 |
| 0.113000E+04 | 0.159440E+02 | 0.166602E+02 | 0.152279E+02 |
| 0.113100E+04 | 0.159472E+02 | 0.166637E+02 | 0.152308E+02 |
| 0.113200E+04 | 0.159504E+02 | 0.166671E+02 | 0.152336E+02 |
| 0.113300E+04 | 0.159538E+02 | 0.166708E+02 | 0.152367E+02 |
| 0.113400E+04 | 0.159577E+02 | 0.166752E+02 | 0.152403E+02 |
| 0.113500E+04 | 0.159618E+02 | 0.166797E+02 | 0.152439E+02 |
| 0.113600E+04 | 0.159657E+02 | 0.166839E+02 | 0.152474E+02 |
| 0.113700E+04 | 0.159694E+02 | 0.166880E+02 | 0.152509E+02 |
| 0.113800E+04 | 0.159731E+02 | 0.166919E+02 | 0.152543E+02 |
| 0.113900E+04 | 0.159765E+02 | 0.166955E+02 | 0.152575E+02 |
| 0.114000E+04 | 0.159795E+02 | 0.166986E+02 | 0.152604E+02 |
| 0.114100E+04 | 0.159821E+02 | 0.167012E+02 | 0.152629E+02 |
| 0.114200E+04 | 0.159844E+02 | 0.167036E+02 | 0.152653E+02 |
| 0.114300E+04 | 0.159866E+02 | 0.167057E+02 | 0.152676E+02 |
| 0.114400E+04 | 0.159887E+02 | 0.167077E+02 | 0.152697E+02 |
| 0.114500E+04 | 0.159906E+02 | 0.167095E+02 | 0.152718E+02 |
| 0.114600E+04 | 0.159925E+02 | 0.167112E+02 | 0.152737E+02 |
| 0.114700E+04 | 0.159940E+02 | 0.167125E+02 | 0.152754E+02 |
| 0.114800E+04 | 0.159950E+02 | 0.167133E+02 | 0.152767E+02 |
| 0.114900E+04 | 0.159957E+02 | 0.167137E+02 | 0.152778E+02 |
| 0.115000E+04 | 0.159962E+02 | 0.167138E+02 | 0.152786E+02 |
| 0.115100E+04 | 0.159965E+02 | 0.167137E+02 | 0.152793E+02 |
| 0.115200E+04 | 0.159966E+02 | 0.167134E+02 | 0.152798E+02 |
| 0.115300E+04 | 0.159966E+02 | 0.167130E+02 | 0.152803E+02 |
| 0.115400E+04 | 0.159965E+02 | 0.167124E+02 | 0.152806E+02 |
| 0.115500E+04 | 0.159962E+02 | 0.167117E+02 | 0.152808E+02 |
| 0.115600E+04 | 0.159956E+02 | 0.167106E+02 | 0.152807E+02 |

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| 0.115700E+04 | 0.159945E+02 | 0.167088E+02 | 0.152802E+02 |
| 0.115800E+04 | 0.159930E+02 | 0.167066E+02 | 0.152793E+02 |
| 0.115900E+04 | 0.159911E+02 | 0.167041E+02 | 0.152782E+02 |
| 0.116000E+04 | 0.159891E+02 | 0.167013E+02 | 0.152769E+02 |
| 0.116100E+04 | 0.159869E+02 | 0.166984E+02 | 0.152754E+02 |
| 0.116200E+04 | 0.159852E+02 | 0.166961E+02 | 0.152744E+02 |
| 0.116300E+04 | 0.159846E+02 | 0.166950E+02 | 0.152742E+02 |
| 0.116400E+04 | 0.159845E+02 | 0.166946E+02 | 0.152745E+02 |
| 0.116500E+04 | 0.159849E+02 | 0.166947E+02 | 0.152751E+02 |
| 0.116600E+04 | 0.159428E+02 | 0.166451E+02 | 0.152404E+02 |
| 0.116700E+04 | 0.158959E+02 | 0.165897E+02 | 0.152021E+02 |
| 0.116800E+04 | 0.159091E+02 | 0.166057E+02 | 0.152125E+02 |
| 0.116900E+04 | 0.159352E+02 | 0.166373E+02 | 0.152331E+02 |
| 0.117000E+04 | 0.159482E+02 | 0.166529E+02 | 0.152434E+02 |
| 0.117100E+04 | 0.159542E+02 | 0.166597E+02 | 0.152486E+02 |
| 0.117200E+04 | 0.159575E+02 | 0.166631E+02 | 0.152518E+02 |
| 0.117300E+04 | 0.159593E+02 | 0.166647E+02 | 0.152539E+02 |
| 0.117400E+04 | 0.159602E+02 | 0.166651E+02 | 0.152553E+02 |
| 0.117500E+04 | 0.157061E+02 | 0.163789E+02 | 0.150332E+02 |
| 0.117600E+04 | 0.155551E+02 | 0.160890E+02 | 0.150211E+02 |
| 0.117700E+04 | 0.156680E+02 | 0.161564E+02 | 0.151796E+02 |
| 0.117800E+04 | 0.158097E+02 | 0.162988E+02 | 0.153207E+02 |
| 0.117900E+04 | 0.159020E+02 | 0.163952E+02 | 0.154089E+02 |
| 0.118000E+04 | 0.159557E+02 | 0.164593E+02 | 0.154521E+02 |
| 0.118100E+04 | 0.159869E+02 | 0.164999E+02 | 0.154740E+02 |
| 0.118200E+04 | 0.160078E+02 | 0.165298E+02 | 0.154859E+02 |
| 0.118300E+04 | 0.160225E+02 | 0.165528E+02 | 0.154922E+02 |
| 0.118400E+04 | 0.160353E+02 | 0.165710E+02 | 0.154997E+02 |
| 0.118500E+04 | 0.160428E+02 | 0.165854E+02 | 0.155002E+02 |
| 0.118600E+04 | 0.160485E+02 | 0.165969E+02 | 0.155001E+02 |
| 0.118700E+04 | 0.160526E+02 | 0.166060E+02 | 0.154991E+02 |
| 0.118800E+04 | 0.160766E+02 | 0.166547E+02 | 0.154985E+02 |
| 0.118900E+04 | 0.160785E+02 | 0.166574E+02 | 0.154996E+02 |
| 0.119000E+04 | 0.160782E+02 | 0.166579E+02 | 0.154985E+02 |
| 0.119100E+04 | 0.160780E+02 | 0.166591E+02 | 0.154970E+02 |
| 0.119200E+04 | 0.160753E+02 | 0.166606E+02 | 0.154901E+02 |
| 0.119300E+04 | 0.160652E+02 | 0.166627E+02 | 0.154678E+02 |
| 0.119400E+04 | 0.159646E+02 | 0.165549E+02 | 0.153743E+02 |
| 0.119500E+04 | 0.158724E+02 | 0.164638E+02 | 0.152809E+02 |
| 0.119600E+04 | 0.158950E+02 | 0.164984E+02 | 0.152916E+02 |
| 0.119700E+04 | 0.159428E+02 | 0.165637E+02 | 0.153219E+02 |
| 0.119800E+04 | 0.159032E+02 | 0.165916E+02 | 0.152148E+02 |
| 0.119900E+04 | 0.158916E+02 | 0.166071E+02 | 0.151761E+02 |
| 0.120000E+04 | 0.158844E+02 | 0.166165E+02 | 0.151523E+02 |
| 0.120100E+04 | 0.158801E+02 | 0.166220E+02 | 0.151382E+02 |
| 0.120200E+04 | 0.158785E+02 | 0.166253E+02 | 0.151316E+02 |
| 0.120300E+04 | 0.158775E+02 | 0.166273E+02 | 0.151277E+02 |
| 0.120400E+04 | 0.158785E+02 | 0.166281E+02 | 0.151289E+02 |
| 0.120500E+04 | 0.158302E+02 | 0.165689E+02 | 0.150915E+02 |
| 0.120600E+04 | 0.157769E+02 | 0.165023E+02 | 0.150514E+02 |
| 0.120700E+04 | 0.158087E+02 | 0.165443E+02 | 0.150730E+02 |
| 0.120800E+04 | 0.158443E+02 | 0.165808E+02 | 0.151079E+02 |
| 0.120900E+04 | 0.158653E+02 | 0.166001E+02 | 0.151305E+02 |
| 0.121000E+04 | 0.158786E+02 | 0.166102E+02 | 0.151469E+02 |

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| 0.121100E+04 | 0.158889E+02 | 0.166169E+02 | 0.151609E+02 |
| 0.121200E+04 | 0.158974E+02 | 0.166217E+02 | 0.151732E+02 |
| 0.121300E+04 | 0.159048E+02 | 0.166253E+02 | 0.151842E+02 |
| 0.121400E+04 | 0.159113E+02 | 0.166284E+02 | 0.151942E+02 |
| 0.121500E+04 | 0.159171E+02 | 0.166310E+02 | 0.152032E+02 |
| 0.121600E+04 | 0.159224E+02 | 0.166335E+02 | 0.152113E+02 |
| 0.121700E+04 | 0.159272E+02 | 0.166357E+02 | 0.152186E+02 |
| 0.121800E+04 | 0.159263E+02 | 0.166278E+02 | 0.152248E+02 |
| 0.121900E+04 | 0.159273E+02 | 0.166248E+02 | 0.152299E+02 |
| 0.122000E+04 | 0.159301E+02 | 0.166254E+02 | 0.152348E+02 |
| 0.122100E+04 | 0.159331E+02 | 0.166269E+02 | 0.152393E+02 |
| 0.122200E+04 | 0.159361E+02 | 0.166288E+02 | 0.152434E+02 |
| 0.122300E+04 | 0.159390E+02 | 0.166308E+02 | 0.152472E+02 |
| 0.122400E+04 | 0.159418E+02 | 0.166328E+02 | 0.152507E+02 |
| 0.122500E+04 | 0.159444E+02 | 0.166350E+02 | 0.152539E+02 |
| 0.122600E+04 | 0.159470E+02 | 0.166371E+02 | 0.152568E+02 |
| 0.122700E+04 | 0.159183E+02 | 0.166029E+02 | 0.152336E+02 |
| 0.122800E+04 | 0.158857E+02 | 0.165640E+02 | 0.152075E+02 |
| 0.122900E+04 | 0.157160E+02 | 0.163768E+02 | 0.150552E+02 |
| 0.123000E+04 | 0.155305E+02 | 0.161578E+02 | 0.149031E+02 |
| 0.123100E+04 | 0.156023E+02 | 0.162576E+02 | 0.149471E+02 |
| 0.123200E+04 | 0.158348E+02 | 0.163672E+02 | 0.153024E+02 |
| 0.123300E+04 | 0.159388E+02 | 0.164432E+02 | 0.154343E+02 |
| 0.123400E+04 | 0.159826E+02 | 0.164817E+02 | 0.154834E+02 |
| 0.123500E+04 | 0.160090E+02 | 0.165129E+02 | 0.155051E+02 |
| 0.123600E+04 | 0.160305E+02 | 0.165364E+02 | 0.155247E+02 |
| 0.123700E+04 | 0.160418E+02 | 0.165544E+02 | 0.155293E+02 |
| 0.123800E+04 | 0.160491E+02 | 0.165688E+02 | 0.155293E+02 |
| 0.123900E+04 | 0.160539E+02 | 0.165807E+02 | 0.155271E+02 |
| 0.124000E+04 | 0.160592E+02 | 0.165906E+02 | 0.155278E+02 |
| 0.124100E+04 | 0.160611E+02 | 0.165986E+02 | 0.155237E+02 |
| 0.124200E+04 | 0.160843E+02 | 0.166467E+02 | 0.155218E+02 |
| 0.124300E+04 | 0.160824E+02 | 0.166463E+02 | 0.155184E+02 |
| 0.124400E+04 | 0.160807E+02 | 0.166474E+02 | 0.155139E+02 |
| 0.124500E+04 | 0.160789E+02 | 0.166485E+02 | 0.155093E+02 |
| 0.124600E+04 | 0.160769E+02 | 0.166490E+02 | 0.155047E+02 |
| 0.124700E+04 | 0.160746E+02 | 0.166490E+02 | 0.155002E+02 |
| 0.124800E+04 | 0.160721E+02 | 0.166484E+02 | 0.154957E+02 |
| 0.124900E+04 | 0.160626E+02 | 0.166474E+02 | 0.154779E+02 |
| 0.125000E+04 | 0.160491E+02 | 0.166458E+02 | 0.154524E+02 |
| 0.125100E+04 | 0.160367E+02 | 0.166437E+02 | 0.154298E+02 |
| 0.125200E+04 | 0.160257E+02 | 0.166410E+02 | 0.154104E+02 |
| 0.125300E+04 | 0.159714E+02 | 0.166378E+02 | 0.153049E+02 |
| 0.125400E+04 | 0.159320E+02 | 0.166324E+02 | 0.152317E+02 |
| 0.125500E+04 | 0.159047E+02 | 0.166241E+02 | 0.151854E+02 |
| 0.125600E+04 | 0.158828E+02 | 0.166139E+02 | 0.151518E+02 |
| 0.125700E+04 | 0.158651E+02 | 0.166026E+02 | 0.151276E+02 |
| 0.125800E+04 | 0.158516E+02 | 0.165919E+02 | 0.151112E+02 |
| 0.125900E+04 | 0.154066E+02 | 0.160746E+02 | 0.147385E+02 |
| 0.126000E+04 | 0.151789E+02 | 0.159923E+02 | 0.143655E+02 |
| 0.126100E+04 | 0.153780E+02 | 0.163057E+02 | 0.144502E+02 |
| 0.126200E+04 | 0.155707E+02 | 0.164931E+02 | 0.146483E+02 |
| 0.126300E+04 | 0.157044E+02 | 0.166117E+02 | 0.147971E+02 |
| 0.126400E+04 | 0.157668E+02 | 0.166547E+02 | 0.148790E+02 |

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| 0.126500E+04 | 0.157988E+02 | 0.166676E+02 | 0.149300E+02 |
| 0.126600E+04 | 0.158190E+02 | 0.166698E+02 | 0.149681E+02 |
| 0.126700E+04 | 0.158328E+02 | 0.166672E+02 | 0.149984E+02 |
| 0.126800E+04 | 0.158402E+02 | 0.166622E+02 | 0.150182E+02 |
| 0.126900E+04 | 0.158443E+02 | 0.166562E+02 | 0.150324E+02 |
| 0.127000E+04 | 0.158475E+02 | 0.166500E+02 | 0.150451E+02 |
| 0.127100E+04 | 0.158502E+02 | 0.166441E+02 | 0.150563E+02 |
| 0.127200E+04 | 0.158528E+02 | 0.166392E+02 | 0.150663E+02 |
| 0.127300E+04 | 0.158547E+02 | 0.166344E+02 | 0.150751E+02 |
| 0.127400E+04 | 0.158561E+02 | 0.166298E+02 | 0.150824E+02 |
| 0.127500E+04 | 0.157561E+02 | 0.165083E+02 | 0.150038E+02 |
| 0.127600E+04 | 0.156436E+02 | 0.163829E+02 | 0.149043E+02 |
| 0.127700E+04 | 0.156838E+02 | 0.164215E+02 | 0.149462E+02 |
| 0.127800E+04 | 0.157466E+02 | 0.164854E+02 | 0.150078E+02 |
| 0.127900E+04 | 0.157763E+02 | 0.165146E+02 | 0.150380E+02 |
| 0.128000E+04 | 0.157896E+02 | 0.165289E+02 | 0.150502E+02 |
| 0.128100E+04 | 0.157950E+02 | 0.165382E+02 | 0.150519E+02 |
| 0.128200E+04 | 0.158011E+02 | 0.165447E+02 | 0.150575E+02 |
| 0.128300E+04 | 0.158063E+02 | 0.165496E+02 | 0.150630E+02 |
| 0.128400E+04 | 0.158105E+02 | 0.165533E+02 | 0.150677E+02 |
| 0.128500E+04 | 0.156789E+02 | 0.164073E+02 | 0.149505E+02 |
| 0.128600E+04 | 0.155441E+02 | 0.162566E+02 | 0.148316E+02 |
| 0.128700E+04 | 0.156781E+02 | 0.163160E+02 | 0.150403E+02 |
| 0.128800E+04 | 0.158254E+02 | 0.164030E+02 | 0.152478E+02 |
| 0.128900E+04 | 0.158797E+02 | 0.164310E+02 | 0.153284E+02 |
| 0.129000E+04 | 0.158992E+02 | 0.164390E+02 | 0.153593E+02 |
| 0.129100E+04 | 0.158601E+02 | 0.163505E+02 | 0.153698E+02 |
| 0.129200E+04 | 0.158519E+02 | 0.163342E+02 | 0.153696E+02 |
| 0.129300E+04 | 0.158478E+02 | 0.163252E+02 | 0.153704E+02 |
| 0.129400E+04 | 0.158432E+02 | 0.163233E+02 | 0.153632E+02 |
| 0.129500E+04 | 0.157342E+02 | 0.162231E+02 | 0.152453E+02 |
| 0.129600E+04 | 0.155999E+02 | 0.160736E+02 | 0.151262E+02 |
| 0.129700E+04 | 0.156462E+02 | 0.161356E+02 | 0.151568E+02 |
| 0.129800E+04 | 0.157221E+02 | 0.162319E+02 | 0.152122E+02 |
| 0.129900E+04 | 0.157562E+02 | 0.162769E+02 | 0.152354E+02 |
| 0.130000E+04 | 0.157744E+02 | 0.163043E+02 | 0.152444E+02 |
| 0.130100E+04 | 0.157867E+02 | 0.163246E+02 | 0.152488E+02 |
| 0.130200E+04 | 0.157957E+02 | 0.163406E+02 | 0.152509E+02 |
| 0.130300E+04 | 0.158027E+02 | 0.163537E+02 | 0.152517E+02 |
| 0.130400E+04 | 0.158081E+02 | 0.163645E+02 | 0.152517E+02 |
| 0.130500E+04 | 0.158125E+02 | 0.163737E+02 | 0.152513E+02 |
| 0.130600E+04 | 0.158160E+02 | 0.163814E+02 | 0.152505E+02 |
| 0.130700E+04 | 0.158188E+02 | 0.163879E+02 | 0.152496E+02 |
| 0.130800E+04 | 0.158210E+02 | 0.163935E+02 | 0.152485E+02 |
| 0.130900E+04 | 0.158229E+02 | 0.163984E+02 | 0.152473E+02 |
| 0.131000E+04 | 0.158244E+02 | 0.164028E+02 | 0.152461E+02 |
| 0.131100E+04 | 0.158258E+02 | 0.164066E+02 | 0.152449E+02 |
| 0.131200E+04 | 0.158269E+02 | 0.164101E+02 | 0.152437E+02 |
| 0.131300E+04 | 0.158278E+02 | 0.164131E+02 | 0.152426E+02 |
| 0.131400E+04 | 0.158286E+02 | 0.164157E+02 | 0.152415E+02 |
| 0.131500E+04 | 0.158292E+02 | 0.164180E+02 | 0.152404E+02 |
| 0.131600E+04 | 0.158297E+02 | 0.164201E+02 | 0.152394E+02 |
| 0.131700E+04 | 0.158301E+02 | 0.164218E+02 | 0.152384E+02 |
| 0.131800E+04 | 0.158304E+02 | 0.164233E+02 | 0.152374E+02 |

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| 0.131900E+04 | 0.158305E+02 | 0.164245E+02 | 0.152365E+02 |
| 0.132000E+04 | 0.158306E+02 | 0.164256E+02 | 0.152356E+02 |
| 0.132100E+04 | 0.158306E+02 | 0.164264E+02 | 0.152347E+02 |
| 0.132200E+04 | 0.158305E+02 | 0.164271E+02 | 0.152339E+02 |
| 0.132300E+04 | 0.158305E+02 | 0.164278E+02 | 0.152332E+02 |
| 0.132400E+04 | 0.158307E+02 | 0.164287E+02 | 0.152327E+02 |
| 0.132500E+04 | 0.158312E+02 | 0.164299E+02 | 0.152324E+02 |
| 0.132600E+04 | 0.158315E+02 | 0.164308E+02 | 0.152322E+02 |
| 0.132700E+04 | 0.158319E+02 | 0.164318E+02 | 0.152321E+02 |
| 0.132800E+04 | 0.158324E+02 | 0.164327E+02 | 0.152320E+02 |
| 0.132900E+04 | 0.157141E+02 | 0.162918E+02 | 0.151363E+02 |
| 0.133000E+04 | 0.156046E+02 | 0.161702E+02 | 0.150389E+02 |
| 0.133100E+04 | 0.156442E+02 | 0.162173E+02 | 0.150711E+02 |
| 0.133200E+04 | 0.157106E+02 | 0.162948E+02 | 0.151263E+02 |
| 0.133300E+04 | 0.157420E+02 | 0.163337E+02 | 0.151504E+02 |
| 0.133400E+04 | 0.157600E+02 | 0.163560E+02 | 0.151639E+02 |
| 0.133500E+04 | 0.157732E+02 | 0.163725E+02 | 0.151740E+02 |
| 0.133600E+04 | 0.157850E+02 | 0.163868E+02 | 0.151833E+02 |
| 0.133700E+04 | 0.157958E+02 | 0.163998E+02 | 0.151919E+02 |
| 0.133800E+04 | 0.158060E+02 | 0.164118E+02 | 0.152001E+02 |
| 0.133900E+04 | 0.158156E+02 | 0.164232E+02 | 0.152080E+02 |
| 0.134000E+04 | 0.158241E+02 | 0.164330E+02 | 0.152151E+02 |
| 0.134100E+04 | 0.158309E+02 | 0.164408E+02 | 0.152210E+02 |
| 0.134200E+04 | 0.158366E+02 | 0.164472E+02 | 0.152260E+02 |
| 0.134300E+04 | 0.158416E+02 | 0.164526E+02 | 0.152305E+02 |
| 0.134400E+04 | 0.158414E+02 | 0.164574E+02 | 0.152255E+02 |
| 0.134500E+04 | 0.157354E+02 | 0.163394E+02 | 0.151314E+02 |
| 0.134600E+04 | 0.156320E+02 | 0.162377E+02 | 0.150263E+02 |
| 0.134700E+04 | 0.156879E+02 | 0.162778E+02 | 0.150981E+02 |
| 0.134800E+04 | 0.157488E+02 | 0.163464E+02 | 0.151512E+02 |
| 0.134900E+04 | 0.157809E+02 | 0.163830E+02 | 0.151788E+02 |
| 0.135000E+04 | 0.157996E+02 | 0.164046E+02 | 0.151947E+02 |
| 0.135100E+04 | 0.158130E+02 | 0.164199E+02 | 0.152062E+02 |
| 0.135200E+04 | 0.158234E+02 | 0.164316E+02 | 0.152152E+02 |
| 0.135300E+04 | 0.158316E+02 | 0.164408E+02 | 0.152224E+02 |
| 0.135400E+04 | 0.158384E+02 | 0.164484E+02 | 0.152285E+02 |
| 0.135500E+04 | 0.158404E+02 | 0.164547E+02 | 0.152261E+02 |
| 0.135600E+04 | 0.158336E+02 | 0.164599E+02 | 0.152073E+02 |
| 0.135700E+04 | 0.158276E+02 | 0.164644E+02 | 0.151908E+02 |
| 0.135800E+04 | 0.157768E+02 | 0.164683E+02 | 0.150853E+02 |
| 0.135900E+04 | 0.157462E+02 | 0.164709E+02 | 0.150215E+02 |
| 0.136000E+04 | 0.157277E+02 | 0.164723E+02 | 0.149831E+02 |
| 0.136100E+04 | 0.157152E+02 | 0.164728E+02 | 0.149575E+02 |
| 0.136200E+04 | 0.157056E+02 | 0.164729E+02 | 0.149383E+02 |
| 0.136300E+04 | 0.157006E+02 | 0.164726E+02 | 0.149286E+02 |
| 0.136400E+04 | 0.156988E+02 | 0.164725E+02 | 0.149252E+02 |
| 0.136500E+04 | 0.156995E+02 | 0.164725E+02 | 0.149264E+02 |
| 0.136600E+04 | 0.157020E+02 | 0.164730E+02 | 0.149309E+02 |
| 0.136700E+04 | 0.157058E+02 | 0.164739E+02 | 0.149378E+02 |
| 0.136800E+04 | 0.157107E+02 | 0.164752E+02 | 0.149461E+02 |
| 0.136900E+04 | 0.157162E+02 | 0.164769E+02 | 0.149555E+02 |
| 0.137000E+04 | 0.157223E+02 | 0.164791E+02 | 0.149655E+02 |
| 0.137100E+04 | 0.157287E+02 | 0.164817E+02 | 0.149756E+02 |
| 0.137200E+04 | 0.157352E+02 | 0.164846E+02 | 0.149859E+02 |

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| 0.137300E+04 | 0.157418E+02 | 0.164878E+02 | 0.149959E+02 |
| 0.137400E+04 | 0.157482E+02 | 0.164909E+02 | 0.150055E+02 |
| 0.137500E+04 | 0.157110E+02 | 0.164436E+02 | 0.149784E+02 |
| 0.137600E+04 | 0.156687E+02 | 0.163899E+02 | 0.149474E+02 |
| 0.137700E+04 | 0.156873E+02 | 0.164093E+02 | 0.149653E+02 |
| 0.137800E+04 | 0.157195E+02 | 0.164457E+02 | 0.149934E+02 |
| 0.137900E+04 | 0.157384E+02 | 0.164659E+02 | 0.150109E+02 |
| 0.138000E+04 | 0.157501E+02 | 0.164773E+02 | 0.150229E+02 |
| 0.138100E+04 | 0.157590E+02 | 0.164854E+02 | 0.150326E+02 |
| 0.138200E+04 | 0.157664E+02 | 0.164918E+02 | 0.150409E+02 |
| 0.138300E+04 | 0.157726E+02 | 0.164971E+02 | 0.150481E+02 |
| 0.138400E+04 | 0.157778E+02 | 0.165012E+02 | 0.150543E+02 |
| 0.138500E+04 | 0.157819E+02 | 0.165043E+02 | 0.150595E+02 |
| 0.138600E+04 | 0.157852E+02 | 0.165066E+02 | 0.150638E+02 |
| 0.138700E+04 | 0.157449E+02 | 0.164583E+02 | 0.150316E+02 |
| 0.138800E+04 | 0.156993E+02 | 0.164031E+02 | 0.149955E+02 |
| 0.138900E+04 | 0.157147E+02 | 0.164210E+02 | 0.150084E+02 |
| 0.139000E+04 | 0.157435E+02 | 0.164554E+02 | 0.150316E+02 |
| 0.139100E+04 | 0.157590E+02 | 0.164736E+02 | 0.150443E+02 |
| 0.139200E+04 | 0.157671E+02 | 0.164825E+02 | 0.150516E+02 |
| 0.139300E+04 | 0.157720E+02 | 0.164876E+02 | 0.150564E+02 |
| 0.139400E+04 | 0.157751E+02 | 0.164904E+02 | 0.150597E+02 |
| 0.139500E+04 | 0.157769E+02 | 0.164918E+02 | 0.150620E+02 |
| 0.139600E+04 | 0.157773E+02 | 0.164916E+02 | 0.150631E+02 |
| 0.139700E+04 | 0.157762E+02 | 0.164896E+02 | 0.150628E+02 |
| 0.139800E+04 | 0.157739E+02 | 0.164864E+02 | 0.150615E+02 |
| 0.139900E+04 | 0.157709E+02 | 0.164823E+02 | 0.150594E+02 |
| 0.140000E+04 | 0.157671E+02 | 0.164775E+02 | 0.150567E+02 |
| 0.140100E+04 | 0.157635E+02 | 0.164729E+02 | 0.150541E+02 |
| 0.140200E+04 | 0.157606E+02 | 0.164693E+02 | 0.150520E+02 |
| 0.140300E+04 | 0.157582E+02 | 0.164663E+02 | 0.150501E+02 |
| 0.140400E+04 | 0.157559E+02 | 0.164635E+02 | 0.150484E+02 |
| 0.140500E+04 | 0.157537E+02 | 0.164607E+02 | 0.150467E+02 |
| 0.140600E+04 | 0.157516E+02 | 0.164581E+02 | 0.150451E+02 |
| 0.140700E+04 | 0.157494E+02 | 0.164554E+02 | 0.150435E+02 |
| 0.140800E+04 | 0.157081E+02 | 0.164067E+02 | 0.150094E+02 |
| 0.140900E+04 | 0.156755E+02 | 0.163786E+02 | 0.149725E+02 |
| 0.141000E+04 | 0.156913E+02 | 0.163894E+02 | 0.149932E+02 |
| 0.141100E+04 | 0.157153E+02 | 0.164161E+02 | 0.150146E+02 |
| 0.141200E+04 | 0.157268E+02 | 0.164294E+02 | 0.150242E+02 |
| 0.141300E+04 | 0.157318E+02 | 0.164352E+02 | 0.150284E+02 |
| 0.141400E+04 | 0.157296E+02 | 0.164381E+02 | 0.150210E+02 |
| 0.141500E+04 | 0.157280E+02 | 0.164384E+02 | 0.150176E+02 |
| 0.141600E+04 | 0.157259E+02 | 0.164361E+02 | 0.150158E+02 |
| 0.141700E+04 | 0.157228E+02 | 0.164319E+02 | 0.150137E+02 |
| 0.141800E+04 | 0.157188E+02 | 0.164266E+02 | 0.150111E+02 |
| 0.141900E+04 | 0.157141E+02 | 0.164204E+02 | 0.150079E+02 |
| 0.142000E+04 | 0.157089E+02 | 0.164136E+02 | 0.150042E+02 |
| 0.142100E+04 | 0.157039E+02 | 0.164070E+02 | 0.150007E+02 |
| 0.142200E+04 | 0.156996E+02 | 0.164015E+02 | 0.149977E+02 |
| 0.142300E+04 | 0.156958E+02 | 0.163965E+02 | 0.149950E+02 |
| 0.142400E+04 | 0.156922E+02 | 0.163918E+02 | 0.149925E+02 |
| 0.142500E+04 | 0.156888E+02 | 0.163876E+02 | 0.149901E+02 |
| 0.142600E+04 | 0.156855E+02 | 0.163834E+02 | 0.149876E+02 |

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| 0.142700E+04 | 0.156822E+02 | 0.163792E+02 | 0.149853E+02 |
| 0.142800E+04 | 0.156782E+02 | 0.163742E+02 | 0.149822E+02 |
| 0.142900E+04 | 0.156789E+02 | 0.163675E+02 | 0.149903E+02 |
| 0.143000E+04 | 0.156736E+02 | 0.163600E+02 | 0.149871E+02 |
| 0.143100E+04 | 0.156667E+02 | 0.163519E+02 | 0.149814E+02 |
| 0.143200E+04 | 0.156592E+02 | 0.163436E+02 | 0.149748E+02 |
| 0.143300E+04 | 0.156511E+02 | 0.163347E+02 | 0.149676E+02 |
| 0.143400E+04 | 0.156158E+02 | 0.162944E+02 | 0.149373E+02 |
| 0.143500E+04 | 0.155802E+02 | 0.162536E+02 | 0.149068E+02 |
| 0.143600E+04 | 0.155822E+02 | 0.162570E+02 | 0.149073E+02 |
| 0.143700E+04 | 0.155892E+02 | 0.162663E+02 | 0.149121E+02 |
| 0.143800E+04 | 0.155886E+02 | 0.162665E+02 | 0.149107E+02 |
| 0.143900E+04 | 0.155844E+02 | 0.162622E+02 | 0.149066E+02 |
| 0.144000E+04 | 0.155787E+02 | 0.162559E+02 | 0.149014E+02 |
| 0.144100E+04 | 0.155718E+02 | 0.162480E+02 | 0.148956E+02 |
| 0.144200E+04 | 0.155658E+02 | 0.162415E+02 | 0.148902E+02 |
| 0.144300E+04 | 0.155610E+02 | 0.162361E+02 | 0.148859E+02 |
| 0.144400E+04 | 0.155568E+02 | 0.162312E+02 | 0.148823E+02 |
| 0.144500E+04 | 0.155533E+02 | 0.162274E+02 | 0.148791E+02 |
| 0.144600E+04 | 0.155500E+02 | 0.162238E+02 | 0.148763E+02 |
| 0.144700E+04 | 0.155471E+02 | 0.162203E+02 | 0.148738E+02 |
| 0.144800E+04 | 0.155443E+02 | 0.162171E+02 | 0.148714E+02 |
| 0.144900E+04 | 0.155417E+02 | 0.162140E+02 | 0.148693E+02 |
| 0.145000E+04 | 0.155392E+02 | 0.162111E+02 | 0.148673E+02 |
| 0.145100E+04 | 0.155369E+02 | 0.162083E+02 | 0.148655E+02 |
| 0.145200E+04 | 0.155344E+02 | 0.162054E+02 | 0.148635E+02 |
| 0.145300E+04 | 0.153648E+02 | 0.160061E+02 | 0.147235E+02 |
| 0.145400E+04 | 0.153195E+02 | 0.157941E+02 | 0.148450E+02 |
| 0.145500E+04 | 0.154361E+02 | 0.158967E+02 | 0.149755E+02 |
| 0.145600E+04 | 0.155373E+02 | 0.160004E+02 | 0.150742E+02 |
| 0.145700E+04 | 0.155806E+02 | 0.160455E+02 | 0.151157E+02 |
| 0.145800E+04 | 0.156014E+02 | 0.160718E+02 | 0.151311E+02 |
| 0.145900E+04 | 0.154650E+02 | 0.159178E+02 | 0.150122E+02 |
| 0.146000E+04 | 0.152136E+02 | 0.156044E+02 | 0.148229E+02 |
| 0.146100E+04 | 0.151641E+02 | 0.155570E+02 | 0.147711E+02 |
| 0.146200E+04 | 0.152892E+02 | 0.157025E+02 | 0.148759E+02 |
| 0.146300E+04 | 0.154100E+02 | 0.158396E+02 | 0.149805E+02 |
| 0.146400E+04 | 0.154736E+02 | 0.159152E+02 | 0.150320E+02 |
| 0.146500E+04 | 0.155104E+02 | 0.159613E+02 | 0.150596E+02 |
| 0.146600E+04 | 0.154927E+02 | 0.159418E+02 | 0.150437E+02 |
| 0.146700E+04 | 0.154644E+02 | 0.159083E+02 | 0.150206E+02 |
| 0.146800E+04 | 0.154940E+02 | 0.159471E+02 | 0.150409E+02 |
| 0.146900E+04 | 0.155300E+02 | 0.159986E+02 | 0.150615E+02 |
| 0.147000E+04 | 0.155468E+02 | 0.160300E+02 | 0.150637E+02 |
| 0.147100E+04 | 0.155793E+02 | 0.160950E+02 | 0.150636E+02 |
| 0.147200E+04 | 0.155849E+02 | 0.161063E+02 | 0.150635E+02 |
| 0.147300E+04 | 0.155898E+02 | 0.161174E+02 | 0.150622E+02 |
| 0.147400E+04 | 0.155946E+02 | 0.161279E+02 | 0.150612E+02 |
| 0.147500E+04 | 0.155993E+02 | 0.161376E+02 | 0.150610E+02 |
| 0.147600E+04 | 0.156039E+02 | 0.161464E+02 | 0.150615E+02 |
| 0.147700E+04 | 0.156090E+02 | 0.161549E+02 | 0.150631E+02 |
| 0.147800E+04 | 0.156148E+02 | 0.161637E+02 | 0.150659E+02 |
| 0.147900E+04 | 0.156212E+02 | 0.161726E+02 | 0.150698E+02 |
| 0.148000E+04 | 0.156280E+02 | 0.161817E+02 | 0.150744E+02 |

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| 0.148100E+04 | 0.155971E+02 | 0.161451E+02 | 0.150490E+02 |
| 0.148200E+04 | 0.155858E+02 | 0.161388E+02 | 0.150329E+02 |
| 0.148300E+04 | 0.155628E+02 | 0.161126E+02 | 0.150129E+02 |
| 0.148400E+04 | 0.155432E+02 | 0.160886E+02 | 0.149978E+02 |
| 0.148500E+04 | 0.155828E+02 | 0.161357E+02 | 0.150299E+02 |
| 0.148600E+04 | 0.156093E+02 | 0.161681E+02 | 0.150505E+02 |
| 0.148700E+04 | 0.156222E+02 | 0.161836E+02 | 0.150608E+02 |
| 0.148800E+04 | 0.156306E+02 | 0.161932E+02 | 0.150679E+02 |
| 0.148900E+04 | 0.156365E+02 | 0.161998E+02 | 0.150733E+02 |
| 0.149000E+04 | 0.156410E+02 | 0.162044E+02 | 0.150776E+02 |
| 0.149100E+04 | 0.156445E+02 | 0.162079E+02 | 0.150811E+02 |
| 0.149200E+04 | 0.156472E+02 | 0.162104E+02 | 0.150840E+02 |
| 0.149300E+04 | 0.156494E+02 | 0.162123E+02 | 0.150864E+02 |
| 0.149400E+04 | 0.156511E+02 | 0.162138E+02 | 0.150885E+02 |
| 0.149500E+04 | 0.156076E+02 | 0.161618E+02 | 0.150535E+02 |
| 0.149600E+04 | 0.155622E+02 | 0.161069E+02 | 0.150174E+02 |
| 0.149700E+04 | 0.155793E+02 | 0.161269E+02 | 0.150317E+02 |
| 0.149800E+04 | 0.156076E+02 | 0.161619E+02 | 0.150533E+02 |
| 0.149900E+04 | 0.156224E+02 | 0.161805E+02 | 0.150643E+02 |
| 0.150000E+04 | 0.156306E+02 | 0.161907E+02 | 0.150704E+02 |
| 0.150100E+04 | 0.156361E+02 | 0.161976E+02 | 0.150746E+02 |
| 0.150200E+04 | 0.156403E+02 | 0.162028E+02 | 0.150778E+02 |
| 0.150300E+04 | 0.156435E+02 | 0.162067E+02 | 0.150804E+02 |
| 0.150400E+04 | 0.156085E+02 | 0.161654E+02 | 0.150517E+02 |
| 0.150500E+04 | 0.155694E+02 | 0.161186E+02 | 0.150201E+02 |
| 0.150600E+04 | 0.155831E+02 | 0.161352E+02 | 0.150310E+02 |
| 0.150700E+04 | 0.156089E+02 | 0.161672E+02 | 0.150506E+02 |
| 0.150800E+04 | 0.156231E+02 | 0.161853E+02 | 0.150609E+02 |
| 0.150900E+04 | 0.156312E+02 | 0.161957E+02 | 0.150667E+02 |
| 0.151000E+04 | 0.156370E+02 | 0.162032E+02 | 0.150708E+02 |
| 0.151100E+04 | 0.156414E+02 | 0.162089E+02 | 0.150739E+02 |
| 0.151200E+04 | 0.156446E+02 | 0.162131E+02 | 0.150761E+02 |
| 0.151300E+04 | 0.156465E+02 | 0.162158E+02 | 0.150773E+02 |
| 0.151400E+04 | 0.156476E+02 | 0.162174E+02 | 0.150777E+02 |
| 0.151500E+04 | 0.156480E+02 | 0.162183E+02 | 0.150777E+02 |
| 0.151600E+04 | 0.156479E+02 | 0.162186E+02 | 0.150772E+02 |
| 0.151700E+04 | 0.156474E+02 | 0.162184E+02 | 0.150763E+02 |
| 0.151800E+04 | 0.156466E+02 | 0.162179E+02 | 0.150752E+02 |
| 0.151900E+04 | 0.156458E+02 | 0.162175E+02 | 0.150740E+02 |
| 0.152000E+04 | 0.156452E+02 | 0.162174E+02 | 0.150730E+02 |
| 0.152100E+04 | 0.156448E+02 | 0.162175E+02 | 0.150721E+02 |
| 0.152200E+04 | 0.156444E+02 | 0.162177E+02 | 0.150711E+02 |
| 0.152300E+04 | 0.156440E+02 | 0.162179E+02 | 0.150702E+02 |
| 0.152400E+04 | 0.156436E+02 | 0.162181E+02 | 0.150692E+02 |
| 0.152500E+04 | 0.156432E+02 | 0.162183E+02 | 0.150682E+02 |
| 0.152600E+04 | 0.156428E+02 | 0.162185E+02 | 0.150671E+02 |
| 0.152700E+04 | 0.155372E+02 | 0.160940E+02 | 0.149803E+02 |
| 0.152800E+04 | 0.154258E+02 | 0.159622E+02 | 0.148893E+02 |
| 0.152900E+04 | 0.154593E+02 | 0.160037E+02 | 0.149149E+02 |
| 0.153000E+04 | 0.155211E+02 | 0.160789E+02 | 0.149633E+02 |
| 0.153100E+04 | 0.155452E+02 | 0.161057E+02 | 0.149848E+02 |
| 0.153200E+04 | 0.155586E+02 | 0.161225E+02 | 0.149946E+02 |
| 0.153300E+04 | 0.155672E+02 | 0.161339E+02 | 0.150006E+02 |
| 0.153400E+04 | 0.155731E+02 | 0.161418E+02 | 0.150044E+02 |

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| 0.153500E+04 | 0.155772E+02 | 0.161475E+02 | 0.150069E+02 |
| 0.153600E+04 | 0.155804E+02 | 0.161519E+02 | 0.150088E+02 |
| 0.153700E+04 | 0.155824E+02 | 0.161547E+02 | 0.150100E+02 |
| 0.153800E+04 | 0.155844E+02 | 0.161578E+02 | 0.150109E+02 |
| 0.153900E+04 | 0.155857E+02 | 0.161601E+02 | 0.150114E+02 |
| 0.154000E+04 | 0.155863E+02 | 0.161610E+02 | 0.150115E+02 |
| 0.154100E+04 | 0.155873E+02 | 0.161630E+02 | 0.150117E+02 |
| 0.154200E+04 | 0.155884E+02 | 0.161649E+02 | 0.150120E+02 |
| 0.154300E+04 | 0.155892E+02 | 0.161661E+02 | 0.150122E+02 |
| 0.154400E+04 | 0.155904E+02 | 0.161683E+02 | 0.150125E+02 |
| 0.154500E+04 | 0.155915E+02 | 0.161702E+02 | 0.150128E+02 |
| 0.154600E+04 | 0.155925E+02 | 0.161719E+02 | 0.150131E+02 |
| 0.154700E+04 | 0.155934E+02 | 0.161735E+02 | 0.150133E+02 |
| 0.154800E+04 | 0.155943E+02 | 0.161750E+02 | 0.150135E+02 |
| 0.154900E+04 | 0.155951E+02 | 0.161765E+02 | 0.150137E+02 |
| 0.155000E+04 | 0.155958E+02 | 0.161778E+02 | 0.150137E+02 |
| 0.155100E+04 | 0.155962E+02 | 0.161788E+02 | 0.150136E+02 |
| 0.155200E+04 | 0.155966E+02 | 0.161797E+02 | 0.150134E+02 |
| 0.155300E+04 | 0.155968E+02 | 0.161804E+02 | 0.150132E+02 |
| 0.155400E+04 | 0.155954E+02 | 0.161809E+02 | 0.150098E+02 |
| 0.155500E+04 | 0.155951E+02 | 0.161813E+02 | 0.150088E+02 |
| 0.155600E+04 | 0.155949E+02 | 0.161817E+02 | 0.150080E+02 |
| 0.155700E+04 | 0.155947E+02 | 0.161820E+02 | 0.150074E+02 |
| 0.155800E+04 | 0.155946E+02 | 0.161823E+02 | 0.150069E+02 |
| 0.155900E+04 | 0.155945E+02 | 0.161826E+02 | 0.150063E+02 |
| 0.156000E+04 | 0.155947E+02 | 0.161833E+02 | 0.150061E+02 |
| 0.156100E+04 | 0.155953E+02 | 0.161844E+02 | 0.150062E+02 |
| 0.156200E+04 | 0.155963E+02 | 0.161860E+02 | 0.150066E+02 |
| 0.156300E+04 | 0.155974E+02 | 0.161877E+02 | 0.150071E+02 |
| 0.156400E+04 | 0.155110E+02 | 0.160943E+02 | 0.149278E+02 |
| 0.156500E+04 | 0.154094E+02 | 0.159699E+02 | 0.148489E+02 |
| 0.156600E+04 | 0.154425E+02 | 0.160088E+02 | 0.148763E+02 |
| 0.156700E+04 | 0.155046E+02 | 0.160843E+02 | 0.149249E+02 |
| 0.156800E+04 | 0.155374E+02 | 0.161253E+02 | 0.149495E+02 |
| 0.156900E+04 | 0.155543E+02 | 0.161476E+02 | 0.149610E+02 |
| 0.157000E+04 | 0.155396E+02 | 0.161316E+02 | 0.149477E+02 |
| 0.157100E+04 | 0.155226E+02 | 0.161116E+02 | 0.149337E+02 |
| 0.157200E+04 | 0.155401E+02 | 0.161324E+02 | 0.149479E+02 |
| 0.157300E+04 | 0.155613E+02 | 0.161582E+02 | 0.149643E+02 |
| 0.157400E+04 | 0.155742E+02 | 0.161740E+02 | 0.149743E+02 |
| 0.157500E+04 | 0.155830E+02 | 0.161848E+02 | 0.149812E+02 |
| 0.157600E+04 | 0.155901E+02 | 0.161932E+02 | 0.149869E+02 |
| 0.157700E+04 | 0.155964E+02 | 0.162007E+02 | 0.149921E+02 |
| 0.157800E+04 | 0.156027E+02 | 0.162081E+02 | 0.149973E+02 |
| 0.157900E+04 | 0.156088E+02 | 0.162152E+02 | 0.150024E+02 |
| 0.158000E+04 | 0.156148E+02 | 0.162221E+02 | 0.150075E+02 |
| 0.158100E+04 | 0.156206E+02 | 0.162288E+02 | 0.150124E+02 |
| 0.158200E+04 | 0.156264E+02 | 0.162354E+02 | 0.150174E+02 |
| 0.158300E+04 | 0.156321E+02 | 0.162419E+02 | 0.150223E+02 |
| 0.158400E+04 | 0.156377E+02 | 0.162482E+02 | 0.150271E+02 |
| 0.158500E+04 | 0.156429E+02 | 0.162542E+02 | 0.150317E+02 |
| 0.158600E+04 | 0.156477E+02 | 0.162596E+02 | 0.150359E+02 |
| 0.158700E+04 | 0.154871E+02 | 0.160651E+02 | 0.149092E+02 |
| 0.158800E+04 | 0.152509E+02 | 0.157880E+02 | 0.147137E+02 |

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| 0.158900E+04 | 0.152486E+02 | 0.157976E+02 | 0.146996E+02 |
| 0.159000E+04 | 0.153508E+02 | 0.159154E+02 | 0.147863E+02 |
| 0.159100E+04 | 0.154407E+02 | 0.160158E+02 | 0.148657E+02 |
| 0.159200E+04 | 0.154879E+02 | 0.160683E+02 | 0.149075E+02 |
| 0.159300E+04 | 0.155184E+02 | 0.161039E+02 | 0.149329E+02 |
| 0.159400E+04 | 0.155419E+02 | 0.161315E+02 | 0.149523E+02 |
| 0.159500E+04 | 0.155616E+02 | 0.161546E+02 | 0.149686E+02 |
| 0.159600E+04 | 0.155788E+02 | 0.161747E+02 | 0.149829E+02 |
| 0.159700E+04 | 0.155940E+02 | 0.161924E+02 | 0.149957E+02 |
| 0.159800E+04 | 0.156078E+02 | 0.162083E+02 | 0.150074E+02 |
| 0.159900E+04 | 0.156174E+02 | 0.162228E+02 | 0.150121E+02 |
| 0.160000E+04 | 0.156392E+02 | 0.162773E+02 | 0.150011E+02 |
| 0.160100E+04 | 0.154486E+02 | 0.160671E+02 | 0.148300E+02 |
| 0.160200E+04 | 0.152501E+02 | 0.158072E+02 | 0.146930E+02 |
| 0.160300E+04 | 0.153277E+02 | 0.159076E+02 | 0.147479E+02 |
| 0.160400E+04 | 0.154305E+02 | 0.160211E+02 | 0.148398E+02 |
| 0.160500E+04 | 0.155049E+02 | 0.161033E+02 | 0.149066E+02 |
| 0.160600E+04 | 0.155498E+02 | 0.161548E+02 | 0.149448E+02 |
| 0.160700E+04 | 0.155790E+02 | 0.161881E+02 | 0.149700E+02 |
| 0.160800E+04 | 0.156009E+02 | 0.162127E+02 | 0.149890E+02 |
| 0.160900E+04 | 0.156182E+02 | 0.162320E+02 | 0.150044E+02 |
| 0.161000E+04 | 0.156313E+02 | 0.162476E+02 | 0.150150E+02 |
| 0.161100E+04 | 0.156314E+02 | 0.162607E+02 | 0.150021E+02 |
| 0.161200E+04 | 0.155915E+02 | 0.162714E+02 | 0.149115E+02 |
| 0.161300E+04 | 0.154522E+02 | 0.161522E+02 | 0.147522E+02 |
| 0.161400E+04 | 0.153279E+02 | 0.160461E+02 | 0.146098E+02 |
| 0.161500E+04 | 0.153687E+02 | 0.160909E+02 | 0.146466E+02 |
| 0.161600E+04 | 0.154274E+02 | 0.161655E+02 | 0.146894E+02 |
| 0.161700E+04 | 0.154562E+02 | 0.162047E+02 | 0.147077E+02 |
| 0.161800E+04 | 0.154727E+02 | 0.162277E+02 | 0.147178E+02 |
| 0.161900E+04 | 0.154856E+02 | 0.162438E+02 | 0.147275E+02 |
| 0.162000E+04 | 0.154970E+02 | 0.162559E+02 | 0.147381E+02 |
| 0.162100E+04 | 0.155077E+02 | 0.162657E+02 | 0.147497E+02 |
| 0.162200E+04 | 0.153965E+02 | 0.161321E+02 | 0.146609E+02 |
| 0.162300E+04 | 0.152880E+02 | 0.160132E+02 | 0.145628E+02 |
| 0.162400E+04 | 0.153391E+02 | 0.160663E+02 | 0.146120E+02 |
| 0.162500E+04 | 0.154177E+02 | 0.161510E+02 | 0.146845E+02 |
| 0.162600E+04 | 0.154616E+02 | 0.161970E+02 | 0.147262E+02 |
| 0.162700E+04 | 0.154892E+02 | 0.162248E+02 | 0.147536E+02 |
| 0.162800E+04 | 0.155104E+02 | 0.162450E+02 | 0.147759E+02 |
| 0.162900E+04 | 0.155279E+02 | 0.162608E+02 | 0.147951E+02 |
| 0.163000E+04 | 0.155429E+02 | 0.162737E+02 | 0.148121E+02 |
| 0.163100E+04 | 0.155560E+02 | 0.162846E+02 | 0.148273E+02 |
| 0.163200E+04 | 0.155668E+02 | 0.162932E+02 | 0.148404E+02 |
| 0.163300E+04 | 0.155732E+02 | 0.162989E+02 | 0.148474E+02 |
| 0.163400E+04 | 0.155760E+02 | 0.163026E+02 | 0.148494E+02 |
| 0.163500E+04 | 0.155782E+02 | 0.163048E+02 | 0.148516E+02 |
| 0.163600E+04 | 0.155799E+02 | 0.163060E+02 | 0.148538E+02 |
| 0.163700E+04 | 0.155812E+02 | 0.163064E+02 | 0.148560E+02 |
| 0.163800E+04 | 0.155821E+02 | 0.163062E+02 | 0.148580E+02 |
| 0.163900E+04 | 0.155828E+02 | 0.163058E+02 | 0.148598E+02 |
| 0.164000E+04 | 0.155836E+02 | 0.163055E+02 | 0.148617E+02 |
| 0.164100E+04 | 0.153829E+02 | 0.160814E+02 | 0.146844E+02 |
| 0.164200E+04 | 0.151608E+02 | 0.158185E+02 | 0.145032E+02 |

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| 0.164300E+04 | 0.153669E+02 | 0.159113E+02 | 0.148225E+02 |
| 0.164400E+04 | 0.155055E+02 | 0.160204E+02 | 0.149907E+02 |
| 0.164500E+04 | 0.155739E+02 | 0.160860E+02 | 0.150619E+02 |
| 0.164600E+04 | 0.156051E+02 | 0.161200E+02 | 0.150903E+02 |
| 0.164700E+04 | 0.156373E+02 | 0.161604E+02 | 0.151142E+02 |
| 0.164800E+04 | 0.156620E+02 | 0.161950E+02 | 0.151291E+02 |
| 0.164900E+04 | 0.156765E+02 | 0.162182E+02 | 0.151347E+02 |
| 0.165000E+04 | 0.156849E+02 | 0.162345E+02 | 0.151354E+02 |
| 0.165100E+04 | 0.156898E+02 | 0.162463E+02 | 0.151332E+02 |
| 0.165200E+04 | 0.156922E+02 | 0.162549E+02 | 0.151295E+02 |
| 0.165300E+04 | 0.156926E+02 | 0.162607E+02 | 0.151245E+02 |
| 0.165400E+04 | 0.156912E+02 | 0.162638E+02 | 0.151187E+02 |
| 0.165500E+04 | 0.156885E+02 | 0.162648E+02 | 0.151122E+02 |
| 0.165600E+04 | 0.156849E+02 | 0.162643E+02 | 0.151055E+02 |
| 0.165700E+04 | 0.156806E+02 | 0.162626E+02 | 0.150986E+02 |
| 0.165800E+04 | 0.156756E+02 | 0.162597E+02 | 0.150916E+02 |
| 0.165900E+04 | 0.156703E+02 | 0.162560E+02 | 0.150845E+02 |
| 0.166000E+04 | 0.156652E+02 | 0.162524E+02 | 0.150780E+02 |
| 0.166100E+04 | 0.156610E+02 | 0.162496E+02 | 0.150725E+02 |
| 0.166200E+04 | 0.156574E+02 | 0.162472E+02 | 0.150676E+02 |
| 0.166300E+04 | 0.156541E+02 | 0.162450E+02 | 0.150631E+02 |
| 0.166400E+04 | 0.156509E+02 | 0.162428E+02 | 0.150590E+02 |
| 0.166500E+04 | 0.156480E+02 | 0.162407E+02 | 0.150553E+02 |
| 0.166600E+04 | 0.156451E+02 | 0.162385E+02 | 0.150518E+02 |
| 0.166700E+04 | 0.155654E+02 | 0.161508E+02 | 0.149799E+02 |
| 0.166800E+04 | 0.154658E+02 | 0.160323E+02 | 0.148994E+02 |
| 0.166900E+04 | 0.154930E+02 | 0.160649E+02 | 0.149212E+02 |
| 0.167000E+04 | 0.155456E+02 | 0.161297E+02 | 0.149615E+02 |
| 0.167100E+04 | 0.155711E+02 | 0.161624E+02 | 0.149799E+02 |
| 0.167200E+04 | 0.155829E+02 | 0.161777E+02 | 0.149881E+02 |
| 0.167300E+04 | 0.155895E+02 | 0.161862E+02 | 0.149927E+02 |
| 0.167400E+04 | 0.154658E+02 | 0.160401E+02 | 0.148914E+02 |
| 0.167500E+04 | 0.153303E+02 | 0.158814E+02 | 0.147792E+02 |
| 0.167600E+04 | 0.153742E+02 | 0.159317E+02 | 0.148167E+02 |
| 0.167700E+04 | 0.154485E+02 | 0.160218E+02 | 0.148752E+02 |
| 0.167800E+04 | 0.154803E+02 | 0.160579E+02 | 0.149028E+02 |
| 0.167900E+04 | 0.154982E+02 | 0.160794E+02 | 0.149169E+02 |
| 0.168000E+04 | 0.155103E+02 | 0.160942E+02 | 0.149264E+02 |
| 0.168100E+04 | 0.154120E+02 | 0.159768E+02 | 0.148473E+02 |
| 0.168200E+04 | 0.153189E+02 | 0.158737E+02 | 0.147641E+02 |
| 0.168300E+04 | 0.153591E+02 | 0.159208E+02 | 0.147975E+02 |
| 0.168400E+04 | 0.154256E+02 | 0.160015E+02 | 0.148498E+02 |
| 0.168500E+04 | 0.154535E+02 | 0.160315E+02 | 0.148754E+02 |
| 0.168600E+04 | 0.154697E+02 | 0.160504E+02 | 0.148890E+02 |
| 0.168700E+04 | 0.154808E+02 | 0.160634E+02 | 0.148982E+02 |
| 0.168800E+04 | 0.154887E+02 | 0.160725E+02 | 0.149050E+02 |
| 0.168900E+04 | 0.154664E+02 | 0.160452E+02 | 0.148876E+02 |
| 0.169000E+04 | 0.154520E+02 | 0.160268E+02 | 0.148772E+02 |
| 0.169100E+04 | 0.154744E+02 | 0.160532E+02 | 0.148956E+02 |
| 0.169200E+04 | 0.154909E+02 | 0.160734E+02 | 0.149084E+02 |
| 0.169300E+04 | 0.155000E+02 | 0.160840E+02 | 0.149160E+02 |
| 0.169400E+04 | 0.155066E+02 | 0.160915E+02 | 0.149218E+02 |
| 0.169500E+04 | 0.153911E+02 | 0.159625E+02 | 0.148198E+02 |
| 0.169600E+04 | 0.152530E+02 | 0.157988E+02 | 0.147072E+02 |

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| 0.169700E+04 | 0.153029E+02 | 0.158552E+02 | 0.147505E+02 |
| 0.169800E+04 | 0.153865E+02 | 0.159574E+02 | 0.148156E+02 |
| 0.169900E+04 | 0.154243E+02 | 0.160005E+02 | 0.148481E+02 |
| 0.170000E+04 | 0.154470E+02 | 0.160275E+02 | 0.148664E+02 |
| 0.170100E+04 | 0.154634E+02 | 0.160470E+02 | 0.148798E+02 |
| 0.170200E+04 | 0.154761E+02 | 0.160618E+02 | 0.148905E+02 |
| 0.170300E+04 | 0.154864E+02 | 0.160735E+02 | 0.148994E+02 |
| 0.170400E+04 | 0.154950E+02 | 0.160830E+02 | 0.149071E+02 |
| 0.170500E+04 | 0.155024E+02 | 0.160910E+02 | 0.149138E+02 |
| 0.170600E+04 | 0.155090E+02 | 0.160980E+02 | 0.149200E+02 |
| 0.170700E+04 | 0.155151E+02 | 0.161045E+02 | 0.149258E+02 |
| 0.170800E+04 | 0.155210E+02 | 0.161107E+02 | 0.149313E+02 |
| 0.170900E+04 | 0.155265E+02 | 0.161165E+02 | 0.149365E+02 |
| 0.171000E+04 | 0.155317E+02 | 0.161221E+02 | 0.149414E+02 |
| 0.171100E+04 | 0.155368E+02 | 0.161274E+02 | 0.149461E+02 |
| 0.171200E+04 | 0.155416E+02 | 0.161326E+02 | 0.149507E+02 |
| 0.171300E+04 | 0.155465E+02 | 0.161379E+02 | 0.149552E+02 |
| 0.171400E+04 | 0.155516E+02 | 0.161434E+02 | 0.149598E+02 |
| 0.171500E+04 | 0.155568E+02 | 0.161491E+02 | 0.149645E+02 |
| 0.171600E+04 | 0.155621E+02 | 0.161550E+02 | 0.149692E+02 |
| 0.171700E+04 | 0.155674E+02 | 0.161609E+02 | 0.149738E+02 |
| 0.171800E+04 | 0.155727E+02 | 0.161669E+02 | 0.149785E+02 |
| 0.171900E+04 | 0.155781E+02 | 0.161730E+02 | 0.149832E+02 |
| 0.172000E+04 | 0.155717E+02 | 0.161791E+02 | 0.149643E+02 |
| 0.172100E+04 | 0.155666E+02 | 0.161861E+02 | 0.149472E+02 |
| 0.172200E+04 | 0.155081E+02 | 0.161946E+02 | 0.148217E+02 |
| 0.172300E+04 | 0.154858E+02 | 0.162031E+02 | 0.147685E+02 |
| 0.172400E+04 | 0.154740E+02 | 0.162115E+02 | 0.147366E+02 |
| 0.172500E+04 | 0.154687E+02 | 0.162198E+02 | 0.147175E+02 |
| 0.172600E+04 | 0.154663E+02 | 0.162284E+02 | 0.147042E+02 |
| 0.172700E+04 | 0.154694E+02 | 0.162372E+02 | 0.147016E+02 |
| 0.172800E+04 | 0.154750E+02 | 0.162454E+02 | 0.147046E+02 |
| 0.172900E+04 | 0.153625E+02 | 0.161226E+02 | 0.146023E+02 |
| 0.173000E+04 | 0.152918E+02 | 0.160248E+02 | 0.145589E+02 |
| 0.173100E+04 | 0.153823E+02 | 0.161184E+02 | 0.146463E+02 |
| 0.173200E+04 | 0.154383E+02 | 0.161748E+02 | 0.147018E+02 |
| 0.173300E+04 | 0.154680E+02 | 0.162052E+02 | 0.147309E+02 |
| 0.173400E+04 | 0.154904E+02 | 0.162274E+02 | 0.147533E+02 |
| 0.173500E+04 | 0.155091E+02 | 0.162452E+02 | 0.147729E+02 |
| 0.173600E+04 | 0.155263E+02 | 0.162612E+02 | 0.147914E+02 |
| 0.173700E+04 | 0.155433E+02 | 0.162770E+02 | 0.148097E+02 |
| 0.173800E+04 | 0.155601E+02 | 0.162925E+02 | 0.148277E+02 |
| 0.173900E+04 | 0.155257E+02 | 0.162484E+02 | 0.148030E+02 |
| 0.174000E+04 | 0.155039E+02 | 0.162188E+02 | 0.147891E+02 |
| 0.174100E+04 | 0.155467E+02 | 0.162658E+02 | 0.148277E+02 |
| 0.174200E+04 | 0.155780E+02 | 0.163026E+02 | 0.148534E+02 |
| 0.174300E+04 | 0.155946E+02 | 0.163232E+02 | 0.148660E+02 |
| 0.174400E+04 | 0.156077E+02 | 0.163386E+02 | 0.148768E+02 |
| 0.174500E+04 | 0.156193E+02 | 0.163514E+02 | 0.148871E+02 |
| 0.174600E+04 | 0.156298E+02 | 0.163626E+02 | 0.148971E+02 |
| 0.174700E+04 | 0.156396E+02 | 0.163726E+02 | 0.149066E+02 |
| 0.174800E+04 | 0.156489E+02 | 0.163819E+02 | 0.149159E+02 |
| 0.174900E+04 | 0.156576E+02 | 0.163905E+02 | 0.149246E+02 |
| 0.175000E+04 | 0.156658E+02 | 0.163986E+02 | 0.149330E+02 |

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| 0.175100E+04 | 0.156737E+02 | 0.164064E+02 | 0.149410E+02 |
| 0.175200E+04 | 0.156813E+02 | 0.164139E+02 | 0.149487E+02 |
| 0.175300E+04 | 0.156885E+02 | 0.164211E+02 | 0.149560E+02 |
| 0.175400E+04 | 0.156955E+02 | 0.164281E+02 | 0.149629E+02 |
| 0.175500E+04 | 0.157023E+02 | 0.164349E+02 | 0.149696E+02 |
| 0.175600E+04 | 0.157087E+02 | 0.164415E+02 | 0.149760E+02 |
| 0.175700E+04 | 0.157148E+02 | 0.164476E+02 | 0.149819E+02 |
| 0.175800E+04 | 0.157205E+02 | 0.164535E+02 | 0.149875E+02 |
| 0.175900E+04 | 0.157260E+02 | 0.164591E+02 | 0.149928E+02 |
| 0.176000E+04 | 0.157313E+02 | 0.164646E+02 | 0.149980E+02 |
| 0.176100E+04 | 0.157364E+02 | 0.164699E+02 | 0.150029E+02 |
| 0.176200E+04 | 0.157414E+02 | 0.164751E+02 | 0.150076E+02 |
| 0.176300E+04 | 0.157462E+02 | 0.164802E+02 | 0.150122E+02 |
| 0.176400E+04 | 0.157506E+02 | 0.164847E+02 | 0.150164E+02 |
| 0.176500E+04 | 0.157541E+02 | 0.164883E+02 | 0.150199E+02 |
| 0.176600E+04 | 0.157571E+02 | 0.164914E+02 | 0.150229E+02 |
| 0.176700E+04 | 0.157599E+02 | 0.164941E+02 | 0.150256E+02 |
| 0.176800E+04 | 0.157624E+02 | 0.164965E+02 | 0.150282E+02 |
| 0.176900E+04 | 0.157646E+02 | 0.164988E+02 | 0.150305E+02 |
| 0.177000E+04 | 0.157667E+02 | 0.165008E+02 | 0.150326E+02 |
| 0.177100E+04 | 0.157687E+02 | 0.165027E+02 | 0.150346E+02 |
| 0.177200E+04 | 0.157699E+02 | 0.165037E+02 | 0.150360E+02 |
| 0.177300E+04 | 0.157698E+02 | 0.165034E+02 | 0.150363E+02 |
| 0.177400E+04 | 0.157690E+02 | 0.165020E+02 | 0.150360E+02 |
| 0.177500E+04 | 0.157676E+02 | 0.165001E+02 | 0.150351E+02 |
| 0.177600E+04 | 0.157658E+02 | 0.164977E+02 | 0.150339E+02 |
| 0.177700E+04 | 0.157636E+02 | 0.164949E+02 | 0.150323E+02 |
| 0.177800E+04 | 0.157610E+02 | 0.164917E+02 | 0.150304E+02 |
| 0.177900E+04 | 0.157583E+02 | 0.164883E+02 | 0.150283E+02 |
| 0.178000E+04 | 0.157559E+02 | 0.164853E+02 | 0.150264E+02 |
| 0.178100E+04 | 0.157543E+02 | 0.164833E+02 | 0.150253E+02 |
| 0.178200E+04 | 0.157533E+02 | 0.164820E+02 | 0.150246E+02 |
| 0.178300E+04 | 0.156334E+02 | 0.163519E+02 | 0.149150E+02 |
| 0.178400E+04 | 0.155417E+02 | 0.162459E+02 | 0.148376E+02 |
| 0.178500E+04 | 0.156190E+02 | 0.163322E+02 | 0.149058E+02 |
| 0.178600E+04 | 0.156622E+02 | 0.163816E+02 | 0.149428E+02 |
| 0.178700E+04 | 0.156803E+02 | 0.164026E+02 | 0.149580E+02 |
| 0.178800E+04 | 0.156926E+02 | 0.164167E+02 | 0.149685E+02 |
| 0.178900E+04 | 0.156547E+02 | 0.163720E+02 | 0.149375E+02 |
| 0.179000E+04 | 0.156117E+02 | 0.163208E+02 | 0.149025E+02 |
| 0.179100E+04 | 0.156331E+02 | 0.163458E+02 | 0.149203E+02 |
| 0.179200E+04 | 0.156662E+02 | 0.163851E+02 | 0.149473E+02 |
| 0.179300E+04 | 0.156849E+02 | 0.164069E+02 | 0.149629E+02 |
| 0.179400E+04 | 0.156960E+02 | 0.164192E+02 | 0.149727E+02 |
| 0.179500E+04 | 0.157041E+02 | 0.164278E+02 | 0.149803E+02 |
| 0.179600E+04 | 0.157104E+02 | 0.164342E+02 | 0.149866E+02 |
| 0.179700E+04 | 0.157155E+02 | 0.164391E+02 | 0.149918E+02 |
| 0.179800E+04 | 0.157196E+02 | 0.164430E+02 | 0.149963E+02 |
| 0.179900E+04 | 0.157231E+02 | 0.164460E+02 | 0.150002E+02 |
| 0.180000E+04 | 0.157260E+02 | 0.164485E+02 | 0.150036E+02 |
| 0.180100E+04 | 0.157285E+02 | 0.164504E+02 | 0.150067E+02 |
| 0.180200E+04 | 0.157306E+02 | 0.164519E+02 | 0.150094E+02 |
| 0.180300E+04 | 0.157324E+02 | 0.164531E+02 | 0.150118E+02 |
| 0.180400E+04 | 0.157340E+02 | 0.164540E+02 | 0.150139E+02 |

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| 0.180500E+04 | 0.157353E+02 | 0.164547E+02 | 0.150159E+02 |
| 0.180600E+04 | 0.157365E+02 | 0.164552E+02 | 0.150177E+02 |
| 0.180700E+04 | 0.157375E+02 | 0.164556E+02 | 0.150194E+02 |
| 0.180800E+04 | 0.157384E+02 | 0.164558E+02 | 0.150210E+02 |
| 0.180900E+04 | 0.155422E+02 | 0.162337E+02 | 0.148508E+02 |
| 0.181000E+04 | 0.153184E+02 | 0.159712E+02 | 0.146656E+02 |
| 0.181100E+04 | 0.155004E+02 | 0.160637E+02 | 0.149371E+02 |
| 0.181200E+04 | 0.156427E+02 | 0.161703E+02 | 0.151151E+02 |
| 0.181300E+04 | 0.157315E+02 | 0.162517E+02 | 0.152114E+02 |
| 0.181400E+04 | 0.157770E+02 | 0.163020E+02 | 0.152519E+02 |
| 0.181500E+04 | 0.155874E+02 | 0.160790E+02 | 0.150958E+02 |
| 0.181600E+04 | 0.153670E+02 | 0.158212E+02 | 0.149128E+02 |
| 0.181700E+04 | 0.154459E+02 | 0.159315E+02 | 0.149603E+02 |
| 0.181800E+04 | 0.155600E+02 | 0.160674E+02 | 0.150526E+02 |
| 0.181900E+04 | 0.156354E+02 | 0.161548E+02 | 0.151161E+02 |
| 0.182000E+04 | 0.156802E+02 | 0.162126E+02 | 0.151478E+02 |
| 0.182100E+04 | 0.157076E+02 | 0.162499E+02 | 0.151652E+02 |
| 0.182200E+04 | 0.157268E+02 | 0.162771E+02 | 0.151766E+02 |
| 0.182300E+04 | 0.157411E+02 | 0.162977E+02 | 0.151844E+02 |
| 0.182400E+04 | 0.157515E+02 | 0.163133E+02 | 0.151898E+02 |
| 0.182500E+04 | 0.157590E+02 | 0.163248E+02 | 0.151933E+02 |
| 0.182600E+04 | 0.157644E+02 | 0.163333E+02 | 0.151955E+02 |
| 0.182700E+04 | 0.157903E+02 | 0.163808E+02 | 0.151999E+02 |
| 0.182800E+04 | 0.157910E+02 | 0.163802E+02 | 0.152018E+02 |
| 0.182900E+04 | 0.157915E+02 | 0.163806E+02 | 0.152024E+02 |
| 0.183000E+04 | 0.157469E+02 | 0.163273E+02 | 0.151666E+02 |
| 0.183100E+04 | 0.155480E+02 | 0.160997E+02 | 0.149963E+02 |
| 0.183200E+04 | 0.154008E+02 | 0.159421E+02 | 0.148595E+02 |
| 0.183300E+04 | 0.154870E+02 | 0.160421E+02 | 0.149320E+02 |
| 0.183400E+04 | 0.155980E+02 | 0.161671E+02 | 0.150288E+02 |
| 0.183500E+04 | 0.155429E+02 | 0.160988E+02 | 0.149871E+02 |
| 0.183600E+04 | 0.154657E+02 | 0.160115E+02 | 0.149200E+02 |
| 0.183700E+04 | 0.155285E+02 | 0.160857E+02 | 0.149713E+02 |
| 0.183800E+04 | 0.156098E+02 | 0.161788E+02 | 0.150409E+02 |
| 0.183900E+04 | 0.156530E+02 | 0.162258E+02 | 0.150801E+02 |
| 0.184000E+04 | 0.156445E+02 | 0.162138E+02 | 0.150752E+02 |
| 0.184100E+04 | 0.156254E+02 | 0.161886E+02 | 0.150623E+02 |
| 0.184200E+04 | 0.156574E+02 | 0.162278E+02 | 0.150869E+02 |
| 0.184300E+04 | 0.156305E+02 | 0.162097E+02 | 0.150513E+02 |
| 0.184400E+04 | 0.156200E+02 | 0.162337E+02 | 0.150063E+02 |
| 0.184500E+04 | 0.156095E+02 | 0.162705E+02 | 0.149484E+02 |
| 0.184600E+04 | 0.156160E+02 | 0.163226E+02 | 0.149095E+02 |
| 0.184700E+04 | 0.156185E+02 | 0.163466E+02 | 0.148904E+02 |
| 0.184800E+04 | 0.156213E+02 | 0.163649E+02 | 0.148777E+02 |
| 0.184900E+04 | 0.156257E+02 | 0.163790E+02 | 0.148723E+02 |
| 0.185000E+04 | 0.156293E+02 | 0.163901E+02 | 0.148686E+02 |
| 0.185100E+04 | 0.156359E+02 | 0.163991E+02 | 0.148728E+02 |
| 0.185200E+04 | 0.156437E+02 | 0.164065E+02 | 0.148808E+02 |
| 0.185300E+04 | 0.156523E+02 | 0.164130E+02 | 0.148915E+02 |
| 0.185400E+04 | 0.156302E+02 | 0.163826E+02 | 0.148778E+02 |
| 0.185500E+04 | 0.156170E+02 | 0.163616E+02 | 0.148724E+02 |
| 0.185600E+04 | 0.156450E+02 | 0.163891E+02 | 0.149009E+02 |
| 0.185700E+04 | 0.156683E+02 | 0.164112E+02 | 0.149253E+02 |
| 0.185800E+04 | 0.156838E+02 | 0.164238E+02 | 0.149437E+02 |

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| 0.185900E+04 | 0.156966E+02 | 0.164333E+02 | 0.149599E+02 |
| 0.186000E+04 | 0.157079E+02 | 0.164412E+02 | 0.149747E+02 |
| 0.186100E+04 | 0.157185E+02 | 0.164485E+02 | 0.149885E+02 |
| 0.186200E+04 | 0.157285E+02 | 0.164555E+02 | 0.150015E+02 |
| 0.186300E+04 | 0.157381E+02 | 0.164624E+02 | 0.150137E+02 |
| 0.186400E+04 | 0.157472E+02 | 0.164692E+02 | 0.150252E+02 |
| 0.186500E+04 | 0.157559E+02 | 0.164759E+02 | 0.150360E+02 |
| 0.186600E+04 | 0.157641E+02 | 0.164822E+02 | 0.150459E+02 |
| 0.186700E+04 | 0.157716E+02 | 0.164882E+02 | 0.150550E+02 |
| 0.186800E+04 | 0.157787E+02 | 0.164939E+02 | 0.150634E+02 |
| 0.186900E+04 | 0.157854E+02 | 0.164996E+02 | 0.150712E+02 |
| 0.187000E+04 | 0.157918E+02 | 0.165051E+02 | 0.150784E+02 |
| 0.187100E+04 | 0.157979E+02 | 0.165105E+02 | 0.150852E+02 |
| 0.187200E+04 | 0.158036E+02 | 0.165158E+02 | 0.150914E+02 |
| 0.187300E+04 | 0.158090E+02 | 0.165209E+02 | 0.150972E+02 |
| 0.187400E+04 | 0.158142E+02 | 0.165259E+02 | 0.151026E+02 |
| 0.187500E+04 | 0.158192E+02 | 0.165308E+02 | 0.151076E+02 |
| 0.187600E+04 | 0.158241E+02 | 0.165358E+02 | 0.151124E+02 |
| 0.187700E+04 | 0.158289E+02 | 0.165408E+02 | 0.151170E+02 |
| 0.187800E+04 | 0.158336E+02 | 0.165458E+02 | 0.151215E+02 |
| 0.187900E+04 | 0.158382E+02 | 0.165508E+02 | 0.151257E+02 |
| 0.188000E+04 | 0.158426E+02 | 0.165556E+02 | 0.151297E+02 |
| 0.188100E+04 | 0.158468E+02 | 0.165602E+02 | 0.151334E+02 |
| 0.188200E+04 | 0.158507E+02 | 0.165646E+02 | 0.151368E+02 |
| 0.188300E+04 | 0.157210E+02 | 0.164220E+02 | 0.150200E+02 |
| 0.188400E+04 | 0.155719E+02 | 0.162501E+02 | 0.148936E+02 |
| 0.188500E+04 | 0.156195E+02 | 0.163059E+02 | 0.149330E+02 |
| 0.188600E+04 | 0.157060E+02 | 0.164062E+02 | 0.150057E+02 |
| 0.188700E+04 | 0.157493E+02 | 0.164560E+02 | 0.150426E+02 |
| 0.188800E+04 | 0.157738E+02 | 0.164846E+02 | 0.150630E+02 |
| 0.188900E+04 | 0.157911E+02 | 0.165045E+02 | 0.150776E+02 |
| 0.189000E+04 | 0.158041E+02 | 0.165194E+02 | 0.150888E+02 |
| 0.189100E+04 | 0.158142E+02 | 0.165308E+02 | 0.150977E+02 |
| 0.189200E+04 | 0.158220E+02 | 0.165394E+02 | 0.151047E+02 |
| 0.189300E+04 | 0.158284E+02 | 0.165464E+02 | 0.151104E+02 |
| 0.189400E+04 | 0.158339E+02 | 0.165524E+02 | 0.151153E+02 |
| 0.189500E+04 | 0.158387E+02 | 0.165578E+02 | 0.151196E+02 |
| 0.189600E+04 | 0.158429E+02 | 0.165624E+02 | 0.151234E+02 |
| 0.189700E+04 | 0.158466E+02 | 0.165666E+02 | 0.151267E+02 |
| 0.189800E+04 | 0.158499E+02 | 0.165702E+02 | 0.151296E+02 |
| 0.189900E+04 | 0.158528E+02 | 0.165735E+02 | 0.151321E+02 |
| 0.190000E+04 | 0.158556E+02 | 0.165766E+02 | 0.151346E+02 |
| 0.190100E+04 | 0.158581E+02 | 0.165795E+02 | 0.151368E+02 |
| 0.190200E+04 | 0.157342E+02 | 0.164393E+02 | 0.150291E+02 |
| 0.190300E+04 | 0.155866E+02 | 0.162729E+02 | 0.149003E+02 |
| 0.190400E+04 | 0.156364E+02 | 0.163265E+02 | 0.149464E+02 |
| 0.190500E+04 | 0.157209E+02 | 0.164212E+02 | 0.150206E+02 |
| 0.190600E+04 | 0.157591E+02 | 0.164660E+02 | 0.150521E+02 |
| 0.190700E+04 | 0.157628E+02 | 0.164753E+02 | 0.150504E+02 |
| 0.190800E+04 | 0.157669E+02 | 0.164809E+02 | 0.150528E+02 |
| 0.190900E+04 | 0.157869E+02 | 0.165043E+02 | 0.150695E+02 |
| 0.191000E+04 | 0.158024E+02 | 0.165223E+02 | 0.150826E+02 |
| 0.191100E+04 | 0.158133E+02 | 0.165343E+02 | 0.150922E+02 |
| 0.191200E+04 | 0.157415E+02 | 0.164498E+02 | 0.150331E+02 |

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| 0.191300E+04 | 0.156730E+02 | 0.163787E+02 | 0.149673E+02 |
| 0.191400E+04 | 0.156750E+02 | 0.163807E+02 | 0.149694E+02 |
| 0.191500E+04 | 0.157107E+02 | 0.164221E+02 | 0.149994E+02 |
| 0.191600E+04 | 0.157579E+02 | 0.164736E+02 | 0.150423E+02 |
| 0.191700E+04 | 0.157884E+02 | 0.165069E+02 | 0.150698E+02 |
| 0.191800E+04 | 0.158088E+02 | 0.165296E+02 | 0.150881E+02 |
| 0.191900E+04 | 0.158250E+02 | 0.165473E+02 | 0.151028E+02 |
| 0.192000E+04 | 0.158386E+02 | 0.165618E+02 | 0.151153E+02 |
| 0.192100E+04 | 0.158508E+02 | 0.165748E+02 | 0.151269E+02 |
| 0.192200E+04 | 0.158628E+02 | 0.165874E+02 | 0.151381E+02 |
| 0.192300E+04 | 0.158743E+02 | 0.165996E+02 | 0.151491E+02 |
| 0.192400E+04 | 0.158345E+02 | 0.165518E+02 | 0.151172E+02 |
| 0.192500E+04 | 0.157885E+02 | 0.164966E+02 | 0.150803E+02 |
| 0.192600E+04 | 0.158139E+02 | 0.165257E+02 | 0.151020E+02 |
| 0.192700E+04 | 0.158561E+02 | 0.165755E+02 | 0.151366E+02 |
| 0.192800E+04 | 0.158823E+02 | 0.166060E+02 | 0.151587E+02 |
| 0.192900E+04 | 0.158999E+02 | 0.166255E+02 | 0.151742E+02 |
| 0.193000E+04 | 0.159137E+02 | 0.166405E+02 | 0.151870E+02 |
| 0.193100E+04 | 0.159263E+02 | 0.166538E+02 | 0.151989E+02 |
| 0.193200E+04 | 0.159388E+02 | 0.166669E+02 | 0.152107E+02 |
| 0.193300E+04 | 0.159511E+02 | 0.166799E+02 | 0.152224E+02 |
| 0.193400E+04 | 0.159633E+02 | 0.166926E+02 | 0.152340E+02 |
| 0.193500E+04 | 0.159752E+02 | 0.167051E+02 | 0.152454E+02 |
| 0.193600E+04 | 0.159852E+02 | 0.167153E+02 | 0.152552E+02 |
| 0.193700E+04 | 0.159919E+02 | 0.167216E+02 | 0.152623E+02 |
| 0.193800E+04 | 0.159965E+02 | 0.167255E+02 | 0.152675E+02 |
| 0.193900E+04 | 0.159998E+02 | 0.167279E+02 | 0.152716E+02 |
| 0.194000E+04 | 0.160020E+02 | 0.167293E+02 | 0.152748E+02 |
| 0.194100E+04 | 0.160056E+02 | 0.167323E+02 | 0.152790E+02 |
| 0.194200E+04 | 0.160123E+02 | 0.167390E+02 | 0.152856E+02 |
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| 0.196600E+04 | 0.159362E+02 | 0.166419E+02 | 0.152306E+02 |

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| 0.198700E+04 | 0.160830E+02 | 0.167974E+02 | 0.153685E+02 |
| 0.198800E+04 | 0.161246E+02 | 0.168447E+02 | 0.154045E+02 |
| 0.198900E+04 | 0.161644E+02 | 0.168900E+02 | 0.154388E+02 |
| 0.199000E+04 | 0.161972E+02 | 0.169266E+02 | 0.154678E+02 |
| 0.199100E+04 | 0.161592E+02 | 0.168724E+02 | 0.154460E+02 |
| 0.199200E+04 | 0.160325E+02 | 0.167425E+02 | 0.153225E+02 |
| 0.199300E+04 | 0.159410E+02 | 0.166259E+02 | 0.152560E+02 |
| 0.199400E+04 | 0.160105E+02 | 0.167030E+02 | 0.153180E+02 |
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| 0.199700E+04 | 0.162201E+02 | 0.169358E+02 | 0.155044E+02 |
| 0.199800E+04 | 0.162686E+02 | 0.169907E+02 | 0.155464E+02 |

```
#          91
# IPCC AR4 Millenium Runs output (vary solar forcing)
# ++++++
#
# Model: Bern2.5CC version with active C-cycle
# -----
# Prescribed forcing timeseries as described in file
# readme_doRuns_IPCC_Chap6_millennium_21jan06.txt
# provided by F. Joos, University of Bern.
#
# Contact:
# -----
# Gian-Kasper Plattner
# Climate and Environmental Physics
# Physics Institute, University of Bern
# Sidlerstrasse 5, CH-3012 Bern, Switzerland
# plattner@climate.unibe.ch
# http://www.climate.unibe.ch/~plattner/
# tel: ++41 (0)31 631-44-67
# fax: ++41 (0)31 631-87-42
#
```

```
# Some model setup informations:
# -----
# All runs with horizontal/vertical diffusion
#
# Run with standard ocean parameters
#   as used in Plattner et al. 2001/2002
#   with Kv (diffusivity) 4*10^-5 m2/s
#
# Climate sens. set to ~ 3.2 degrees C
# parameterized see Knutti et al. (Clim. Dyn. 2003)
#
# Model version is annual mean.
#
# No radiation code, CO2 radiative forcing calculated
# for as RF=5.35*ln(CO2/CO2_preind),
# Non-co2 radiative forcing prescribed according to
# Joos et al. GBC 2001 with updates for solar forcing
#
# More model description:
# -----
# Zonally averaged dynamical ocean with 3 basins and
# Southern Ocean, zonally averaged one layer energy
# and moisture balance atmosphere, thermodynamic
# sea ice (Stocker et al., J. Climate 1992).
#
# Carbon cycle components: Ocean/Atm/Terr.biosphere;
# Ocean carbon cycle is a description of the cycles
# of organic carbon and CaCO3 (Marchal et al., Tellus
# Tellus B), based on Redfield approach using PO4 as
# biolimiting nutrient.
#
# Land Biota: Lund-Jena-Postdam Dynamical Global
#               Vegetation Model (LPJ-DGVM)
# at GCM resolution (Gerber et al. 2003, Climate
# Dynamics; Sitch et al. 2003, Global Change Biology)
#
# LPJ forced by Cramer/Leemans annual mean
# climatology plus interannual climate variability
# from Hadley simulation (30-recycled climate) plus
# changes in the fields of surface temperature,
# precipitation, and cloudcover as simulated with the
# Impulse-EOF version of ECHAM-3/LSG in response to
# projected radiative forcing changes.
#
# Land use changes are not explicitly considered.
#
# Impact of climate change on terrestrial C-storage
# included
#
# References:
# -----
# Carbon cycle Ocean: Marchal et al., Tellus 1998
# Carbon cycle Terr. Bio: Sitch et al., GCB 2003
#               Gerber et al., Clim. Dyn. 2003
```

```

# Cycle-climate feedbacks and global warming:
#     Plattner et al., Tellus 2001
#     Plattner et al., GBC 2002
# Non-CO2 forcing: Joos et al., GBC 2001
# Climate model: Stocker et al., J. Climate 1992
# Sea level: Knutti et al., J. Climate 2000
# Global warming Physics: Knutti et al., Nature 2002
#     Knutti et al., Cl. Dyn. 2003
#     and refs therein.
#
# Output columns:
# -----
# Time (yr AD)
# Global mean air temperature (deg C)
# NH-averaged air temperature (deg C)
# SH-averaged air temperature (deg C)
0.100100E+04 0.160288E+02 0.166890E+02 0.153687E+02
0.100200E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.100300E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.100400E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.100500E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.100600E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.100700E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.100800E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.100900E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.101000E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.101100E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.101200E+04 0.160288E+02 0.166890E+02 0.153687E+02
0.101300E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.101400E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.101500E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.101600E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.101700E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.101800E+04 0.160288E+02 0.166890E+02 0.153687E+02
0.101900E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.102000E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.102100E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.102200E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.102300E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.102400E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.102500E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.102600E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.102700E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.102800E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.102900E+04 0.160288E+02 0.166890E+02 0.153687E+02
0.103000E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.103100E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.103200E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.103300E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.103400E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.103500E+04 0.160288E+02 0.166890E+02 0.153687E+02
0.103600E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.103700E+04 0.160288E+02 0.166889E+02 0.153687E+02
0.103800E+04 0.160288E+02 0.166889E+02 0.153687E+02

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| | | | |
|--------------|--------------|--------------|--------------|
| 0.195700E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.195800E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.195900E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.196000E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.196100E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.196200E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.196300E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.196400E+04 | 0.160288E+02 | 0.166890E+02 | 0.153687E+02 |
| 0.196500E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.196600E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.196700E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.196800E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.196900E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.197000E+04 | 0.160288E+02 | 0.166890E+02 | 0.153687E+02 |
| 0.197100E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.197200E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.197300E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.197400E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.197500E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.197600E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.197700E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.197800E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.197900E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.198000E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.198100E+04 | 0.160288E+02 | 0.166890E+02 | 0.153687E+02 |
| 0.198200E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.198300E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.198400E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.198500E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
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| 0.198900E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.199000E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.199100E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.199200E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.199300E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.199400E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
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| 0.199600E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
| 0.199700E+04 | 0.160288E+02 | 0.166889E+02 | 0.153687E+02 |
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Attachment Converted:

"c:\eudora\attach\Dgmairtnorm_millenum_CLIMBER2_1000_1998_ipccar4.eps"

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Eystein Jansen <eystein.jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>
Subject: MWP paper / possible figure / data
Date: Tue, 31 Jan 2006 14:03:29 +0000
Cc: Keith Briffa <k.briffa@uea.ac.uk>

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Dear Eystein and Peck,

sorry for the overlong silence at this end. We **are** working on the revised figures, etc. and thanks for the CLIMBER and BERN EMIC data - Keith and I must look at this and see how best to show it.

In the meantime, I just wanted to forward to you a paper that we have coming out in Science next Friday - see the **uncorrected** page proofs attached. Please treat this in confidence and for IPCC purposes only - I'm sure you're aware of their strict embargo policy.

The reason we thought it worth forwarding was because it is useful for comparing implied MWP and 20th century NH temperatures and thus might be appropriate for use in the IPCC "MWP box". The approach is similar to that which Susan Solomon seemed to be keen on - looking at individual series, but simply counting how many simultaneously imply warmth or cold conditions. There's also the possibility that one of its figures (perhaps panel 3B) might be useful in the "MWP box". If you have time for a quick read, please tell us what you think.

Eystein - you were also wanting some regional proxy series and I thought I'd send you the data shown in Fig 1 of this paper, because I'm preparing a file to accompany the paper anyway and this will kill two birds with one stone. Are these data what you were hoping for? I'll send them later today if they are.

Cheers

Tim

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Attachment Converted: "c:\eudora\attach\osborn_uncorrectedproofs.pdf"

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sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>, Eystein.Jansen@geo.uib.no
Subject: Fwd: new fig
Date: Fri Feb 3 14:31:09 2006

Peck and Eystein

we are having trouble to express the real message of the reconstructions - being scientifically sound in representing uncertainty, while still getting the crux of the information across clearly. It is not right to ignore uncertainty, but expressing this merely in an arbitrary way (and as a total range as before) allows the uncertainty to swamp the magnitude of the changes through time. We have settled on this version (attached) of the Figure which we hope you will agree gets the message over but with the rigor required for such an important document.

We have added a box to show the "probability surface" for the most likely estimate of past temperatures based on all published data. By overlapping all reconstructions and giving a score of 2 to all areas within the 1 standard error range of the estimates for each reconstruction, and a score of 1 for the area between 1 and 2 standard errors, you build up a composite picture of the most likely or "consensus" path that temperatures took over the last 1200 years (note - now with a linear time axis). This still shows the outlier ranges, preserving all the information, but you see the central most likely area well, and the comparison of past and recent temperature levels is not as influenced by the outlier estimates. What do you think? We have experimented with different versions of the shading and this one shows up quite well - but we may have to use some all grey version as the background to the overlay of the model results.

We have also experimented with changing the normalisation base for the model/reconstruction Figure, but using the same short modern period as for the first Figure is not satisfactory - more on this later. We have added in Oerlemans curve as many insisted - but we only have the GLOBAL curve - can you get the separate North and Southern Hemisphere curves (with uncertainty). I do not see that the new model runs from Germany/Switzerland will fit easily in the existing Figure and need to be separate! I am really struggling with the text also - really need more time!!!! More later

Keith

X-Mailer: QUALCOMM Windows Eudora Version 7.0.0.16

Date: Fri, 03 Feb 2006 10:42:15 +0000

To: Keith Briffa <k.briffa@uea.ac.uk>

From: Tim Osborn <t.osborn@uea.ac.uk>

Subject: new fig

Dr Timothy J Osborn

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web: [1]<http://www.cru.uea.ac.uk/~timo/>

suncllock: [2]<http://www.cru.uea.ac.uk/~timo/suncllock.htm>

--

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Phone: +44-1603-593909

Fax: +44-1603-507784

[3]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/suncllock.htm>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>
Subject: Re: Fwd: new fig
Date: Fri Feb 3 17:45:52 2006

Eystein

can you also check that Fortunat is addressing the few comments (ie revising the text) that relate to his bit of my section and Henry Pollack is helping us to asses the comments and revise the text to do with the Ground Surface Temperature section. I presume Ricardo and Peck are dealing with all the regional stuff. Thanks

At 17:32 03/02/2006, you wrote:

Hi,

I can contact Oerlemans, have met him a few times.

Cheers,
Eystein

thanks for this - the new runs I think best in a separate panel .

Keith

At 16:44 03/02/2006, Jonathan Overpeck wrote:

Hi Keith and Tim (and Eystein): Your new figure is quite compelling, and a nice complement to the other two panels. I agree it would be good to get the Northern Hem Oerleman's plot - Eystein do you know him well enough to ask? (I never even met him, but could ask if you don't know him).

What you have created will take some good work on the caption to explain, but it has my vote.

What is your plan for dealing with the new German/Swiss model results? A new figure? Are you sure these runs can't be worked in, perhaps as a new panel? At least we have Susan's support for the new runs, so we do what we have to do.

As for work and time, we are running out. Just do the best you can, and hopefully the new section will emerge sometime next week.

Highest priority (please do first) - we need 3 TS-contender figures (and captions) by early next week:

- 1) the new fig showing all the sites used in the recons - with caption
- 2) the fig you've attached to this email - with caption (were we going to try to put all the model runs/refs/color key into a table, so the caption could be shorter than in the FOD? Think this would be better, so caption is shorter)
- 3) the new fig comparing the obs to the model runs (update of the fig we showed for first time in ChCh - using a version of the lower panel you attached to this email - with caption

There is little doubt you guys have the hardest job of all LAs in our chapter, and

possibly the entire WG1 report. Your work will have huge impact, and the extra effort is really appreciated well beyond me and Eystein. I wish we could offer up a time machine to make it easier, but... just keep plugging.

thanks! Peck

Peck and Eystein

we are having trouble to express the real message of the reconstructions - being scientifically sound in representing uncertainty, while still getting the crux of the information across clearly. It is not right to ignore uncertainty, but expressing this merely in an arbitrary way (and as a total range as before) allows the uncertainty to swamp the magnitude of the changes through time. We have settled on this version (attached) of the Figure which we hope you will agree gets the message over but with the rigor required for such an important document.

We have added a box to show the "probability surface" for the most likely estimate of past temperatures based on all published data. By overlapping all reconstructions and giving a score of 2 to all areas within the 1 standard error range of the estimates for each reconstruction, and a score of 1 for the area between 1 and 2 standard errors, you build up a composite picture of the most likely or "consensus" path that temperatures took over the last 1200 years (note - now with a linear time axis). This still shows the outlier ranges, preserving all the information, but you see the central most likely area well, and the comparison of past and recent temperature levels is not as influenced by the outlier estimates. What do you think? We have experimented with different versions of the shading and this one shows up quite well - but we may have to use some all grey version as the background to the overlay of the model results.

We have also experimented with changing the normalisation base for the model/reconstruction Figure, but using the same short modern period as for the first Figure is not satisfactory - more on this later. We have added in Oerlemans curve as many insisted - but we only have the GLOBAL curve - can you get the separate North and Southern Hemisphere curves (with uncertainty). I do not see that the new model runs from Germany/Switzerland will fit easily in the existing Figure and need to be separate! I am really struggling with the text also - really need more time!!!! More later

Keith

X-Mailer: QUALCOMM Windows Eudora Version 7.0.0.16

Date: Fri, 03 Feb 2006 10:42:15 +0000

To: Keith Briffa <k.briffa@uea.ac.uk>

From: Tim Osborn <t.osborn@uea.ac.uk>

Subject: new fig

Dr Timothy J Osborn

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[3]<http://www.cru.uea.ac.uk/cru/people/briffa/>
Attachment converted: Macintosh HD:ipcc_nhrecon_new1.pdf (PDF /«IC») (0010B41B)

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References

1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>
4. <http://www.geo.arizona.edu/>
5. <http://www.ispe.arizona.edu/>
6. <http://www.cru.uea.ac.uk/cru/people/briffa/>
7. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Olga Solomina" <olgasolomina@yandex.ru>
To: "Jonathan Overpeck" <jto@u.arizona.edu>, "Eystein Jansen" <Eystein.Jansen@geo.uib.no>
Subject: glacier box sod
Date: Mon, 6 Feb 2006 11:09:24 +0300
Cc: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>, "Keith Briffa" <k.briffa@uea.ac.uk>, Valérie Masson-Delmotte <Valerie.Masson@cea.fr>

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Dear Eystein and Peck,

Many thanks for your reply and contribution for the glacier box. Everything is fine with me except for the sentence:

"Comparing the ongoing retreat of glaciers with the reconstructed records, it is evident that the current global pattern is unprecedented within the Holocene, as there is no known period with a global homogenous trend of retreating glaciers over centennial and shorter timescales."

The reason of my disagreement is the following: the resolution and the spatial and temporal coverage of the Holocene glacial records is not enough to compare it seriously at the century level. For most of regions we even cannot estimate the synchronicity of the records. Looking at the figure a reader will see that there was actually a period with "a global homogenous trend of retreating glaciers" during at least a millennium (at least 7000-8000 bp) - not a century like now! To resolve this problem we can discuss in a broader audience and ask the opinion of more experts if you wish - I can think of Luckman, Nesje, Grove, Porter, Karlen.

I corrected a little the second paragraph - removed three references - they are not used in our picture and, in fact not that good in terms of real reconstructions. I think we should stress clearly that the records from Scandinavia is now the most reliable and detailed.

Regards,

olga

----- Original Message -----

From: "Eystein Jansen" <Eystein.Jansen@geo.uib.no>
To: "Olga Solomina" <olgasolomina@yandex.ru>
Cc: "Jonathan Overpeck" <jto@u.arizona.edu>
Sent: Saturday, February 04, 2006 3:04 AM
Subject: Fwd: Re: glacier box

Dear Olga,
both Peck and I like the new version, both figure and shorter text.
Please find enclosed a suggestion from us with
some revisions, one file with track changes, one
with all changes accepted. I have added a little
to your short text, but not much. If you are
happy with this, please send the final version
inserted into the template of the SOD we sent out
so that the style is correct, the figure
separately, and an endnote file with references.
Best wishes and thanks for all your efforts,
Eystein

--

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Attachment Converted: "c:\eudora\attach\glboxsodso.doc"

From: simon.tett@metoffice.gov.uk
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: congratulations for the Science paper!
Date: Tue, 07 Feb 2006 09:40:14 +0000
Cc: Eduardo Zorita <Eduardo.Zorita@gkss.de>, Tim Osborn
<t.osborn@uea.ac.uk>, Hans von Storch <Hans.von.Storch@gkss.de>, Simon
Tett <simon.tett@metoffice.gov.uk>, Eystein Jansen
<eystein.jansen@geo.uib.no>

My view is that paleo is not important enough for the Hadley Centre for us to spend much (or any) time helping Millennium unless there is some cash on the table to buy some staff time. I am working 75% at the moment so need to focus on staff management. If I do have time it will be focused on completing the SOAP work.

Simon

On Tue, 2006-02-07 at 09:34, Keith Briffa wrote:

> Hi Eduardo
> Thanks for this and for letting us know about Millennium.
> I think it is outrageous that the millennium group submitted what
> was patently an inferior proposal compared to Imprint. Having then
> succeeded in getting the funding, they are now resorting to
> "poaching" members of the Imprint team to provide the essential model
> simulation element that was pitifully deficient in their original
> submission. To me this is tantamount to receiving money under false
> pretences! I believe the European system for allocating research
> funds has been seriously abused .
>
> Keith
>
>
> At 23:30 06/02/2006, Eduardo Zorita wrote:
>
> >Tim, Keith
> >
> >Hans and myself were in Oxford last week to meet Myles Allen and
> >Danny McCarroll,
> >among others. Myles has been in contact with us in the last couple of
months,
> >and they are interested in a GKSS participation in Millennium. It
seems
> >that our collaboration there is getting clearer, although we will not
get
> >funding from the EU. We will probably assist in the design of their
> >global simulations
> >and perhaps also in some regional simulations. Likely GKSS will
perform some
> >ensembles for certain periods to estimate the internal variability
> >at regional scales.
> >
> >Simon could not attend the meeting in the last moment, but probably he
will be
> >involved too, although I do not know exactly how.
> >

> >
> >eduardo
>
> --
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From: Tim Osborn <t.osborn@uea.ac.uk>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Data for IPCC
Date: Tue, 07 Feb 2006 12:00:21 +0000

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Hi Eystein and Peck,

sorry, but I'm **still** working on the figures. On the simulations one, we were requested to include results from the new Stendel et al. (2005, Clim. Dyn.) simulation with ECHAM4-OPYC3 for the last 500 years. Did you get these data already? I've just emailed Martin Stendel to ask for them, but thought I'd check in case you already had them.

Cheers

Tim

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>
Subject: Re: Fwd: Re: Data for IPCC
Date: Tue, 7 Feb 2006 19:11:56 -0700
Cc: Keith Briffa <k.briffa@uea.ac.uk>, tshanaha@email.arizona.edu

<x-flowed>

Hi Eystein, Keith and Tim - this seems odd to me, given that the N hem data must completely dominate his global recon. BUT, since the data and recon are his, and our job is to assess what is published, we don't have much choice. We have three options (or more if you can think of them):

option 1) forget about his recon. Although I sense that there might be some interest in this, we must include his study/data/fig

option 2) we could make a separate fig to highlight just his global recon, perhaps compared to the global borehole recon. We are dying for space, so I suspect this option isn't ideal either. Expert review of the SOD might suggest it, but in the meantime, I suggest we try to get away with...

option 3) we include it in the big recon plot, and just make it clear in the caption (and table that goes with the caption if you're going with the table idea) that the Oerleman's curve, though labeled global in the original paper, appears to be representative of (or weighted mostly by, or ?) glaciers in the Northern Hemisphere (per his Fig 3a). I think we should leave it to Keith and Tim to figure out the best language, but I think this will work. Could be done as a footnote to the table instead of the caption.

Make sense? thanks, Peck

>Hi, this is what I got from Oerlemans.

>If we go with his data it has to be the global curve it seems....

>

>Eystein

>

>>From: "J. Oerlemans" <J.Oerlemans@phys.uu.nl>

>>Subject: Re: Data for IPCC

>>Date: Sun, 5 Feb 2006 22:31:19 +0100

>>To: Eystein Jansen <Eystein.Jansen@geo.uib.no>

>>X-checked-clean: by exiscan on noralf

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>>X-UiB-SpamReport: spamassassin found;

>> -15 From is listed in 'whitelist_SA'

>>

>>Dear Eystein,

>>

>>Just returned from abroad and have some time now to look at your request.

>>

>>I don't think it is a very good idea to

>>consider hemispheric temperatures from glacier

>>records separately. The error bars are just too

>>large. I am currently extending the dataset

>>substantially, but it will take some time

>>before hemispheric averages have a similar

>>error bar as the global mean right now (figure

>>3b in my paper).

>>So I propose you only present the estimated

>>global mean temperature, which I give below.

>>

>>With best wishes,

>>Hans

>>=====

>>

>>

>>

>>

>>

>>

>>

>>

>>On Feb 3, 2006, at 7:08 PM, Eystein Jansen wrote:

>>

>>>Dear Hans,

>>>I am co-ordinating lead author for the IPCC

>>>AR4 Paleoclimate chapter. In our section on

>>>the last 2000 years we would like to include
>>>your T-reconstruction from glaciers that was
>>>published in Science. We would like to have
>>>the data separate for each hemisphere plus the
>>>global mean and include this into a figure
>>>showing a suite of T reconstructions. There is
>>>an urgency to this and we hope that you could
>>>send us the data very soon, in order for the
>>>data to bbe incorporated into the 2nd draft of
>>>the report.

>>>
>>>Best wishes
>>>Eystein

>>>--

>>>

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>

>--

>

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: just checking - important

Date: Wed, 8 Feb 2006 10:42:06 -0700

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Tim - thanks for the update. Just think of the beer at the end of the tunnel. This week's deadline is a TSU deadline for figs being considered for the Tech Summary. You're looking good to get some of your figs/science in the TS, and this means big impact. Hopefully, provides the extra juice to find the extra time needed to get them done.

thx, peck

>update:

>

>reconstructions + observations: you've seen the multiple shading
>extra panel already, but I've made a few more tweaks to this and
>added oerlemans (global, but looks similar to his NH regions, by
>eye) reconstruction.

>

>forcings + model NH temps: waiting for Stendel data, added new
>ECHO-G run without drift problem, tried replacing reconstruction
>"envelope" with the multiple shading approach used in the extra
>panel of the first figure. Not sure how clear it is - obviously
>adding shades of grey behind coloured lines can make it a little
>harder to distinguish them.

>

>extra runs from EMICS: draft plot of NH temps made, got to put the
>reconstruction shading under that too, not yet done and the whole
>thing needs some tidying up so that it can be an extra panel of the
>previous figure.

>

>extra panel showing a volcanic forcing time series unsmoothed (i.e.,
>with spikes): draft done but again needs tidying so it can be an
>extra panel of the forcings/models figure.

>

>maps of proxy locations - still lots of work to be done.

>

>Cheers

>
>Tim
>
>
>
>At 03:01 08/02/2006, you wrote:

>>Hi Tim - I did, thanks. And this is where the "hybid" MWP box idea
>>came from. Speaking of which, how are all your figs going? We
>>really need those being considered for the Tech Summary asap
>>(deadline is this week). Please update at least. Thanks, Peck

>>>Hi Peck - sorry, forgot to reply to this. Yes, please do share it
>>>with them, if you haven't already. - Tim

>>>>At 05:38 01/02/2006, Jonathan Overpeck wrote:
>>>>Hi Tim and Keith - I assume I can share your pre-pub Science pdf
>>>>with Susan and Martin? Of course, I'll point out the need for
>>>>confidentiality, but I'm sure they know the deal and can be
>>>>trusted. Just wanted to make sure this is ok w/ you, so that we
>>>>can get their opinions on what's best for the MWP box.

>>>>thanks, Peck
>>>>--
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>>

>>

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From: Jesus Fidel Gonzalez Rouco <fidelgr@fis.ucm.es>
To: t.osborn@uea.ac.uk
Subject: Re: erik2
Date: Wed, 08 Feb 2006 18:34:25 +0100
Cc: k briffa <k.briffa@uea.ac.uk>

Dear Tim,

attach the data (erikII.dat): NH averages with standard latitude weighting.

Yes, the forced simulations are identical in forcing, just different initial conditions.

Just for complementary info on the data file a rough plot of the data in the file erikII.dat in comparison with NH avgs in ErikI: ErikI-II.ps

Also for commplementary info, I attach nhavg.jpg and nhano.jpg...plots of anomalies and absolute values in the NH for all the forced runs:

columbus erikI,II and control.

Let me know if there are any queries or problems.

Best regards

Fidel

ps.- I will be glad to have a pdf of the magicc paper when you consider it appropriate.

Congratulations for this.

Tim Osborn wrote:

Hi again Fidel,

we are in the very final stages of preparing a revised figure for the IPCC and so your email has come at just the right time (if you can provide the data quickly). Assuming the forcings are identical to erik1, then all we would need is a time series (in plain text) of annual-mean NH temperature.

If you can provide this, then we can include it. (My paper comparing erik1 against a simulation with the simple energy balance model MAGICC has at last been accepted by Climate Dynamics - there were no problems at all, just very slow reviewers and very slow editorial decisions!).

Best wishes

Tim

On Mon, February 6, 2006 8:37 pm, Jesus Fidel Gonzalez Rouco wrote:

Dear Tim and Keith,

the erik2 paper which I mentioned in Bern was under review came out some weeks ago.

You mentioned then that it might be of interest to include these data in the IPCC rep. Let me know what you need for this when it suits you.

I attach the pdf.

Thanks a lot for that btw.

Best regards from Madrid

Fidel

--

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References

1. <http://chubasco.fis.ucm.es/%7Efi/>
2. <http://chubasco.fis.ucm.es/%7Efi/>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: paper in this Friday's Science
Date: Thu, 09 Feb 2006 12:07:02 -0500
Reply-to: mann@psu.edu
Cc: k.briffa@uea.ac.uk

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Guys,

A final revised version attached. I'm expecting the embargo to lift at midnight east coast U.S., but let me know if you hear otherwise. I will make sure the science website has posted the paper before posting myself...

mike

Tim Osborn wrote:

> Hi Mike,
>
> thanks for putting this together, Mike. It is a nice summary plus
> drawing out of the important strands etc. I especially like "might be
> likened in shape to a certain implement used in a popular North
> American winter sport" - Keith thinks you must mean a "ski"?
>
> The only negative thing I have to say is that you get in a couple of
> "digs" at the sceptics which might unnecessarily rankle readers. e.g.
> *astronomers* Soon and Baliunas; *unbridled* cherry picking. Still,
> it's your name that's attached to this piece, so it's up to you to dig
> if you want.
>
> Cheers and thanks again
>
> Tim
>
> At 13:42 09/02/2006, Michael E. Mann wrote:
>
>> Hi Tim,
>>
>> Maybe Science can still fix (at least, the online version?). I
>> wouldn't lose sleep over this though. As typos go, its relatively minor.
>>

>> I must confess that I scavenged a figure off your page proofs. As the
>> piece won't go online until after the article goes up on Science's
>> website, shouldn't matter what the source was though...

>>
>> I've attached the piece in word format. Hyperlinks are still there,
>> but not clickable in word format. I've already given it a good
>> go-over w/ Gavin, Stefan, and William Connelley (our internal "peer
>> review" process at RC), so I think its in pretty good shape. Let me
>> know if any comments...

>>
>> thanks,

>>
>> Mike

>>
>> Tim Osborn wrote:

>>
>>> Bugger. You read and re-read the manuscript and the proofs and
>>> *still* you miss things! Yes, it should be 1856. Thanks for
>>> spotting this.

>>>
>>> I didn't reply yet about RealClimate because I thought Keith or I
>>> would have to prepare something and wasn't sure if we'd have time
>>> (IPCC deadlines!), but as you've done the work instead, that's great
>>> - though we'd like to see it beforehand if possible. Did you
>>> need/want a copy of a figure or have you got hold of one from
>>> Science/journalist?

>>>
>>> Cheers

>>>
>>> Tim

>>>
>>> At 19:53 08/02/2006, Michael E. Mann wrote:

>>>
>>>> Tim/Keith,

>>>>
>>>> I've worked up an article for RC to go online when the embargo is
>>>> lifted. Will send later when finalized. One issue came up in an
>>>> interview w/ a writer at Science, and I didn't know the answer. Is
>>>> the shorter reference period you mention in caption of fig 3 really
>>>> 1865, or is that a typo (i.e., supposed to be 1856). I couldn't
>>>> think of a reason for why the latter date would be used, and
>>>> guessed that "65" just got transposed accidentally? Please let me
>>>> know if you can what the answer is. Its a minor point, but nice to

>>>> get things right if possible...

>>>>

>>>> mike

>>>>

>>>> --

>>>> Michael E. Mann

>>>> Associate Professor

>>>> Director, Earth System Science Center (ESSC)

>>>>

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Attachment Converted: "c:\eudora\attach\OsbornBriffa06Post1.doc"

From: Jonathan Overpeck <jto@u.arizona.edu>
To: t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>
Subject: progress
Date: Thu, 9 Feb 2006 14:56:51 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith and Tim - Eystein and I just talked about Henry's request to be able to read and comment on your SOD text, and it seems highly appropriate that we work super hard to make this possible. It is taking place w/ other sections of the SOD, and your section is the one that has to be the most perfect.

I'm guessing that we'll have final figs this week or over the weekend (please!), and the edited section a day or two later (at the most). As per the last email to you and Henry, you can save everyone time if you send sections relevant to him (all the multi-proxy and proxy sections) as soon as they are done.

Sorry to keep the pressure on, but we are running out of time.

thanks, peck

--

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</x-flowed>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>
Subject: update
Date: Thu, 09 Feb 2006 16:51:53 -0500
Reply-to: mann@psu.edu
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

<x-flowed>

guys, I see that Science has already gone online w/ the new issue, so we put up the RC post. By now, you've probably read that nasty McIntyre thing. Apparently, he violated the embargo on his website (I don't go there personally, but so I'm informed).

Anyway, I wanted you guys to know that you're free to use RC in any way you think would be helpful. Gavin and I are going to be careful about what comments we screen through, and we'll be very careful to answer any questions that come up to any extent we can. On the other hand, you might want to visit the thread and post replies yourself. We can hold comments up in the queue and contact you about whether or not you think they should be screened through or not, and if so, any comments you'd like us to include.

You're also welcome to do a followup guest post, etc. think of RC as a resource that is at your disposal to combat any disinformation put forward by the McIntyres of the world. Just let us know. We'll use our best discretion to make sure the skeptics don't get to use the RC comments as a megaphone...

mike

--

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From: Jonathan Overpeck <jto@u.arizona.edu>

To: "Wahl, Eugene R" <wahl@alfred.edu>

Subject: RE: Wahl-Ammann paper and UAZ position

Date: Fri, 10 Feb 2006 12:05:44 -0700

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Eugene - this is good news... I hope. Please contact Steve and see if we will have "in press" status before the end of the month. He knows the drill, but also the downside of not being precise. Let me, Eystein and Keith know as soon as you know. Bit nuts right now, really appreciate your help.

thanks, peck

>Hi Peck:

>

>Well, as I have understood it in our communications with Steve,
>final acceptance is equivalent to being in press for Climatic Change
>because it is a "journal of record". However, this would need to be
>confirmed to be quite sure.

>

>If that is the case, then in press is still possible by the end of
>the month. I think.

>

>Which would be best at this point, for me to write and ask Steve
>this, or would it be better for you to ask? I'm happy to do so, I
>just want to act in the most time-effective and appropriate way.

>

>I apologize for the fact that it is coming right down to the wire.
>The status right now is that I am waiting for final analytical
>results from Caspar re: Pearson's r and CE results on all the
>scenarios we have done. These results will go in an appendix table
>and I have to write a brief text to go with them for
>contextualization purposes--I already have in mind what I want to
>say. The entire rest of the document is essentially done.

>

>Steve turned around the change from "in review" to "provisionally
>accepted" within days last December after receiving back the final
>independent re-review (it had been due a month earlier), so I can
>imagine that he could potentially turn around the change from
>"provisional acceptance" to "full acceptance" similarly quickly.

>
>
>Please advise about who is best to contact Steve--and if me I will
>get on it today.

>
>
>Peace, Gene
>Dr. Eugene R. Wahl
>Asst. Professor of Environmental Studies
>Alfred University

>
>_____

>
>
>From: Jonathan Overpeck [mailto:jto@u.arizona.edu]
>Sent: Fri 2/10/2006 12:39 PM
>To: Wahl, Eugene R
>Cc: Eystein Jansen; Keith Briffa; t.osborn@uea.ac.uk
>Subject: RE: Wahl-Ammann paper and UAZ position

>
>
>
>Hi Gene - First the IPCC, then I'll send another email wrt UA Geography

>
>Based on your update (which is much appreciated), I'm not sure we'll
>be able to cite either in the SOD due at the end of this month
>(sections will have to be done this week, or earliest next week to
>meet this deadline). The rule is that we can't cite any papers not in
>press by end of Feb.

>
> From what you are saying, there isn't much chance for in press by the
>end of the month? If this is not true, please let me, Keith, Tim and
>Eystein know, and make sure you send the in press doc as soon as it
>is officially in press (as in you have written confirmation). We have
>to be careful on these issues.

>
>Thanks again, Peck

>
>>Hi Peck:

>>
>>Two quick things...

>>
>>1) Regarding the Wahl-Ammann (WA) Climatic Change paper...Caspar
>>and I are in the very final stages of completing the requirements

>>Steve Schneider set for bringing this paper into full (vs.
>>provisional) acceptance. We have an internal goal of a week from
>>now for resubmission.

>>
>>We have had an equally pressing deadline with Science re: our
>>comment on the vonStorch et al. 2004 criticism of MBH [that was
>>based on an improper (and undisclosed) detrending step], which has
>>taken some extra work to be sure we have our mathematics exactly
>>correct. We have been multitracking on both this and WA, and so far
>>have been quite close to meeting our internal time goals. I feel the
>>week time frame will be fairly accurate.

>>
>>
>>2) I am aware of a position now open at UAZ in the Geography and
>>Regional Development Dept. I think I make a good fit with the
>>position profile--actually quite good--however, I have met
>>roadblocks in geography departments before because my degree is not
>>in geography. Geographers seem to have particular sensitivities to
>>their discipline being "watered down". Also, the geography depts at
>>some research grade institutions (UMN for example) require pretty
> >heavy teaching loads, which makes a nice challenge to keep up with
>>research--don't I know!! And finally, the position is subject to
>>budgetary approval, which makes me wonder if there are significant,
>>deeper budgetary issues that it would be good to know about.

>>
>>Do you have any read on this position and the budget issues? I have
>>a lot of contacts there in climatology/earth system-related
>>areas--including you, Malcolm Hughes, Tom Swetnam, Owen Davis, and
>>also Julio Betancourt of the USGS--which is something that would be
>>considered a strength for this position. From my perspective, the
>>fit would be very good, but I don't want to invest effort in the
>>application process if it is clear that not being a geography PhD is
>>a stopper, or if there is some other significant red flag I should
>>know about. Any thoughts you might have will be welcome.

>>
>>I'll be contacting Malcolm for his read also, and then talk to the
>>search chair.

>>
>>
>>Peace, Gene
>>Dr. Eugene R. Wahl
>>Asst. Professor of Environmental Studies
>>Alfred University

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>>607-871-2604
>>1 Saxon Drive
>>Alfred, NY 14802
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From: Jonathan Overpeck <jto@u.arizona.edu>
To: rahmstorf@ozean-klima.de, joos <joos@climate.unibe.ch>, Eystein Jansen <eystein.jansen@geo.uib.no>, t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>
Subject: Fwd: some figures at last!
Date: Fri, 10 Feb 2006 12:21:17 -0700

<x-flowed>

Hi Stefan and Fortunat: Attached are the draft figs that include proxy obs, simulations, and comparisons of the two. As you can see, Tim just sent them. Big job, but they look great in my eyes.

See Tim's email below for more background info.

We need fast feedback from you both, specifically:

- 1) any general comments on the figs - this is a crux set of figures and we need your eyes to look at them carefully
- 2) is it wise to keep the new EMIC run panel attached to the second figure as attached? I vote yes, but what do you think. It fits w/ the other panels pretty well.
- 3) either way, we need caption prose from you (perhaps Fortunat start, and Stefan edit, or vice versa if Stefan can start first) on the new EMIC panel.
- 4) also, we need a new para, or prose that can be added to a para, that describes the panel and it's implications as it informs our assessment. Keith will then integrate this into the section. I'm not sure of this, but perhaps you could start with a new question heading, and then have a short para to go under it - something like "What is the significance of the new reduced-amplitude estimates of past solar variability?"

Of course, we need your feedback and prose asap. Please send to me, Eystein, Keith and Tim.

Thanks in advance for the help. Best, peck

>X-Sieve: CMU Sieve 2.2
>Date: Fri, 10 Feb 2006 18:00:19 +0000
>To: Jonathan Overpeck <jto@u.arizona.edu>,
> Eystein Jansen <eystein.jansen@geo.uib.no>
>From: Tim Osborn <t.osborn@uea.ac.uk>
>Subject: some figures at last!
>Cc: Keith Briffa <k.briffa@uea.ac.uk>
>X-UEA-Spam-Score: -102.8
>X-UEA-Spam-Level: -----
>X-UEA-Spam-Flag: NO
>
>Dear Peck and Eystein,
>

>the attached word file contains the latest versions of two of our figures.
>
>First, is the reconstructions with many requests now done: linear time scale, dotted early instrumental temperatures not solid line, Oerlemans added, new panel showing shading for the overlapping regions of temperature reconstructions.
>
>Second, is the forcings and models. Stendel ECHAM simulation added (1500-2000). New ECHO-G Erik2 simulation just published in GRL from Gonzalez-Ruoco et al. added (1000-1990). Reconstruction "envelope" replaced by new shading of overlaps in the temperature reconstructions. Correction of some labelling errors. Those runs that did not include 20th century sulphate aerosol cooling are dotted or dashed after 1900 (the two low ones also omitted CH4, N2O, CFCs, O3, hence still cool despite omitting aerosol cooling). The ECHO-G Erik1 simulation with the very out-of-equilibrium initial conditions is dashed. Finally, the extra panel with the new EMIC runs is included as panel (e), again with the new shading of overlapping temperature reconstructions.
>
>Keith suggests sending to Stefan and Fortunat too for their views - can you do that (they may now be gone for the weekend, of course).
>
>Best wishes and sorry this is late. Am I right in thinking that the only other possible-TS figure is the location maps? Still working on those (had very little time in last 2 days due to media etc. attention re. Science paper).
>
>Cheers
>
>Tim
>
>
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From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: pulling teeth and hair out
Date: Fri, 10 Feb 2006 12:59:50 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Keith - figs look great for now, and hopefully the flurry of emails just cc'd to you will take care of everything except Oerlemans. To help here, I've dug up the Chap 4 pdfs. (going to the CLA would not be quick, nor necessarily any better).

In the Ch04 figs file, go to Fig 4.5.4 on pg 4-72 for caption material that seems pretty bland.

In the Ch04 Text file, go to first full para on p 4-22 for what chap 4 had on the Oerleman's work. I suspect this is the last time they thought about it.

You can keep this really short and sweet - main thing is that it's another independent data set that shows unprecedented recent warming. A short para should do it.

Are you going to use a table to help with the figure captions?

On the weekend/evenings, I can be reached at home 1-970-728-0780, and during the week on my cell 520-907-6480. I'm single parenting, so on the weekends and evenings I might have to call back if 4 yr old Jack is doing something less than enjoyable to 1 yr old Eli. Julie is in Germany for IODP sampling. During the week, the boys are in school, and Julie's Mom arrives in time for next weekend. After the boys go to bed, I also work.

We're getting there - thanks!

best, peck

>Peck (tried to phone) -

>i please get Henry P to correct the text regarding the Section on

>Ground Surface temperatures. I am not going to mess with this and I

>can not get into which refs we need to include. Generally , I am

>happy to go with what we have for this section but the comments ,
>especially by Beltrami need to be at least considered. Thanks
>We have come to the best that we can re the Figures. The text of
>course now needs to expand , especially re the justification for the
>the new EMIC runs . How about you think on this and get the input
>from Fortunat and Stefan especially re what we need to say and ,
>whether the last panel on second Figure ought to be in another
>Figure with the specific forcings above as in the original second
>Figure? These Figures (and even the few new additions to the
>original model/data comparison) are opening cans of worms re having
>to explain/justify different results. Someone also promised (from
>the Cyrosphere chapter) presumably the CLA to send the appropriate
>text to describe the Oerlemans Figure - but nothing has been sent .
>Can you check this out - or I will just write something naive.
>Remind Fortunat he is editing in relation to his section in my
>section!!!!!!

>Keith

>

>

>

>Keith

>

>

>

>--

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Attachment Converted: "c:\eudora\attach\Ch04_FOD_Text_TSU_FINAL.pdf"

From: Jonathan Overpeck <jto@u.arizona.edu>

To: t.osborn@uea.ac.uk

Subject: Re: some figures at last!

Date: Fri, 10 Feb 2006 16:43:24 -0700

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

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Hi Tim - it is a wonderful figure, and we are writing about a paper's worth about it - very condensed stuff, but loaded with impact.

Let's see what Keith and Eystein suggest, but I'm happy w/ the fig and ref period you've used. Would rather have you working on more award winning figs than updating this one. Can do that later depending on feedback the SOD gets.

Well done, thx, peck

>Glad you like it. Regarding the positive radiative forcing, the volcano
>series (smoothed and spikey) were expressed as anomalies from the
>1500-1899 mean, as were all other data in all panels of this figure. I
>can provide the entire figure expressed as anomalies from their 1961-1990
>mean on Monday, but the volcanic forcing will again have +ve and -ve
>values because the 1961-1990 mean has some volcanic events during it.

>
>We could set maxima of each volcanic series to zero. But I like to think
>of it in this way: positive volcanic forcing *can* occur during periods
>with *less* volcanic activity than "normal", where "normal" is defined as
>the mean volcanic activity during the reference period (this is partly why
>we prefer the longer 1500-1899 reference than the shorter 1961-1990
>reference, because a 30-year reference period can't really be
>representative for a sporadic forcing like volcanoes). So, while I'm
>personally comfortable with both positive and negative volcanic forcing
>values, I'm happy to shift them to peak at zero during quiescent periods.
>Just let me know... and Keith/Eystein?

>
>I can't believe how much info there is in this figure now. We could write
>an entire paper on the construction of this one diagram!

>
>Cheers

>

>Tim

>

>On Fri, February 10, 2006 10:33 pm, Jonathan Overpeck wrote:

>> Hi Tim - nice service, thanks! This will help
>> with the diplomacy, since Susan did want to see
>> these data. Also, maybe we'll get a prize for the
>> most information backed figure in the AR4?

>>

>> I like it, and I don't think it's too
>> distracting. How did you decide to put the
>> baseline where you did? And how do we get
>> positive volcanic radiative forcing? Why not
>> bottom out all the raw and smoothed curves at
>> zero? Suspect you have a good reason, but thought
>> I'd check.

>>

>> I think I know have all the figs I'm supposed to
>> have for transmission to TSU for TS
>> consideration, and they all look good. Not that
>> they are all finished, but that's ok for this
>> fine day.

>>

>> Thanks again, Peck

>>

>>>Hi again Peck,

>>>

>>>sorry, forgot about the raw volcanic series. Originally I had it as a
>>>separate panel - yes! yet another panel! - but then I tried underlaying
>>> it

>>>on the smoothed series in a pale grey. Please see attached files (pdf
>>> and

>>>gif of the model/forcing figure). What do you think? Is it too
>>>distracting to have these grey spikes? Also note that they are on the
>>>same scale as the rest of the forcings and unfortunately some spikes are
>>>truncated at the bottom of the forcings panel - especially 1259 event.
>>>This particular series I've used is from Ammann and you can see the link
>>>between the spikes and the smoothed green-colour volcanic curve.

>>>

>>>Finally, note that this is just panels A-D. If you like this version,
>>>then you can insert it into the Word file I sent before, in place of
>>>panels A-D (use the gif file for this purpose). You'll see that panel E
>>>is a separate piece of figure, and can stay unchanged in the Word file.
>>>Hope you follow this bit.

>>>

>>>PS. Keith hasn't seen this version - not sure what his views are on the
>>>distraction of the grey spikes!

>>>

>>>Must go now,

>>>

>>>Tim

>>>

>>>

>>>On Fri, February 10, 2006 7:40 pm, Jonathan Overpeck wrote:

>>>> Hi Tim - see prev email regarding this fig. I do
>>>> like it, and I'll get feedback fast from
>>>> Susan/Martin.

>>>>

>>>> What happened to the more raw volcanic series?
>>>> Susan really wants this, but I'm not sure how to
>>>> best provide. Could we include at the top of the
>>>> forcing fig - underneath the smoothed volc
> >>> forcing curves?

>>>>

>>>> If Keith is doing all the text revision, I guess
>>>> the next fig job would be to try to create the
>>>> new hybrid MWP fig - the old FOD concept merged
>>>> with (new panel or just right below?) the curve
>>>> from your just-out Science paper (which is great,
>>>> by the way).

>>>>

>>>> Thanks again for getting these by today - it's a huge help.

>>>>

>>>> best, peck

>>>>

>>>>>Dear Peck and Eystein,

>>>>>

>>>>>proxy location maps are half done! I've
>>>>>attached what I have. Do not use this for real
>>>>>because they are not correct!!!

>>>>>

>>>>>I've done them for 1000, 1500 and 1750. They include:

>>>>>

>>>>>boreholes (circles) and this is correct for all 3 times.

>>>>>

>>>>>schweingruber tree-ring density/width network as used by briffa and
>>>>> this

>>>>

>>>>Tim

>>>>

>>>>Attachment converted: Macintosh HD:proxylocations.pdf (PDF /«IC»)

>>>> (00112850)

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>>>>

>>>> --

>>>> Jonathan T. Overpeck

>>>> Director, Institute for the Study of Planet Earth

>>>> Professor, Department of Geosciences

>>>> Professor, Department of Atmospheric Sciences

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>>>>

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>>>Attachment converted: Macintosh HD:modelsA-D.gif (GIFf/«IC») (00112AB2)

>>>Attachment converted: Macintosh HD:modelsA-D.pdf (PDF /«IC») (00112AB3)

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>> --

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</x-flowed>

From: "Tim Osborn" <t.osborn@uea.ac.uk>
To: "Jonathan Overpeck" <jto@u.arizona.edu>
Subject: Re: some figures at last!
Date: Fri, 10 Feb 2006 22:16:10 -0000 (GMT)
Reply-to: t.osborn@uea.ac.uk
Cc: "Tim Osborn" <t.osborn@uea.ac.uk>, "Eystein Jansen" <eystein.jansen@geo.uib.no>, k.briffa@uea.ac.uk

Hi again Peck,

sorry, forgot about the raw volcanic series. Originally I had it as a separate panel - yes! yet another panel! - but then I tried underlaying it on the smoothed series in a pale grey. Please see attached files (pdf and gif of the model/forcing figure). What do you think? Is it too distracting to have these grey spikes? Also note that they are on the same scale as the rest of the forcings and unfortunately some spikes are truncated at the bottom of the forcings panel - especially 1259 event. This particular series I've used is from Ammann and you can see the link between the spikes and the smoothed green-colour volcanic curve.

Finally, note that this is just panels A-D. If you like this version, then you can insert it into the Word file I sent before, in place of panels A-D (use the gif file for this purpose). You'll see that panel E is a separate piece of figure, and can stay unchanged in the Word file. Hope you follow this bit.

PS. Keith hasn't seen this version - not sure what his views are on the distraction of the grey spikes!

Must go now,

Tim

On Fri, February 10, 2006 7:40 pm, Jonathan Overpeck wrote:

> Hi Tim - see prev email regarding this fig. I do
> like it, and I'll get feedback fast from
> Susan/Martin.
>
> What happened to the more raw volcanic series?
> Susan really wants this, but I'm not sure how to
> best provide. Could we include at the top of the
> forcing fig - underneath the smoothed volc
> forcing curves?
>

> If Keith is doing all the text revision, I guess
> the next fig job would be to try to create the
> new hybrid MWP fig - the old FOD concept merged
> with (new panel or just right below?) the curve
> from your just-out Science paper (which is great,
> by the way).
>
> Thanks again for getting these by today - it's a huge help.
>
> best, peck
>
>>Dear Peck and Eystein,
>>
>>proxy location maps are half done! I've
>>attached what I have. Do not use this for real
>>because they are not correct!!!
>>
>>I've done them for 1000, 1500 and 1750. They include:
>>
>>boreholes (circles) and this is correct for all 3 times.
>>
>>schweingruber tree-ring density/width network as used by briffa and this
>>is correct for all 3 times (triangles)
>>
>>esper tree-ring data is also correct for all 3 times (also triangles)
>>
>>squares are a few selected records from Mann et
>>al. (1998) and although they are in the correct
>>locations, they are a strange subset and they
>>also currently appear in all three panels EVEN
>>THOUGH ACTUALLY SOME OF THESE ARE SHORTER AND
>>SHOULD DROP OUT OF THE EARLIER PANELS. I can
>>fix this soon but not yet.
>>
>>I can add extra locations from Mann et al., Mann
>>and Jones, Crowley etc. It's a little time
>>consuming but not too bad.
>>
>>I can also change symbols and colour etc. pretty easily.
>>
>>What is harder to do is to change the years for
>>which we want the subsets. So I really need to
>>be told what years to do - here I've done 1000,
>>1500 and 1750. We need to pin down exactly what
>>you want before I do more on this. And please

>>don't tell me to try loads of different ones and
>>show them all to you before deciding - that
>>won't help me! I wasn't sure if you wanted the
>>whole globe or just NH, but thought whole globe
>>looked good. Also did you want pre-1000, e.g.
>>AD 500 coverage?
>>
>>Hope this is ok as a "placeholder"
>>
>>Cheers
>>
>>Tim
>>
>>
>>
>>
>>
>>
>>
>>
>>Cheers
>>
>>Tim
>>
>>Attachment converted: Macintosh HD:proxylocations.pdf (PDF /«IC»)
>> (00112850)
>>Dr Timothy J Osborn
>>Climatic Research Unit
>>School of Environmental Sciences, University of East Anglia
>>Norwich NR4 7TJ, UK
>>
>>e-mail: t.osborn@uea.ac.uk
>>phone: +44 1603 592089
>>fax: +44 1603 507784
>>web: <http://www.cru.uea.ac.uk/~timo/>
>>sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
>
>
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>

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\modelsA-D.gif"

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\modelsA-D.pdf"

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: IN CONFIDENCE - opinion sought
Date: Mon, 13 Feb 2006 08:01:03 -0500
Reply-to: mann@psu.edu

<x-flowed>
Hi Keith,

I'm pretty sure they're just asking for a neutral discussion of the science that you've done that is relevant to the issues being reviewed by the committee (after all this is the U.S. National Academy of Sciences, not the U.S. Senate, etc). But I understand where you're coming from nonetheless. Perhaps you could suggest an alternate? Any possibility Tim could do this instead? He's less intimately involved w/ the paleo chapter of IPCC, so I think it might be less of a worry for him? Or Phil? Its your prerogative to suggest alternates, and I think they'll take your suggestions very seriously. My greatest fear is that McIntyre dominates the discussion. Its important that they hear from the legitimate scientists.

Thanks,

mike

Keith Briffa wrote:

> Mike
> thanks for this but after a lot of soul searching this weekend , I
> have decided to decline the invitation. Pressure of stuff here is
> intense - but the real reason is that I really think it could be
> politic to retreat into "neutral" mode , at least until after the IPCC
> Report is out. I know you can argue this various ways but the sceptics
> are starting to attack on this "non neutral" stance, and the less
> public I am at the moment the better I think. Hope you do not think I
> am a wimp here - just trying to go the way I think best.
> best wishes
> Keith

>
> At 17:14 09/02/2006, you wrote:

>
>> Hi Keith,

>>
>> I think you really **should** do this if you possibly can. The panel is
>> entirely legititimate, and the report was requested by Sherwood
>> Boehlert, who as you probably know has been very supportive of us in
>> the whole Barton affair. The assumption is that an honest
>> review of the science will buttress us against any attempt for Barton
>> to continue his attacks (there is some indication that he hasn't
>> given up yet). Especially, with the new Science article by you and
>> Tim I think its really important that one of you attend, if at all
>> possible.

>>

>> I'm scheduled to arrive Thursday March 2rd, and give a presentation

>> friday morning March 2nd. I believe Malcolm is planning on
>> participating, not sure about Ray. I would guess that Tom C and
>> Caspar A have been invited as well, but haven't heard anything.
>>
>> The panel is solid. Gerry North should do a good job in chairing
>> this, and the other members are all solid. Chrisy is the token
>> skeptic, but there are many others to keep him in check:
>>
<http://www4.nas.edu/webcr.nsf/8f6526d9731740728525663500684166/2dbbe64b5fe9981b8525710f007025b2?OpenDocument>
>>
>>
>> So I would encourage you to strongly reconsider! Let me know if you'd
>> like to chat over the phone at all about any of this. My cell phone
>> number is 814-876-0485. I teach in about an hour, for about 1.5
>> hours, but then free most of the day...
>>
>> mike
>>
>> Keith Briffa wrote:
>>
>>> Mike
>>> IN STRICT CONFIDENCE I am sending this for your opinion. To be
>>> frank, I am inclined to decline . What do think?
>>> Presumably you and others are already in the frame?
>>> Keith
>>>
>>>
>>>> X-SBRS: None
>>>> X-REMOTE-IP: 144.171.38.41
>>>> X-IronPort-AV: i="4.02,98,1139202000";
>>>> d="doc'32?scan'32,208,32"; a="8557254:sNHT39904420"
>>>> Subject: Invitation to speak to the NRC Committee on Surface
>>>> Temperature Reconstructions
>>>> Date: Wed, 8 Feb 2006 14:55:58 -0500
>>>> X-MS-Has-Attach: yes
>>>> X-MS-TNEF-Correlator:
>>>> Thread-Topic: Invitation to speak to the NRC Committee on Surface
>>>> Temperature Reconstructions
>>>> Thread-Index:
>>>>
AcYce3i/tURJ1nRBSbezvDYAmbiDhQAAJeAgAABmHeAAAFz5YAABterwAAAqT9AAKTmk4AAFc
V2QAAGRMBAAADHXgALyVAvAAJatBwAAACel8AABGFiwAAGtjsAAXF4z0A==
>>>>
>>>> From: "Kraucunas, Ian" <IKraucunas@nas.edu>
>>>> To: <k.briffa@uea.ac.uk>
>>>> X-UEA-Spam-Score: 0.0
>>>> X-UEA-Spam-Level: /
>>>> X-UEA-Spam-Flag: NO
>>>>
>>>> Dear Dr. Briffa,
>>>>
>>>> The National Research Council of The National Academies of the
United

>>>> States is empanelling a committee to study "Surface Temperature
>>>> Reconstructions for the Past 1,000-2,000 Years". The committee
>>>> will be
>>>> asked to summarize the current scientific information on the
>>>> temperature
>>>> record over the past two millennia, describe the proxy records that
>>>> have
>>>> been used to reconstruct pre-instrumental climatic conditions,
>>>> assess
>>>> the methods employed to combine multiple proxy data over large
>>>> spatial
>>>> scales, evaluate the overall accuracy and precision of such
>>>> reconstructions, and explain how central the debate over the
>>>> paleoclimate temperature record is to the state of scientific
>>>> knowledge
>>>> on global climate change. I have attached the complete study
>>>> proposal
>>>> (Word document).
>>>>
>>>> Since this issue has been the subject of considerable controversy,
>>>> we
>>>> have taken great care to assemble an unbiased panel of scientific
>>>> experts with the appropriate range of expertise to produce an
>>>> authoritative report on the subject. The committee slate will be
>>>> formally announced on Wednesday, but I can tell you that Jerry North
>>>> (Texas A&M) will be chairing the committee, and NAS Members Mike
>>>> Wallace, Karl Turekian, and Bob Dickinson will be on the panel, in
>>>> addition to a half-dozen other scientists with expertise in
>>>> statistics,
>>>> climate variability, and several different types of paleoclimate
>>>> proxy
>>>> data.
>>>>
>>>> The committee would like to invite you to come to Washington DC on
>>>> Thursday, March 2nd to speak about your extensive work with this
>>>> area
>>>> and to discuss your perspective on the issues noted above and in the
>>>> study proposal. The committee will be familiar with the relevant
>>>> peer-reviewed literature, but is also interested in any recently
>>>> submitted or accepted papers. We will be inviting 8-10 other
>>>> experts to
>>>> speak; a complete agenda will be made available prior to the
>>>> meeting,
>>>> and the meeting will be open to the public. Speakers will be
>>>> reimbursed
>>>> for travel expenses and invited to stay for the entire open session
>>>> of
>>>> the meeting (which will include a reception on Thursday evening and
>>>> a
>>>> few speakers on Friday morning).
>>>>
>>>> Thank you in advance for your time and interest, I hope that you are
>>>> available and willing to meet with our committee. If you are not
>>>> available on March 2nd, we have a limited number of timeslots

>>>> available
>>>> on March 3rd. We are trying to finalize the meeting schedule by
>>>> Friday
>>>> so please let me know if there is a particularly convenient time
>>>> that I
>>>> could call you this week to discuss details and answer any
>>>> questions you
>>>> might have (or feel free to call me directly).

>>>>

>>>> Sincerely,

>>>>

>>>> Ian Kraucunas

>>>>

>>>> ~~~

>>>> Ian Kraucunas, Ph.D.

>>>> Board on Atmospheric Sciences and Climate

>>>> National Research Council of The National Academies

>>>> 500 Fifth Street NW, Keck 705

>>>> Washington, DC 20001

>>>> Email: ikraucunas@nas.edu

>>>> Phone: (202) 334-2546

>>>> Fax: (202) 334-3825

>>>

>>>

>>> --

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>>> Climatic Research Unit

>>> University of East Anglia

>>> Norwich, NR4 7TJ, U.K.

>>>

>>> Phone: +44-1603-593909

>>> Fax: +44-1603-507784

>>>

>>> <http://www.cru.uea.ac.uk/cru/people/briffa/>

>>

>>

>>

>>

>> --

>> Michael E. Mann

>> Associate Professor

>> Director, Earth System Science Center (ESSC)

>>

>> Department of Meteorology

Phone: (814) 863-4075

>> 503 Walker Building

FAX: (814) 865-3663

>> The Pennsylvania State University

email: mann@psu.edu

>> University Park, PA 16802-5013

>>

>> <http://www.met.psu.edu/dept/faculty/mann.htm>

>>

>

> --

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--

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| | |
|-----------------------------------|---|
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| University Park, PA 16802-5013 | |

<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: "Wahl, Eugene R" <wahle@alfred.edu>
Subject: Re: FW: Wahl and Ammann ms 3321
Date: Mon, 13 Feb 2006 10:48:09 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

thanks Gene - let us know if you can get it in press. I think that's what he's saying. Best, peck

>Hi Peck and Caspar:

>

>Here is Steve Schneider's response to what "in press" means for Climatic
>Change. It is hopeful.

>

>OK Caspar, here we go! Let's do it.

>

>Peace, Gene

>

>

>*****

>

>-----Original Message-----

>From: Stephen H Schneider [mailto:shs@stanford.edu]

>Sent: Saturday, February 11, 2006 1:56 AM

>To: Wahl, Eugene R

>Cc: katarina kivel

>Subject: RE: Wahl and Ammann ms 3321

>

>your interpretation is fine--get me the revision soon so I have time to
>assess your responses in light of reviews in time! Look forward to
>recievieng it, Steve

>

>

>*****

>

>On Sat, 11 Feb 2006, Wahl, Eugene R wrote:

>

>> Hello Steve:

>>

>> Caspar and I expect to have the final manuscript to you in 7-10 days
>with all the revisions you requested in December. I have recently had

>some correspondance with Jonathan Overpeck about this, in his IPCC role.

>He says that the paper needs to be in press by the end of February to be

>acceptable to be cited in the SOD.

>>

>> He and I have communicated re: what "in press" means for Climatic

>Change, and I agreed to contact you to have a clear definition. What I

>have understood from our conversations before is that if you receive the

>mss and move it from "provisionally accepted" status to "accepted", then

>this can be considered in press, in light of CC being a journal of

>record.

>>

>> However, I recognize that this may not be a correct interpretation.

>If you can clarify, I'd be very grateful. Also, if I do have these

>definitions interpreted correctly--and if Caspar and I meet the target

>set above (paper to you by Feb 17-20)--is there any chance it might be

>fully "accepted" by the end of the month? I realize this is very close,

>for which I accept all responsibility. And of course, I also fully

>recognize that this kind of timeline is very likely out of the realm of

>possibility for you. I mean no pressure in asking, I only want to get

>info to then bring back to Peck.

>>

>> I hope this finds you well, and look forward to your response.

>>

>>

>> Peace, Gene

>> Dr. Eugene R. Wahl

>> Asst. Professor of Environmental Studies

>> Alfred University

>>

>> 607-871-2604

>> 1 Saxon Drive

>> Alfred, NY 14802

>>

>>

>

>-----

>Stephen H. Schneider

>Melvin and Joan Lane Professor for Interdisciplinary

> Environmental Studies;

>Professor, Department of Biological Sciences;

>Co-Director, Center for Environmental Science and Policy at

>the Stanford Institute for International Studies

>

>Mailing Address:

> Stephen Schneider

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>Gilbert Building

>371 Serra Mall

>Stanford University

>Stanford, CA 94305-5020 U.S.A.

>

>Tel: (650)725-9978

>Fax: (650)725-4387

>e-mail: shs@stanford.edu

>climate change website: <http://stephenschneider.stanford.edu>

> (or: climatechange.net)

>cancer book website: patientfromhell.org

--

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</x-flowed>

From: Anders Levermann <Anders.Levermann@pik-potsdam.de>
To: Fortunat Joos <joos@climate.unibe.ch>
Subject: Re: Millennium Simulations
Date: Mon, 13 Feb 2006 11:20:14 +0100
Cc: Jonathan Overpeck <jto@u.arizona.edu>, Stefan Rahmstorf
<rahmstorf@ozean-klima.de>, Anders Levermann <levermann@pik-potsdam.de>,
Eva Bauer <eva.bauer@pik-potsdam.de>, plattner@climate.unibe.ch, Eystein
Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>
Dear all,

here is the data from the Climber-3alpha simulations. I know they are too late, but perhaps there is still a way to include them. The structure of the files is the same as Eva's. The file names correspond to the ones you gave in the simulation protocol.

Cheers,
Anders

Fortunat Joos wrote:

> Dear all,
>
> Please find attached an update of the simulation protocol and input
> data description.
>
> Kasper Plattner pointed out that I forgot the obvious. We need of
> course a control run to correct for potential model drift. The readme
> file has been modified accordingly adding a brief description on how
> the control should be done.
>
> I am looking forward to any additional comments. Hope everything is
> clear.
>
> Kasper is currently working to perform the simulation with the
Bern2.5CC.
>
> Regards, Fortunat
>
> Fortunat Joos wrote:
>
>> Dear all,
>>
>> I have now compiled the input data set and written a protocol how to
>> perform the runs. It seems to me that it would make sense if we
>> perform the simulations first with the Bern Model and with the
>> Climber 2 model. We can then still decide if we need Climber 3.
>>
>> Please let me know if there are any questions.
>>

>> I could also provide files where the radiative forcing of solar,
>> volcanoes and non-CO2-anthropogenic has been added together.
>>
>> With best wishes,
>>
>> Fortunat
>>
>>
>>
>> Jonathan Overpeck wrote:
>>
>>> Dear Eva and Fortunat - thanks for working on getting things moving.
>>> It seems that the detailed forcing recommendations laid out below by
>>> Fortunat build nicely on what Eva first suggested, and that going
>>> with the forcing series suggested below by Foortunat (and the 6
>>> simulations) is going to be just right for the IPCC AR4 Chap 6
>>> needs. Does everyone agree?
>>>
>>> Thanks Fortunat for preparing/sharing the standard forcing series.
>>>
>>> Best, peck
>>>
>>>> Dear Eva,
>>>>
>>>> We are working on the forcing series and they should be ready by
>>>> the end of the week. Stefan assured us that you can run this
>>>> within a few hours.
>>>>
>>>> What we are preparing are the following series of radiative forcing
>>>> in W/m²:
>>>>
>>>> a) RF from atmospheric constituents (well-mixed GHGs (CO₂, CH₄,
>>>> N₂O, many Halocarbons) tropo and strato Ozone, various
>>>> anthropogenic aerosols) as used in the Bern CC TAR version and the
>>>> TAR (see Joos et al., GBC, 2001; pdf is on my homepage and TAR
>>>> appendix).
>>>> b) volcanic from Crowley, Sci, 2000
>>>> c) solar based on Lean and Bard et al.
>>>>
>>>> For the solar we will prepare 3 combinations:
>>>>
>>>> c1) original serie from Lean (2005) provided to you already
>>>> c2) Bard et al., Be-10 record linearly scaled to match the Maunder
>>>> Minimum Average of Lean-AR4
>>>> c3) Bard et al., Be-10 scaled to a MM reduction of 0.25 permil,
>>>> i.e. the low case in the Bard et, Tellus, publication corresponding
>>>> to the Lean et al, 1995 scaling
>>>>
>>>> For the RF by atmospheric components two cases are foreseen:
>>>> a1) standard case with reconstructed evolution over past 1150 years
>>>> a2) RF kept at 1765 value after 1765, i.e. a simulation with
>>>> natural forcings only.
>>>>
>>>> This will yield in total 6 simulations 3 over the full length from

>>>> 850 AD to 2000 and 3 brach-off simulatons from 1765 with natural
>>>> only forcing.
>>>>
>>>> An important point in IPCC is that things are published, consistent
>>>> among chapters, and it helps if approaches are tracable to earlier
>>>> accepted and approved IPCC work. The arguments for these series are
>>>> as follows:
>>>>
>>>> a) Considering as many components relevant for RF as possible (more
>>>> than just CO2). The series are fully compatible with TAR and that
>>>> the setup is tracable to the TAR for the industrial era increase.
>>>> The same series will be used in the projection chapter 10 for the
>>>> SRES calculation
>>>>
>>>> b) volcanic: a widely cited record
>>>>
>>>> c) solar: c1) and c3) are published series; c2 follows the same
>>>> approach and spirit as used to derive c3, i.e. scaling the Be-10
>>>> serie linearly with a given Maunder Minimum reduction. The impact
>>>> of the 11-yr solar cycle can be looked at in the original Lean-AR4
>>>> serie.
>>>>
>>>> I hope this help.
>>>>
>>>> With kind regards,
>>>>
>>>> Fortunat
>>>>
>>>> Eva Bauer wrote:
>>>>
>>>>>
>>>>> Dear Jonathan, dear Fortunat:
>>>>>
>>>>> Happy New Year!
>>>>>
>>>>>
>>>>> Stefan, Anders and me just have discussed how to set up our
>>>>> CLIMBER2/3alpha runs, to produce something useful for the IPCC WGI
>>>>> chapter 6. This chapter appears to touch the impact on the NH
>>>>> temperature related to low and high solar forcing.
>>>>>
>>>>> For a reasonable comparison, we think two 1000-year simulations
>>>>> differing only by a low and a high solar forcing, conducted with
>>>>> both
>>>>> CLIMBER models, would be ideal. To do so, we would have to extend
>>>>> the
>>>>> solar forcing time series based on Lean (GRL, 2000) and on Wang et
>>>>> al. (2005) distributed in previous e-mails back to the year 1000.
>>>>> This
>>>>> would require some splicing as was done, for instance, by Crowley.
>>>>>
>>>>> I'm thinking of some scaling applied to a series of Crowley (say
>>>>> the
>>>>> data called Be10/Lean splice in Science, 2000) such that the

>OVERVIEW

>-----

>

>A total of 7 simulations is planned.

>

>A control simulation without any forcing

>

>Two millennium-long simulations with solar forcing following Bard et al. with a Maunder Minimum reduction of 0.08 and 0.25 percent in total irradiance and volcanic and anthropogenic forcing included

>

>A simulation from 1610 to 1998 with solar forcing from Wang et al, 2005 and

>volcanic and anthropogenic forcing included

>

>Three simulations from 1765 to 1998 with only solar and volcanic forcing included, but no anthropogenic forcings. These are branches from the above three simulation.

>

>A range of input data files have been prepared. Each contains a header with additional descriptions of the data.

>

>Solar irradiance has been taken from Bard et al., Tellus, 1999 and from Wang, Lean, Shirley, JAp, 2005.

>

>It is estimated that the Maunder Minimum irradiance is reduce by 0.08 percent

>relative to today and that the present irradiance is 1366 W/m² from the Wang et al. data.

>

>A case with a Maunder Minimum reduction of 0.08 percent is calculated from the Bard et al. data by scaling the original Bard series appropriately.

>The original Bard series are offset by 1.3 W/m² in irradiance to bring them to

>a present irradiance of 1366 W/m². For this excercise we will utilize a Maunder

>Minimum reduction in irradiance relative to today of 0.08 percent and of 0.25 percent (other cases with high MM reduction are included in the files).

>

>Irradiance has been converted to radiative forcing: $RF = (IRR - 1366) / 4 * 0.7$

>

>Volcanic forcing is from Crowley Science, 2000, with albedo factored in (e.g. as for solar forcing). To avoid a cold start of the model, the serie is extended to 850 AD by mirroring the Crowley data from 1001 to 1150 to the period 850 to 1000.

>

>NonCO₂ forcing is following TAR (updated for an error in tropo O₃ in the TAR).

>

>CO₂ is a spline through the Etheridge, JGR, 97 data and the Siegenthaler, TELLUS, 2005 data.

>

```
>
>INPUT FILES DESCRIPTION:
>-----
>
>It is recommended to linearly interpolate between data points.
>
>A1: Solar irradiance and radiative forcing following Bard from 850 to
2000
>
>(Tag      description)
>solBard08  2. col: Maunder Minimum reduction of  0.08 percent
>solBard25  3. col: Maunder Minimu reduction of 0.25 percent
>
>Note: data from Bard have been linearlz interplated on an annual time
step
>
>  files:
>  bard00tel_solar_RF_IPCC_Chap6_Joos_11jan06.out
>  bard00tel_solar_irradiance_offset-13_IPCC_Chap6_Joos_11jan06.out
>
>
>A2: Solar irradiance and radiative forcing following Wang, Lean,
Shirley, 2005
>  from 1610 to 2004
>
>  annual resolution
>
>Tag: WLS-05
>
>  files:
>  wang05jastr_lean_RF_IPCC_chap6_Joos_11jan06.out
>  wang05jastr_lean_irradiance_IPCC_chap6_Joos_11jan06.out
>
>A3: CO2 concentration in ppm from 850 to 2000
>
>  annual resolution
>
>Tag: CO2
>  file: co2_850-2000_splined_IPCC_Chap6_Joos_11jan06.out
>
>A4: volcanic forcing after Crowley from 1001 to 1998 AD, extended by
artificial
>  data from 850 to 1000 AD by mirroring the forcing from 1000 to 1150
to the period 850 to 1000
>
>Tag: volcCrow
>
>  annual resolution
>
>  file: crowley00sci_RFvolcanic_IPCC_Chap6_Joos_11jan05.out
>
>A5: radiative forcing by non-CO2 agents
>
>  annual resolution
```

```
>
>Tag: nonco2
>
>   files
>   rf_nonco2_1yr_1765_2000_individ_IPCC_Chap6_Joos_11jan06.out
>   rf_nonco2_1yr_850_2000_IPCC_Chap6_Joos_11jan06.out
>
>
>
>B) SIMULATIONS
>-----
>
>B1. 2 Long simulations from 850 AD to 1998
>
>-----
>
>Simulation B1.1. tag: bard08_volcCrow_CO2_nonCO2_850-1998
>
>Solar forcing from Bard et al. with MM reduction of 0.08 percent,
volcanic forcing and forcing from CO2 and other anthropogenic (non-CO2)
agents.
>
>Start of simulation 850 AD
>End of simulation: 1998 AD
>initial condition: model spinup for year 850 (or similiar)
>
>Analysis period: 1001 AD to 1998 AD
>start-up period: 850 to 1000 with artificial volcanic data
>
>-----
>
>Simulation B1.2 tag: bard25_volcCrow_CO2_nonCO2_850-1998
>
>as B1.1 but with solar forcing from Bard et al. reduced by 0.25 percent
for the Maunder Minimum.
>
>Start of simulation 850 AD
>End of simulation: 1998 AD
>initial condition: model spinup for year 850 (or similiar)
>
>Analysis period: 1001 AD to 1998 AD
>start-up period: 850 to 1000 with artificial volcanic data
>
>-----
>
>Simulation B2: A simulation from 1610 to 1998 restarted from
bard08_volcCrow_CO2_nonCO2
>
>With solar forcing from Wang et al., 2005, volcanic forci
ng and forcing from CO2 and other anthropogenic (non-CO2) agents.
>
>B2 tag: WLS-2005_volcCrow_CO2_nonCO2_1610-1998
>
>Start of simulation: 1610 AD
```

```
>End of simulation: 1998 AD
>initial condition: restart from simulation B1.1.
bard08_volcCrow_CO2_nonCO2
>          at year 1610
>
>Analysis period: 1610 AD to 1998 AD
>
>
>-----
>
>B3: 3 Simulations from 1765 to 1998 with natural forcing only
>
>    non-CO2 radiative forcing is kept to zero
>    (except for volcanoes and solar)
>
>    CO2 is kept at its 1765 value.
>
>Simulation B3.1: tag bard08_volcCrow_1765_1998
>
>Start of simulation: 1765 AD
>End of simulation: 1998 AD
>initial condition: restart from simulation B1.1.
bard08_volcCrow_CO2_nonCO2
>          at year 1765
>
>Analysis period: 1765 to 1998 AD
>
>-----
>
>Simulation B3.2: tag bard25_volcCrow_1765_1998
>
>Start of simulation: 1765 AD
>End of simulation: 1998 AD
>initial condition: restart from simulation B1.2.
bard25_volcCrow_CO2_nonCO2
>          at year 1765
>
>Analysis period: 1765 to 1998 AD
>
>-----
>
>Simulation B3.1: tag WLS-2005_volcCrow_1765_1998
>
>Start of simulation: 1765 AD
>End of simulation: 1998 AD
>initial condition: restart from simulation B2. WLS-
2005_volcCrow_CO2_nonCO2
>          at year 1765
>
>Analysis period: 1765 to 1998 AD
>
>-----
>
>Simulation B4: tag ctrl_850-1998
```

>
>Control simulation without any forcing
>
>Start of simulation 850 AD
>End of simulation: 1998 AD
>initial condition: model spinup for year 850 (or similiar)
>
>Analysis period: 850 to 1998
>
>
>OUTPUT
>-----
>
>I guess minimal output is global and NH mean surface temperature.
>
>

--

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</x-flowed>

Attachment Converted: "c:\eudora\attach\c3a_b1_1.dat"

Attachment Converted: "c:\eudora\attach\c3a_b1_2.dat"

Attachment Converted: "c:\eudora\attach\c3a_b2.dat"

Attachment Converted: "c:\eudora\attach\c3a_b3_1.dat"

Attachment Converted: "c:\eudora\attach\c3a_b3_2.dat"

Attachment Converted: "c:\eudora\attach\c3a_b3_3.dat"

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Henry Pollack <hpollack@umich.edu>

Subject: Re: Fwd: [Wg1-ar4-ch06] SOD- template and FOD document

Date: Mon, 13 Feb 2006 12:15:06 -0700

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Henry (and Keith) - thanks for the quick effort! Regarding your comments, here's some feedback - it's good Keith beat me too it.

1. For Fig. 6.9b, there is a new version that resulted in lots of discussion at our last meeting. Keith can elaborate when he has time (we're pushing him real hard now for the SOD text), but we agree the caption has to be clear.
2. I'm worried about your discussion of southern hemisphere records, and trust Keith will get it right. Too bad your paper isn't in press too - it would be nice to include.
3. Hope you can help Keith with uncertainty prose. We are over length and hence we can't have more figures (e.g., with confidence intervals shown for all data). Please help him work it into the SOD text.
4. It is unclear if we'll have time for review of the whole chapter, but I'm still hoping Keith will send you all of Section 6.6 to look at. That assumes he has it done today or very soon at least. The more people that can look at text the better, but we also have to get the draft done - it can then be reviewed, and we will make sure CAs get to review in a more timely fashion this time.

Thanks again, Peck

>Hi Peck, Eystein and Keith,

>

>Attached in Borehole SOD.doc is a 'rewrite' of the borehole stuff. You
>will recognize the 'rewrite', as it still addresses everything in
>the FOD draft sent to me, with much the same language. It is, however,
>an improvement in
>structure, and has a more balanced discussion. Keith, if you want more
>insight into why I
>have presented the material this way, I'll be happy to elaborate.
>
>The rewrite occupies lines 32-57 of page 6-30 SOD and lines 1-12 of page 6-31.
>
>Also attached is the full SOD template with the 'rewrite' and
>references inserted. It is not clear from your instructions that you
>wanted this to be done, but now you have it if you want it.
>
>Also attached are my replies to the reviewers of the FOD.
>
>I am sending everything today (Sunday), so everyone will get it as
>early as possible.
>
>Some additional comments in areas outside the narrowly defined
>'borehole' section:
>
>In Figure 6.9b, I recommend removing the instrumental record prior to
>1860, because it
>apparently represents only four European stations. The figure is
>captioned to represent
>the entire northern hemisphere.
>
>In section 6.6.2 Southern Hemisphere Temperature Variability page 6-32,
>lines 56-57: The
>two geothermal reconstructions shown, for southern Africa and
>Australia, do NOT indicate
>unusually warm conditions prevailing in the 20th century. Both
>reconstructions miss the
>rapid warming in the last two decades of the 20th century because many
>of the boreholes
>were logged prior to that excursion. The two reconstructions do match
>well the pre-1980
>SAT trends. I discuss this in a paper now in review by J. Quaternary
>Sci., titled "Five
>centuries of climate change in Australia: the view from underground."
>The southern
>hemisphere is NOT discussed in Pollack and Smerdon (2004), which you

>have cited there.

>

>If you will find it helpful, I can scan the entire chapter and provide

>comments, but

>perhaps that could wait until you have passed the immediate deadline in

>front of you.

>

>Cheers,

>Henry

>

>

> ___ ___ Henry N. Pollack

>[\ /] Professor of Geophysics

> | \ | Department of Geological Sciences

> [MICHIGAN] University of Michigan

>[___]V[___] Ann Arbor, Michigan 48109-1005, U.S.A.

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> URL: www-personal.umich.edu/~hpollack/book.html

>

>-----

>Quoting Jonathan Overpeck <jto@u.arizona.edu>:

>

>>Hi Henry - yes, it's true, but that's why we all get things done. Thanks.

>>

>>We have a serious space problem with the chapter, and need to

>>generally reduce it's size. However, if you need a couple more lines

>>to do it well, and to get the proper refs in there (there are

>>undoubtedly new ones?), you may do so. We can always cut later... (so

>>don't add more than just a few lines max).

>>

>>As soon as you're done, pls email to me, Eystein and Keith. The

>>sooner Keith can finish the complete section, the sooner we can all

>>look at it and edit.

>>

>>The NAS/NRC mtg is at a crappy time. I can't travel then since I'm

>>alone w/ the kids, but I've been discussing helping by phone if

>>possible. The problem is that March 3 (the day they really want my

>>input) is the deadline for the SOD. If it's anything like last time

>>(FOD), I won't have time but for a quick trip to the bathroom now and

>>then to recycle coffee. But, I'm glad to hear you're in the loop. I

>>might still be able to help, since we're trying to do this so it
>>isn't a madhouse at the very end.

>>

>>Best, peck

>>

>>>Hi Peck,

>>>

>>>Yes, I will be working weekends -- don't we always??

>>>

>>>Are you attending the NAS/NRC hearing on surface temperature
>>>reconstructions on March 2?

>>>

>>>I will take you up on the invitation to (re)write the 40 lines of the
>>>borehole section.

>>>

>>>Cheers,

>>>Henry

>>> ___ ___ Henry N. Pollack

>>>[\ /] Professor of Geophysics

>>> | \ | Department of Geological Sciences

>>> [MICHIGAN] University of Michigan

>>> [___] \ [___] Ann Arbor, Michigan 48109-1005, U.S.A.

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>>>

>>>

>>>Quoting Jonathan Overpeck <jto@u.arizona.edu>:

>>>

>>>>Hi Henry - see the notes below on how to best update your section

>>>>using the attached files (and comments you already have).

>>>>

>>>>Julie is flying to Germany tomorrow, so I'll be single-parenting and

>>>>my email will be at night on the weekend. If you have urgent need for

>>>>input, you can call me:

>>>>

>>>>970-728-0780 (home)

>>>>520-907-6480 (cell - only good if I'm in town - best to use home on

>>>>weekends, and cell weekdays)

>>>>

>>>>Thanks again, peck

>>>>
>>>>>X-Sieve: CMU Sieve 2.2
>>>>>Date: Tue, 17 Jan 2006 08:59:33 +0100
>>>>>To: wg1-ar4-ch06@joss.ucar.edu
>>>>>From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
>>>>>Subject: [Wg1-ar4-ch06] SOD- template and FOD document
>>>>>X-BeenThere: wg1-ar4-ch06@joss.ucar.edu
>>>>>List-Id: <wg1-ar4-ch06.joss.ucar.edu>
>>>>>List-Help: <mailto:wg1-ar4-ch06-request@joss.ucar.edu?subject=help>
>>>>>List-Post: <mailto:wg1-ar4-ch06@joss.ucar.edu>
>>>>>List-Subscribe: <http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>,
>>>>> <mailto:wg1-ar4-ch06-request@joss.ucar.edu?subject=subscribe>
>>>>>List-Archive: <http://www.joss.ucar.edu/mailman/private/wg1-ar4-ch06>
>>>>>List-Unsubscribe:
>>>>><http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>,
>>>>> <mailto:wg1-ar4-ch06-request@joss.ucar.edu?subject=unsubscribe>
>>>>>Sender: wg1-ar4-ch06-bounces@joss.ucar.edu

>>>>>
>>>>>Dear friends,
>>>>>In preparation for your rewriting of the FOD as SOD, we send you
>>>>>the following documents.
>>>>>1. A new template for the FOD which is restructured so that the
>>>>>decisions on structure we made in Christchurch have been taken into
>>>>>account. We also send you the word version of the FOD which is the
>>>>>final version used for the review, in case you do not have this.
>>>>>This is the version for which the comments refer to.
>>>>>In the rewriting we would ask you to rewrite into the SOD template
>>>>>document, thus:
>>>>>1. Find the relevant comment or section to be rewritten in the FOD.
>>>>>2. Then the corresponding section in the SOD document, and rewrite
>>>>>the text there. References should also be inserted into the SOD
>>>>>document.
>>>>>You have to work in parallel with both documents, but we do not see
>>>>>any way around this in order to arrive at a SOD without too many
>>>>>problems of technical sort.

>>>>>
>>>>>Cheers, and best luck.
>>>>>Peck and Eystein
>>>>>--

>>>>>

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>>>>>

>>>>>Wg1-ar4-ch06 mailing list
>>>>>Wg1-ar4-ch06@joss.ucar.edu
>>>>><http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06>
>>>>>
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>Attachment converted: Macintosh HD:Boreholes SOD.doc (WDBN/«IC») (001131FA)
>Attachment converted: Macintosh HD:Ch06_SOD_1A 2.doc (WDBN/«IC») (001131FC)
>Attachment converted: Macintosh HD:Pollack_comm.doc (WDBN/«IC») (00113211)

--
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</x-flowed>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: update
Date: Tue, 14 Feb 2006 08:27:43 -0500
Reply-to: mann@psu.edu
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>

<x-flowed>

Hi Tim,

Thanks, I agree. I don't think there is any need for you/Keith to do this. We've pretty much got things under control at RC and it is probably wise to hold your ammunition for any possible comment to Science. In my view the McIntyre criticisms are weak and disingenuous. But what's new w/ that?

mike

Tim Osborn wrote:

> Hi Mike and Gavin,
>
> thanks for the things that are doing at RC, it has developed into an
> excellent resource for this type of situation. I think we'll hold off
> from posting any reply to criticisms for the moment, I somehow don't
> think that we would even then make much headway with the hard-core
> critics. They might even submit some formal criticism to Science and
> we can reserve our response for that if they do.
>
> So, no need to hold up any comments etc., we'll just let things run.
> Sorry if this puts the onus upon you or others at RC, but the comments
> on this particular thread seem to be petering out anyway, so hopefully
> not too much left to deal with.
>
> Best wishes and thanks for your support,
>
> Tim
>
> At 21:51 09/02/2006, Michael E. Mann wrote:
>
>> guys, I see that Science has already gone online w/ the new issue, so
>> we put up the RC post. By now, you've probably read that nasty

>> McIntyre thing. Apparently, he violated the embargo on his website (I
>> don't go there personally, but so I'm informed).

>>

>> Anyway, I wanted you guys to know that you're free to use RC in any
>> way you think would be helpful. Gavin and I are going to be careful
>> about what comments we screen through, and we'll be very careful to
>> answer any questions that come up to any extent we can. On the other
>> hand, you might want to visit the thread and post replies yourself.
>> We can hold comments up in the queue and contact you about whether or
>> not you think they should be screened through or not, and if so, any
>> comments you'd like us to include.

>>

>> You're also welcome to do a followup guest post, etc. think of RC as
>> a resource that is at your disposal to combat any disinformation put
>> forward by the McIntyres of the world. Just let us know. We'll use
>> our best discretion to make sure the skeptics don't get to use the RC
>> comments as a megaphone...

>>

>> mike

>>

>>

>>

>>

>> --

>> Michael E. Mann

>> Associate Professor

>> Director, Earth System Science Center (ESSC)

>>

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>>

>

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Fortunat Joos <joos@climate.unibe.ch>
Subject: Re: Fwd: some figures at last!
Date: Tue, 14 Feb 2006 10:56:19 -0700
Cc: rahmstorf@ozean-klima.de, Eystein Jansen <eystein.jansen@geo.uib.no>, t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi all - I commented on the reference period issue in my previous email, and hope we can resolve it today, or tomorrow at the latest? Tim and Keith should help convince Fortunat that their choice is strong.

Tim - can you make the other changes suggested by Fortunat?

Thanks, peck

>Hi,

>

>I have now found the time to look over the figures. First
>congratulations to this effort. Looks great! A tremendous job - I
>assume many hours of work.

>

>I have, however, a few points

>

>1) The instrumental record - our best piece of information is
>missing in panel e. Please add to the EMIC panel.

>

>2) I am not very enthusiastic to normalize model results with
>respect to 1500-1899. The EMIC panel is to illustrate two points -
>the difference between low and high solar forcing and with/without
>anthropogenic forcing.

>

>I think panel e (EMIC panel) would be more informative in this
>respect if all runs with anthropogenic forcing and the proxies are
>normalized as in panel b) (19061-1990) and the runs without anth.
>forcing start at the same point as the ones with anth. forcing

>

>I have no strong opinion on panel d.

>

>3) Please change Bern2.5c to Bern2.5CC

>

>Thanks for considering this.

>

>Best regards,

>

>Fortunat

>

>Jonathan Overpeck wrote:

>>Hi Stefan and Fortunat: Attached are the draft figs that include
>>proxy obs, simulations, and comparisons of the two. As you can see,
>>Tim just sent them. Big job, but they look great in my eyes.

>>

>>See Tim's email below for more background info.

>>

>>We need fast feedback from you both, specifically:

>>

>>1) any general comments on the figs - this is a crux set of figures

>>and we need your eyes to look at them carefully

>>

>>2) is it wise to keep the new EMIC run panel attached to the second

>>figure as attached? I vote yes, but what do you think. It fits w/

>>the other panels pretty well.

>>

>>3) either way, we need caption prose from you (perhaps Fortunat

>>start, and Stefan edit, or vice versa if Stefan can start first) on

>>the new EMIC panel.

>>

>>4) also, we need a new para, or prose that can be added to a para,

>>that describes the panel and it's implications as it informs our

>>assessment. Keith will then integrate this into the section. I'm

>>not sure of this, but perhaps you could start with a new question

>>heading, and then have a short para to go under it - something like

>>"What is the significance of the new reduced-amplitude estimates of

>>past solar variability?"

>>

>>Of course, we need your feedback and prose asap. Please send to me,

>>Eystein, Keith and Tim.

>>

>>Thanks in advance for the help. Best, peck

>>

>>>X-Sieve: CMU Sieve 2.2

>>>Date: Fri, 10 Feb 2006 18:00:19 +0000

>>>To: Jonathan Overpeck <jto@u.arizona.edu>,

>>> Eystein Jansen <eystein.jansen@geo.uib.no>

>>>From: Tim Osborn <t.osborn@uea.ac.uk>

>>>Subject: some figures at last!

>>>Cc: Keith Briffa <k.briffa@uea.ac.uk>

>>>X-UEA-Spam-Score: -102.8

>>>X-UEA-Spam-Level: -----

>>>X-UEA-Spam-Flag: NO

>>>

>>>Dear Peck and Eystein,

>>>

>>>the attached word file contains the latest versions of two of our figures.

>>>

>>>First, is the reconstructions with many requests now done: linear

>>>time scale, dotted early instrumental temperatures not solid line,

>>>Oerlemans added, new panel showing shading for the overlapping

>>>regions of temperature reconstructions.

>>>

>>>Second, is the forcings and models. Stendel ECHAM simulation

>>>added (1500-2000). New ECHO-G Erik2 simulation just published in

>>>GRL from Gonzalez-Ruoco et al. added (1000-1990). Reconstruction

>>>"envelope" replaced by new shading of overlaps in the temperature

>>>reconstructions. Correction of some labelling errors. Those runs

>>>that did not include 20th century sulphate aerosol cooling are

>>>dotted or dashed after 1900 (the two low ones also omitted CH4,

>>>N2O, CFCs, O3, hence still cool despite omitting aerosol cooling).

>>>The ECHO-G Erik1 simulation with the very out-of-equilibrium

>>>initial conditions is dashed. Finally, the extra panel with the
>>>new EMIC runs is included as panel (e), again with the new shading
>>>of overlapping temperature reconstructions.

>>>
>>>Keith suggests sending to Stefan and Fortunat too for their views
>>>- can you do that (they may now be gone for the weekend, of
>>>course).

>>>
>>>Best wishes and sorry this is late. Am I right in thinking that
>>>the only other possible-TS figure is the location maps? Still
>>>working on those (had very little time in last 2 days due to media
>>>etc. attention re. Science paper).

>>>
>>>Cheers

>>>
>>>Tim

>>>
>>>
>>>Dr Timothy J Osborn
>>>Climatic Research Unit
>>>School of Environmental Sciences, University of East Anglia
>>>Norwich NR4 7TJ, UK

>>>
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>>
>
>--
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--
Jonathan T. Overpeck
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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: c.goodess@uea.ac.uk,k.briffa@uea.ac.uk
Subject: Fwd: Invitation to an EU project
Date: Tue, 14 Feb 2006 13:47:11 +0000

Clare, Keith,
Any thoughts on this?
Phil

From: "Andras Vag" <andras.vag@atlasco.hu>
To: <p.jones@uea.ac.uk>
Subject: Invitation to an EU project
Date: Tue, 14 Feb 2006 13:00:25 +0100
Organization: ATLAS
X-Mailer: Microsoft Outlook Express 6.00.2900.2180
X-UEA-Spam-Score: 0.1
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO

Dear Prof. Jones

My name is Andras Vag, I am working for a Hungarian organization (ATLAS Innoglobe), which deals with environmental consultancy.

We are preparing an EU project proposal for the following call: Scientific Support to Policies, Identifier: [FP6-2005-SSP-5-A] Budget: 77 million

Closing Date(s): 22 March 2006 at 17.00 (Brussels local time)

Specific programme: [Integrating and Strengthening the European Research Area] ,
Activity area(s): [Policy-orientated research]

[1]http://fp6.cordis.europa.eu.int/index.cfm?fuseaction=UserSite.FP6DetailsCallPage&call_id=268

Are you / CRU is interested in the cooperation? The co-work with you would be a great honour for us and definitely would improve the quality of the project.

Please see the attached Letter of Invitation to the planned project. I hope you like the idea.

Best wishes

Andras Vag
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Hungary
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Attachment Converted: "c:\eudora\attach\LetterOfInvitation.pdf"

References

1. file:///localhost/tmp/convertmbox13876.html??
2. file:///localhost/tmp/convertmbox13876.html??

From: Fortunat Joos <joos@climate.unibe.ch>
To: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Fwd: Re: Millennium Simulations
Date: Wed, 15 Feb 2006 08:25:27 +0100
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Anders Levermann <levermann@pik-potsdam.de>, Eva Bauer <eva.bauer@pik-potsdam.de>, plattner@climate.unibe.ch, Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, oyvind.paasche@bjerknes.uib.no

<x-flowed>

O.k. EMIC caption noted. Can go with the 1500-1899 ref period.

Stefan, Anders, and Eva can you provide me the appropriate references for your models and the official names.

Regards, Fortunat

Jonathan Overpeck wrote:

> Hi Tim and Fortunat: This looks nice (thanks) and my slight bias is that
> we should include the Climber3a results. What do you think, Fortunat? I
> think Stefan likes it based on his email.
>
> Regarding the reference period, I would side w/ Tim and Keith on using
> 1500-1899. We need to use the same ref period for everything on these
> two figs (obs and forcing/simulations), and I think the EMIC panel
still
> convey's the main message. Keith/Tim/Fortunat - we have to resolve this
> FAST, so please weigh in more on this issue. Thanks.
>
> Regarding captions, yes, you should do all but the EMICS, and you
should
> make sure you send to Stefan so he can help make sure it makes sense
> (e.g., the red/grey shading). We have asked Fortunat to do the EMIC
> caption. Can you do this Fortunat? Thanks.
>
> Best, Peck
>
>
>
>
>> Dear all,
>>
>> please see the attached diagram (both the same, PDF or GIF) with all
>> three EMICs on now. Climber3a seems to lie between Climber2 and
>> Bern2.5CC mostly. Does it add to the message of the figure to use all
>> three? If so, please use this version from now on, for drafting
>> captions etc.
>>
>> Nobody said much about the previous version, so hopefully this
>> indicates general agreement! I didn't show the "Bard08" runs, because
>> they were so close to the runs I have labelled "WLS", but of course in
>> those runs the pre-1610 solar forcing is Bard08 - so maybe the labels

>> should be altered to somehow indicate them, or this could just be
>> stated in the caption.
>>
>> Am I right that Keith and I need to provide an updated caption for
>> panels (a)-(d), but that someone else will write a caption for the
>> EMIC panel (e)?
>>
>> Cheers
>>
>> Tim
>>
>> At 19:20 13/02/2006, Jonathan Overpeck wrote:
>>
>>> Hi Anders and Tim - It could be too late, but this is up to Tim. Can
>>> you get these data onto the new EMIC panel? I think it'd be worth
>>> it, but only if you and Keith can get everything else done first.
>>> Best make sure you have all the data needed, just in case.
>>>
>>> thanks Anders too.
>>>
>>> best, peck
>>>
>>>> X-Sieve: CMU Sieve 2.2
>>>> Date: Mon, 13 Feb 2006 11:20:14 +0100
>>>> From: Anders Levermann <Anders.Levermann@pik-potsdam.de>
>>>> Organization: PIK
>>>> X-Accept-Language: en-us, en
>>>> To: Fortunat Joos <joos@climate.unibe.ch>
>>>> Cc: Jonathan Overpeck <jto@u.arizona.edu>,
>>>> Stefan Rahmstorf <rahmstorf@ozean-klima.de>,
>>>> Anders Levermann <levermann@pik-potsdam.de>,
>>>> Eva Bauer <eva.bauer@pik-potsdam.de>,
>>>> plattner@climate.unibe.ch,
>>>> Eystein Jansen <eystein.jansen@geo.uib.no>,
>>>> Keith Briffa <k.briffa@uea.ac.uk>
>>>> Subject: Re: Millennium Simulations
>>>>
>>>> Dear all,
>>>>
>>>> here is the data from the Climber-3alpha simulations. I know they
>>>> are too late, but
>>>> perhaps there is still a way to include them. The structure of the
>>>> files is the
>>>> same as Eva's. The file names correspond to the ones you gave in the
>>>> simulation
>>>> protocol.
>>>>
>>>> Cheers,
>>>> Anders
>>>>
>>>> Fortunat Joos wrote:
>>>>
>>>>> Dear all,
>>>>>

>>>> Please find attached an update of the simulation protocol and input
>>>> data description.
>>>>
>>>> Kasper Plattner pointed out that I forgot the obvious. We need of
>>>> course a control run to correct for potential model drift. The
>>>> readme file has been modified accordingly adding a brief
>>>> description on how the control should be done.
>>>>
>>>> I am looking forward to any additional comments. Hope everything is
>>>> clear.
>>>>
>>>> Kasper is currently working to perform the simulation with the
>>>> Bern2.5CC.
>>>>
>>>> Regards, Fortunat
>>>>
>>>> Fortunat Joos wrote:
>>>>
>>>>> Dear all,
>>>>>
>>>>> I have now compiled the input data set and written a protocol how
>>>>> to perform the runs. It seems to me that it would make sense if we
>>>>> perform the simulations first with the Bern Model and with the
>>>>> Climber 2 model. We can then still decide if we need Climber 3.
>>>>>
>>>>> Please let me know if there are any questions.
>>>>>
>>>>> I could also provide files where the radiative forcing of solar,
>>>>> volcanoes and non-CO2-anthropogenic has been added together.
>>>>>
>>>>> With best wishes,
>>>>>
>>>>> Fortunat
>>>>>
>>>>>
>>>>>
>>>>> Jonathan Overpeck wrote:
>>>>>
>>>>>> Dear Eva and Fortunat - thanks for working on getting things
>>>>>> moving. It seems that the detailed forcing recommendations laid
>>>>>> out below by Fortunat build nicely on what Eva first suggested,
>>>>>> and that going with the forcing series suggested below by
>>>>>> Foortunat (and the 6 simulations) is going to be just right for
>>>>>> the IPCC AR4 Chap 6 needs. Does everyone agree?
>>>>>>
>>>>>> Thanks Fortunat for preparing/sharing the standard forcing
>>>>>> series.
>>>>>>
>>>>>> Best, peck
>>>>>>
>>>>>>> Dear Eva,
>>>>>>>
>>>>>>> We are working on the forcing series and they should be ready by
>>>>>>> the end of the week. Stefan assured us that you can run this

>>>>>>> within a few hours.
>>>>>>>
>>>>>>> What we are preparing are the following series of radiative
>>>>>>> forcing in W/m2:
>>>>>>>
>>>>>>> a) RF from atmospheric constituents (well-mixed GHGs (CO2, CH4,
>>>>>>> N2O, many Halocarbons) tropo and strato Ozone, various
>>>>>>> anthropogenic aerosols) as used in the Bern CC TAR version and
>>>>>>> the TAR (see Joos et al., GBC, 2001; pdf is on my homepage and
>>>>>>> TAR appendix).
>>>>>>> b) volcanic from Crowley, Sci, 2000
>>>>>>> c) solar based on Lean and Bard et al.
>>>>>>>
>>>>>>> For the solar we will prepare 3 combinations:
>>>>>>>
>>>>>>> c1) original serie from Lean (2005) provided to you already
>>>>>>> c2) Bard et al., Be-10 record linearly scaled to match the
>>>>>>> Maunder Minimum Average of Lean-AR4
>>>>>>> c3) Bard et al., Be-10 scaled to a MM reduction of 0.25 permil,
>>>>>>> i.e. the low case in the Bard et, Tellus, publication
>>>>>>> corresponding to the Lean et al, 1995 scaling
>>>>>>>
>>>>>>> For the RF by atmospheric components two cases are foreseen:
>>>>>>> a1) standard case with reconstructed evolution over past 1150
years
>>>>>>> a2) RF kept at 1765 value after 1765, i.e. a simulation with
>>>>>>> natural forcings only.
>>>>>>>
>>>>>>> This will yield in total 6 simulations 3 over the full length
>>>>>>> from 850 AD to 2000 and 3 brach-off simulatons from 1765 with
>>>>>>> natural only forcing.
>>>>>>>
>>>>>>> An important point in IPCC is that things are published,
>>>>>>> consistent among chapters, and it helps if approaches are
>>>>>>> tracable to earlier accepted and approved IPCC work. The
>>>>>>> arguments for these series are as follows:
>>>>>>>
>>>>>>> a) Considering as many components relevant for RF as possible
>>>>>>> (more than just CO2). The series are fully compatible with TAR
>>>>>>> and that the setup is tracable to the TAR for the industrial era
>>>>>>> increase. The same series will be used in the projection chapter
>>>>>>> 10 for the SRES calculation
>>>>>>>
>>>>>>> b) volcanic: a widely cited record
>>>>>>>
>>>>>>> c) solar: c1) and c3) are published series; c2 follows the same
>>>>>>> approach and spirit as used to derive c3, i.e. scaling the Be-10
>>>>>>> serie linearly with a given Maunder Minimum reduction. The
>>>>>>> impact of the 11-yr solar cycle can be looked at in the original
>>>>>>> Lean-AR4 serie.
>>>>>>>
>>>>>>> I hope this help.
>>>>>>>
>>>>>>> With kind regards,

>>>>>>>
>>>>>>> Fortunat
>>>>>>>
>>>>>>> Eva Bauer wrote:
>>>>>>>
>>>>>>>
>>>>>>> Dear Jonathan, dear Fortunat:
>>>>>>>
>>>>>>> Happy New Year!
>>>>>>>
>>>>>>>
>>>>>>> Stefan, Anders and me just have discussed how to set up our
>>>>>>> CLIMBER2/3alpha runs, to produce something useful for the IPCC
WGI
>>>>>>> chapter 6. This chapter appears to touch the impact on the NH
>>>>>>> temperature related to low and high solar forcing.
>>>>>>>
>>>>>>> For a reasonable comparison, we think two 1000-year simulations
>>>>>>> differing only by a low and a high solar forcing, conducted
>>>>>>> with both
>>>>>>> CLIMBER models, would be ideal. To do so, we would have to
>>>>>>> extend the
>>>>>>> solar forcing time series based on Lean (GRL, 2000) and on Wang
et
>>>>>>> al. (2005) distributed in previous e-mails back to the year
>>>>>>> 1000. This
>>>>>>> would require some splicing as was done, for instance, by
Crowley.
>>>>>>>
>>>>>>> I'm thinking of some scaling applied to a series of Crowley
>>>>>>> (say the
>>>>>>> data called Bel0/Lean splice in Science, 2000) such that the
>>>>>>> amplitude
>>>>>>> of the solar variability from the 11-year cycle is conserved
after
>>>>>>> ~1720. I have to check but it appears that the variation in the
>>>>>>> TSI
>>>>>>> due to the 11-year cycle contained in the Crowley series agrees
>>>>>>> perfectly with the 11yr-cycle data in the file based on Lean
>>>>>>> (2000).
>>>>>>> Before starting such an exercise I like to ask you what you
think
>>>>>>> about. We would be happy to receive your response quite soon to
be
>>>>>>> able to finish the calculations with our slow model in time for
>>>>>>> the
>>>>>>> IPCC report.
>>>>>>>
>>>>>>> Could you please also comment on the other forcings we should
>>>>>>> include,
>>>>>>> namely the volcanic forcing and the CO2 forcing. For the
>>>>>>> present study
>>>>>>> we suggest to use the forcing as in Bauer et al (2000) but
>>>>>>> omitting

>>>>>>> the land-use. This means, using the volcanic forcing from
Crowley,
>>>>>>> 2000 and the CO2 forcing based on Etheridge et al 1996 and
>>>>>>> Keeling and
>>>>>>> Whorf, 1996. (If you wish we can distribute these data
series.)
>>>>>>>
>>>>>>> Also, thinking beyond the IPCC study, the model results may
become
>>>>>>> interesting enough to be discussed in a 3-model comparison
study!?
>>>>>>>
>>>>>>> Looking forward to your reply.
>>>>>>>
>>>>>>> Best wishes
>>>>>>>
>>>>>>> Eva
>>>>>>>
>>>>>>>
>>>>>>> --
>>>>>>>
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>>>>>>> Physics Institute, University of Bern
>>>>>>> Sidlerstr. 5, CH-3012 Bern
>>>>>>> Phone: ++41(0)31 631 44 61 Fax: ++41(0)31 631 87
42
>>>>>>> Internet: <http://www.climate.unibe.ch/~joos/>
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>>>>>>>
>>>>>>> -----

>>>>>
>>>>>
>>>>> Last Millennium Simulations for IPCC AR4 WG1 Chap 6
>>>>> -----
>>>>>
>>>>> F. Joos,
>>>>> joos@climate.unibe.ch
>>>>> 18 Januar 2006
>>>>>
>>>>> OVERVIEW
>>>>> -----
>>>>>
>>>>> A total of 7 simulations is planned.
>>>>> A control simulation without any forcing
>>>>>
>>>>> Two millennium-long simulations with solar forcing following Bard
>>>>> et al. with a Maunder Minimum reduction of 0.08 and 0.25 percent in
>>>>> total irradiance and volcanic and anthropogenic forcing included
>>>>> A simulation from 1610 to 1998 with solar forcing from Wang et al,

```

>>>> 2005 and volcanic and anthropogenic forcing included
>>>>
>>>> Three simulations from 1765 to 1998 with only solar and volcanic
>>>> forcing included, but no anthropogenic forcings. These are branches
>>>> from the above three simulation.
>>>>
>>>> A range of input data files have been prepared. Each contains a
>>>> header with additional descriptions of the data.
>>>> Solar irradiance has been taken from Bard et al., Tellus, 1999 and
>>>> from Wang, Lean, Shirley, JAp, 2005.
>>>>
>>>> It is estimated that the Maunder Minimum irradiance is reduce by
>>>> 0.08 percent
>>>> relative to today and that the present irradiance is 1366 W/m2 from
>>>> the Wang et al. data.
>>>>
>>>> A case with a Maunder Minimum reduction of 0.08 percent is
>>>> calculated from the Bard et al. data by scaling the original Bard
>>>> series appropriately.
>>>> The original Bard series are offset by 1.3 W/m2 in irradiance to
>>>> bring them to a present irradiance of 1366 W/m2. For this excercise
>>>> we will utilize a Maunder
>>>> Minimum reduction in irradiance relative to today of 0.08 percent
>>>> and of 0.25 percent (other cases with high MM reduction are
>>>> included in the files).
>>>>
>>>> Irradiance has been converted to radiative forcing: RF=
>>>> (IRR-1366)/4*0.7
>>>> Volcanic forcing is from Crowley Science, 2000, with albedo
>>>> factored in (e.g. as for solar forcing). To avoid a cold start of
>>>> the model, the serie is extended to 850 AD by mirroring the Crowley
>>>> data from 1001 to 1150 to the period 850 to 1000.
>>>> NonCO2 forcing is following TAR (updated for an error in tropo O3
>>>> in the TAR).
>>>> CO2 is a spline through the Etheridge, JGR, 97 data and the
>>>> Siegenthaler, TELLus, 2005 data.
>>>>
>>>>
>>>> INPUT FILES DESCRIPTION:
>>>> -----
>>>>
>>>> It is recommended to linearly interpolate between data points.
>>>>
>>>> A1: Solar irradiance and radiative forcing following Bard from 850
>>>> to 2000
>>>> (Tag      description)
>>>> solBard08  2. col: Maunder Minimum reduction of  0.08 percent
>>>> solBard25  3. col: Maunder Minimu reduction of 0.25 percent
>>>>
>>>> Note: data from Bard have been linearlz interplated on an annual
>>>> time step
>>>> files:
>>>>   bard00tel_solar_RF_IPCC_Chap6_Joos_11jan06.out
>>>>   bard00tel_solar_irradiance_offset-13_IPCC_Chap6_Joos_11jan06.out

```

```

>>>>
>>>>
>>>> A2: Solar irradiance and radiative forcing following Wang, Lean,
>>>> Shirley, 2005
>>>>   from 1610 to 2004       annual resolution
>>>> Tag: WLS-05
>>>>
>>>>   files:
>>>>     wang05jastr_lean_RF_IPCC_chap6_Joos_11jan06.out
>>>>     wang05jastr_lean_irradiance_IPCC_chap6_Joos_11jan06.out
>>>>
>>>> A3: CO2 concentration in ppm from 850 to 2000
>>>>
>>>>   annual resolution
>>>> Tag: CO2
>>>>   file: co2_850-2000_splined_IPCC_Chap6_Joos_11jan06.out
>>>>
>>>> A4: volcanic forcing after Crowley from 1001 to 1998 AD, extended
>>>> by artificial
>>>>   data from 850 to 1000 AD by mirroring the forcing from 1000 to
>>>> 1150 to the period 850 to 1000
>>>> Tag: volcCrow
>>>>
>>>>   annual resolution
>>>>   file: crowley00sci_RFvolcanic_IPCC_Chap6_Joos_11jan05.out
>>>>
>>>> A5: radiative forcing by non-CO2 agents
>>>>   annual resolution
>>>> Tag: nonco2
>>>>
>>>>   files
>>>>     rf_nonco2_1yr_1765_2000_individ_IPCC_Chap6_Joos_11jan06.out
>>>>     rf_nonco2_1yr_850_2000_IPCC_Chap6_Joos_11jan06.out
>>>>
>>>>
>>>>
>>>> B) SIMULATIONS
>>>> -----
>>>>
>>>> B1. 2 Long simulations from 850 AD to 1998
>>>>
>>>> -----
>>>>
>>>> Simulation B1.1. tag: bard08_volcCrow_CO2_nonCO2_850-1998
>>>>
>>>> Solar forcing from Bard et al. with MM reduction of 0.08 percent,
>>>> volcanic forcing and forcing from CO2 and other anthropogenic
>>>> (non-CO2) agents.
>>>>
>>>> Start of simulation 850 AD
>>>> End of simulation: 1998 AD
>>>> initial condition: model spinup for year 850 (or similiar)
>>>>
>>>> Analysis period: 1001 AD to 1998 AD

```

```
>>>> start-up period: 850 to 1000 with artificial volcanic data
>>>>
>>>> -----
>>>>
>>>> Simulation B1.2 tag: bard25_volcCrow_CO2_nonCO2_850-1998
>>>>
>>>> as B1.1 but with solar forcing from Bard et al. reduced by 0.25
>>>> percent for the Maunder Minimum.
>>>>
>>>> Start of simulation 850 AD
>>>> End of simulation: 1998 AD
>>>> initial condition: model spinup for year 850 (or similiar)
>>>>
>>>> Analysis period: 1001 AD to 1998 AD
>>>> start-up period: 850 to 1000 with artificial volcanic data
>>>>
>>>> -----
>>>>
>>>> Simulation B2: A simulation from 1610 to 1998 restarted from
>>>> bard08_volcCrow_CO2_nonCO2
>>>>
>>>> With solar forcing from Wang et al., 2005, volcanic forci
>>>> ng and forcing from CO2 and other anthropogenic (non-CO2) agents.
>>>>
>>>> B2 tag: WLS-2005_volcCrow_CO2_nonCO2_1610-1998
>>>>
>>>> Start of simulation: 1610 AD
>>>> End of simulation: 1998 AD
>>>> initial condition: restart from simulation B1.1.
>>>> bard08_volcCrow_CO2_nonCO2
>>>> at year 1610
>>>>
>>>> Analysis period: 1610 AD to 1998 AD
>>>>
>>>> -----
>>>>
>>>> B3: 3 Simulations from 1765 to 1998 with natural forcing only
>>>>
>>>> non-CO2 radiative forcing is kept to zero (except
>>>> for volcanoes and solar)
>>>>
>>>> CO2 is kept at its 1765 value.
>>>>
>>>> Simulation B3.1: tag bard08_volcCrow_1765_1998
>>>>
>>>> Start of simulation: 1765 AD
>>>> End of simulation: 1998 AD
>>>> initial condition: restart from simulation B1.1.
>>>> bard08_volcCrow_CO2_nonCO2
>>>> at year 1765
>>>>
>>>> Analysis period: 1765 to 1998 AD
>>>>
```

```
>>>> -----
>>>>
>>>> Simulation B3.2: tag bard25_volcCrow_1765_1998
>>>>
>>>> Start of simulation: 1765 AD
>>>> End of simulation: 1998 AD
>>>> initial condition: restart from simulation B1.2.
>>>> bard25_volcCrow_CO2_nonCO2
>>>> at year 1765
>>>>
>>>> Analysis period: 1765 to 1998 AD
>>>>
>>>> -----
>>>>
>>>> Simulation B3.1: tag WLS-2005_volcCrow_1765_1998
>>>>
>>>> Start of simulation: 1765 AD
>>>> End of simulation: 1998 AD
>>>> initial condition: restart from simulation B2.
>>>> WLS-2005_volcCrow_CO2_nonCO2
>>>> at year 1765
>>>>
>>>> Analysis period: 1765 to 1998 AD
>>>>
>>>> -----
>>>>
>>>> Simulation B4: tag ctrl_850-1998
>>>>
>>>> Control simulation without any forcing
>>>>
>>>> Start of simulation 850 AD
>>>> End of simulation: 1998 AD
>>>> initial condition: model spinup for year 850 (or similiar)
>>>>
>>>> Analysis period: 850 to 1998
>>>>
>>>>
>>>> OUTPUT
>>>> -----
>>>>
>>>> I guess minimal output is global and NH mean surface temperature.
>>>>
>>>>
>>>> --
>>>> Anders Levermann
>>>> phone: +49-331-288-2560 Potsdam Institute for Climate Impact
Research
>>>> fax: +49-331-288-2570 Telegraphenberg A26, 14473 Potsdam, Germany
>>>> anders.levermann@pik-potsdam.de www.pik-potsdam.de/~anders
>>>>
>>>>
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>>>>
>>>
>>>
>>> --
>>> Jonathan T. Overpeck
>>> Director, Institute for the Study of Planet Earth
>>> Professor, Department of Geosciences
>>> Professor, Department of Atmospheric Sciences
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>> Attachment converted: Macintosh HD:modelsE.gif (GIFf/¼IC¶) (00113719)
>> Attachment converted: Macintosh HD:modelsE.pdf (PDF /¼IC¶) (0011371A)
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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Bullet debate number 1
Date: Wed, 15 Feb 2006 11:46:17 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, t.osborn@uea.ac.uk

<x-flowed>

Hi Keith (and Eystein - we need your opinion) - thanks for the quick response. I think it easier (imagining the mess of email that could result) if we focus on one bullet/email. So I'll start w/ the first, and hope that Eystein can also weigh in.

With regard to the first one below, I agree that we can leave statistics out of it. Good point.

But, I think we must at least address Susan's concern. To do otherwise would be counterproductive. She makes sense. I think your MWP results is quite appropriate - they were published in Science, and in my reading of the paper, you are convincing. If it's in the chapter, it makes sense to draw on it for the exec summary. Please defend more convincingly, or suggest an alternative way to deal with Susan's concern - what is the significance (not statistical) of this one record being warmer? We need to say it.

If you really want to leave as is, please write your response in a way that I can forward to Susan - we can't ignore the comment in this case, because other (me, at least) think it makes sense. So we have to convince her too - this is big stuff for the AR4, and will be in the TS/SPM. We can't be as vague as the current bullet is.

And as for the MWP box fig, I think it should be as you suggest - combine the existing fig w/ the new one from Tim and your paper. I think Tim might already be working on it?

Sorry to be a tough guy, but this bullet needs to be more clear.

Thanks, peck

>Peck

>do not think you will like what I say here , but I am going to give
>straight answers to your questions.

>

>First
>
>The new draft says enough in the text now about "far-less-accurately
>dated" and "low-resolution proxy records that can not be rigorously
>calibrated" in relation to this paper (Moberg et al.) . It is not
>appropriate to single the one series out for specific criticism in
>the summary . The use of the word "only" implies we do not believe
>it. Mike Mann's suggestion begs a lot of questions about what
>constitutes "significantly warmer". You need to have a Null
>Hypothesis to test . If you mean would the estimates in Moberg and
>the other reconstructions (during medieval time) show significantly
>different means using a t-test - then of course not , but this tells
>us nothing other than they are not likely samples from totally
>different populations - an almost impossible test to pass given the
>wide uncertainties on all reconstructions . Incidentally, we do not
>have formal (calibration) uncertainties for Moberg anyway (just
>boot-strapped uncertainty on the average low-frequency curve).
>
>I think the vagueness is necessary - "suggests slightly" and is appropriate.
>
>I would not call out The results of Tim and my paper either. It is
>just an aside in the Medieval box at present , perhaps with a Figure
>to accompany the original if you agree, but without more text in the
>Chapter , which I do not consider appropriate, it should not be
>highlighted as a bullet.
>

--
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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>
Subject: Re: Bullet debate number 2
Date: Wed, 15 Feb 2006 16:36:46 -0700
Cc: Keith Briffa <k.briffa@uea.ac.uk>

thanks. Agree on the attribution front, but what about being more specific (at least a little) about what the "subsequent evidence" is. Is there really anything new that gives us more confidence?

Keith? Eystein?

thx, peck

Hi,

I think this version of bullet two is best:

- o The TAR pointed to the "exceptional warmth of the late 20th century, relative to the past 1000 years". Subsequent evidence reinforces this conclusion. Indeed, it is very likely that average Northern Hemisphere temperatures during the second half of the 20th century were warmer than any other 50-year period in the last 500 years. It is also likely that this was the warmest period in the past 1300 years. The uneven coverage and characteristics of the proxy data mean that these conclusions are most robust over summer, extra-tropical, land areas.

I agree with Keith we cannot enter into the attribution aspects that Susan alludes to.

Eystein

At 11:57 -0700 15-02-06, Jonathan Overpeck wrote:

Hi again - as for bullet issue number 2, I agree that we don't need to go with the suggest stuff on solar/forcing, BUT, I agree w/ Susan that we should try to put more in the bullet about "Subsequent evidence" Would you pls send a new bullet that has your suggested changes below, and that includes something like:

"Subsequent evidence, including x, y and z, reinforces this conclusion." Need to convince readers that there really has been an increase in knowledge - more evidence. What is it? The bullet can be longer if needed.

Thanks, Peck

Second

Simply make "1000" "1300 years. " and delete "and unusually warm compared with the last 2000 years."

It is certainly NOT our job to be discussing attribution in the 20th century - this is Chapter 9 - and we had no room (or any published material) to allow a discussion of relative forcing contributions in earlier time. Therefore a vague statement about "perhaps due to solar forcing" seems unjustified.

Third

I suggest this should be

Taken together , the sparse evidence of Southern Hemisphere temperatures prior to the period of instrumental records indicates that overall warming has occurred during the last 350 years, but the even fewer longer regional records indicate earlier periods that are as warm, or warmer than, 20th century means.

Fourth

fine , though perhaps "warmth" instead of "warming"?
and need to see EMIC text

Fifth

suggest delete

Sixth

suggest delete

Peck, you have to consider that since the TAR , there has been a lot of argument re "hockey stick" and the real independence of the inputs to most subsequent analyses is minimal. True, there have been many different techniques used to aggregate and scale data - but the efficacy of these is still far from established. We should be careful not to push the conclusions beyond what we can securely justify - and this is not much other than a confirmation of the general conclusions of the TAR . We must resist being pushed to present the results such that we will be accused of bias - hence no need to attack Moberg . Just need to show the "most likely" course of temperatures over the last 1300 years - which we do well I think. Strong confirmation of TAR is a good result, given that we discuss uncertainty and base it on more data. Let us not try to over egg the pudding.

For what it worth , the above comments are my (honestly long considered) views - and I would not be happy to go further . Of course this discussion now needs to go to the wider Chapter authorship, but do not let Susan (or Mike) push you (us) beyond where we know is right.

--

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From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: bullet debate #3
Date: Thu, 16 Feb 2006 00:28:11 +0100

<x-flowed>

This version is fine with me:

At 12:03 -0700 15-02-06, Jonathan Overpeck wrote:

>Hi again... thanks for the work on number #3. It

>seems a bit awkward/vague, so how about:

>

>Taken together, the sparse evidence of Southern

>Hemisphere temperatures prior to the period of

>instrumental records indicates that overall

>warming has occurred during the last 350 years.

>The even sparser records longer than 350 years

>indicate that there may have been periods of

>regional warmth in the past 1000 years that were

>as warm, or warmer than, 20th century means.

>

Eystein

>Thanks, Peck

>

>>Third

>>

>>I suggest this should be

>>

>>Taken together , the sparse evidence of

>>Southern Hemisphere temperatures prior to the

>>period of instrumental records indicates that

>>overall warming has occurred during the last

>>350 years, but the even fewer longer regional

>>records indicate earlier periods that are as

>>warm, or warmer than, 20th century means.

>>

>>Fourth

>>

>>fine , though perhaps "warmth" instead of "warming"?

>>

>>and need to see EMIC text

>>

>>Fifth

>>

>>suggest delete

>>

>>Sixth

>>

>>suggest delete

>>

>>Peck, you have to consider that since the TAR ,

>>there has been a lot of argument re "hockey

>>stick" and the real independence of the inputs

>>to most subsequent analyses is minimal. True,

>>there have been many different techniques used

>>to aggregate and scale data - but the efficacy

>>of these is still far from established. We

>>should be careful not to push the conclusions

>>beyond what we can securely justify - and this

>>is not much other than a confirmation of the

>>general conclusions of the TAR . We must resist

>>being pushed to present the results such that

>>we will be accused of bias - hence no need to

>>attack Moberg . Just need to show the "most

>>likely"course of temperatures over the last

>>1300 years - which we do well I think. Strong

>>confirmation of TAR is a good result, given

>>that we discuss uncertainty and base it on more

>>data. Let us not try to over egg the pudding.

>>For what it worth , the above comments are my

>>(honestly long considered) views - and I would

>>not be happy to go further . Of course this

>>discussion now needs to go to the wider Chapter

>>authorship, but do not let Susan (or Mike) push

>>you (us) beyond where we know is right.

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>>--

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Bullet debate number 1
Date: Thu, 16 Feb 2006 17:49:58 +0000
Cc: t.osborn@uea.ac.uk

<x-flowed>

Dear Peck and Eystein

I have to come back again on this.

FIRST

Happy with first sentence.

Then following largely on a suggestion made by Tim , I suggest

The additional variability implies mainly cooler temperatures (predominantly in the 12th-14th, 17th and 19th centuries) and only one new reconstruction suggests slightly warmer conditions (in the 11th century), but well within the uncertainty range indicated in the TAR.

Failing this, I suggest we omit everything after the first closing bracket.

SECOND

Now suggest insert the bit about our work (Tim and I) in the second point - after the sentence ending "1300 years." That is..

The regional extent of Northern Hemisphere warmth was very likely greater during the 20th century than in any other century during the last 1300 years.

Will finish corrections to my text tomorrow - but hope Fortunat has checked it all, and is doing a paragraph on the EMICS still?

cheers

Keith

At 23:19 15/02/2006, Eystein Jansen wrote:

>Hi,
>I think we should avoid discussing the Moberg et
>al results in the exec. bullet. I also think we
>need to have a statement about the MWP in the
>bullet, and I cannot really understand why the
>most central conclusion from the very nice
>recent Osborn et al. Science paper cannot be
>highlighted in the first bullet. My suggestion is:
>o Some of the post-TAR studies indicate
>greater multi-centennial Northern Hemisphere
>temperature variability than was shown in the
>TAR, due to the particular proxies used, and the
>specific statistical methods of processing
>and/or scaling them to represent past
>temperatures. The additional variability implies
>cooler temperatures, predominantly during the
>12th to 14th, the 17th, and the 19th centuries.
>The warmer period in the 11th century is in
>general agreement with the results shown in the
>TAR. Consideration of the regional records of
>temperature for the 11th century indicate that
>it is unlikely that the spatial extent of
>warming during this time period was as
>significant as in the second half of the 20th century.

>
>Cheers,
>Eystein

>
>
>
>

>At 11:46 -0700 15-02-06, Jonathan Overpeck wrote:

>>Hi Keith (and Eystein - we need your opinion) -
>>thanks for the quick response. I think it
>>easier (imagining the mess of email that could
>>result) if we focus on one bullet/email. So
>>I'll start w/ the first, and hope that Eystein can also weigh in.

>>
>>With regard to the first one below, I agree
>>that we can leave statistics out of it. Good point.
>>

>>But, I think we must at least address Susan's
>>concern. To do otherwise would be
>>counterproductive. She makes sense. I think
>>your MWP results is quite appropriate - they
>>were published in Science, and in my reading of
>>the paper, you are convincing. If it's in the
>>chapter, it makes sense to draw on it for the
>>exec summary. Please defend more convincingly,
>>or suggest an alternative way to deal with
>>Susan's concern - what is the significance (not
>>statistical) of this one record being warmer? We need to say it.

>>
>>If you really want to leave as is, please write
>>your response in a way that I can forward to
>>Susan - we can't ignore her comment in this
>>case, because other (me, at least) think it
>>makes sense. So we have to convince her too -
>>this is big stuff for the AR4, and will be in
>>the TS/SPM. We can't be as vague as the current bullet is.

>>
>>And as for the MWP box fig, I think it should
>>be as you suggest - combine the existing fig w/
>>the new one from Tim and your paper. I think
>>Tim might already be working on it?

>>
>>Sorry to be a tough guy, but this bullet needs to be more clear.

>>
>>Thanks, peck

>>>Peck
>>>do not think you will like what I say here ,
>>>but I am going to give straight answers to your questions.

>>>
>>>First

>>>
>>>The new draft says enough in the text now
>>>about "far-less-accurately dated" and
>>>"low-resolution proxy records that can not be
>>>rigorously calibrated" in relation to this
>>>paper (Moberg et al.) . It is not appropriate
>>>to single the one series out for specific
>>>criticism in the summary . The use of the word
>>>"only" implies we do not believe it. Mike
>>>Mann's suggestion begs a lot of questions

>>>about what constitutes "significantly warmer".
>>>You need to have a Null Hypothesis to test .
>>>If you mean would the estimates in Moberg and
>>>the other reconstructions (during medieval
>>>time) show significantly different means using
>>>a t-test - then of course not , but this tells
>>>us nothing other than they are not likely
>>>samples from totally different populations -
>>>an almost impossible test to pass given the
>>>wide uncertainties on all reconstructions .
>>>Incidentally, we do not have formal
>>>(calibration) uncertainties for Moberg anyway
>>>(just boot-strapped uncertainty on the average low-frequency curve).
>>>
>>>I think the vagueness is necessary -
>>>"suggests slightly" and is appropriate.
>>>
>>>I would not call out The results of Tim and my
>>>paper either. It is just an aside in the
>>>Medieval box at present , perhaps with a
>>>Figure to accompany the original if you agree,
>>>but without more text in the Chapter , which I
>>>do not consider appropriate, it should not be highlighted as a bullet.
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>>--
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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Robust Findings/ Key Uncertainties Table V3
Date: Fri, 17 Feb 2006 10:15:28 -0700
Cc: joos <joos@climate.unibe.ch>

Hi Keith and Eystein - good additions. Thanks. You can see how I edited them in the attached. The only tough issue was Eystein's proposed key uncertainty on ocean circulation. I think it would be awkward to have multiple abrupt change uncertainties listed (our list is already pretty long in general), so I combined your suggested bullet w/ the existing one (to include drought and other types of abrupt change:

"The mechanisms of abrupt climate change (for example, in ocean circulation and drought frequency) are not well understood, nor are the key climate thresholds that, when crossed, could trigger an acceleration in regional climate change."

If either of you thinks we can improve further, pls track changes edit the attached.

Thanks again, Peck

--

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Attachment Converted: "c:\eudora\attach\Chap6RobustKeyTableV3.doc"

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Fwd: URGENT review requested
Date: Fri Feb 17 15:52:41 2006
Cc: Fortunat Joos <joos@climate.unibe.ch>, eystein.jansen@geo.uib.no

Date: Thu, 16 Feb 2006 23:01:57 -0700
To: Eystein Jansen <eystein.jansen@geo.uib.no>,
Keith Briffa <k.briffa@uea.ac.uk>, joos <joos@climate.unibe.ch>
From: Jonathan Overpeck <jto@u.arizona.edu>
Subject: URGENT review requested
X-UEA-Spam-Score: 0.0
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO

Hi Eystein, Keith and Fortunat - this is a special request for help from the Euro team, so I know I have solid feedback by the time I get to work tomorrow am. Please respond asap (using track changes if you can).

1) Tomorrow I have to send the TSU our Robust Findings and Key Uncertainties Table. I have attached this table. Please edit, and if you think a Finding or Uncertainty is missing, please suggest exactly how you think it should be worded, and, if it is a Finding, suggest which existing one it should replace (I suspect they don't want more, but we could try). Please keep in mind this table will be part of the TS (not our chapter), and they must be VERY policy relevant - this is not the place for things a policy maker would not understand. Also, we need to use plainer English than in our Exec Summary bullets.

2) I also attach the latest Exec Summary, with the latest from Keith and Fortunat (e.g., reordered as you suggested). I will send this in to the TSU tomorrow too, so if you want to read and edit (PLEASE USE TRACK CHANGES), that'll help too, but this is less important than working on the Robust/Key table.

Many thanks! Cheers, peck

--

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Suggestions re Box - see attached

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References

1. <http://www.geo.arizona.edu/>
2. <http://www.ispe.arizona.edu/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Tim Osborn <t.osborn@uea.ac.uk>

To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

Subject: Re: Figures - urgent

Date: Fri, 17 Feb 2006 17:00:44 +0000

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Hi Peck and Eystein,

just working on this MWP box fig update. Just trying to clarify what is wanted.

The old MWP box fig had 8 series on it. 7 of these were straight from our recent Science paper anyway, and the 8th was the average of 2 more from the Science paper. The other 5 in the paper (making a total of $7+2+5 = 14$ series) were not used in the old MWP box fig, as they are too short to cover the MWP period.

(1) Are you asking me to use exactly the 14 series from the Science paper, overlaid like in the old MWP fig or, if space permits, plotted like fig 1 in our Science paper. And then add below the exact fig 3B of our paper (you say "3b-like" which implied maybe some changes).

(2) Or do you want to stick with the original 8 series, and then have the exact fig 3B from our paper, which wouldn't correspond exactly to the 8 series above because it would be based on the 14.

(3) Or do you want to stick with the original 8 series, and then show a panel similar to our fig 3B, but *recalculated* using just the 8 series shown?

So many questions! ;-)

I attached the original MWP fig (8 series), plus a new one from option (1) above (14 series, looks a bit of a mess, also I removed the "composite mean" which might have been agreed in New Zealand?).

Cheers

Tim

At 05:28 02/02/2006, Jonathan Overpeck wrote:

>Hi Tim and Keith - I have some feedback on the MWP box fig, but
>would to first ask that you update us (me and Eystein) about the
>status of your other figs. We have a particularly urgent need to see
>those that are likely to be elevated to the TS (Tech Summary) - a
>big deal for paleo. Can you promise us these by the end of this
>week, Monday at the latest? Again, see my emails of Dec for details.
>

>It would be great to see a new MWP box fig asap too, but this isn't
>as high priority as the TS figs. Eystein and I agree with both Susan
>and Martin that it would be good to see a new MWP box fig that was a
>hybrid of the old fig concept and the new Fig 3b from your Science

>paper. It would be good to have two versions - if space allows, we
>go with the first, otherwise the 2nd:

>
>Both would have your 3b-like plot, and both would have all the
>normalized time series that were used to create the 3b plot (i.e.,
>those in Fig. 1 of your paper).

>
>Version 1 - has all the input series stacked on top of each other as
>in your Fig. 1, with the summary Fig 3b-like plot below.

>
>Version 2 - is the same, but the input series are all on the same
>axis like in the FOD MWP box fig.

>
>Now, if you think Version 1 plus caption would be smaller than
>Version 2 plus caption, no need for Version 2. Ditto if Version 1
>plus caption was only a little bigger than V 2 plus caption.

>
>Again, thanks for getting all of your new figs to us asap,
>particularly those targeted for TS consideration.

>
>Many thanks, Peck

>--
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Attachment Converted: "c:\eudora\attach\ipccar4_mwpbox4.pdf"

Attachment Converted: "c:\eudora\attach\ipccar4_mwpbox_a.pdf"

<x-flowed>

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From: Fortunat Joos <joos@climate.unibe.ch>
To: Keith Briffa <k.briffa@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: section 6.6 material Solar-CO2-aerosols-EMIC figure
Date: Fri, 17 Feb 2006 21:52:42 +0100
Cc: ""@kup.unibe.ch

Hi,

Robust finding/uncertainty table is fine with me. Good job!

Here the 6.6 material from Bern. It includes an update on solar forcing, an update on the section on compatibility of the GHG-proxz-forcing records, new text for the sulfate aerosol figure, new text for the EMIC figure panel e) and a proposed bullet for the last millennium modeling.

Will send an update of the ice core sulfate figure next week with one additional curve from Antarctica and an updated figure caption. Otherwise, I think this is all you need from me for 6.6. Will also hunt recent references for alpine cores highlighted as missing.

Let me know if I missed something else for the last 2ka section.

- The solar subsection in 6.6.3 requires coordination with chapter 2 – Suggest to send the text to Dorland and Lean as soon as agreed among us. Note that we do not have an exec summary bullet on solar forcing – probably captured by chap 2
- should probably also send the para on sulfate aerosols to chap 2 for checking consistency and cross-referencing
- Have tried to be brief and not to add much, have also suggested to delete paragraphs.
- note new bullet proposed for exec summary on model results. Would be nice if sufficient space, but no strong feeling whether this should be included or not; may be covered to some extent by the attribution chapter.

KEITH:

Can you or Tim please provide the number for the smoothing shown in figure 11e: 'The simulated range in decadal-smoothed NH surface temperature is about 60% KEITH/TIM CAN YOU CHECK/PROVIDE THIS NUMBER WITH YOUR FILTERED DATA larger for the high amplitude than for the low amplitude case prior to the industrialization.'

Looking forward to your feedbacks and improvements. We can forward to Oyvind when finalized.

With best regards,

Fortunat

--

Climate and Environmental Physics

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Internet: <http://www.climate.unibe.ch/~joos/>

Attachment Converted: "c:\eudora\attach\sec6.6-solar-EMIC-CO2_fjoos_17feb06.doc"

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>
Subject: Re: Science letter
Date: Mon, 20 Feb 2006 11:35:39 +0000
Cc: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Thanks Eystein. We submitted the data to WDC-Paleo in advance and they went online on the day of publication. We didn't provide an "accession" number however.

Cheers

Tim

At 03:14 20/02/2006, you wrote:

>Tim,
>in case you did not see this yet: <http://www.climateaudit.org/?p=537>

>

>Eystein

>--

>

>Eystein Jansen

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>Bjerknes Centre for Climate Research and

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>NORWAY

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>Fax: +47-55-584330

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web: <http://www.cru.uea.ac.uk/~timo/>

sunlock: <http://www.cru.uea.ac.uk/~timo/sunlock.htm>

</x-flowed>

From: "Rob Wilson" <rob.dendro@virgin.net>
To: <Sandy.Tudhope@ed.ac.uk>, "Tim Osborn" <t.osborn@uea.ac.uk>
Subject: Re: Fw: 2005JC003188R Decision Letter
Date: Tue, 21 Feb 2006 15:37:10 -0000
Cc: <K.briffa@uea.ac.uk>, "Brohan, Philip" <philip.brohan@metoffice.com>, <simon.tett@metoffice.com>

Thanks Tim,

am working my way through the comments

Have also re-read Mike Evans 2002 paper.

I am frustrated with the associate editors comments. He seems to be overtly defending

Mike's reconstruction which are quite different in nature - i.e. he reconstructed 2 spatial

fields - the 1st being ENSO related and the 2nd being probably related to the PDO although

it is not clear from the text.

The coral data-sets are also quite different, with only ~ 4 series being common to both

studies. In fact, many of the coral series used by Mike did not pass my screening process.

Lastly, the only statistic use by Mike for validation is the correlation coefficient. I

like to think I have been a little more robust at least in this regard.

I need to diplomatically word all this. I never wanted to criticise Mike's work in anyway

way. It was for that reason that I made little mention to it initially.

anyway, I hope to get a more cleaner version done by early next week.

will keep you all posted

Rob.

PS. do you have the FORTRAN code for Ed Cook's SSA software?

----- Original Message -----

From: [1]Tim Osborn

To: [2]Rob Wilson ; [3]Sandy.Tudhope@ed.ac.uk

Cc: [4]K.briffa@uea.ac.uk ; [5]Brohan, Philip ;
[6]simon.tett@metoffice.com

Sent: Tuesday, February 21, 2006 3:00 PM

Subject: Re: Fw: 2005JC003188R Decision Letter

Hi Rob et al.,
seems like there are many points to address - some reasonable, some rather picky.

Some easy things to do... change "all time scales" to "annual to centennial time scales", minor inconsistencies pointed out.

Near the end the comments get a bit picky/stupid. e.g. "according

to

CE reconstruction is less skillful than climatology". Doesn't RE assume "climatology" (= calibration period mean) while CE compares the skill against the assumption that the mean over the verification period is known (which of course it isn't known for a general period outside the instrumental period)? And I really don't think your average reader will be confused into thinking that you calibrated using observations before 1840! Though wording could be changed to "the explained variance of the reconstruction using records

available

before 1840 is quite low" or something similar that fits the flow of the sentence. Also, earlier on, isn't it obvious from the editor's own description of the method that you can indeed estimate verification errors for all "networks", including those available

during

the instrumental period, and thus it is obvious why verification statistics can cover this entire period in Figure 2C,D. The editor just needs to think about things a bit more!
The description of the calibration method can be written in the way that is requested, I'm sure. The difficulty is actually in countering the criticisms that (1) the reconstruction error obtained by regression may no longer be appropriate after the "inflation" step, (2) the use of calibration period residuals rather than verification period residuals to provide the error bars (though here the editor contradicts this suggestion by pointing out that the verification errors apply to no period other than the verification period, but if you assume the same for the calibration errors then where can you get the errors from?).

Hope these quick comments help,

Cheers

Tim

At 11:41 18/02/2006, Rob Wilson wrote:

>Greetings All,

>have just been away for a week to return to this reply from JGR.

>Have only gone through it quickly, but we obviously have a fussy

>associate editor to please.
>Should have gone for 'atmospheres' rather than 'oceans'.
>
>will go through it properly on Monday.
>Hope you are around over the next few days or so.
>
>regards
>Rob
>PS. have used this e-mail address as the Uni server seems to be
down
>
>----- Original Message -----
>From: <[7]mailto:jgr-oceans@agu.org>jgr-oceans@agu.org
>To: <[8]mailto:rob.wilson@ed.ac.uk>rob.wilson@ed.ac.uk
>Cc: <[9]mailto:rob.dendro@virgin.net>rob.dendro@virgin.net
>Sent: Thursday, February 16, 2006 8:06 PM
>Subject: 2005JC003188R Decision Letter
>
>Dear Dr. Wilson:
>
>Thank you for submitting your manuscript "250-years of
reconstructed
>and modeled tropical temperatures" [Paper #2005JC003188R].
>
>I am in agreement with the associate editor and the reviewers that
>your revisions fail to adequately address the original concerns
>about the reconstruction methodologies. If you want to convey that
>this is somehow far superior to earlier reconstructions of SST,
then
>it is only fair that readers of JGR get a very very clear
>description of the methods used and a convincing argument as to why
>the reconstruction is better than prior published reports on such
>reconstructions. Please heed the detailed comments and carefully
>address each of the comments with appropriate revisions and clear
>responses. I will be obliged to reject the manuscript if you do not
>address these concerns since the main claim of an improved
>reconstruction of historic temperatures is not scientifically
>rigorous enough for publication in JGR-Oceans.
>
>Please submit your revised manuscript by March 28, 2006. If you do
>not plan to submit a revision, or if you cannot do so in the time
>allotted, I would be grateful if you could let me know as soon as
>possible.
>
>Please review the Important Links to JGR Information attached below
>before uploading your revised manuscript.
>
>When you are ready to submit your revision, please use the link
>below.
>
><<[10]http://jgr-oceans-submit.agu.org/cgi-
bin/main.plex?el=A7D3BjvY2B7CcrO6I3A9KGXg2FZ
afNJvsZyA2JF0mAZ>http://jgr-oceans-submit.agu.org/cgi-
bin/main.plex?el=A7D3BjvY2B7CcrO6I

3A9KGXg2FZafNJvsZyA2JF0mAZ>
>
>
>Sincerely,
>
>Raghu Murtugudde
>Editor, Journal of Geophysical Research - Oceans
>
>-----IMPORTANT PUBLICATION INFORMATION-----

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>
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>
>
>Adobe Acrobat Reader is available, free, on the internet at the
>following URL:
><[13]<http://www.adobe.com/prodindex/acrobat/readstep.html>><http://www.adobe.com/prodindex/acrobat/readstep.html>
>
>
>*****END*****

>
>
>Reviewer Comments
>
>Associate Editor(Comments):
>
>The authors adequately addressed many of the reviewers'
>remarks and requests for revisions.
>

>However, there are significant outstanding issues detailed
>below. The paper needs a thorough revision to become
>acceptable.

>

>1. The paper lacks a clear description of the reconstruction
>technique. From the text, figures, tables, and the authors'
>responses, one can guess that the following approach was
>used, in order to produce the main ("full period")
>reconstruction that the authors use for model comparison and
>interpretation: (1) for each year before 1870 the subset of
>coral records for which this year's value is available
>("nest") is identified; (2) standardized values of the
>"nest" records are averaged together for each year for which
>the entire nest is available; (3) a linear regression of the
>nest values is performed on the instrumental annual tropical
>SST averages for the period 1897-1981 (or its subperiod for
>which the nest values are available); (4) the obtained
>linear regression formula for that nest is tested on the
>period 1870-1896, and the verification statistics is
>derived; (5) the reconstruction of the target year is
>performed using the same linear regression for this nest,
>and the "verification" statistics is attributed to this
>year.

>

>Very small percentage of the readers will be able to
>understand this procedure from the paper in its current
>form. There are a few reasons for that: (a) the paper lacks
>an explicit coherent description of this procedure, (b) the
>additional "inflation" of the reconstruction (p.9, lines
>2-3) is performed, but neither the explicit formula for it
>is given, nor how this inflation affects the reconstruction
>error in verification is discussed, (c) it would seem
>natural to use the verification error for the error bars,
>but it appears that the authors are using the calibration
>error, although no adequate description is given, (d) the
>authors are taking a lot of liberty with using verification
>statistics - unlike error bar estimates these are not
>supposed to be attributed to the periods other than those
>for which they were computed, or at least it is highly
>unusual to do that, (e) what values are given as coral
>reconstructions for the instrumental period is not
>explained: calibration values for corresponding nests? (f)
>why "verification" statistics in Fig 2C,D cover the entire
>calibration period is unclear, (g) the presence of the
>specific calibration formula in the upper right corner of
>Fig 2 is very confusing in the context of this work, but the
>authors failed to take any action despite the hint from
>Reviewer 2 (remark 3.3).

>

>The authors have to provide an unambiguous description of all
>aspects of their reconstruction procedure. But all
>additional information they provide about their
>reconstruction should help the reader to understand the main
>message, rather than to get confused or completely drowned

>under the confusing information flow. Therefore the
>"split-period" calibrations need to be reported only if they
>help to deliver the main message, which is not the case in
>the present version. Same with statistics: a lot of it is
>reported, but what purpose it serves is unclear. All
>statistics more complicated than correlation coefficient
>needs to be explicitly defined, to make the presentation
>unambiguous. In their reply, the authors call Durbin-Watson
>statistic "standard". Well it's not for JGR-Oceans, where at
>least since 1994 it's never been used (in the entire body of
>all AGU journals it was only about 15 times). Same with sign
>test: the readers of JGR-Oceans should not be expected to
>have dendroclimatological textbooks by Cook and Kairiukstis
>or by Fritts in their possession in order to look up and
>interpret the authors' results. Some of these statistics
>are only introduced in table captions, and in a puzzling
>way, e.g. Table 2A, lines 3-4: LIN r = correlation of linear
>trend in residual series. What is meant here is probably the
>correlationcoefficient of residual with the time variable,
>but in any case, LIN r is not a good notation.

>

>2. The authors resisted the gentle insistence of Reviewer 2
>(remark 5.1) on quantifying the role of trends in the
>model-reconstruction intercomparison. To put it more
>bluntly, the significant correlations reported on p.11 and
>Table 3 are only significant because of the long term
>trends. If the 50- or 100-year trends were subtracted,
>no significant correlation of residuals would be
>left. Trends themselves have such a small number of degrees
>of freedom (6, if separate trends are computed for 50 yr
>periods), that reported correlations are not significant for
>them. Therefore the authors' claim in conclusions of "a
>strong mutual agreement between the reconstruction and two
>global coupled-climate models" (p.14, lines 21-22) is not
>properly supported by the presented results and most likely
>incorrect. The authors have to change somehow their line of
>argument about model-data consistency to make it correct and
>acceptable for publication.

>

>3. The authors claim to develop "first coral-based, large
>scale temperature reconstruction, exclusive to the tropics,
>that represents past SST variability at all time-scales."
>First, how can it possibly do this at "all" time-scales and
>what scales other reconstructions of similar length exclude?
>Second, why Evans et al 2002 reconstruction doesn't count?
>In general, the authors seem to operate with understanding
>that their reconstruction is superior to that by Evans et al
>2002 (e.g. their reply to remark 3.4 by Reviewer 2). The
>basis for that is unclear, since they use a simpler
>technique, a similar coral data set, and they only try to
>reconstruct the tropical mean, rather than the entire
>field. The actual advantages of their product compared with
>earlier works need to be made clear in the paper.

>

>4. The revision seems to have been made in a great haste, so
>that the changes the authors made often result in
>inconsistencies with the surrounding text.
>
>Abstract, lines 14-16: this sentence is grammatically
>incorrect.
>
>p.4, line 15: raw records are not data transforms
>
>p.4 lines 18-19 and p.5 lines 11-12 are in conflict. Logical
>way to present the material is to say that 16 records passed
>the screening, but then 2 of them were excluded for that and
>this reason.
>
>p.6, line 7: MTA is mentioned here, but it is only in the
>captions to Table 2 that it is explained that MTA is a
>combined mean of MAI and TAR. This is inappropriate use of
>caption, not to mention that (1) TAR is called MaiTar in the
>Table header, (2) the number of records is reduced to 13
>now, to confuse the reader further.
>
>p. 7, line 6: add "here" after "was used" to break the false
>attribution of this sentence to Evans et al 1998 work.
>
>p.8, line 5. ST abbreviation introduced earlier is not used
>here.
>
>p.8 lines 9-11: "calculated" used twice.
>
>p.8 line 20 - p.9 line 5. Ambiguous, confusing description of
>the crucial part of the procedure.
>
>
>p.9, lines 6-18. (1) attribution of the statistics to the
>entire nest record creates very bad effects here: "prior to
>1840, the explained calibration variance is quite low". For
>a reader who hasn't internalize the authors approach, the
>reference to calibration before 1840 will be shocking. (2)
>Strictly speaking, for the entire period before 1850 the
>reconstruction has less skill than climatology, according to
>CE in the Figure 2B. The authors have to deal with a
>complicated task of explaining that to the reader, while
>also arguing that since after 1750 the CE is a bit better
>that before 1750, they chose to use the reconstruction after
>1750 for comparison with the models. (The Reviewer 1 was
>concerned about this too in the first remark).
>
>p.9, line 19: ". . . appear improved" compared to what?
>
>p.15, lines 19-21. Again, it needs to be explained better
>what is the contribution of the present paper to evaluating
>the potential for reconstructing large scale tropical
>temperatures from a network of coral proxies, as compared to
>Evans et al papers, where this task seems to have been

>accomplished before from a few different angles.
>
>p.24, line 1: "Simple zero order OLS regression" is not
>simple: what does zero order mean in this context?
>
>p.24, line 5. "model residual" is confusing, because the
>only models called so in the paper are GCMs. But here
>"model" denotes a linear regression model.
>
>Page 41. Table S1. (1) it would be helpful to explain that
>left part of these tables are calibration statistics and
>right are verification statistics. (2) What is aR^2 :
>"multiple" correlation coefficient? Is R different from r?
>(3) Why full-period verification statistics are missing for
>nests after 1879?
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web: [15]http://www.cru.uea.ac.uk/~timo/
sunclock: [16]http://www.cru.uea.ac.uk/~timo/sunclock.htm

References

1. mailto:t.osborn@uea.ac.uk
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6. mailto:simon.tett@metoffice.com
7. mailto:jgr-oceans@agu.org>jgr-oceans@agu.org
8. mailto:rob.wilson@ed.ac.uk>rob.wilson@ed.ac.uk
9. mailto:rob.dendro@virgin.net>rob.dendro@virgin.net
10. http://jgr-oceans-submit.agu.org/cgi-bin/main.plex?el=A7D3BjvY2B7CcrO6I3A9KGXg2FZafNJvsZyA2JF0mAZ>http://jgr-oceans-submit.agu.org/cgi-bin/main.plex?el=A7D3BjvY2B7CcrO6I3A9KGXg2FZafNJvsZyA2JF0mAZ
11. http://www.agu.org/pubs/inf4aus.html>http://www.agu.org/pubs/inf4aus.html

12. http://www.agu.org/cgi-bin/ms_status/ms_status.cgi>http://www.agu.org/cgi-bin/ms_status/ms_status.cgi
13. <http://www.adobe.com/prodindex/acrobat/readstep.html>><http://www.adobe.com/prodindex/acrobat/readstep.html>
14. <mailto:t.osborn@uea.ac.uk>
15. <http://www.cru.uea.ac.uk/~timo/>
16. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

From: Stefan Rahmstorf <rahmstorf@ozean-klima.de>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: latest draft of 2000-year section text
Date: Tue, 21 Feb 2006 19:15:54 +0100
Cc: jto@u.arizona.edu, eystein.jansen@geo.uib.no, Fortunat Joos
<joos@climate.unibe.ch>, Tim Osborn <t.osborn@uea.ac.uk>,
drind@giss.nasa.gov, Henry Pollack <hpollack@umich.edu>

<x-flowed>
Hi Keith,

will try to look at your text asap. Concerning the issue of the drift in the Von Storch run: they now have at least one paper plus one submitted comment where they redid their model run without the drift, they call this ECHO-G II, the version with drift is now ECHO-G I. I think this argues for leaving the ECHO-G I curve out of the graphs, and just having one sentence in the text stating this is not shown as it was found to drift, and has been superseded. It is an outlier that messes up the graph, and if it is known and even acknowledged by its authors that it is a model artifact, why show it in IPCC?

Stefan

</x-flowed>

From: "Wahl, Eugene R" <wahle@alfred.edu>
To: "Jonathan Overpeck" <jto@u.arizona.edu>
Subject: RE: Wahl and Ammann Climatic Change article on MBH
Date: Tue, 21 Feb 2006 19:26:44 -0500
Cc: "Keith Briffa" <k.briffa@uea.ac.uk>, "Eystein Jansen" <eystein.jansen@geo.uib.no>

OK:

Here is the mss. Yes, fingers crossed. Note, this is not for general dissemination until actually "in press".

The article is quite long, due to all the MM issues we address and the extensive discussions concerning use of validation measures we get into.

As a first pass, the Abstract, Discussion, and Summary would be good places to start.

Peace, Gene

Dr. Eugene R. Wahl
Asst. Professor of Environmental Studies
Alfred University

1 Saxon Drive
Alfred NY, 14802

607.871.2604

-----Original Message-----

From: Jonathan Overpeck [mailto:jto@u.arizona.edu]
Sent: Tuesday, February 21, 2006 3:59 PM
To: Wahl, Eugene R
Cc: Keith Briffa; Eystein Jansen
Subject: Re: Wahl and Ammann Climatic Change article on MBH

Hi Gene - might be better to send the ms now - at least to Keith, since final text is being worked out now. Fingers crossed, thanks, peck

>Hello all:

>

>The re-revised mss. of the Wahl-Ammann article on the MBH-MM controversy

>is now to Stephen Schneider of Climatic Change for his approval.

>

>It is possible that we might hear from him within days. If so, and the

>decision is full approval of "in press" status, I will let you all know

>immediately. At that time I also will send the mss. itself.

>

>Peace, Gene

>

Attachment Converted: "c:\eudora\attach\Wahl-Ammann_3321_Figures.pdf"

Attachment Converted: "c:\eudora\attach\Wahl_Ammann_3321_Final_21Feb.doc"

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Wahl, Eugene R" <wahl@alfred.edu>, "Jonathan Overpeck" <jto@u.arizona.edu>
Subject: RE: Wahl and Ammann Climatic Change article on MBH
Date: Wed Feb 22 08:53:55 2006
Cc: "Eystein Jansen" <eystein.jansen@geo.uib.no>

Thanks for this Eugene. It has been very difficult in drafting the 2000-year section text for us to get the balance between too much concentration on the controversy as you call it and the need to describe subsequent work. Sounds like your paper is an important one to signpost in the text.

best wishes

Keith

At 00:26 22/02/2006, Wahl, Eugene R wrote:

OK:

Here is the mss. Yes, fingers crossed. Note, this is not for general dissemination until actually "in press".

The article is quite long, due to all the MM issues we address and the extensive discussions concerning use of validation measures we get into.

As a first pass, the Abstract, Discussion, and Summary would be good places to start.

Peace, Gene

Dr. Eugene R. Wahl

Asst. Professor of Environmental Studies

Alfred University

1 Saxon Drive

Alfred NY, 14802

607.871.2604

-----Original Message-----

From: Jonathan Overpeck [[1]mailto:jto@u.arizona.edu]

Sent: Tuesday, February 21, 2006 3:59 PM

To: Wahl, Eugene R

Cc: Keith Briffa; Eystein Jansen

Subject: Re: Wahl and Ammann Climatic Change article on MBH

Hi Gene - might be better to send the ms now - at least to Keith, since final text is being worked out now. Fingers crossed, thanks, peck

>Hello all:

>

>The re-revised mss. of the Wahl-Ammann article on the MBH-MM controversy

>is now to Stephen Schneider of Climatic Change for his approval.

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>It is possible that we might hear from him within days. If so, and the

>decision is full approval of "in press" status, I will let you all know

>immediately. At that time I also will send the mss. itself.

>

>Peace, Gene

>

--

Professor Keith Briffa,
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[2]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <mailto:jto@u.arizona.edu>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Valerie.Masson@cea.fr, Henry Pollack <hpollack@umich.edu>
Subject: Re: latest draft of 2000-year section text
Date: Thu Feb 23 10:14:30 2006
Cc: jto@u.arizona.edu, Eystein Jansen <Eystein.Jansen@geo.uib.no>

Valerie and Henry

these are really great and useful comments - I am going to try to get these incorporated , in the time allowed , though today again I am busy with exam question scrutiny board meeting and teaching. Thanks a lot for your help

Keith

At 10:02 23/02/2006, you wrote:

Dear Keith,

A few rapid comments on the section 6.6 revised text. I have enjoyed reading it, more concise, less defensive and key conclusions appear more solid. Sometimes the text is written in the past tense, sometimes in the present tense : it could be homogenised.

Please remove the sentence page 6-15 "The paleohydrologic record of North America is the most complete and diverse of any of the world in part due to the proximity to many well equipped labs but also due to the concern of the frequent change in drought, flood...".

This has nothing to do in a scientific assesment (equipment versus motivation). The same motivation should hold true for all tropical areas!

It would be worth to discuss in one paragraph somewhere (possibly together with the text page 6-6 about the proxies) the methods of tree ring standardisation which seem to have changed over time and lead to larger low frequency signals in the tree ring width based reconstructions.

Comments on the structure :

6.6.1 I think that the italic question for the section does not work. I suggest to add sub questions such as :

What do early instrumental records tell us? (p6-2, lines 7 to 39)

What new reconstruction efforts have been conducted since TAR for NH temperatures (6-2 lines 41 to 6-6 25)

What are the main sources of uncertainties in large scale climate reconstructions (6-6 lines 27 to 49) - should refer to the section introduction / description of proxies

What do NH temperature reconstructions tell us (6-6 lines 51 to 6-8 line 5)

Regarding climate forcings and simulations (6.6.3 and 6.6.4) there must be a cross verification with chapter 9, have you looked at their revised text? The title 6.6.3 includes too much reference to modelling. They have been also statistical efforts to relate forcings and respondes (not only physical models) which have to be mentioned. Then modelling should be in 6.6.4 only. Another way could be to combine both in one section : 6.6.3 would be model-data comparisons with 1) forcings and 2) simulations versus reconstructions. Section 6.6.5 is too long compared to the # of studies conducted here.

Minor comments :

6-3 2 line 20 add "North European records"

line 27 and onwards I think that Boehm reconstruction should be cited around the Alps back to 1780 (it really deserves to be cited).

line 33 Chuine et al puts the French heat wave in a 700 perspective with grape harvest dates, which could be mentioned.

line 36 shorten to "detailed changes in various climate forcings"

line 44 : what are the documentary sources incorporated by Mann? I understand essentially early instrumental records.

6-3 line 49 : this paragraph is a bit vague. Maybe mention more clearly areas where no data are available. Goosse et al GRL 2004 used a synthesis of Antarctica data + simulations to discuss the pb of phase with Antarctica and could be mentioned. I suggest to replace "assimilated" which has a special meaning for meteorologists by "combined"

6-4 line 9 change "are" to "is"

line 16 : how many such long records are available (= what are "very few"?)

6-3 line 39 : is it the rapidity of the 20th c warming or the level of late 20th c temperatures that have to be discussed?

6-5 line 8 use reconstruction, not "series". I understand that one series is one proxy record and a mixture of records with various statistical methods is a reconstruction.

Line 31 : add "many of the individual annually resolved proxy series".

6-6 line 30 change "over a fixed calendar based time window such as J-A or J-D" to "over a specific season"

6-8 line 29 : I propose to change the text about tropical ice cores.

There are few strongly temperature-sensitive proxies from tropical latitudes. Water stable isotope records from high latitude tropical glaciers were first used as temperature proxies but recent calibration and modelling studies have confirmed that tropical precipitation isotopic composition is mostly sensitive to precipitation changes ("amount effect") at seasonal to decadal time scales both in south America and south Tibet.

References :

Hoffmann G, *Ramirez E*, Taupin JD, et al.

Coherent isotope history of Andean ice cores over the last century

<[1]<http://wos.isiknowledge.com/?SID=W1hPnja@D7cM8186jFa&Func=Abstract&doc=17/3>> GEOPHYSICAL RESEARCH LETTERS 30 (4): Art. No. 1179 FEB 25 2003

Vuille M, Werner M, Bradley RS, et al.

Stable isotopes in precipitation in the Asian monsoon region

<[2]<http://wos.isiknowledge.com/?SID=W1hPnja@D7cM8186jFa&Func=Abstract&doc=19/1>> JOURNAL OF GEOPHYSICAL RESEARCH-ATMOSPHERES 110 (D23): Art. No. D23108 DEC 8 2005

By the way, in the same paragraph, you cite tropical glacier retreat as caused by temperature changes. I suggest to refer to chapter 3 on this topic because many studies have also shown that precipitation / relative humidity / albedo effects can be very important for tropical glacier mass balance (see for instance Vincent et al, Comptes rendus Geosciences 2005).

Page 6-8, ground surface temperatures : are there tropical records available that could be explicitly discussed?

The problem of calibration mentioned line 29 (lack of the last decades of the 20th

century) also holds true for many of the long tree ring records... should it be explicitly highlighted here?

6-9 : line 9-10, what is a "much longer warm period", I do not understand. I think that this could be shortened. I still suffer that Antarctica is not mentioned at all. In Goosse et al 2004 I made a stack of 6 records from East Antarctica. There is also one good borehole record from Law Dome (Dahl Jensen Annals of Glacio 1998) showing the same features.

6-10 line 28 : I do not think that it is appropriate to discuss the Solanki paper here.

6-10 and 11 : why mix volcanic and anthropogenic surface aerosols rather than 2 sections? Why not discuss changes in surface occupation (land use) in the forcings for the last millenium at least in one sentence?

6-12, lines 38 and onwards : it seems that this is attribution and detection and should be a summary of chapter 9 or just a cross reference to chapter 9.

Section 6.6.5 (6-12 and 13) is too long compared to the studies cited. Maybe Fortunat could help to make this section more punchy. Should the PhD thesis of MacFarling Meure be cited in this assessment?

Remove "the best known aspects of the records"

Refer to chapter XX for biogeochemical cycles

The last paragraph is probably redondant with respect to the carbon cycle climate feedback discussed in that chapter.

Page 6-14 line 43 : redundancy in this paragraph. Does the coldest European winter have to be discussed in such detail? I would skip this (remove line mid 42 to beg of 45 and keep the last sentence of the paragraph which basically says the same thing.

The section on Asian monsoon variability is not focused on the last 2000 years but on millenial variability => mix with 6.4? Why not cite the Tibet ice core records here (ex Dasuopu 18O which should be a local precip record). There are also high res speleothem records with high resolution. Ramesh should help on this paragraph.

I hope that you find this useful, congratulations for the large improvements of this section and taking into account a record number of comments...

Valérie.

--

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[3]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://wos.isiknowledge.com/?SID=W1hPnja@D7cM8l86jFa&Func=Abstract&doc=17/3>
2. <http://wos.isiknowledge.com/?SID=W1hPnja@D7cM8l86jFa&Func=Abstract&doc=19/1>

3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Wahl, Eugene R" <wahle@alfred.edu>
To: "Caspar Ammann" <ammann@ucar.edu>
Subject: Wahl and Ammann ms 3321
Date: Fri, 24 Feb 2006 22:33:22 -0500
Cc: <kivel@stanford.edu>, "Jonathan Overpeck" <jto@u.arizona.edu>, <k.briffa@uea.ac.uk>, <eystein.jansen@geo.uib.no>

Hello all:

Here is a slightly revised update of the Wahl-Ammann final submission to Climatic Change. It is entirely unaltered in substance, design, methods, results, and conclusions.

The alterations are concentrated in Appendix 1 (the entire rest of the text is unaffected except for three words on p. 17)--focused on eliminating a small gap in logic in our description of the performance of the CE statistic (and to streamline the statements about the sign test and the product means test).

Stephen Schneider has these corrections and is still reviewing the manuscript.

Please replace the version sent earlier this week with this one.

Peace, Gene
Dr. Eugene R. Wahl
Asst. Professor of Environmental Studies
Alfred University

607-871-2604
1 Saxon Drive
Alfred, NY 14802

Attachment Converted: "c:\eudora\attach\Wahl_Ammann_3321_Final_21Feb-Revision1.doc"

From: "Rob Wilson" <rob.wilson@ed.ac.uk>
To: "Tim Osborn" <t.osborn@uea.ac.uk>
Subject: Re: Emailing: Wilson et al. technical comment
Date: Mon, 27 Feb 2006 14:28:29 -0000
Reply-to: "Rob Wilson" <rob.wilson@ed.ac.uk>
Cc: "rosanne" <rdd@ldeo.columbia.edu>, <K.briffa@uea.ac.uk>

Hi Tim,

yes, we processed our own RCS chronology using Jan's Jaemtland data.

I also agree that using Jaemtland or not would make little difference to the results.

Rosanne is presenting at this NAS meeting on Thursday which McIntyre is obviously going to use as a forum to muddy the waters even further. He has given us a hard time about the use of Gaspe and the Polar Urals chronologies and their influence on the 'hockey stick' trend over the past 2 centuries. However, removing these series makes little difference to our results in the past few centuries.

am just going through your e-mails w.r.t. the coral paper - it is a huge help

thanks

Rob

----- Original Message -----

From: [1]Tim Osborn

To: [2]Rob Wilson

Cc: [3]rosanne ; [4]K.briffa@uea.ac.uk

Sent: Monday, February 27, 2006 2:23 PM

Subject: Re: Emailing: Wilson et al. technical comment

Thanks for the very clear answers Rob.

We didn't use Jaemtland and you did, that is why McIntyre suggested that we disagreed. But in fact our reason for excluding it was not that it didn't correlate with temperature positively, but that we didn't even calculate a correlation because the RCS chronology series we received stopped in 1827 rather than 1978.

It is true that the full set of core data from Jan Esper span the range 1107-1978, but the RCS chronology we received spanned the range 1316-1827 only - and this matches the replication diagram in Esper et al.

([5]http://www.sciencemag.org/content/vol295/issue5563/images/data/2250/DC1/1066208S2_mend.gif)

which stops then for Jaemtland.

Presumably you obtained the set of core data and did your own RCS processing etc., rather than using the Esper et al. RCS chronologies?

Anyway, I think that clears up our supposed "differences" over Jaemtland, though do let me know if you have any more points to add. Our results would have been very little affected by including Jaemtland anyway!

Cheers

Tim

At 09:58 25/02/2006, Rob Wilson wrote:

>Moring Tim,

>answers in red.

>

>

>on a related matter, Science have forwarded me some

>questions/requests from McIntyre about our paper that they'd like our

>response to. One of them states that "D'Arrigo et al. (2006) have

>reported directly opposite findings in respect to the correlation

>between their RCS chronology and gridcell temperature for Jaemtland

>and the two foxtail series."

>I am not sure where he got that from.

>We used Jaemtland - it is a good site.

>We did not use the foxtail data for similar reasons for us not using

>the Bristlecone pine data (see below) .

>

>We didn't give a correlation for Jaemtland so it is hard for you to

>have obtained the "opposite of nothing"! But anyway, I wanted to ask

>whether in fact your Jaemtland differed from the one we used. The

>one we used should be the same as Esper et al., with data provided by

>Ed Cook. You seem to be citing Naurzbaev and Vaganov (1999) for your

>Jaemtland record which seems odd. And its start and finish years

>differ from the series I got, so I'm guessing that the data are

>different and thus there's no reason why different data would have
>consistent correlations. Also, do you know what correlation and for
>what season (annual-mean?) you got for Jaemtland?
>We also used the Esper data.
>The N+V reference is completely wrong. I checked with Rosanne. Not
>sure how that got in. The N+V reference is actually for Taymir.
>Apologies for that - hopefully there are no more mistakes like that.
>Anyway, to clarify what we did to the data, here is an exert from
>the report I wrote for Rosanne 2 years ago.
>
>"The data from this site were those utilised by Jan Esper for his
>Science paper. After removing a few low correlated series, the final
>data-set consists of 156 radii over the period 1106-1978.
>Unfortunately however, the period 1292-1315 is represented by only
>one radius and replication is only reasonable from the mid 14th century. "
>
>In the end, I used the period represented by 10 or more series - 1340-1978.
>This should agree with the data you have.
>
>As for correlations with temperature, Jaemtland is OK.
>Against the relevant local 5x5 Land CRU (version 1) grid, the STD
>and RCS chrons correlate with the Jun-Sep season at 0.48 over the
>1956-1970 period. No residual problems were found with this
>relationship. All screening was done up to 1970 so that potential
>divergence would not effect the screening process. In this situation
>though, there was no divergence for the 1971-1978 period.
>
>
>On your (D'Arrigo et al.) exclusion of the Boreal/Upperwright series,
>it wasn't clear which (one or more) of the 3 reasons listed applied
>to these: (1) no significant temperature correlation, (2) significant
>precip correlation, (3) too far south.
>I know that the temperature signal is debatable in such records, but
>I seem to recall you saying that on the longer time scales they (and
>I think you were referring to Boreal/Upperwright, but I may have been
>mistaken) showed some agreement with the N. American series from this
>recent paper, giving some support at least for a temperature
>signal. Is my recollection correct?
>As I said earlier, I did not look at the Foxtail data.
>However, I have played with the BP data.
>The sites I utilised are described in this extract.
>
>"Of the 10 Bristlecone pine chronologies sent to me, 3 chronologies

>were identified to express a significant summer temperature signal
>using correlation analysis against local gridded data. These three
>sites also load upon the same principal component in a PCA using all
>10 chronologies. These three sites are: Hermit Hill (N = 38;
>1048-1983) and Windy Ridge (N = 29; 1050-1985) from Colorado and
>Sheep Mountain (N = 71; 0 - 1990) from California (Figure 1)."

>
>The correlation of the STD and RCS chronologies against local
>gridded July-Sep mean temperatures is 0.38 and 0.34 respectively.

>
>I have also showed you a comparative plot of the RCS chronology with
>my North American average series and the comparison is pretty good
>for most of the record and certainly there does not seem to be any
>obvious inflation of index values in the 20th century.

>
>So - why did we not use this site:
>well

>(1) Steve Macintyre was kicking up a fuss about these data and we
>felt that perhaps it might be opening us to criticism if we used them

>(2) These data are have been reported to also show a precipitation
>signal. I did some analysis on a site basis, but cannot find the
>results. However, the precipitation signal in the 3 chrons used was
>also weak. The temperature signal is stronger. This agrees with the
>BP vs NA chronology comparison.

>(3) As this was a low latitude site, then we would also need to
>include other low latitude sites - e.g. from the Himalayas. Jan
>would not let me use his data for this region, so in the end, we
>decided to keep the data-set as high latitude as possible. Quebec,
>Alps and Mongolia being the most southerly sites.

>
>I hope this answers your queries. Rosanne is presenting at the NAS
>meeting next week, and we have been trying to address many of the
>criticisms of Macintyre that he is posting on his blog. I think Jan
>making his data available was probably bad timing.

>
>Rob

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sunclock: [8]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

References

1. <mailto:t.osborn@uea.ac.uk>
2. <mailto:rob.wilson@ed.ac.uk>
3. <mailto:rdd@ldeo.columbia.edu>
4. <mailto:K.briffa@uea.ac.uk>
5. http://www.sciencemag.org/content/vol295/issue5563/images/data/2250/DC1/1066208S2_med.gif
6. <mailto:t.osborn@uea.ac.uk>
7. <http://www.cru.uea.ac.uk/~timo/>
8. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>
Subject: Re: latest (as of time and date)draft of 2000 bit
Date: Mon, 27 Feb 2006 15:34:31 -0700
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith and Øyvind - I agree, this is great, and your priorities are on target. I'll prepare to help on those non-temp subsections after you take a look at the (especially the North Atlantic/NAO one - which is closer to your strength than mine, I suspect).

As for captions, they're in the Figs/Captions Worddoc that Øyvind sent on the 24th.

Thanks to Øyvind for doing the references job as suggested by Keith.

Best, peck

>Keith - I'll see what I can do. Nice going with 6.6.

>

>Cheers,

>Øyvind

>

>>Peck and all

>>here is version containing all Fortunat,
>>Valerie and Henry comments that are feasible to
>>do. PLEASE NOTE (at Valerie's suggestion) the
>>renaming of sections - which need to be
>>reproduced on contents page.

>>As for Figure captions , I am lost as I tried
>>to follow Fortunat , but do not think he has it
>>right - and our printer here has died (til
>>tomorrow) so can not see definitive list.
>>Tomorrow , with the full version and look at
>>the Figures I will sort this - do we have a
>>full list of Figure captions as a separate file?
>>I will look at the regional stuff tomorrow Peck
>>- but I suspect it is all weak and I can not
>>really help it much now.Please look also

>>yourself but I think at this stage we need to
>>go with what we have.
>>More important tomorrow , is for me to go
>>through what Gabi sent and check for
>>consistency.
>>
>>As for overall things not done - as I said
>>before , we have not really covered issue of
>>possible CO2 fertilization and "decline " issue
>>in trees , but this can not get done without a
>>early section rewrite , and I have to think
>>about where to say that lots of proxies do not
>>come up to the present - but again - more
>>important now to get all figures correctly
>>called out ,cross references to other Chapters
>>consistently called out, and especially
>>references sorted.
>>
>>How about Oyvind gets everyone now to check
>>that all refs in their sections are included in
>>list - and mark in our colour , on the list,
>>which are called out in these sections (just by
>>shading them . Then we can check what is not
>>needed and what is still missing.
>>I have to go home now but will work on final
>>consolidated draft when it returns asap
>>tomorrow from Oyvind (with most up to date
>>reference list if you can Oyvind - (thank
>>goodness you are helping)
>>cheers
>>Keith
>>
>>
>>
>>--
>>Professor Keith Briffa,
>>Climatic Research Unit
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>>Norwich, NR4 7TJ, U.K.
>>
>>Phone: +44-1603-593909
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>>

>><http://www.cru.uea.ac.uk/cru/people/briffa/>

>>

>>Attachment converted: Øyvind:Keith2000section.doc (WDBN/«IC») (003B260C)

>

>

>--

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--

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: latest draft of 2000-year section text
Date: Tue, 28 Feb 2006 11:21:28 -0700
Cc: Keith Briffa <k.briffa@uea.ac.uk>, rahmstorf@ozean-klima.de,
cddhr@giss.nasa.gov, joos <joos@climate.unibe.ch>, Eystein Jansen
<eystein.jansen@geo.uib.no>

<x-flowed>

Hi Tim, Keith and Stefan - We certainly can't get into the details of the debate, both for space reasons, and because K & T have gotten us away from the more "defensive" impression our FOD gave reviewers and others. Although I share Stefan's concern that we almost have to hammer the misinformation to death, I think we'll be ok dealing with it succinctly, and focusing on the bigger picture - Mann et al., and all the controversy is history - we know much more now, and it makes for stronger statements. Keith and Tim have done a nice job balancing all this, and we have to hope that all the Mann et al controversy will start sounding as dated as it is. I know I make that point pretty clearly when I talk to the media.

BUT, I leave it to Keith and Tim to tweak the discussion to reflect Stefan's concern as appropriate.

thanks, Peck

>Hi Stefan,

>

>our (Keith and mine) understanding of this issue is that Burger et al. (2006, Tellus, already published and therefore citable) already point out the von Storch et al. (2004) mistake in implementing the Mann et al. (1998) method. But we haven't stated this (or cited the Science in press comment) because Burger et al. also demonstrate that when they implement the method without the detrending step (i.e., following the Mann et al. approach more accurately than von Storch et al. did) then the bias is still there, though of smaller magnitude than von Storch et al. (2004) suggested. Given that we already say that the extent of any bias is uncertain, it does not seem necessary to go into the details any further by discussing the implementation by von Storch et al. of the Mann et al. method.

>

>Finally, I think (though here it is less clear from their paper and I am relying on my recollection of talking to Gerd Burger) that Burger et al. also show that the amount of noise von Storch et al. added to create the pseudo-proxies yields a pseudo-reconstruction that has much better verification skill than obtained by Mann et al. (1998) for their real reconstruction. If they increase the noise added (deteriorating the "skill" of the pseudo-proxies) until they get similar verification statistics as Mann et al. report, then the size of the bias gets bigger. In fact, the bias they obtain with the higher noise but "correct" no-detrending method is actually very similar to the bias von Storch et al. reported with lower noise but incorrect detrending method! So where does that leave us? I don't think there's room to put all this in. Of course the magnitude of

>the bias cannot be determined from any pseudo-proxy simulation
>anyway, and will be different for different models.
>
>We'd be interested to know if your (or others on the cc list)
>interpretation of Burger et al. (2006) is significantly different to
>this.
>
>Cheers
>
>Tim
>
>At 16:42 28/02/2006, Stefan Rahmstorf wrote:
>>Hi Keith and others,
>>
>>attached is the draft Keith sent on 21 Feb of the 2000-year
>>section, with comments and edits (grey) from me.
>>
>>I note that Von Storch et al. 2004 is cited without it being
>>mentioned that they did not implement the Mann et al. method
>>correctly - by detrending before calibration, the performance of
>>the method was greatly degraded in their model. I guess you left
>>this out because the comment to Science showing this is still in
>>press? Will it be added once this has been published? I think it is
>>a major point, as it was such a high-profile paper - Von Storch's
>>contention that the "hockey stick" is "nonsense" (cited in the US
>>Senate) is based on a mistake.
>>
>>Cheers, Stefan
>>
>>--
>>To reach me directly please use: rahmstorf@ozean-klima.de
>>(My former addresses @pik-potsdam.de are read by my assistant
Brigitta.)
>>
>>Stefan Rahmstorf
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--
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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: "Wahl, Eugene R" <wahle@alfred.edu>
Subject: Fwd: RE: Wahl Ritson Ammann Science article on vonStorch 04
Date: Tue, 28 Feb 2006 11:50:28 -0700
Cc: "Keith Briffa" <k.briffa@uea.ac.uk>, "Eystein Jansen"
<eystein.jansen@geo.uib.no>, "Caspar Ammann" <ammann@ucar.edu>,
rahmstorf@ozean-klima.de, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Eugene - quite timely. Keith and Tim are doing the final revision tomorrow, and we've actually been debating if the vonStorch issue was handled just right.

thx, peck

>X-Sieve: CMU Sieve 2.2
>Subject: RE: Wahl Ritson Ammann Science article on vonStorch 04
>Date: Tue, 28 Feb 2006 13:38:06 -0500
>Thread-Topic: Wahl Ritson Ammann Science article on vonStorch 04
>Thread-Index: AcY3ZrWjPf6A8R9vTWeseSE3GvqmgKLAFLDcogAACcoIA=
>From: "Wahl, Eugene R" <wahle@alfred.edu>
>To: "Jonathan Overpeck" <jto@u.arizona.edu>
>Cc: "Keith Briffa" <k.briffa@uea.ac.uk>,
> "Eystein Jansen" <eystein.jansen@geo.uib.no>,
> "Caspar Ammann" <ammann@ucar.edu>
>
>Sorry, I sent the message without the text. [The "send" button is next
>to the "insert" button on my software!!] Here it is.

>-----Original Message-----

>From: Wahl, Eugene R
>Sent: Tuesday, February 28, 2006 1:32 PM
>To: 'Jonathan Overpeck'
>Cc: Keith Briffa; Eystein Jansen; 'Caspar Ammann'
>Subject: RE: Wahl Ritson Ammann Science article on vonStorch 04
>

>Hello Jonathan, Keith, and Eystein:

>
>I don't yet have any word from Steve Schneider concerning the
>Wahl-Ammann article on the MBH/MM issues...

>
>...HOWEVER, here is something that slipped under my radar screen, about
>which I should have made you aware previously. I've attached the
>ACCEPTED version of the Wahl-Ritson-Ammann comment article on the
>vonStorch et al. 2004 Science paper. This the article that criticizes
>MBH for very large low-frequency amplitude losses. The final acceptance
>from Science just came TODAY, and is copied below.

>
>In this comment article (specifically requested to be expanded to 1000
>words by the Science editors), we note that the calibration and
>verification performance of the MBH method as implemented in VS04 show
>really poor LF fidelity--which cannot happen if the MBH method is
>implemented according to its original form. We note this, which is
>explained by a significant omission on the part of VS04 in implementing

>the MBH methodology (a detrending step that was only disclosed later
>last year in a conference proceedings paper). We also comment on
>physical and statistical reasons why detrending is not appropriate in
>this context. We conclude that the large amplitude losses VS04 claims
>are simply not correct.

>
>I am imagining that this contextualization of the VS04 critique would
>also be relevant for your chapter, and it can now be considered "in
>press" as the from our Science correspondent notes below. I would think
>this acceptance makes it "citable". If not, I understand.

>
>
>NOTE THAT THIS ARTICLE IS SUBJECT TO THE USUAL SCIENCE EMBARGO RULES. I
>DO NOT BELIEVE THAT THIS MEANS CITATION IS EMBARGOED. (Cf. 5th
>paragraph in copied message below, which supports citation.)

>
>
>Peace, Gene

>
>*****

>
>Dr. Eugene R. Wahl
>Asst. Professor of Environmental Studies
>Alfred University

>
>607.871.2604

>
>
>***** copied message below *****

>
>
>February 28, 2006 received 10:31 am EST

>
>Dear Dr. Wahl,

>
>Below is the formal acceptance of your manuscript. The paper is
>technically not "in press" yet, though I assume that either "accepted"
>or "in press" would be acceptable.

>
>
>Dear Dr. Wahl,

>
>We are pleased to accept your revised Technical Comment on the paper by
>von Storch et al. for publication.

>
>The text of your comment will be edited to conform to *Science* style
>guidelines. Before publication you will receive galley proofs for
>author corrections. Please return the marked and corrected proofs, by
>fax or overnight express, within 48 hours of receipt.

>
>For authors with NIH grants intending to deposit the accepted version of
>their paper on PubMed Central, the following text must be displayed as a
>footnote with an asterisk to the manuscript title:

>

>"This manuscript has been accepted for publication in Science. This
>version has not undergone final editing. Please refer to the complete
>version of record at <http://www.sciencemag.org/>. This manuscript may
>not be reproduced or used in any manner that does not fall within the
>fair use provisions of the Copyright Act without the prior, written
>permission of AAAS."

>

>As noted in our License for Publication, the manuscript cannot be posted
>sooner than 6 months after final publication of the paper in Science.

>

>As you know, the full text of technical comments and responses appears
>on our website, Science Online, with abstracts published in the Letters
>section of the print *Science*.

>

>Thanks for your patience during this long process, and thanks for
>publishing in *Science*.

>

>Sincerely,

>

>Tara S. Marathe

>Associate Online Editor, Science

>tmarathe@aaas.org

>

>***** end copied message *****

>

>Content-Type: application/msword;

> name="1120866RevisedText.doc"

>Content-Description: 1120866RevisedText.doc

>Content-Disposition: attachment;

> filename="1120866RevisedText.doc"

>

>

>Content-Type: image/jpeg;

> name="1120866Fig.jpg"

>Content-Description: 1120866Fig.jpg

>Content-Disposition: attachment;

> filename="1120866Fig.jpg"

>

--

Jonathan T. Overpeck

Director, Institute for the Study of Planet Earth

Professor, Department of Geosciences

Professor, Department of Atmospheric Sciences

Mail and Fedex Address:

Institute for the Study of Planet Earth

715 N. Park Ave. 2nd Floor

University of Arizona

Tucson, AZ 85721

direct tel: +1 520 622-9065

fax: +1 520 792-8795

<http://www.geo.arizona.edu/>
<http://www.ispe.arizona.edu/>
</x-flowed>

Attachment Converted: "c:\eudora\attach\1120866RevisedText1.doc"

Attachment Converted: "c:\eudora\attach\1120866Fig1.jpg"

From: "Wahl, Eugene R" <wahl@alfred.edu>
To: "Jonathan Overpeck" <jto@u.arizona.edu>
Subject: RE: Wahl Ritson Ammann Science article on vonStorch 04
Date: Tue, 28 Feb 2006 13:32:19 -0500
Cc: "Keith Briffa" <k.briffa@uea.ac.uk>, "Eystein Jansen" <eystein.jansen@geo.uib.no>, "Caspar Ammann" <ammann@ucar.edu>

Hello Jonathan, Keith, and Eystein:

I don't yet have any word from Steve Schneider concerning the Wahl-Ammann article on the MBH/MM issues...

...HOWEVER, here is something that slipped under my radar screen, about which I should have made you aware previously. I've attached the ACCEPTED version of the Wahl-Ritson-Ammann comment article on the vonStorch et al. 2004 Science paper. This the article that criticizes MBH for very large low-frequency amplitude losses. The final acceptance from Science just came today, and is copied below.

In this comment article (specifically requested to be expanded to 1000 words by the Science editors), we note that the calibration and verification performance of the MBH method as implemented in VS04 show really poor LF fidelity--which cannot happen if the MBH method is implemented according to its original form. We note this, which is explained by a significant omission on the part of VS04 in implementing the MBH methodology (a detrending step that was only disclosed later last year in a conference proceedings paper). We also comment on physical and statistical reasons why detrending is not appropriate in this context. We conclude that the large amplitude losses VS04 claims are simply not correct.

I am imagining that this contextualization of the VS04 critique would also be relevant for your chapter, and it can now be considered "in press" as the from our Science correspondent notes below. I would think this acceptance makes it "citable". If not, I understand.

NOTE THAT THIS ARTICLE IS SUBJECT TO THE USUAL SCIENCE EMBARGO RULES. I DO NOT BELIEVE THAT THIS MEANS CITATION IS EMBARGOED. (Cf. 4th paragraph in copied message below that supports citation.)

Peace, Gene

Dr. Eugene R. Wahl
Asst. Professor of Environmental Studies
Alfred University

607.871.2604

***** copied message below *****

Dear Dr. Wahl,

Below is the formal acceptance of your manuscript. The paper is technically not "in press" yet, though I assume that either "accepted" or "in press" would be acceptable.

Dear Dr. Wahl,

We are pleased to accept your revised Technical Comment on the paper by von Storch et al. for publication.

The text of your comment will be edited to conform to *Science* style guidelines. Before publication you will receive galley proofs for author corrections. Please return the marked and corrected proofs, by fax or overnight express, within 48 hours of receipt.

For authors with NIH grants intending to deposit the accepted version of their paper on PubMed Central, the following text must be displayed as a footnote with an asterisk to the manuscript title:

"This manuscript has been accepted for publication in Science. This version has not undergone final editing. Please refer to the complete version of record at <http://www.sciencemag.org/>. This manuscript may not be reproduced or used in any manner that does not fall within the fair use provisions of the Copyright Act without the prior, written permission of AAAS."

As noted in our License for Publication, the manuscript cannot be posted sooner than 6 months after final publication of the paper in Science.

As you know, the full text of technical comments and responses appears on our website, Science Online, with abstracts published in the Letters section of the print *Science*.

Thanks for your patience during this long process, and thanks for publishing in *Science*.

Sincerely,

Tara S. Marathe
Associate Online Editor, Science
tmarathe@aaas.org

***** end copied message *****

From: Tim Osborn <t.osborn@uea.ac.uk>

To: Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: latest draft of 2000-year section text

Date: Tue, 28 Feb 2006 17:10:45 +0000

Cc: jto@u.arizona.edu, eystein.jansen@geo.uib.no, Fortunat Joos <joos@climate.unibe.ch>, drind@giss.nasa.gov

<x-flowed>

Hi Stefan,

our (Keith and mine) understanding of this issue is that Burger et al. (2006, Tellus, already published and therefore citable) already point out the von Storch et al. (2004) mistake in implementing the Mann et al. (1998) method. But we haven't stated this (or cited the Science in press comment) because Burger et al. also demonstrate that when they implement the method without the detrending step (i.e., following the Mann et al. approach more accurately than von Storch et al. did) then the bias is still there, though of smaller magnitude than von Storch et al. (2004) suggested. Given that we already say that the extent of any bias is uncertain, it does not seem necessary to go into the details any further by discussing the implementation by von Storch et al. of the Mann et al. method.

Finally, I think (though here it is less clear from their paper and I am relying on my recollection of talking to Gerd Burger) that Burger et al. also show that the amount of noise von Storch et al. added to create the pseudo-proxies yields a pseudo-reconstruction that has much better verification skill than obtained by Mann et al. (1998) for their real reconstruction. If they increase the noise added (deteriorating the "skill" of the pseudo-proxies) until they get similar verification statistics as Mann et al. report, then the size of the bias gets bigger. In fact, the bias they obtain with the higher noise but "correct" no-detrending method is actually very similar to the bias von Storch et al. reported with lower noise but incorrect detrending method! So where does that leave us? I don't think there's room to put all this in. Of course the magnitude of the bias cannot be determined from any pseudo-proxy simulation anyway, and will be different for different models.

We'd be interested to know if your (or others on the cc list) interpretation of Burger et al. (2006) is significantly different to this.

Cheers

Tim

At 16:42 28/02/2006, Stefan Rahmstorf wrote:

>Hi Keith and others,

>

>attached is the draft Keith sent on 21 Feb of the 2000-year section,
>with comments and edits (grey) from me.

>

>I note that Von Storch et al. 2004 is cited without it being
>mentioned that they did not implement the Mann et al. method
>correctly - by detrending before calibration, the performance of the
>method was greatly degraded in their model. I guess you left this
>out because the comment to Science showing this is still in press?
>Will it be added once this has been published? I think it is a major
>point, as it was such a high-profile paper - Von Storch's contention
>that the "hockey stick" is "nonsense" (cited in the US Senate) is
>based on a mistake.

>

>Cheers, Stefan

>

>--

>To reach me directly please use: rahmstorf@ozean-klima.de
>(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)

>

>Stefan Rahmstorf

>www.ozean-klima.de

>www.realclimate.org

>

>

>

Dr Timothy J Osborn
Climatic Research Unit
School of Environmental Sciences, University of East Anglia
Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk

phone: +44 1603 592089

fax: +44 1603 507784

web: <http://www.cru.uea.ac.uk/~timo/>

suncllock: <http://www.cru.uea.ac.uk/~timo/suncllock.htm>

</x-flowed>

From: Stefan Rahmstorf <rahmstorf@ozean-klima.de>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: latest draft of 2000-year section text
Date: Tue, 28 Feb 2006 18:32:25 +0100
Cc: Keith Briffa <k.briffa@uea.ac.uk>, jto@u.arizona.edu, eystein.jansen@geo.uib.no, Fortunat Joos <joos@climate.unibe.ch>, drind@giss.nasa.gov

Hi Tim,

my simplistic interpretation as an outside observer of this field is:

VS04 published a high-profile analysis in Science concluding that the performance of the MBH method is disastrously bad. Subsequently, VS in the media called the MBH result "nonsense", accused Nature of putting their sales interests above peer review when publishing MBH, and called the IPCC "stupid" and "irresponsible" for highlighting the results of MBH. This had *major* political impact - I know this e.g. from EU negotiators who were confronted with this stuff by their US colleagues.

Then it turns out that they implemented the method incorrectly. If it is done as MBH did, variance is still somewhat underestimated in the same pseudoproxy test, but only a little, within the error bars given by MBH and shown by IPCC. Certainly nothing dramatic - one could conclude that the method works reasonably well but needs improvement. This would have been a technical discussion with not much political impact.

What VS and their colleagues are doing now, rather than publishing a correction of their mistake, is saying: "well, but if we add a lot more noise, or use red noise, then the MBH method is still quite bad..."

The question here is: should our IPCC chapter say something to correct the wrong impression which had the political impact, namely that the MBH method is disastrously bad? This is not the same as the legitimate discussion about the real errors in proxy reconstructions, which accepts that these reconstructions have some errors but are still quite useful, rather than being "nonsense".

Cheers, Stefan

--

To reach me directly please use: [1]rahmstorf@ozean-klima.de
(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)

Stefan Rahmstorf
[2]www.ozean-klima.de
[3]www.realclimate.org

References

1. <mailto:rahmstorf@ozean-klima.de>
2. <http://www.ozean-klima.de/>
3. <http://www.realclimate.org/>

From: "Wahl, Eugene R" <wahl@alfred.edu>
To: "Jonathan Overpeck" <jto@u.arizona.edu>
Subject: RE: Wahl Ammann Climatic Change article on MBH/MM
Date: Tue, 28 Feb 2006 21:42:42 -0500
Cc: "Keith Briffa" <k.briffa@uea.ac.uk>, "Eystein Jansen" <eystein.jansen@geo.uib.no>, "Caspar Ammann" <ammann@ucar.edu>

Hello all:

Good news this day. The Wahl-Ammann paper also has been given fully accepted status today by Stephen Schneider. I copy his affirmation of this below, and after that his remark from earlier this month regarding this status being equivalent to "in press". I hope this meets the deadline of before March 1 for citation.

Peace, Gene

***** first copied message

RE: provision of Wahl and Ammann ms 3321 to NAS committee
Stephen H Schneider [shs@stanford.edu]
 You replied on 2/28/2006 9:33 PM.
 Follow up
To: Wahl, Eugene R
Cc: katarina kivel

Hello from Sydney. I have now read your responses the the rereviewer and am satisfied you have done more than an adequate job. The paper is now accepted and you can post it where you wish with that designation. Let me know if there is anything else to do. Congratulations, Steve

***** second copied message

RE: Wahl and Ammann ms 3321
Stephen H Schneider [shs@stanford.edu]
 You replied on 2/28/2006 7:06 PM.
 Follow up
To: Wahl, Eugene R
Cc: katarina kivel

your interpretation is fine--get me the revision soon so I have time to assess your responses in light of reviews in time! Look forward to receiving it, Steve

On Sat, 11 Feb 2006, Wahl, Eugene R wrote:

> Hello Steve:
>

> Caspar and I expect to have the final manuscript to you in 7-10 days with all the revisions you requested in December. I have recently had some correspondance with Jonathan Overpeck about this, in his IPCC role. He says that the paper needs to be in press by the end of February to be acceptable to be cited in the SOD. [I had thought that we had passed all chance for citation in the next IPCC report back in December, but Peck has made it known to me this is not so.]

>

> He and I have communicated re: what "in press" means for Climatic Change, and I agreed to contact you to have a clear definition. What I have understood from our conversations before is that if you receive the mss and move it from "provisionally accepted" status to "accepted", then this can be considered in press, in light of CC being a journal of record.

>

> Peace, Gene
> Dr. Eugene R. Wahl
> Asst. Professor of Environmental Studies
> Alfred University

>

***** end of copied messages *****

Dr. Eugene R. Wahl
Asst. Professor of Environmental Studies
Alfred University

607-871-2604
1 Saxon Drive
Alfred, NY 14802

From: "Wahl, Eugene R" <wahl@alfred.edu>
To: "Jonathan Overpeck" <jto@u.arizona.edu>
Subject: RE: Wahl Ritson Ammann Science article on vonStorch 04
Date: Tue, 28 Feb 2006 23:23:25 -0500
Cc: "Keith Briffa" <k.briffa@uea.ac.uk>, "Eystein Jansen"
<eystein.jansen@geo.uib.no>

Hello Jonathan, Keith, and Eystein:

I want to make a reminder about the embargo for release of the WRA Science comment article. Please do not disseminate this article to anyone else, or discuss it publically until it is actually published, which I know Science wants to do soon. I still believe citation is appropriate, and I have asked for clarification on this from the editors. I will let you know what/if I hear from them.

FYI, this issue is also related to the NAS committee looking into last millenium surface temperature reconstructions this week, as I think you are aware. Today, the NAS staff person working with this committee said he talked to Jesse Smith of Science about this article, who mentioned he could say nothing, but referred the staff person to me. I was not really sure what this meant, and so I did not say anything specific on this myself, to ensure that I would not be in conflict with the embargo. That is where it stands in that arena for now.

As you saw in the message from Steve Schneider that I copied to you, however, there is no embargo of any kind on use of the Climatic Change article.

Peace, Gene
Dr. Eugene R. Wahl
Asst. Professor of Environmental Studies
Alfred University

607-871-2604
1 Saxon Drive
Alfred, NY 14802

From: Susan Solomon <ssolomon@al.noaa.gov>
To: <wgl-ar4-las@joss.ucar.edu>, wgl-ar4-las@joss.ucar.edu, <wgl-ar4-re@joss.ucar.edu>, wgl-ar4-re@joss.ucar.edu
Subject: [Wgl-ar4-las] Inappropriate Press Reports
Date: Wed, 1 Mar 2006 10:17:35 -0700
Cc: reate christ <RChrist@wmo.int>, bubu jallow2 <dwr@gamtel.gm>, bubu jallow1 <bubujallow@hotmail.com>, Jian Liu <Jianliu@wmo.int>, jouzel <jouzel@dsm-mail.saclay.cea.fr>, IPCC Chair <chairipcc@teri.res.in>

Dear Colleagues,

It has come to our attention that certain preliminary results of the WGI draft report may

have been provided inappropriately to the press, particularly the Guardian and the BBC.

Due to the nature of some of the specific material now appearing in the press (i.e.,

specific numbers discussed in our last LA meeting but not yet presented to others; see

<http://www.guardian.co.uk/frontpage/story/0,,1719608,00.html>),

and the nature in which it is being cited (i.e., a 'source' as indicated in

<http://news.bbc.co.uk/1/hi/sci/tech/4761804.stm>), there may be a connection to someone

inside our team, and this is both extremely disappointing and concerning to us.

As you will all be well aware, all of our findings are currently under development and

cannot be quoted or cited until the report is officially finalized at the end of January,

2007. Please do not give anyone the impression that you can currently represent

information on behalf of the IPCC, or provide information about the draft material in the

report. To do so would be not only a great discourtesy to your colleagues but may allow

others to question the credibility of the IPCC process.

We have previously circulated the attached LAGuide.pdf and are recirculating that here. We

would like to emphasize here that this applies to everyone involved in the report,

including review editors as well as authors, co-chairs, and bureau members. Please let us

know immediately at ipcc-wgl@al.noaa.gov if you find any aspect of this document

unacceptable to you.

We cannot overstate the importance of our all paying scrupulous attention to ensuring that

IPCC draft results are not revealed in any way that could lead to their appearing in a

press venue prior to formal approval. Please redouble your efforts to avoid being

misquoted, or misidentified as representing the IPCC's draft fourth assessment report.

Best regards,

Susan Solomon, Martin Manning and Qin Dahe

Attachment Converted: "c:\eudora\attach\LAGuide1.pdf"

list

Wg1-ar4-las@joss.ucar.edu

<http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-las>

From: Stefan Rahmstorf <rahmstorf@ozean-klima.de>
To: Fortunat Joos <joos@climate.unibe.ch>
Subject: Re: latest draft of 2000-year section text
Date: Wed, 01 Mar 2006 15:55:41 +0100
Cc: Jonathan Overpeck <jto@u.arizona.edu>, Tim Osborn
<t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>,
cddhr@giss.nasa.gov, Eystein Jansen <eystein.jansen@geo.uib.no>

Hi all,
let me add to Fortunat that I feel Keith and Tim have done a tremendous job in very thorny terrain. And I agree with Peck - science has moved way past the "hockey stick" debate, and it is great how our chapter shows that. Nevertheless, we should remember that the Von Storch et al. (2004) critique was a fundamental methodological critique that applies to *all* (or at least most) proxy reconstructions - it is not just a Storch vs. Mann quarrel (although it is that as well, of course). Hence it is worth mentioning their error, else this could still call the entirety of our conclusions from that section into question. Currently, our draft just says:

At present, the extent of any such bias in specific reconstructions is uncertain

This is true, but leaves in my view slightly too much room for interpretation - like, it would still encompass the interpretation that the bias of all reconstructions is disastrous, so they are all "nonsense" in Von Storch's words. What about saying something along the lines: "At present, the extent of any such bias in specific reconstructions is uncertain, although probably not as large as suggested by Von Storch et al. (2004), whose work was affected by a calibration error (Wahl, Ritson and Amman, 2006)."
Regards, Stefan
p.s. Tim: Are you convinced the more recent papers by the VS group use the correct calibration? In those curves that are intended to show the pseudoproxies perform poorly even when calibrated correctly, as long as you add a lot more noise, I wonder why the pseudoproxies perform poorly even within the calibration interval, where they now should be calibrated to properly reproduce the 20th C warming trend, and they don't?

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Fortunat Joos <joos@climate.unibe.ch>
Subject: Re: latest draft of 2000-year section text
Date: Wed, 01 Mar 2006 16:59:37 +0000
Cc: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, cddhr@giss.nasa.gov, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi again Stefan,

At 14:55 01/03/2006, Stefan Rahmstorf wrote:
What about saying something along the lines:

>"At present, the extent of any such bias in specific reconstructions
>is uncertain, although probably not as large as suggested by Von
>Storch et al. (2004), whose work was affected by a calibration error
>(Wahl, Ritson and Amman, 2006)."

This sounds good and Keith is currently working your suggested
wording into the paragraph in question.

>p.s. Tim: Are you convinced the more recent papers by the VS group
>use the correct calibration? In those curves that are intended to
>show the pseudoproxies perform poorly even when calibrated
>correctly, as long as you add a lot more noise, I wonder why the
>pseudoproxies perform poorly even within the calibration interval,
>where they now should be calibrated to properly reproduce the 20th C
>warming trend, and they don't?

I am not certain, of course. And yes, there is a link between the
degree to which the trend over the calibration period is captured and
the amplitude of long-term fluctuations in the reconstruction. That
many of Burger's multitude of methods do not obtain the full warming
trend, while Mann et al. do, is certainly a concern here. But it is
also true (and I have myself analysed this one year before von Storch
et al. was published - if only I'd realised the implications I could
have had another Science paper! :-)) that correct implementation of a
regression method, keeping the trend in, can still lead to a massive
underestimation of that trend. So there's still more work to be done
on this topic!

Cheers

Tim

Dr Timothy J Osborn
Climatic Research Unit
School of Environmental Sciences, University of East Anglia
Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk
phone: +44 1603 592089
fax: +44 1603 507784
web: <http://www.cru.uea.ac.uk/~timo/>
sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: oyvind.paasche@bjerknes.uib.no
Subject: Text here for 6.6 BUT not references -help
Date: Wed Mar 1 21:50:02 2006
Cc: jto@u.arizona.edu,Eystein Jansen <eystein.jansen@geo.uib.no>,t.m.melvin@uea.ac.uk

Peck

here is a version you can look at. The text in blue , I suggest deleting.

please also see my message to Oyvind below

Oyvind

here is a word file that is very near to the final version for this stage, of the 6.6 section.

NOTE that we (really Tom Melvin here) have had a nightmare with trying to get references in endnote and keeping the text as I wrote it . We need to work on finding and sorting a few references - but in working today , Tom found endnote reordering the references being called out in the text - actually moving them into incorrect places! To meet todays deadline I am sending this word file version of my text , which except for possible minor typos , is the version that I consider done (with the exception of changes Peck may wish to make to the Regional section).

Tomorrow , could you please liaise with Tom here (see his email cc'd) to discuss how to get the same text associated with the correct references in the way you want. Tom, as far as I understand is mostly there - but whether his version of this text corresponds with what it should say now - is beyond my comprehension. I have had enough of this system and I think we should have simply used word. I am sure there will be minor formatting problems and inconsistencies in the way cross referencing is done in what I am sending . I am also sure that knowing which reference was meant and which is now cited will take some sorting . Please let Tom know how you wish to proceed with this as soon as you know and he may be able to comply. Thanks - now I am going home

Keith

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784

[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: <oyvind.paasche@bjerkes.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: last minute changes.
Date: Fri, 3 Mar 2006 08:43:34 -0700

<x-flowed>

Hi Keith - pls cc everything regarding change to Øyvind, as there is basically no time left for changes now. thanks

Ø - can you remove ref to Wilson et al in 6.6 at the end? Keith is going to tell us more about the cited Tett et al submitted..

thanks, peck

>X-Sieve: CMU Sieve 2.2
>Date: Fri, 03 Mar 2006 09:30:41 +0000
>To: Jonathan Overpeck <jto@u.arizona.edu>,
> Eystein Jansen <eystein.jansen@geo.uib.no>
>From: Keith Briffa <k.briffa@uea.ac.uk>
>Subject: Re: Fwd: gabi's 1500-year reconstruction
>Cc: "Susan Solomon" <Susan.Solomon@noaa.gov>
>X-UEA-Spam-Score: -102.8
>X-UEA-Spam-Level: -----
>X-UEA-Spam-Flag: NO

>
>Let us stay with Gabi as it is in Figure etc.
>and as you say in the Nature paper anyway. We
>may have more problem with Tett et al. - need to
>check status , and Wilson et al is not as it
>turns out accepted yet - awaiting corrections.
>So this will definitely NOT make it and will
>have to come out - so wondered if Peck Julie
>could just look at the coral bits where this is
>cited and see if it can be removed easily. Will
>be in touch re Tett et al.
>Keith

>
>At 06:51 03/03/2006, Jonathan Overpeck wrote:
>>Hi guys - great timing here for this message
>>from Francis, and I don't think we can (or
>>should) do anything. It seems Gabi's recon is
>>in press, and that's the way it is. I suspect
>>Gabi's J Clim paper will come out before the
>>TOD too, but since it's in press in Nature,
>>it's published.
>>
>>I don't think the IPCC has to provide anything

>>>beyond the report - in fact, I'm almost sure
>>>Susan made this point to me/a bigger group
>>>already. I'll cc this to her, just so she
>>>know's what might be coming, but I think we're
>>>fine. M&M can get Congress to ask the FBI to
>>>secret Gabi away forever for doing her science
>>>the accepted way. Seriously, it's up to her to
>>>make things available as appropriate.

>>>Of course, I could be too sleep-deprived too.
>>>Am I correct in my assessment? I don't feel
>>>like calling Gabi at 2am (her time) to discuss
>>>making changes (e.g., to text, let along figs)
>>>that it's too late to make anyhow. I'll respond
>>>to Francis after I hear from you.

>>>Anyhow, I'm just about to send the full SOD
>>>text back to Norway for final minor editing. It
>>>looks good.

>>>Best, peck

>>>>X-Sieve: CMU Sieve 2.2
>>>>Date: Thu, 02 Mar 2006 17:11:24 -0800
>>>>From: Francis Zwiers <francis.zwiers@ec.gc.ca>
>>>>To: Jonathan Overpeck <jto@u.arizona.edu>
>>>>Cc: Gabi Hegerl <hegerl@duke.edu>
>>>>Subject: gabi's 1500-year reconstruction
>>>>Hi Peck,

>>>>I just got a call from Gabi, who spent the day
>>>>in Washington at that NAS panel on the hockey
>>>>stick. She doesn't have access to e-mail
>>>>today, and so asked me to convey a message.

>>>>McIntyre and McKittrick were there, and seem
>>>>to have left Gabi with the strong impression
>>>>that they will be insisting on having access
>>>>to supporting data, etc., used to build
>>>>reconstructions. Gabi says that this is
>>>>making her nervous, wants to make sure that
>>>>you are aware of the status of her
>>>>reconstruction, and wants to be sure that you
>>>>are comfortable with continuing to use it in
>>>>Ch 6. She says that if you feel it necessary
>>>>to exclude her reconstruction from your SOD of
>>>>Ch 6, you should do so. The reconstruction is
>>>>used in her Nature paper on sensitivity, which
>>>>has been accepted, but the Nature paper does
>>>>not describe the reconstruction or the

>>>supporting data in any detail. There is a
>>>paper under review at J. Climate that does do
>>>that (which is cited in the Nature paper), but
>>>unfortunately, an editorial decision is still
>>>pending.

>>>
>>>I hope that I've conveyed her message
>>>correctly. If you have a few minutes, it
>>>might be a good idea to give Gabi a call on
>>>her cell at bit later this evening (919 451
>>>2773).

>>>
>>>Cheers, Francis

>>>
>>>PS - hope things are progressing with your
>>>chapter. Things are a bit hectic here!

>>>--
>>>Francis Zwiers, Chief
>>>Canadian Ctr for Climate Modelling and Analysis
>>>Climate Research Division, Environment Canada
>>>c/o University of Victoria
>>>PO Box 1700, STN CSC
>>>Victoria, BC V8W 2Y2

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>>>Fax: (250)363-8247
>>>Web: <<http://www.cccma.bc.ec.gc.ca>><http://www.cccma.bc.ec.gc.ca>

>>
>>
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>>Jonathan T. Overpeck
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>
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--

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: photographs and other visuals for Science
Date: Fri, 3 Mar 2006 10:07:17 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith - thanks. Plan sounds good, and I will use this email to start the "do for next draft" file. Thanks, peck

>Peck

>

>we do need to say something , but as I said in an earlier message ,
>not without more consideration. We should not write something curt
>on this - ditto the Co2 possible fertilisation . In the push to do
>all this other stuff , we have had to leave it - to discuss later
>how to include an uncertainty issues bit about recent environmental
>mess ups . The D'arrigo paper is not convincing , but we have to do
>some work to show why , instead of just saying this . The divergence
>issue is NOT universal , and not unrelated to very recent period
>bias arising from processing methods . It is VERY LIKELY not the
>threshold problem D'Arrigo thinks it is. We need money here to work
>on this and losing our last application to Europe has messed us up.
>For now we can not include anything. I will work on text for the
>next iteration.

>

>At 16:05 03/03/2006, you wrote:

>>Hi Richard - this issue is one that we refer to in our key
>>uncertainty table. I believe Keith Briffa was one of the first to
>>write about it, and it is an important issue. I haven't seen R's
>>paper or results myself, but I bet Keith has. I'm cc'ing this to
>>him to see what he thinks.

>>

>>thanks, peck

>>

>>>Know anything about the "divergence problem" in tree rings? R D'arrigo
>>>talked to the NRC yesterday. I didn't get to talk to her afterward, but
>>>it looked to me that they have redrilled a bunch of the high-latitude tree
>>>rings that underlie almost all of the high-res reconstructions, and the
>>>tree rings are simply missing the post-1970s warming, with reasonably high
>>>confidence. She didn't seem too worried, but she apparently has a paper
>>>just out in JGR. It looked to me like she had pretty well killed the

>>>hockey stick in public forum--they go out and look for the most-sensitive
>>>trees at the edge of the treeline, flying over lots and lots of
>>>trees that are
>>>less sensitive but quite nearby, and when things get a little warmer, the
>>>most-sensitive trees aren't anymore, and so the trees miss the extreme
>>>warming of the recent times, and can't reliably be counted as catching
>>>the extreme warmth of the MWP if there was extreme warmth then.
>>>Because as far as I can tell the hockey stick really was a
>>>tree-ring
>>>record, regardless of how it was labelled as multiproxy, this looks to me
>>>to be a really big deal. And, a big deal that may bite your chapter...
>>>--Richard

>>
>>
>>--
>>Jonathan T. Overpeck
>>Director, Institute for the Study of Planet Earth
>>Professor, Department of Geosciences
>>Professor, Department of Atmospheric Sciences

>>

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>--

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--

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</x-flowed>

From: "Tim Osborn" <t.osborn@uea.ac.uk>
To: k.briffa@uea.ac.uk
Subject: [Fwd: Re: data request to SCIENCE for 1120514]
Date: Tue, 7 Mar 2006 08:22:22 -0000 (GMT)
Reply-to: t.osborn@uea.ac.uk
Cc: t.osborn@uea.ac.uk

Keith - see below. I bet it won't be the end of the episode! - Tim

----- Original Message -----

-
Subject: Re: data request to SCIENCE for 1120514
From: "Jesse Smith" <hjsmith@aaaas.org>
Date: Mon, March 6, 2006 8:03 pm
To: t.osborn@uea.ac.uk

-
Dear Dr. Osborn,

Thank you for your clear and careful response to the requests made by Dr. McIntyre, which we forwarded to you: it was quite satisfactory, we believe, and will greatly help Brooks (Hanson) in crafting his reply to Dr. McIntyre. I hope that this will be the end of this episode, but if it is not, we will be in touch again.

Best regards,

Jesse Smith

=====
Dr. Jesse Smith
Senior Editor

Science
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Washington, DC 20005
USA

(202) 326-6556
(202) 408-1256 (FAX)
hjsmith@aaaas.org
=====

>>> Tim Osborn <t.osborn@uea.ac.uk> 3/3/2006 11:22:17 AM >>>

Dear Jesse Smith and Brooks Hanson,

thank you for your patience while waiting for our reply. Before responding to the specific data requests, we would like to say that it is our view that we should provide sufficient data to enable all the main elements of our analysis to be checked, but that we are not obliged to provide the data that would enable the research reported in other papers to be checked, even if we cite those other papers or

use results reported in those other papers. You will see how this view has determined our response to some of the requests.

Now to the requests themselves, numbered according to the numbering system of Steve McIntyre's email.

(1) As you know, we provided (in advance of publication) the 14 smoothed and normalised proxy records to WDC-Paleo that enable the main parts of our analysis to be replicated. The only part of our analysis for which the unsmoothed data are required is to calculate the correlations against temperature that we reported for some of the series (not those that had already been reported by Mann and Jones, as indicated in our Table S1). These unsmoothed data for all 14 series are now also archived at WDC-Paleo, which will enable those correlations that we reported in Table S1 to be checked. These unsmoothed data were archived on Thursday 23rd February, in response to a request by a different colleague. This should cover this request in full.

(2) Our Table S1 provides the full citation to the source of our data, funnily enough given in the column labelled "Data source". Some of these may or may not have publicly archived their data, but our WDC-Paleo entry now contains the series that we were originally provided with (i.e., the unsmoothed data that we refer to in item (1) above). The "Orig source" column in our table was our effort to ensure that original work on collecting/processing these data is acknowledged, because it is important for us to acknowledge that work even when we obtained the data from a secondary study. We did not intend to imply that the data that we had used would match the data in these original sources, because various different versions might exist (due, e.g., to different methods of processing the data, or due to updated measurements, etc.). That is why we made the source of our data clear.

(a-c) We have not yet had time to double check the ITRDB citations that we provided for these three records, but we will do so as soon as we have time. Our data source was in fact Esper et al. (2002) and this is correct, so the concern over the accuracy of these ITRDB citations does not limit the ability for others to check our work.

(d-f) The original studies that we cite are definitely correct for these two records. We have provided sufficient data for our analysis of these records to be checked. We have not provided extra data to enable other people's studies to be checked, nor do we feel obliged to do so.

(g) These series from Esper et al. (2002) were considered by us and then rejected. As we understand it, Esper et al. have made available their site RCS records and therefore these four records could be obtained from Esper et al. If this is not the case, we could provide these four rejected series.

(3) D'Arrigo et al. (2006) do **not** report directly opposite findings in respect to the correlations we obtain for Jaemtland and

Boreal/Upperwright. Neither paper reports any correlations involving these series versus temperature. Both papers list more than one reason why series might be rejected. For example, our reasons were "We removed series from (S1) that did not correlate positively with their local annual or summer temperatures (Table S1), or which did not extend into the period with instrumental temperature to allow a correlation to be calculated." The latter is our reason for excluding Jaemtland, not the former: the Jaemtland series that we obtained from Esper et al. (2002) has no data after 1827 and so no correlation was calculated. The Jaemtland series used by D'Arrigo et al. continues through to 1978 due to the inclusion of additional data. Similarly, D'Arrigo et al. list a number of reasons for excluding series, but they do not state which one(s) were used to exclude Boreal and Upperwright, though in fact none disagreed with our criteria anyway!

We have not separately stored the temperature time series used to obtain the correlations reported in our Table S1 and to do so requires some changes to our program, which we have not done because there does not appear to be a need to do so (given our explanation above of the situation regarding our paper versus D'Arrigo et al., 2006).

Because Steve McIntyre has explicitly stated that he is unable to verify our results for the Boreal/Upperwright case, we have extracted the temperatures we used for that case only and attach them here as a text file. We hope that he can use them to reassure himself about the correlations that we obtained.

(4)

(a) We explicitly state that we did not use the Esper et al. (2002) Jasper series, so there is no expectation that they should be identical. Esper et al. (2002) have, we believe, made their version available and we have made available the series that we used via WDC-Paleo.

(b) Similarly, we explicitly state that we did not use the Esper et al. (2002) Tornetrask series and data are available as for (a).

(c) We are not obliged to confirm anything that Esper et al. (2002) did.

(5) This request is not relevant to our paper, as discussed at the start of this email.

(6) Same as (5).

We hope that we have dealt with these requests to a more than satisfactory extent, but please let us know if you feel that we should do more.

Best regards

Tim Osborn and Keith Briffa

At 19:30 23/02/2006, you wrote:

>Dear Dr. Osborn,

>

>We have just received an email from Steve McIntyre (pasted below),
>with a long and very specific list of alleged deficiencies in the
>availability of data by which to evaluate your recent paper, "The
>Spatial Extent of 20th-Century Warmth in the Context of the Past
>1200 Years," and others. Wishing to deal with this issue in a
>conscientious and reasonable way, we are passing the email along to
>you as a request for data, without taking a position on the validity
>of any particular point. We would like to have your confidential
>response to this request, keeping in mind the stated policy of
>SCIENCE that "Any reasonable request for materials, methods, or data
>necessary to verify the conclusions of the experiments reported must
>be honored." Please return your response by email directly to me,
>and CC: Brooks Hanson, our Deputy Editor
>(<<mailto:bhanson@aaas.org>>bhanson@aaas.org). We appreciate your
>cooperation, as well as the time and effort that a reply may
>take. Feel free to contact me if you have any questions about this
issue.

>

>

>Sincerely,

>

>Jesse Smith

>

>*****START OF EMAIL FROM S. MCINTYRE*****

>Dear Dr Hanson,

>

>Thank you for your prompt response to my letter in respect to Osborn
>and Briffa [2006], Esper et al [2002] and Thompson et al [1989;
>1997]. I appreciate your efforts in this and realize that you are
>frustrated at being criticized. However, if you reflect on the
>matter, I'm sure that you will agree that the problem stems entirely
>from the original authors failing to comply with Science's data
>archiving policy.

>

>It will come as no surprise to you that I do not believe that the
>additional data, useful as it is, comes anywhere near discharging
>Science's obligations under its data policies for reasons that I
>will set out in detail below. I will discuss the shortfalls in
>connection with what I understand to be one of Science's governing
>policies
><http://www.sciencemag.org/feature/contribinfo/prep/gen_info.dtl#datadep
>http://www.sciencemag.org/feature/contribinfo/prep/gen_info.dtl#datadep

>:

>

>Science supports the efforts of databases that aggregate published
>data for the use of the scientific community. Therefore, before
>publication, large data sets . must be deposited in an approved
>database and an accession number provided for inclusion in the

published paper.

>

>Since the issue pertains to how Science discharges its policies, it
>is my position that you, rather than the original authors, are the
>appropriate arbiter of that. (Additionally, the authors have refused
>all requests in the past and I see no reason why their behavior
>would now differ.)

>

>Status of Each Request:

>

> 1. Digital versions of all 14 series as used in their
> final compilations;

>

>I have inspected the archive at

><ftp://ftp.ncdc.noaa.gov/pub/data/paleo/contributions_by_author/osborn20
06/osborn2006.txt>ftp://ftp.ncdc.noaa.gov/pub/data/paleo/contributions_by
_author/osborn2006/osborn2006.txt,

>to which you directed me. This consists of smoothed (and re-scaled)
>versions of the 14 series and is relevant to the request, but does
>not satisfy it. The authors specifically discuss correlations of
>these series to temperature, which requires consideration of the
>pre-smoothed series. Accordingly, I re-iterate my original request
>for digital versions of the 14 series.

>

>2. For each of the tree ring sites analysed (both the 11
>retained and Esper site not used, including Gotland, Jaemtland,
>Mackenzie Mts and Zhaschiviersk), an exact data citation to a public
>archive (e.g. WDCP) for the data set used; or, in the alternative,
>an archive of the data set at the Science website. In cases, where
>the publicly archive dataset for a site is related to but different
>from the version used by Osborn and Briffa, please archive the data
>set as used.

>

>I was able to reasonably reconcile the smoothed series to original
>sources in public archives and accordingly have no issue with data
>provenance for the following Osborn and Briffa series: the Mann PC1
>(#1); #5 Chesapeake; - #6 - Fisher's Greenland O18 stack; #7 -
>Netherlands documentary; #14 - Yang's China composite (although
>there are problems in the Thompson series used in this composite).
>For other users less familiar with nuances of series versions, I
>recommend that the SI be modified to provide accurate data citations
>for these 5 series.

>

>The problems mostly pertain to tree ring data, which make up the
>other 9 series. In three cases, Osborn and Briffa provided data
>citations for sites in public archives (#4 - Quebec- canal69; #8 -
>Tirol - germ21; #11 - Mangazeja - russ067, russ068). In each of
>these 3 cases, the Esper version reconciles to the Osborn version
>(up to re-scaling). However, they do not reconcile to the original
data sets.

>

>a) the dataset germ21, cited by Osborn-Briffa for series #8-
>Tirol, has values from 1466 to 1837, while the archived version goes

>from 1324 to 1975. Obviously the data set has not been cited
>accurately or is incomplete.
>
>b) the series canal69 goes from 1352 to 1989, while the Osborn
>version (#4 - Quebec) goes from 1352 to 1947. Again, it appears
>that the data set has not been cited accurately or is incomplete.
>Additionally, while I have been able to substantially replicate the
>features of other RCS chronologies, my efforts to reproduce the
>archived result from canal69 lead to a series with a significantly
>different shape.
>
>c) one of the two cited data sets (russ067) does not contain
>measurements at WDCP. However, the versions "mangazla" and
>"mangazpc" in the Schweingruber section of WDCP appear to have the
>data for russ067 and russ068. However, these data sets only yield
>values from 1246 to 1969, while the archived Osborn version (#11 -
>Mangazeja) goes from 1246 to 1990. Some additional data must exist
>somewhere, but has not been archived at WDCP to date.
>
>Two sites (#9 - Tornetrask; #13 - Mongolia) have WDCP measurement
>archives (swed019; mong003 respectively), but there are
>inconsistencies between the data as archived and the length of the
>Osborn and Briffa versions.
>
>d) the WDCP archive for Tornetrask ends in 1990, which is
>inconsistent with the Osborn version which ends in 1993. This
>indicates that the data sets are not the same.
>
>e) similarly, the WDCP archive for Sol Dav, Mongolia begins in
>900, while the Osborn version begins in 800.
>
>For the following 5 sites, no archive of the measurements exists at
>all - a direct breach of Science's archiving policy:
>
>f) Jasper/Icefields, Boreal, Upper Wright, Taimyr, Yamal,
>
>Accordingly, I re-iterate my request that the measurement data
>consistent with the archived site chronologies be archived for each
>of the above items 2(a)- 2(f), as well as corresponding information
>for the following 4 sites considered in Osborn and Briffa:
>
>g) Gotland, Jaemtland, Mackenzie; Zhaschiviersk
>
>3. Digital versions of the specific gridcell temperature series
>used in each of the reported temperature correlations together with
>version date.
>
>As noted in my previous request, D'Arrigo et al [2006] have reported
>directly opposite findings in respect to the correlation between
>their RCS chronology and gridcell temperature for: Jaemtland and the
>two foxtail series. I have specifically been unable to verify their
>claim in respect to bristlecones. Accordingly, I re-iterate the
>request for the digital versions of the temperature data used in
>these calculations. (In connection with a similar request, Nature

>required Mann et al. to archive the exact temperature data used in MBH98.)

>

>4. Exact data citations to a public archive for all datasets used, or, if such do not exist, an archive of the data set at the Science website.

>

>While most Osborn versions match Esper versions up to re-scaling, they differ in three cases, and a separate Esper version is required in two of them:

>

>a) the Esper version for the Jasper data is different than the Osborn and Briffa version (as noted in Osborn and Briffa) and both data sets need to be made available;

>

>b) similarly, there are differences between the version of the Tornetrask series archived by Esper and the one archived by Osborn, again requiring examination of both data sets;

>

>c) the Polar Urals version of Esper differs from the Yamal version of Briffa. It is possible that the Esper version used a combination of data sets russ021 and russ176 (if so, would you please confirm this.)

>

>5. A clear and operational definition distinguishing "linear" and "nonlinear" trees, preferably with source code showing any differences in methodology.

>

>While the provision of site chronologies for 13 Esper sites is appreciated, one site (Mongolia) was unaccountably omitted. The corresponding information is requested.

>

>While the provision of the site chronologies was interesting and appreciated, according to my reading of Esper et al [2002], these site chronologies were not used in the calculations in the article, which distinguished between "linear" and "nonlinear". No operational definition is provided. combined with the unavailability of the bulk of the data, the calculations of "linear" and "nonlinear" chronologies cannot be replicated even from the recent information regarding Esper et al [2002] and this remains unresolved.

>

>6. Thompson provides a complete archive of both Dundee and Guliya ice cores, including both isotope and chemical data.

>

>While I appreciate that Thompson has provided sample information on (only) 2 Kilimanjaro cores, he did not provide the requested accompanying chemical information necessary for their interpretation. The Kilimanjaro data is obviously of little help with the Dundee and Guliya data.

>

>The U.S. Global Change Research Program required archiving of data commencing in 1991 and the World Data Center for Paleoclimatology has been in existence since then and has been online since 1994. Accordingly there was an adequate facility for the archiving of the

>Guliya core when it was published in 1997.
>
>I realize that the Dundee core was published in 1989, at a time when
>your present archiving policies were not in effect. However,
>Thompson has published versions of this series in other journals
>which are inconsistent with the version published in Science. I
>cannot imagine that you are content with such a situation. Even if
>you did not have policies at the time, I am sure that you can give a
>very firm request to Thompson and I find it difficult to believe
>that Thompson would refuse a direct request from Science to provide
>this data. If he has refused a direct request, then that too is
>relevant information, upon which I would appreciate confirmation.
>
>Again, I apologize for putting you in the middle of this and for the
>public nature of the exchange. However, some of this has been going
>on far too long with minimal results, leaving no alternative.
>However, I assure you that I will be equally public in commending
>you if and when you resolve matters. In my opinion, you should
>simply do the following:
>
>(1) send a copy of your data archiving policy to each
>of the authors: Osborn-Briffa; Esper et al. and Thompson;
>
>(2) tell Osborn-Briffa and Esper et al. that you
>expect them to comply with the policy which was in effect at the
>time of publication or else you will retract the article.
>
>(3) tell Thompson that, if he wants to publish at
>Science in the future, he should immediately clean up his archive
>for the earlier articles.
>
>Obviously there has been some inadequate housekeeping in the past. I
>can understand this and my concern is not with the past. My concern
>is with the present. You have an opportunity to remedy the situation
>now and no one will criticize Science for ensuring that paleoclimate
>authors meet Science's data archiving policies. On the other hand,
>you will be justly criticized both by me and others if you don't do
>so.
>
>
>
>Regards,
>
>Stephen McIntyre
>
>*****END OF EMAIL FROM S. MCINTYRE*****
>*****
>
>=====

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>Senior Editor
>-----
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>USA
>-----
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>(202) 408-1256 (FAX)
><mailto:hjsmith@aaas.org>hjsmith@aaas.org
>=====

Attachment Converted: "c:\eudora\attach\untitled-23.htm"

From: Jonathan Overpeck <jto@u.arizona.edu>
To: cddhr@giss.nasa.gov, Eystein Jansen <eystein.jansen@geo.uib.no>, rahmstorf@ozean-klima.de, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, joos <joos@climate.unibe.ch>, Dominique Raynaud <raynaud@lgge.obs.ujf-grenoble.fr>, "James Zachos" <jzachos@emerald.ucsc.edu>, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>, Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk
Subject: Fwd: Re: [Wgl-ar4-ch06] Chapter 6 glossary edited version
Date: Tue, 7 Mar 2006 12:02:12 -0700

Hi folks - seems the listserv is down again. Please take a look at the attached draft chap

6 glossary and send comments to me and David Rind today if you have any (Jim Z - hope you can look at the way we've butchered the preQ defns). Eystein and I would like to send to TSU tonight if we can.

Thanks, Peck

Date: Tue, 7 Mar 2006 11:45:06 -0700
To: David Rind <drind@giss.nasa.gov>
From: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: [Wgl-ar4-ch06] Chapter 6 glossary edited version
Cc:
Bcc: wgl-ar4-ch06@joss.ucar.edu, fons_baede@hetnet.nl
X-Attachments: :Macintosh HD:329718:Chapter 6 glossaryJTO.doc:

Hi David (and those who have contributed) - thanks!
I've attached a revised version, with my edited sections highlighted in yellow. I've tried to update some definitions to be more accurate (agree w/ Stefan, by the way, regarding D/O events), and also to standardize mention of time intervals. Also, I don't think we want to cite the sources you have cited, since these were only the sources used to get going. I think many of the definitions are updated significantly by our team. If you get any other feedback today, great. Please forward me and Eystein your final version at the end of the day, and we'll send to the TSU (and Fons). If you get no additional input, just let us know and we can send in the attached version w/ the yellow shading removed.

Thanks again, Peck

--

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Attachment Converted: "c:\eudora\attach\Chapter 6 glossaryJTO.doc"

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Fwd: divergence
Date: Wed, 8 Mar 2006 15:18:54 -0700
Cc: ralley@geosc.psu.edu, Eystein Jansen <eystein.jansen@geo.uib.no>, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, joos <joos@climate.unibe.ch>, t.osborn@uea.ac.uk, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>

Hi gang - Richard is raising important issues, and Keith is going to respond in some detail on Friday when he gets back. I am cc'ing this to a broader group of IPCC Chap 6 folks so that we make sure we (chap 6) deal with the issues correctly. I'm hoping that Keith will cc to us all, and we'll go from there.

For those just in on the issue raised by Richard. There is a paper written by Rosanne D'Arrigo that apparently casts serious doubt on the ability of tree ring data to reconstruct the full range of past temperature change - particularly temperatures above mid-20th century levels. Chap 6 obviously has to deal with this more in the next draft, so Eystein and I would like to get on top of it starting this week.

Keith or Richard - do you have a copy of this paper? Is it accepted?

Thanks, Peck

>X-Sieve: CMU Sieve 2.2
>Date: Wed, 8 Mar 2006 11:55:46 -0500 (EST)
>From: <ralley@geosc.psu.edu>
>To: jto@u.arizona.edu
>Cc: k.briffa@uea.ac.uk
>Subject: divergence
>

>Peck--Thanks. The big issue may be that you don't just have to convince me
>now; if the NRC committee comes out as being strongly negative on the
>hockey stick owing to RD'A's talk, then the divergence between IPCC and NRC
>will be a big deal in the future regardless. The NRC committee is accepting
>comments now (I don't know for how long)... As I noted, my observations
>of the NRC committee members suggest rather strongly to me that they now
>have serious doubts about tree-rings as paleothermometers (and I do,
>too...at least until someone shows me why this divergence problem really
>doesn't matter). --Richard

--

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From: Phil Jones <p.jones@uea.ac.uk>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Climate Audit
Date: Thu, 09 Mar 2006 13:48:31 +0000
Cc: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Dear All,

A lot of good points raised by the horizontal Eystein. Keith is hoping to do something on the recent tree growth issue.

What this sad crowd (nice words - I'll use the phrase again) don't realise is that the satellite data now agree with the surface. This is said in Ch 3 and will come home more forcefully once the CCSP report on vertical temperature trends comes out. This should be April or May according to Tom Karl who is overseeing it all. I say should as it apparently has to be approved by the White House! Peck will know why this is and the expertise of the people doing the approval!

I can say for certain (100% - not any probable word that IPCC would use) is that the surface temperature data are correct.

McIntyre is determined and the blog does influence people, unfortunately the media. As you say as issues are partially closed, they will move on to others.

Cheers
Phil

At 12:50 09/03/2006, Eystein Jansen wrote:

>Hi Phil, thanks for the greetings. The back is status quo-like, so today
>the neurosurgeons concluded I need a surgery to take care of the hernia
>that creates the pains. Will take place in a week or two, and I will be
>out of work for a month afterwards, but should be up and going in good
>time for Wengen and for LA4.

>

>One side effect of being stranded and in horizontal working mode is more
>time to browse the net, thus I have monitored the Climate Audit page.
>Looking at the discussions after the NAS panel meeting we should expect

>focus now to be sidetracked from PC-analyses and over to the issue of bad
>proxies and divergence from temperature in the last 50 years. Thus this
>last aspect needs to be tackled more candidly in AR4 than in the SOD, and
>we need to discuss how to do this, soon. The Key expert here is Keith and
>I guess we should be able to assess the situation based on his and
>D'Arrigo's work and the expertise at hand.
>The rather sad crowd of followers who put their confused ideas onto the
>blog is one thing - they can't make up their mind if tree-rings are
>correct over the past 50 years and the Instrumental data wrong (UHI story)
>or vice versa. The more important aspect is that the blog is now used a
>lot by media and McIntyre has immediate access to the international media
>in the form of being one of the key players in terms of paleoclimate,
>ironocally enough. He is extremely determined, has his skewed viewpoints
>and is of course very pompous, but the blog is effective for his goals.

>
>Cheers,
>Eystein

>
>At 08:39 +0000 09-03-06, Phil Jones wrote:

>> Peck,
>> I should stop looking at these sites. Was just looking during a
>> break yesterday pm.

>>
>> Spent part of yesterday going through the TS and SPM and
>> sent some comments in, only to be told they weren't specific
>> enough by Susan. Probably the last time I waste my time
>> doing that. I knew she had an agenda, but I hadn't fully
>> realised how extensive it was.

>>
>> We need to revisit AR4 at some stage. Let's talk about this
>> over some beers at the Wengen meeting - to decide if we do
>> anything at the Bergen one. I'm sure Susan is aware of most
>> of the issues..... well, I'd like to believe that. The trouble is
>> that the blog sites keep promoting the same arguments, it just
>> doesn't seem to matter how we try and respond - they are oblivious
>> to it. One issue we could discuss is data availability. Keith says
>> you're going to make all your series (in the plots available). This
>> should be across all chapters if done. This is a load of work, but
>> they'll just say it isn't enough. So, impossible to win, or even get a
>> draw.

>>
>> Keith is hoping to do something re Rosanne, but like all
>> of us we're not finding the time. There are a load of things

>> we want to write, but responding (even reading) all this

>> rubbish takes time.

>>

>> Hope you're better Eystein ! Looking forward to Bergen - partly

>> as we're closer then to seeing the back of IPCC!

>>

>> Cheers

>> Phil

>>

>>

>>

>>

>>At 23:15 08/03/2006, Jonathan Overpeck wrote:

>>>Hi Phil - I'm not a big blog guy - not enough time, nor good enough

>>>internet here. So, I'm not following the audit junk. Am I nuts?

>>>

>>>And, I'm not sure I understand what's going to happen when the AR4 comes

>>>out. Should we have some discussion on this - as a broader group w/

>>>Susan - in Norway? Or is some other strategy advised?

>>>

>>>What fun...

>>>

>>>I'm hearing about D'Arrigo's splash from other sources (Richard Alley) -

>>>hope Keith et al., have good counter arguments.

>>>

>>>best, peck

>>>

>>>>>> Caspar,

>>>> I guess you've seen the site in the last day or so.

>>>> Did you give them your CC paper to post up and attack?

>>>> They clearly shouldn't have it.

>>>>

>>>> There are some funny things (#32 on the verification r*r revealed),

>>>> but much has gone beyond that.

>>>>

>>>> D'Arrigo's Cherry Pie - where did Briffa graduate from!

>>>> Keith's web page isn't up-to-date as he's a professor now!

>>>>

>>>> I'm the greatest hoarder of climate information!

>>>>

>>>> It's the pages on Mike that are no longer funny.

>>>>

>>>> Peck - do you think Susan really understands what will

>>>> happen when the AR4 comes out?

>>>>

>>>> I heard from Jerry North thinks they will have a report
>>>> from the NAS meeting by April.

>>>>

>>>> Cheers

>>>> Phil

>>>>

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>>>>School of Environmental Sciences Fax +44 (0) 1603 507784

>>>>University of East Anglia

>>>>Norwich Email p.jones@uea.ac.uk

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</x-flowed>

From: <ralley@geosc.psu.edu>

To: eystein.jansen@geo.uib.no, joos@climate.unibe.ch, jto@u.arizona.edu, k.briffa@uea.ac.uk, ottobli@ncar.ucar.edu, p.jones@uea.ac.uk, ricardo@lab.cricyt.edu.ar

Subject: NRC and IPCC millennial temperatures

Date: Sat, 11 Mar 2006 15:27:19 -0500 (EST)

Cc: mmanning@al.noaa.gov, ssolomon@al.noaa.gov

Friends in the IPCC WG1 AR4--

My impression is that, for good reasons, the US NRC panel looking at the record of temperatures over the last millennium or two is not going to strongly endorse the ability of proxies to detect warming above the level of a millennium ago, and that a careful re-examination of the Chapter 6 wording and its representation in the TS and SPM would be wise. Some of you have seen some of the discussion that follows, in some of the rapid-fire emails over the last day or two, but I'd like to clarify a little.

Please note that I am NOT on the NRC committee, do not speak for them, and have no "inside" knowledge of what they are doing. I was asked to testify to them, and I heard remarks from some other speakers and questions from the committee in public forum. I did NOT represent the IPCC to the committee, either; I stated that although I was proud to be participating with the IPCC, I absolutely was not speaking for, representing, or presaging anything in the IPCC. (I was, however, favorably quite impressed with the NRC committee and their efforts.) Someone else may have a different impression of what went on; this is mine.

Among the presentations, involving borehole temperatures, corals, glaciers and ice cores, and historical records, that which to me seemed to interest the committee most was from Rosanne d'Arrigo, who reported (among many other things) on a just-published study in which northern tree-ring sites were revisited and updated, and in which many of those sites failed to track the recent warming documented instrumentally. She did not make a big deal out of this, but several of the questions afterward from the committee focused on this "divergence" problem. (And to note, Rosanne did not discover the divergence problem, which has been around and discussed for a while; her testimony, including the recent large effort to update some tree-ring records, stirred interest from some committee members.)

I would also note that one of the committee members was asking each presenter whether the presenter believed that temperatures could be reconstructed for

1000 years ago within 0.5 C, and that the presenters were answering with some qualified version of "no".

My guess is that the NRC committee will put these things together, find some papers on ozone damage and CO2 fertilization, consider Rosanne's statement that the preferred temperature-sensitive trees are rare and in restricted places (and thus that a prolonged warming could easily move those trees out of the sensitive band), and conclude that tree-ring reconstructions include larger errors than are returned by any of the formal statistics from calibration or aggregation of records, and thus that there is less confidence than previously believed in the relative warmth of recent versus Medieval times. I also consider it possible that they will point out the difficulty of using a composite temperature history consisting of proxy and instrumental data if some of the proxy data do not track the more-recent part of the instrumental data.

The IPCC must be the IPCC, not the NRC. But, if the IPCC and NRC look very different, there will be much comment, and we will have to be very sure. More importantly, I believe that real issues are raised here, and that better discussion of this should be included in chapter 6, and probably brought forward at least into the TS. I know I'm not in chapter 6, I know I'm not a tree-ring expert, and I know I'm sticking my nose in where it might not belong or be welcome. But the flurry of emails in the last couple of days has not convinced me that this one can be ignored; indeed, I am more convinced that there exist issues that the IPCC must discuss more thoroughly.

My impression of the status (and my thoughts about what chapter 6 might say) from a whole lot of quick reading, your emails, and the testimony and questions I heard, is along the lines of:

--> The TAR highlighted a temperature history composited from multi-proxy paleoclimatic indicators plus the instrumental record, showing anomalous recent warmth, with the recent warmth emerging well above the 95% confidence interval for the last millennium.

--> The multi-proxy paleoclimatic indicators reflect tree-ring results more than any other source.

--> Tree-ring records are responsive to many factors, and great care and effort go into isolating the temperature signal from other signals.

--> Tree-ring data, in common with essentially all paleoclimatic data, are not collected in a continually updated "operational" fashion analogous to that used for meteorological data, so the data sets end at different times; data used in the multi-proxy reconstructions cited in the TAR ended between the 1990s and the 1940s. This difficulty motivated the need to include instrumental as well as proxy data in the reconstructions.

--> In those data, there was some suggestion of non-temperature influences on the tree-ring reconstructions; in particular, some of the most-recent records did not record the full amplitude of the instrumental warming. This has come to be known as the "divergence" issue.

--> Much research has been conducted since the TAR, and additional evidence of divergence has emerged in some records, causing some aggregated reconstructions from proxy records to show less warming than does the instrumental record.

--> There are many hypotheses for non-temperature influences on tree-ring records, including: (i) recent damage (as by ozone); (ii) recent fertilization (as by CO₂); and (iii) decreasing sensitivity of tree-ring growth to temperature with increasing temperature (once it's warm enough, the trees are primarily responsive to other things). The nature of these and their timing relative to the interval in which tree-ring data were calibrated to instrumental records would control the effects on climate reconstructions. In general: (i) would mean that recent warmth is underestimated but warmth from a millennium ago is not; (ii) would mean that recent warmth is overestimated but warmth from a millennium ago is not; and (iii) would mean that both recent warmth and warmth from a millennium ago are underestimated.

--> Various arguments have been advanced to support (i), (ii), or (iii), with many workers in the field favoring (i). Nonetheless, further characterizing recent non-temperature influences on tree-ring growth remains an open research question, and no broad consensus has emerged on (i), (ii), (iii), or something else.

--> These considerations do not affect the conclusion that recent warmth is anomalous over the last few centuries; the strong correlations of the proxy data with temperature over the instrumental record, and the strong tree-ring signals, are evident.

--> These considerations do not affect the best estimate that recent warmth is greater than that of a millennium ago; the central estimate from proxy data

of latter-twentieth-century warmth is still above that of a millennium ago, with greater spatial coherence recently in the signal.

--> These considerations do somewhat affect the confidence that can be attached to the best estimate of recent warmth versus that of a millennium ago. If the paleoclimatic data could be confidently be interpreted as paleotemperatures, then joining the paleoclimatic and instrumental records would be appropriate, and the recent warmth would clearly be anomalous over the last millennium and beyond. By demonstrating that some tree-ring series chosen for temperature sensitivity are not fully reflecting temperature changes, the divergence issue widens the error bars and so reduces confidence in the comparison between recent and earlier warmth.

--Richard

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From: Jonathan Overpeck <jto@u.arizona.edu>

To: drdendro@ldgo.columbia.edu, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

Subject: NRC study

Date: Mon, 13 Mar 2006 16:29:34 -0700

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Hi Ed and Keith - I hate to say it, but Richard's take on the political aspects of the NRC vs. IPCC reports seem worth some extra effort. Since you were both invited to speak with the NRC committee, I would suggest that you both (together or separately) submit formal comments asap. I don't know when the comment period starts or ends, but I'm guessing you have to work fast. I'm also thinking that you two might want to get out a peer-reviewed paper on the topic really soon too. I worry that the hole will continue to deepen for dendroclimate if you two don't act to clarify what we know/don't know, and when it is safe (and why) to use dendroclimate data to address the issue of long-term variation in temperature.

Please don't construe my suggestions or comments as pro/con dendro, but rather just as someone who wants the truth - whatever it is - to be communicated clearly, and as best we know it. But, I do think that if Richard is suspect, dendro has a real problem. He doesn't have a personal bias in this, and is clearly trying harder than most to understand what's really going on with climate and the proxies.

Effort now might save time later.

Also, are you both going to be at the Swiss mtg in June? We really have to get this all ironed out better before the next (last) draft of the IPCC AR4.

Thanks, Peck

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: edwardcook <drdendro@ldeo.columbia.edu>

Subject: Re: NRC study

Date: Tue, 14 Mar 2006 00:32:37 -0700

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

Hi Ed - thanks for trying to fit something in quick for the NRC group. I'm not sure about Richard's full motives, but I think he has his heart in the right place - that the NRC Committee might have gotten the impression he did, and this will be reflected in their report, perhaps in a way that is even less satisfactory to you and Keith. And, this report will likely have enormous political potential. It needs to get things as right as possible from the start. So... time well spent on the part of you and Keith. Thanks much, peck

Hi Peck,

Being in Bangkok, on to PACLIM, on to CONCORD in Mendoza, back to Bangkok, and back to NY on May 1 makes it difficult for me to do much, but I will do what I can to salvage a bad situation. The longish emails I sent out to you all contain much of what I would write. The main point to make, one that Richard seems to be totally oblivious to, is that there is no evidence for loss of sensitivity prior to the 20th century in a large-scale NH sense like that seen in the 20th century. On the other hand, there is evidence that there was not a loss of sensitivity in a large-scale NH sense in my QSR paper (Fig. 6). I acknowledge the weakness in the data prior to about 1200, but even so the regional comparisons only show divergence between north and south in the 20th century, with none indicated during the putative MWP. So why is Richard and the NRC panel apparently stating without evidence that divergence probably is a problem in the past and, therefore, tree rings cannot be trusted to reconstruct past temperatures? It is honestly unscientific when the only evidence that I have seen refutes that premise, and it plays unfairly into McIntyre's hand. I almost admit to being very irritated that Richard should anoint himself as the arbitrator of this debate. He knows nothing substantive about tree rings. In that sense, he is just like Ray Bradley.

Cheers,

Ed

On Mar 14, 2006, at 6:29 AM, Jonathan Overpeck wrote:

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References

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From: Jonathan Overpeck <jto@u.arizona.edu>
To: edwardcook <drdendro@ldeo.columbia.edu>
Subject: Re: NRC Committee on Surface Temperature Reconstructions
Date: Tue, 14 Mar 2006 21:29:28 -0700
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>

Hi Ed (and Keith) - this looks good. For what it's worth, here are some comments:

1. I agree Keith should send in an independent letter by email too (I'd put both on letterhead or at least include as pdf attachments, so email forwarding wouldn't have the chance of messing it up) .
2. I would say right up front - first line that you'd like your letter (s) to go to all committee members, if possible with a cc to you. Don't leave any wriggle room.
3. cc to G. North and B. Otto-Bliesner - again, so there is no doubt that this gets to everyone
4. no need to mention IPCC. Focus on the science and the NRC review. Don't want to introduce extra politics.

Thanks both for doing this - I agree there is a real need to ensure that the panel has the science from the experts.

Best, peck

Hi everyone,
Here is a draft of what I want to quickly send to
Ian Kraucunas, Ph.D.
Board on Atmospheric Sciences and Climate
National Research Council of The National Academies
500 Fifth Street NW, Keck 705
Washington, DC 20001
Email: ikraucunas@nas.edu
Phone: (202) 334-2546
Fax: (202) 334-3825

He originally invited me to talk before the NRC. I do not have any other information on who to send it too. Please let me know what you think, but don't be too pedantic or critical at this stage. I get the feeling we have very little time to make an impact on the NRC committee and its report. I personally think that I am correct as far as I can take the argument. Let me know if I should send this on to Richard as well.

Ed

Dear Ian,

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Somewhat alarmingly, it is my impression now the the NRC committee members and other influential participants of the meeting have come to the conclusion that the observed 20th century "divergence" calls into serious question the value of the tree-ring reconstructions of temperatures over the past millennium. The implicit assumption being made is that the "divergence" is being caused by climatic change related to 20th century warming, conditions that could have also prevailed back during the Medieval Warm Period (MWP) some 800-1000 years in the past. If this were the case, then the concerns of the committee would be justified. However, the available evidence does not support such a conclusion. In a paper I published in *Quaternary Science Reviews* in 2004 (Cook et al., 2004), I reviewed the properties and interpretation of the tree-ring data used in the Esper et al. (2002) paper published in *Science*. The reasonably well distributed set of tree-ring data in both boreal and more temperate latitude sites around the Northern Hemisphere allowed me to split up the data into sub-regional ensembles, including 8 sites in the 55-70° north band and 6 sites in the 30-55° south band. The purpose was to show the overall robustness of the multi-centennial temperature signal in the tree-ring data. This plot from the QSR paper is attached below as is the paper itself.

In his 1998 paper, Briffa showed that the divergence was largely restricted to the region covered by the north band described in Cook et al. (2004). Consistent with that finding, the north ensemble mean shown below reveals a serious downturn in growth after about 1950. This is an expression of the "divergence" that has been described first by Briffa and also by D'Arrigo in her NRC talk. In contrast, the south ensemble mean shows the opposite, i.e. a substantial growth increase which is much more consistent with 20th century warming. If one then follows the plots back in time, all of the sub-region ensemble means track each other remarkably well at multi-centennial time scales even when they enter the putative MWP 800-1000 years ago. In fact, at no time prior to the 20th century is there separation between north and south that is remotely comparable to that found after ca. 1950. This result suggests that no large-scale "divergence" of the order found during the 20th century occurred during the MWP even though that period is suggested to have been somewhat warmer than average overall. This result clearly refutes the argument that "divergence" of the kind noted in the 20th century happened in the past. It also suggests a unique anthropogenic cause to the 20th century divergence.

I am not aware of ANY evidence that demonstrates the occurrence of large-scale "divergence" in the past. It is therefore unjustified to call into question the use of tree rings for reconstructing temperatures over the past millennium based on a naive extrapolation of growth "divergence" into the past when it appears to be unique to the 20th century. The NRC committee members must be made aware of this if their report is to have the necessary scientific credibility that is expected of it.

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References

Briffa, K.R., Schweingruber, F.H., Jones, P.D., Osborn, T.J., Shiyatov, S.G., Vaganov, E.A. 1998. Reduced sensitivity of recent tree-growth to temperature at high northern latitudes. *Nature* 391: 678-682.

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Attachment converted: Macintosh HD:2004_Cook_QSR 1.pdf (PDF /«IC») (0011FEF2)

Attachment converted: Macintosh HD:Cook_QSR_Fig6.gif (GIFf/«IC») (0011FEF5)

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Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences
Mail and Fedex Address:

Institute for the Study of Planet Earth

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<http://www.geo.arizona.edu/>

<http://www.ispe.arizona.edu/>

References

1. <mailto:ikraucunas@nas.edu>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: oyvind.paasche@geo.uib.no,Eystein Jansen <Eystein.Jansen@geo.uib.no>
Subject: Re: Fwd: Re: Fwd: Ch06 Figure Check
Date: Wed, 15 Mar 2006 16:16:25 +0000
Cc: Jonathan Overpeck <jto@u.arizona.edu>,Keith Briffa <k.briffa@uea.ac.uk>

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Here's the next one

Dear Oyvind,

sorry for the delay, I've been off work for a couple of days due to unexpected family illness.

here are the EPS format figures. I'll send in separate emails due to their size. I've tried to number them correctly according to the new numbering, though please open them to check they look ok.

One thing to note is that I have separate files for the two panels of the MWP box figure, and also the forcings/models figure is also in two files. They are all labelled appropriately, with the panel part in the filename, so I hope this is no problem!?

You should get these files:

ipccar4_fig6.10.eps
ipccar4_fig6.11.eps
ipccar4_fig6.12.eps
ipccar4_fig6.13abcd.eps & ipccar4_fig6.13e.eps
ipccar4_box6.4_fig1a.eps & ipccar4_box6.4_fig1b.eps

Please let me know if they don't all arrive!

Cheers

Tim

At 09:00 08/03/2006, Eystein Jansen wrote:

>Dear Tim and Bette,
>first I wish to thank you again for your particularly outstanding
>and hard work for the SOD. Your work in particular really has made

>the new draft a lot better than the FOD.
>There is one small remaining issue, however, as noted by the TSU in
>the message posted below. We need high res version, i.e. eps. files
>of your figures. At present we only have the ones sent in word files.
>Could you send the eps. files to us and Øyvind asap so we can get
>the whole delivery uploaded in a finished state to the TSU server.
>Best wishes,
>Eystein
>
>
>
>>Envelope-to: eystein.jansen@geo.uib.no
>>Date: Tue, 07 Mar 2006 19:00:19 -0700
>>From: IPCC-WG1 <ipcc-wg1@al.noaa.gov>
>>To: Jonathan Overpeck <jto@u.arizona.edu>, eystein.jansen@geo.uib.no
>>Subject: Ch06 Figure Check
>>X-checked-clean: by exiscan on noralf
>>X-UiB-SpamFlag: NO UIB: 0 hits, 8.0 required
>>X-UiB-SpamReport: spamassassin found;
>>
>>
>>Greetings Peck and Eystein!
>>
>>I have gone through the Chapter 6 figure files submitted to the ftp
>>site. Problems with any of the Chapter 6 figures are noted in the
>>attached spreadsheet. Luckily, none of your figures need revision
>>prior to the SOD.
>>
>>However, you will need to provide the TSU with information about
>>the spacing of figures... i.e. one or two columns. Please input
>>that information into the attached spreadsheet and forward that
>>information to the TSU as soon as possible.
>>
>>Another small item: some of your figures were submitted as .pdf
>>files rather than in .eps format. Please note that these files will
>>need to be resubmitted as .eps files in the near future. Although
>>replacing these figures is not urgent, I wanted to let you know now
>>since I know how long it can take to generate high-resolution
>>images. When you do resubmit these files, please be sure to send an
>>email to the TSU to let us know to check the ftp site.
>>
>>If you have any questions, please let me know. Thanks!
>>

>>Regards, Kristen

>>

>>+++++

>>Kristen Averyt, Ph.D.

>>Project Scientist

>>Intergovernmental Panel on Climate Change

>>Working Group I TSU

>>325 Broadway SDRC CSD08

>>Boulder, CO 80305 USA

>>

>>Tel: 1.303.497.4885

>>Fax: 1.303.497.5686

>>Email: averyt@ucar.edu

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>Eystein Jansen

>Professor/Director

>Bjerknes Centre for Climate Research and

>Dep. of Earth Science, Univ. of Bergen

>Allégaten 55

>N-5007 Bergen

>NORWAY

>e-mail: eystein.jansen@geo.uib.no

>Phone: +47-55-583491 - Home: +47-55-910661

>Fax: +47-55-584330

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>#Attachment converted: Øyvind:Ch06_Figure_Check.xls (XLS8/XCEL) (003D0E85)

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>Dr. Øyvind Paasche

>Bjerknes Centre for Climate Research/

>Department of Earth Science

>University of Bergen

>Allé gt. 55

>N-5007, Bergen

>Norway

>Phone direct: +47 55583297

>Cell phone: +47 93048919

>E-mail: oyvind.paasche@bjerknes.uib.no

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<x-flowed>

Dr Timothy J Osborn

Climatic Research Unit

School of Environmental Sciences, University of East Anglia

Norwich NR4 7TJ, UK

e-mail: t.osborn@uea.ac.uk

phone: +44 1603 592089

fax: +44 1603 507784

web: <http://www.cru.uea.ac.uk/~timo/>

sunlock: <http://www.cru.uea.ac.uk/~timo/sunlock.htm>

</x-flowed>

From: edwardcook <drdendro@ldeo.columbia.edu>

To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

Subject: Fwd: Comment on NRC Workshop

Date: Wed, 15 Mar 2006 19:33:48 +0700

Cc: edwardcook <drdendro@ldeo.columbia.edu>

Hi Gents, Here is what I just sent off to NRC, cc'd to Gerry North and Bette Otto-Bliesner.

Ed Begin forwarded message: > From: edwardcook > Date: March 15, 2006 7:23:23 PM GMT+07:00

> To: "Kraucunas, Ian" > Cc: edwardcook , g-north@tamu.edu, > ottobli@ucar.edu > Subject:
> Comment on NRC Workshop >> Ian Kraucunas, Ph.D. > Board on Atmospheric Sciences and
> Climate > National Research Council of The National Academies > 500 Fifth Street NW, Keck
> 705 > Washington, DC 20001 >> Dear Dr. Kraucunas, >> I request that this document (also
> attached as Cook_NRC.pdf) and > the attached scientific paper (2001_Cook_QSR.pdf) be
> forwarded to > all NRC committee members who participated in the recent NRC > workshop
> "Surface Temperature Reconstructions for the Past 2,000 > Years: Synthesis of Current
> Understanding and Challenges for the > Future", ideally with a cc to me when this is done.

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> about tree rings by some committee members and invited > participants at the NRC workshop.

This concern could have an > unfairly negative impact on the use of tree rings for >
> reconstructing past climate, especially that related to surface air > temperatures, hence
> my letter to you and the committee. As part of > her talk, Dr. Rosanne D'Arrigo mentioned
> the discovery of > "divergence" between instrumental temperatures and tree growth > during
> the last few decades of the 20th century at selected boreal > sites in the Northern
> Hemisphere. The affected trees > systematically under-responded to increasing temperatures,
> i.e. > they grew more slowly than they should have based on a well-fitted > linear response
> model applied to the data prior to the onset of > "divergence". The large-scale occurrence
> of this change in > responsiveness has also been described by Keith Briffa (Briffa et >
> al., 1998) in Nature. A number of hypotheses have been proposed to > explain it, which
> range from natural (climatic change) to > anthropogenic (pollution related), but the actual
> cause is still > unknown. This phenomenon needed to be mentioned by Dr. D'Arrigo, > but it
> appears to have taken on a level of specious importance that > is not justified by the
> evidence. >> Perhaps not surprisingly, but also somewhat alarmingly, it is my >

> understanding that some NRC committee members and other influential > participants have
> come to the conclusion that the observed 20th > century "divergence" calls into serious
> question the value of the > tree-ring reconstructions of temperatures over the past >
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> during times like the > Medieval Warm Period (MWP) some 800-1000 years in the past. If >
> this were the case, then the concern raised by some at the workshop > would be justified.

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> published in Quaternary > Science Reviews in 2004 (Cook et al., 2004), I reviewed the >
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> from the QSR paper is embedded below and the paper is sent being > sent as an attachment.
> The importance of this plot to the > "divergence" debate follows next. >> In their paper,
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Cc: edwardcook <[3]drdendro@ldeo.columbia.edu>, [4]g-north@tamu.edu, [5]ottobli@ucar.edu

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In their paper, Briffa et al. (1998) showed that the "divergence" between tree growth and

temperatures was largely restricted to the region covered by the north band described in Cook et al. (2004). Consistent with that finding, the north ensemble mean shown below (blue curve) reveals a serious downturn in growth after about 1950. This is an expression of the large-scale "divergence" described by Briffa et al. (1998) and also by Dr. D'Arrigo in her NRC talk. In contrast, the south ensemble mean (red curve) shows the opposite growth trajectory after 1950, i.e. a substantial growth increase that is much more consistent with 20th century warming. If one then follows the plots back in time, all sub-region ensemble means track each other remarkably well at multi-centennial time scales even when they enter the putative MWP 800-1000 years ago. In fact, at no time prior to the 20th century is there a separation between north and south that is at all comparable to that found after 1950. This result indicates that no large-scale "divergence" of the order found during the 20th century occurred during the MWP even though that period is suggested to have been somewhat warmer than average overall. It thus refutes the argument that "divergence" of the kind found in the 20th century could very well have happened in the past, thus implying that tree rings cannot produce reliable reconstructions of past temperatures. It also supports the existence of an admittedly unknown anthropogenic cause of the 20th century "divergence". The lack of any known cause is unfortunate, but this would be true regardless of how the importance of "divergence" is interpreted.

I am not aware of ANY evidence that demonstrates the occurrence of large-scale "divergence" between tree growth and climate prior to the 20th century. Indeed, the available evidence indicates just the opposite. In my opinion it is therefore unjustified to call into question the use of tree rings for reconstructing temperatures over the past millennium based on a naïve and inappropriate extrapolation of the growth "divergence" problem into the past when it appears to be unique to the 20th century. The NRC committee members must consider this in their report if it is to have the necessary scientific credibility that is expected of it.

References

Briffa, K.R., Schweingruber, F.H., Jones, P.D., Osborn, T.J., Shiyatov, S.G., Vaganov, E.A. 1998. Reduced sensitivity of recent tree-growth to temperature at high northern latitudes. *Nature* 391: 678-682.

Esper, J., Cook, E.R., Schweingruber, F.H. 2002. Low-frequency signals in long tree-ring chronologies for reconstructing past temperature variability. *Science* 295: 2250-2253.

Cook, E.R., Esper, J., D'Arrigo, R.D. 2004. Extra-tropical Northern Hemisphere land temperature variability over the past 1000 years. *Quaternary Science Reviews* 23(20-22): 2063-2074.

Sincerely,

Edward R. Cook

=====

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Attachment Converted: "c:\eudora\attach\2004_Cook_QSR3.pdf"

Attachment Converted: "c:\eudora\attach\Cook_NRC.pdf"

Attachment Converted: "c:\eudora\attach\Cook_QSR_Fig61.gif"

References

1. <mailto:drdendro@ldeo.columbia.edu>
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5. <mailto:ottobli@ucar.edu>
6. <mailto:drdendro@ldeo.columbia.edu>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Martin Manning <mmanning@al.noaa.gov>
Subject: SUPER URGENT IPCC help needed
Date: Thu, 23 Mar 2006 13:17:44 -0700
Cc: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk, Eystein Jansen <eystein.jansen@geo.uib.no>, <oyvind.paasche@bjerknes.uib.no>, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, Melinda Marquis <Marquis@ucar.edu>, averyt@ucar.edu, ssolomon@al.noaa.gov

Sounds good Martin. Keith, Tim - are you out there? Please help by ensuring we're doing the right thing w Fig 6.13 and table 6.2

Hi Peck
Thanks for the provisional "go ahead" - we can (and so will) wait till Monday before changing the master copy of the chapter here.
Regards
Martin
At 11:16 AM 3/23/2006, Jonathan Overpeck wrote:

Hi Martin - this seems ok to me. I hope we hear from Tim and Keith - they are the key folks on this one. If we don't hear from them, then we go with what you have done. Seems quite reasonable to me, and I'm sorry we caused the TSU this extra work.
Thanks again, Peck

X-Sieve: CMU Sieve 2.2
Date: Wed, 22 Mar 2006 19:11:36 -0700
To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk, Eystein Jansen <eystein.jansen@geo.uib.no>, <oyvind.paasche@bjerknes.uib.no>, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, Melinda Marquis <Marquis@ucar.edu>
From: Martin Manning <mmanning@al.noaa.gov>
Subject: Re: SUPER URGENT IPCC help needed
Cc: ssolomon@al.noaa.gov, averyt@ucar.edu
Dear Jonathan
Thanks for trying to sort this out quickly for us and for the information that the Ammann et al paper is not available.
Susan and I have discussed your two options and have to say that we can not agree to option 1 in the circumstances. Although the Jones and Mann (2004) paper shows the NCAR simulation, the key point is that it cites it as "C. Ammann et al private communication 2003". So in effect option 1 would be bringing in material that was not peer reviewed and not even separately documented. Anyone wanting to discredit your chapter would

highlight the fact that you appear to be depending on work done in 2003 that had still not been peer-reviewed.

Option 2 is the only way to meet the standard that we have set all along of basing the assessment very firmly on peer reviewed literature.

Kristen Averyt found that she could edit the EPS files that you had sent us earlier for

Fig 6.13 and take out the curves in question labelled AJS2006. The result is attached.

If you can confirm that this edited figure looks correct we are now proposing to drop

that into your chapter in place of the original one. We would also remove the [S4] row

in Table 6.2 referring to this study. We would also of course use the edited version of

the figure in the TS (Fig TS-26 in current draft).

If you can see any other implications of this approach to resolving the problem that we

need to be aware of please let me know. If the author team wants to provide a redrawn

figure that might be an improvement on the attached version we can still wait until

Monday morning for that.

Best regards

Martin

At 04:25 PM 3/22/2006, Jonathan Overpeck wrote:

Hi Keith and Tim - need FAST help. Figure 6.13, and Table 6.2 cite Amman et al., for the

CSM curve. Since this paper doesn't yet exist in "in press" form (I checked w/ Bette,

who is a co-author), we have two choices. I think choice one below could be ok, but want

to have confirmation from Keith or Tim, and if it's not ok, (NOTE) Tim and Keith need to

get new Fig and Table to Melinda and Martin at the TSU by Monday.

Option 1: we can cite Jones, P.D., and M.E. Mann, 2004: Climate over past millennia.

Reviews of Geophysics, 42(2) - this paper (already in references - there is hope!) has

the CSM simulation in its Fig 8, but of course it's not the idea original reference

describing the simulation.

Option 2: we (Tim) creates new fig 6.13, and Table 6.2 without any reference to this

simulation.

PLEASE NOTE - if Keith and Tim (or Martin) feels we must go w/ option 2, Tim has to send

the new fig and table to TSU (Melinda Marguis and Martin) by Monday AM at the absolute

latest.

Thanks for your quick help, Peck

--

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Professor, Department of Atmospheric Sciences
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--

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** Please note that problems may occur with my @noaa.gov address
Dr Martin R Manning, Director, IPCC WG I Support Unit
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--

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--

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: Martin Manning <mmanning@al.noaa.gov>, Jonathan Overpeck
<jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen
<eystein.jansen@geo.uib.no>, <oyvind.paasche@bjerknes.uib.no>, Bette
Otto-Bleisner <ottobli@ncar.ucar.edu>, Melinda Marquis <Marquis@ucar.edu>
Subject: Re: SUPER URGENT IPCC help needed
Date: Fri, 24 Mar 2006 14:16:19 +0000
Cc: ssolomon@al.noaa.gov, averyt@ucar.edu

<x-flowed>
Dear all,

we (Keith and I) agree that it isn't appropriate to cite only Jones and Mann (2004) as a reference for the NCAR CSM curves in figure 6.13.

Another alternative to deleting the curves, however, would be to reference Mann, Rutherford, Wahl and Ammann (2005), which should already be in the reference list. This might be an appropriate reference because it includes Ammann as a co-author and provides a more information about the simulation than Jones and Mann (2004). However it still relies upon the submitted Ammann et al. paper as the main reference -- so maybe still not good enough? I've attached a PDF of Mann et al. (2005) for you to consider.

From earlier discussions (and perhaps also in relation to chapters using new model runs of future climate), I thought that a new unpublished run with an existing published model under published forcing might be allowed (in the same way that updated 2005 or 2006 instrumental temperatures could be included, even if not published, providing they were compiled following the procedures described in an earlier paper). For instance, the EMIC runs we included as an extra panel probably fall in this category. Maybe the CSM run falls in this category too? Have other runs with this model been published? And the forcing used in this run was presented in Goosse et al. (2005; GRL 32, L06710, again it includes Ammann as a co-author) as well as in Jones and Mann (2004). So, maybe CSM can be included under this reasoning?

I don't want to sound as if we are arguing strenuously to keep the CSM curves in the figure -- if the preferred decision is to drop it, then so be it. If so, then the modified figure looks ok.

Cheers

Tim

At 02:11 23/03/2006, Martin Manning wrote:

>Dear Jonathan

>

>Thanks for trying to sort this out quickly for us and for the
>information that the Ammann et al paper is not available.

>

>Susan and I have discussed your two options and have to say that we
>can not agree to option 1 in the circumstances. Although the Jones

>and Mann (2004) paper shows the NCAR simulation, the key point is
>that it cites it as "C. Ammann et al private communication 2003". So
>in effect option 1 would be bringing in material that was not peer
>reviewed and not even separately documented. Anyone wanting to
>discredit your chapter would highlight the fact that you appear to
>be depending on work done in 2003 that had still not been peer-reviewed.

>

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>along of basing the assessment very firmly on peer reviewed literature.

>

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>sent us earlier for Fig 6.13 and take out the curves in question
>labelled AJS2006. The result is attached.

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>proposing to drop that into your chapter in place of the original
>one. We would also remove the [S4] row in Table 6.2 referring to
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>If you can see any other implications of this approach to resolving
>the problem that we need to be aware of please let me know. If the
>author team wants to provide a redrawn figure that might be an
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>morning for that.

>

>Best regards

>Martin

>

>At 04:25 PM 3/22/2006, Jonathan Overpeck wrote:

>>Hi Keith and Tim - need FAST help. Figure 6.13, and Table 6.2 cite
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>>in "in press" form (I checked w/ Bette, who is a co-author), we
>>have two choices. I think choice one below could be ok, but want to
>>have confirmation from Keith or Tim, and if it's not ok, (NOTE) Tim
>>and Keith need to get new Fig and Table to Melinda and Martin at
>>the TSU by Monday.

>>

>>Option 1: we can cite Jones, P.D., and M.E. Mann, 2004: Climate
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>>

>>Option 2: we (Tim) creates new fig 6.13, and Table 6.2 without any
>>reference to this simulation.

>>

>>PLEASE NOTE - if Keith and Tim (or Martin) feels we must go w/
>>option 2, Tim has to send the new fig and table to TSU (Melinda
>>Marguis and Martin) by Monday AM at the absolute latest.

>>

>>Thanks for your quick help, Peck

>>

>>

>>--

>>

>>Jonathan T. Overpeck

>>Director, Institute for the Study of Planet Earth

>>Professor, Department of Geosciences

>>Professor, Department of Atmospheric Sciences

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>

>--

>Recommended Email address: mmanning@al.noaa.gov

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</x-flowed>

Attachment Converted: "c:\eudora\attach\mann 2005 pseudoproxy.pdf"

<x-flowed>

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</x-flowed>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Gustafson, Diane" <DGustafs@nas.edu>
Subject: Re: Proxy time series
Date: Wed, 29 Mar 2006 14:36:50 +0100
Cc: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Dear Diane / Mike / NRC Committee,

At 22:18 28/03/2006, Gustafson, Diane wrote:

>Dear Tim:

>

>Our National Research Council Committee on Surface Temperature
>Reconstructions has been considering your paper with Keith Briffa
>published in a recent issue of Science. Could you please elaborate
>on your criterion for selecting the proxy time series included in
>the analysis. We are interested in how you computed the correlation
>between the proxy time series and local temperature time series. Is
>the correlation based on filtered or detrended time series? How
>would you counter the potential criticism that your selection method
>tends to favor proxy time series that show a strong 20th century warming?

>

>It would be most helpful for us if you could reply in time for us to
>consider your response at our meeting tomorrow morning. Thanks in
>advance for your help.

>

>Mike Wallace

We (Tim Osborn and Keith Briffa) will first respond to these specific questions about our recent Science paper. In addition, copied below are some further comments by Keith Briffa on issues related to tree-ring proxy records, that may be of interest to the committee.

The primary purpose of our paper was to implement an alternative, and possibly complementary, method of proxy-data analysis to the methods used in most previously published reconstructions of past NH temperature variations. We did not want to introduce an entirely new selection of proxy records (even if this were possible), because that would obscure whether differences in our conclusions, compared with published work, arose from our method or a different selection of proxy records.

We decided, therefore, to make use of as many of the individual records used in almost all the previously published NH temperature reconstructions, excluding any records for which an indication of at least partial temperature sensitivity was lacking. So, very low resolution records for which comparison with instrumental temperatures is problematic were excluded.

We used records specifically from Mann and Jones (2003) and Esper et al. (2002). In addition we included records from Mann et al. (2003), which I think just adds the van Engelen documentary record from the Low Countries in Europe, because the others were already in the Mann and Jones set. We excluded duplicates, and our paper explains which series we used where duplicates were present. We did not average the Tornetrask, Yamal and Taimyr tree-ring records as done by Mann and Jones, because we could see no reason not to use them as individual series.

The series used by Mann and Jones had already been correlated with their local instrumental temperatures -- using decadal-smoothed, non-detrended, values -- so we accepted this as an indication of some temperature sensitivity. For the other series, we calculated our own correlations against local instrumental temperatures, trying both annual-mean or summer-mean temperatures. In our paper's supplementary information, we state that we used the HadCRUT2 temperatures for this purpose, which combines land air temperatures with SST observations. In fact, we used the CRUTEM2 land-only temperature data set for this purpose. These should be identical where the proxy locations are not coastal. For these correlations, we did not filter the data, nor did we detrend it, and we used the *full* period of overlap between the proxy record and the available instrumental record.

We excluded records that did not show a *positive* correlation with their local temperatures. The remaining set includes most of the long, high resolution records used by others, such as Moberg et al., Crowley and Lowery, Hegerl et al., Mann, Bradley and Hughes, etc. as well as by Mann and Jones and Esper et al.

The final question, regarding the selection method favouring records that show a strong 20th century warming trend, is a more philosophical issue. As stated above, we did not actually use strongly selective criteria, preferring to use those records that others had previously used and only eliminating those that were clearly lacking in temperature sensitivity. To some extent,

therefore, the question is then directed towards the studies whose selection of data we used. Certainly we did not look through a whole host of possibilities and just pick those with a strong upward trend in the last century! And we don't think the scientists whose work we selected from would have done this either. There are very few series to choose from that are >500 years long and are from proxy types/locations where temperature sensitivity might be expected. It would be entirely the wrong impression to think that there are 140 such a priori suitable possible series, and that we picked (either explicitly or implicitly) just those 10% that happened by chance to exhibit upward 20th century trends.

The correlation with local temperature is an entirely appropriate factor to consider when selecting data; these could be computed using detrended data, though for those that we calculated, our use of unfiltered data means that the trend is unlikely to dominate the correlation. One would need to inspect the trend in the temperature data at each location to evaluate how much influence it would have on the results; but in locations where a strong upward trend is present, it would be right to exclude proxy records that did not reproduce it, though also correct that a proxy shouldn't be included solely on the basis of it having the trend, especially where the proxy resolution is sufficient to test its ability to capture shorter term fluctuations.

Finally, note that our method has not selected only those records with a strong 20th century warming trend. Of the 14 proxies selected (see our figure 1), 7(!) do not have strong upward 20th century trends: Quebec, Chesapeake Bay, W Greenland, Tirol, Tornetrask, Mangazeya, and Taimyr. Our method gives equal weight to all records, so it should not be biased towards a single record, or a small number of records, that do show strong upward trends.

Here are the additional comments on tree-ring issues:

I would also like to take the opportunity, if you will allow, to comment briefly on some reports that have reached me concerning the contribution made by Rosanne D'Arrigo to your Committee. Apparently, this is being interpreted by some as reflecting adversely on the validity of numerous temperature reconstructions that involve significant dependence on tree-ring data. This is related to Rosanne's focus in her presentation on the apparent difference between measured temperatures and tree growth in recent decades - a so-called "divergence" problem.

First let me make it clear that as I did not attend the Committee meeting I am not able to comment specifically on the details of Rosanne D'Arrigo's actual presentation, though I am aware of her papers with various co-authors related to this "divergence" in the recent (circa post 1970) trends in tree-growth and temperature changes as recorded in instrumental data, at near tree-line sites in the Canadian Arctic. There are also other papers dealing with 'changing growth responses' to climate in North American trees.

I have co-authored a paper in Nature on the reduced response to warming as seen in tree-ring densitometric data at high-latitude sites around the Northern Hemisphere, increasingly apparent in the last 30 years or so.

First, it is important to note that the phenomena is complicated because it is not clearly identifiable as a ubiquitous problem. Rather it is a mix of possible regionally distinct indications, a possible mix of phenomena that is almost certainly in part due to the methodological aspects of the way tree-ring series are produced. This applies to my own work, but also very likely to other work.

The implications at this stage for the 'hockey stick' and other reconstructions are not great. That is because virtually all long tree-ring reconstructions that contribute to the various reconstructions, are NOT affected by this. Most show good coherence with temperature at local levels in recent decades. This is not true for one series (based on the density data). As these are our data, I am able to say that initial unpublished work will show that the "problem" can be mitigated with the use of new, and again unpublished, chronology construction methods.

In the case of the work by Rosanne and colleagues, I offer my educated opinion that the phenomenon they describe is likely also, at least in part, a chronology construction issue. I am not saying that this is a full explanation, and certainly there is the possibility of increased moisture stress on these trees, but at present the issue is still being defined and explored. As the issue needs more work, this is only an opinion, and until there is peer-reviewed and published evidence as to the degree of methodological uncertainty , it is not appropriate to criticize this or other work . For my part, I have been very busy, lately with teaching and IPCC commitments, but we will do some work on this now, though again lack of funds to support

a research assistant do not help.

The matter is important but I do not believe that the facts yet support Rosanne's contention, in her Global Biogeochemical Cycles paper (Vol. 18, GB3021, doi:10.1029/2004GB002249, 2004) that an optimum physiological threshold has been consistently exceeded at a site in the Yukon. This conclusion should certainly not be taken as indicating a widespread threshold exceedence.

It was my call not to "overplay" the importance of the divergence issue, knowing the subtlety of the issues, in the forthcoming IPCC Chapter 6 draft. We did always intend to have a brief section about the assumption of uniformitarianism in proxy interpretation , including mention of the possible direct carbon dioxide fertilization effect on tree growth (equally controversial), but it is likely to conclude that here as well , there is no strong evidence of any major real-world effect. This and the divergence problem are not well defined, sufficiently studied, or quantified to be worthy of too much concern at this point. The uncertainty estimates we calibrate when interpreting many tree-ring series will likely incorporate the possibility of some bias in our estimates of past warmth, but these are wide anyway. This does not mean that temperatures were necessarily at the upper extreme of the reconstruction uncertainty range 1000 years ago, any more than they may have been at the bottom. The real problem is a lack of widespread (and non-terrestrial) proxies for defining the level of early warmth, and the vital need to up-date and study the responses of proxies in very recent times.

Best regards,

Tim Osborn and Keith Briffa

--

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Fax: +44-1603-507784

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sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

</x-flowed>

From: Eduardo Zorita <Eduardo.Zorita@gkss.de>
To: t.osborn@uea.ac.uk (Tim Osborn)
Subject: Re: Response to Wahl et al in Science
Date: Thu, 30 Mar 2006 16:18:00 +0200 (MET DST)
Cc: Eduardo.Zorita@gkss.de, k.briffa@uea.ac.uk

Tim,

yes, I also found it strange. We noticed that Amman and Wahl cited their Science comment as accepted in their manuscript that is now in press in Climatic Change.

Personally I think it is convenient that this clarification gets published but I am somewhat disapointed by the fact that a very similar content was submitted by Buerger and Cubasch about one year ago and it was not even sent to reviewers (it is the paper that finally appeared in Tellus). I think that comment was of much higher quality than Wahl's.

Science knew of the Tellus paper, since we cite it in our response. So actually there is scientifically nothing new in this exchange, but it will be published in Science...

Anyway, I am happy to have more time now for more productive work and hope that Ritson doe not bomb me with more mails in the future

eduardo

> Thanks for letting us know, Eduardo. It is strange that Science
> accepted the Wahl et al. comment before yours; we were told of this
> on 28-Feb and that is why you will notice, if you get to see the
> latest IPCC draft, that Wahl et al. is cited but your response is not
> cited! This will look strange, given that they will be published
> together. Maybe it can be changed later?

>

> Cheers

>

> Tim

>

> At 11:31 29/03/2006, Eduardo Zorita wrote:

>>Dear Tim,

> >
> >the comment by Wahl, Ritson and Amman and our response have been now
> >accepted for
> >publication in Science
> >
> >eduardo
>
> Dr Timothy J Osborn
> Climatic Research Unit
> School of Environmental Sciences, University of East Anglia
> Norwich NR4 7TJ, UK
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> e-mail: t.osborn@uea.ac.uk
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> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: "Brooks Hanson" <bhanson@aaas.org>
Subject: Re: data request to SCIENCE for 1120514
Date: Fri, 31 Mar 2006 10:30:06 +0100
Cc: "Jesse Smith" <hjsmith@aaas.org>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Dear Brooks Hanson,

of the two additional questions/requests, the first one is quick to respond to and so I can do that immediately.

In fact my previous reply answers the first question already, as does our paper itself in a very clear way. It is something of a waste of time, therefore, to have to write another answer, but here goes anyway...

We clearly state (in the SOM to our paper) what the data sources were, and Esper et al. was not the source for the four series in question. There is, therefore, no need for anyone to "surmise" that this is the case, because we explicitly state it!

Further, we state in our paragraph (d) that we replaced Athabasca with a new, "better-replicated series" from Luckman and Wilson. "Better-replicated" clearly indicates that there are more data in the new series than were available to Esper et al., as is also clear from even a cursory read of the Luckman and Wilson paper. So it should be obvious that you cannot expect to reproduce the results using the fuller data set by using only the more limited data available from Esper et al. -- otherwise what would be the point of going out and collecting all that new data?

The other three series are covered in our paragraph (c), "The data sets contain some non-identical tree-ring series derived from the same sites; we have favoured series from (S3) because they are based on a greater number of tree core measurements than the series generated by (S1)". So we clearly did not use the Esper et al. data (S1) and it should also be clear that the series we did use can not be reproduced using the Esper et al. data because they are "non-identical" and there are fewer tree cores in the Esper et al. data. The source we gave for these three series is Briffa (2000).

We did not use tree-core measurement data in our paper, only

chronologies that had previously been assembled by others from core measurement data. I don't have any core measurement data and therefore have none to give out! And in my first reply I explained why I didn't think that this was appropriate anyway, since I consider that our obligation is limited to providing data to allow the replication of the steps reported in our paper, none of which involved any processing of core measurement data.

I will reply next week regarding the second question/request.

Best regards

Tim

At 20:35 30/03/2006, Brooks Hanson wrote:

> Dear Dr. Osborn:

>

>Thank you for your assistance in resolving the request for data for
>your recent paper. I have passed along the relevant information you
>have provided (I assure you not your email). In response, i've received
>two additional questions. I'm wondering if it would be possible to
>clarify these.

>

>In 4 cases, the Osborn site chronology differs from the Esper site
>chronology, although in the other cases the versions are identical. In
>some cases, the date ranges do not match. I do not believe that it is
>possible to replicate the Osborn version from the Esper measurement data
>in these 4 cases and surmise that Osborn used a different measurement
>data set. I therefore request measurement data used by Osborn for the
>following sites: Polar Urals, Tornetrask, Taymir and Athabaska.

>

>The HadCRU2 data set contains temperature data for the gridcell 37.5N,
>117.5W commencing in 1870. However, the gridcell information provided
>by Osborn commenced only in 1888 and the differences are material to
>the final result (0.045 versus 0.18 reported). What is the reason for
>commencing this comparison in 1888 rather than the available 1870? Since
>there is a material difference in this example, could you please provide
>the gridcell temperature sets in a comparable format for the other 13
>Osborn and Briffa series

>

>I appreciate that the latter request may take some additional effort as
>you noted. I'm hopeful that this will provide a resolution to this
>matter.

>

>Sincerely,

>Brooks Hanson

Dr Timothy J Osborn
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Norwich NR4 7TJ, UK

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phone: +44 1603 592089

fax: +44 1603 507784

web: <http://www.cru.uea.ac.uk/~timo/>

sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

</x-flowed>

From: Rainer Zahn <rainer.zahn@uab.es>

To: Eystein Jansen <Eystein.Jansen@geo.uib.no>, "k.briffa-uea.ac.uk" <k.briffa@uea.ac.uk>

Subject: Re:

Date: Fri, 07 Apr 2006 12:29:58 +0200

<x-flowed>

At 18:17 03/04/2006, you wrote:

>Hi Rainer, we drafted a complaint, which Keith Briffa still sits on, and
>I don't think it will be sent. Some of our partners, e.g. Hadley Centre,
>MPI and CNRS were reluctant as they thought complaining might backfire.
>If there was foul play, we had no proof of it. We did some checks with
>commission representatives, but did not learn much. I think the problem
>was that the review panel was biased against us, and that the commission
>did not follow up with instructions that was coherent with their own policies.
>
>Best wishes,
>Eystein

Hi Eystein,

not sure if I comprehend the mentality of not sending a statement, keeping a low profile I do not perceive a good strategy. I am mentioning this as I have become increasingly weary of FWP programmes and proposals. Over the past four years I was involved in 4 initiatives none of which came through. Beyond the immediate frustration on the basis of the individual failures I do note in all these instances is an unfavourable degree of ambiguity in the reviewing process such that it appears the reviewers are being kept in the dark about the vision of the call beyond what the call says in printing. I can see the challenge from the programme managerial side that one wishes not to interfere with the reviewing progress and yet I feel that the reviews offered, perhaps the reviewing process at large, do not live up to the standards set for proposals. Quite frankly, from my few conversations I had with the programme managers and their assistants I have come to the conclusion that they are helpful in providing assistance with logistics and proposal structuring, yet on a managerial front they are not up to speed with what I would perceive professionalism in handling their tasks. So to me it seems there are various levels involved in the issue that in the end mount to the impression that FWPs are not an immediate option for proposals much longer. This view is shared by quite a number of colleagues and it is for this reason that I am convince we must respond to the Imprint failing.

If Keith doesn't mind perhaps forward the statement so I can glance through it.

Best, Rainer

</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Henry Pollack <hpollack@umich.edu>
Subject: Re: IPCC FAR draft
Date: Tue, 18 Apr 2006 15:32:11 -0600
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

<x-flowed>

Hi Henry - thanks for the email. Just earlier today, Eystein and I were soliciting approval from our team on how to best get feedback from chapter authors - Lead Authors and Contributing Authors alike. Since we're all authors, it isn't appropriate to comment officially as expert reviewers, but rather to work as a team to take expert reviews - AND chapter 6 author feedback - and use them to create a better final draft. One key, as promised earlier, is to have a process that makes sure we get all comments and are able to respond to them. The other key is that we ensure time to allow the needed debate. Eystein and I are going to ask LAs (including Keith) to do their work sooner in the draft cycle than before so that we have the time for this.

So... I would suggest you keep these comments in a safe place for a bit longer, and then send them in to the Eystein and I when we ask (should be in the next week). Note that the current draft has only officially been available for a bit over a week (indeed, I didn't see it until today since the IPCC TSU had to check for all sorts of things after we submitted it over a month ago), and we won't be working on the new draft until June. So we have time to be thoughtful and complete in the feedback gathering process.

Is this ok? Seems more suitable than giving review via the gov process on your own work (you are an author of our chapter).

Also, I can anticipate one thing that is going to come up again, and that I don't think we had your

feedback on (nor Keith's). What about the borehole recons that you and colleagues have done extending back beyond the last couple centuries. I don't have my paper pdf collection here, but I believe you have some recons going back many centuries. Does this need more attention in the chapter?

Thanks for being proactive and quick to send feedback. We'll be sending our email to all CA's soon, if you're willing to wait a couple more days.

Thanks, peck

>Hi Keith (and Peck and Eystein),

>

>I have recently been sent the current draft of the IPCC FAR by the US
>Global Change Research Program, asking for comments on the draft. This
>is the first time I have seen this product since we were feverishly
>exchanging e-mails in February. Let me call to your attention some
>small but not insignificant corrections to be made to the next draft.

>

>Page 6-33, Section 6.6.1.2, line 22. The title of this section (in
>italics) should be changed to "What do ground surface temperature
>reconstructions derived from subsurface temperature measurements tell
>us?"

>

>Page 6-33, lines 49 and 52, there is a reference (Smerdon et al., in press).
>This paper has now been published, so substitute "2006" for "in press",
>and in the list of references the citation should include the following:

>

>J. Geophys. Res. 111, D07101, doi:10.1029/2004JD005578

>

>Page 6-34, lines 43 and 44. This section is dealing with the southern
>hemisphere. The sentence "...these both indicate unusually warm
>conditions prevailing in the 20th century (Pollack and Smerdon, 2004)"
>, and the reference therein, are both incorrect.

>

>The ground surface temperature changes over the last 500 years DO NOT
>indicate unusually warm conditions prevailing in the 20th century in
>Australia and southern Africa. This is because the unusually warm
>conditions developed late in the century, after most of the boreholes

>had already been logged. What the borehole reconstruction for
>Australia does show is very good correspondence with the Cook et al
>(2000) reconstruction for Tasmania and the Cook et al. (2002) recon for
>New Zealand. The Australia work is described in a manuscript "Five
>centuries of Climate Change in Australia: The View from Underground" by
>Pollack, Huang and Smerdon now under review in the Journal of
>Quaternary Science. The Africa work is unpublished.

>
>Is this e-mail to you sufficient to activate these changes? Or should I
>submit these comments to the US Government Review Panel? If I am to
>submit to the latter, they require all comments to be filed by May 9.

>
>Cheers,
>Henry

>
>
> _____ Henry N. Pollack
>[\ /] Professor of Geophysics
> | \ | Department of Geological Sciences
> |MICHIGAN| University of Michigan
>[____]V[____] Ann Arbor, Michigan 48109-1005, U.S.A.

>
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> URL: www-personal.umich.edu/~hpollack/book.html

--
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</x-flowed>

From: "Michael E. Mann" <mann@meteo.psu.edu>

To: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: Fwd: Re: Ruherford et al 2005

Date: Wed, 26 Apr 2006 10:49:23 -0400

Reply-to: mann@psu.edu

Cc: Scott Rutherford <srutherford@rwu.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>

<x-flowed>

thanks Tim,

I'm saddened to hear that this bozo is bothering you too, in addition to NCAR, NSF, NAS, IPCC and everyone else. Rest assured that I won't ever respond to McIntyre should he ever contact me, but I will forward you any email he sends related to this. I assume Scott feels the same way...

I hope you're having as nice a spring as we are here. See you in June?

mike

p.s. we have some interesting new reconstruction based on RegEM using a greatly expanded multiproxy network (which includes the MXD data). I hope to send you guys shortly. It is our hope that you'll consider being co-authors. This to come soon...

Tim Osborn wrote:

> Hi Scott and Mike,

>

> as lead author and co-author on the Rutherford et al. paper, I thought

> I'd let you know that we are dealing with some requests for the MXD

> data set used in this paper, including the one copied below from

> McIntyre. We should have got this organised a bit quicker but we will

> (eventually!) get the data and its description available for

> interested parties. So if you get any more requests for the MXD data

> that were used in our joint paper, please pass them on to me.

>

> Hope everything's well with you,

>

> Tim

>

>

>> Date: Wed, 26 Apr 2006 15:08:39 +0100

>> To: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>

>> From: Tim Osborn <t.osborn@uea.ac.uk>

>> Subject: Re: Ruherford et al 2005

>> Cc: "Andrew Weaver" <jclim@uvic.ca>, Keith Briffa <k.briffa@uea.ac.uk>

>>

>> Dear Steve,

>>

>> I have just finished responding to Science about your latest request
>> to them concerning our recent paper, so I can now turn to your
>> request copied below.

>>

>> I can answer your first request immediately:

>>

>> The MXD data used in Rutherford et al. were *derived* from the
>> Schweingruber network, but aren't actually the raw site-by-site data
>> values. The reason why we didn't use the latter is that the
>> site-by-site MXD chronologies have only been processed using a
>> "traditional" approach to standardization that removes low-frequency
>> climate variations. Our age-band decomposition approach (Briffa et
>> al., 2001, JGR), which retains more low-frequency variability, had
>> only been applied at the regional-average level. So we gridded the
>> site-by-site chronologies onto a 5x5 grid and added to each grid box
>> the "missing" regional-scale low-frequency information identified by
>> comparing the age-band and traditionally-standardized results at a
>> regional scale.

>>

>> I will respond with information and/or data to your requests (2)-(4)
>> soon.

>>

>> Regards

>>

>> Tim

>>

>> At 19:37 18/04/2006, Steve McIntyre wrote:

>>

>>> Dear Tim, I presume that the sites used in the MXD network in
>>> Rutherford et al., Journal of Climate 2005 came from the
>>> Schweingruber network. Could you provide me with (1) confirmation as
>>> to whether this is the case; (2) identification of the sites; (3)
>>> the protocol for site selection from the larger Schweingruber
>>> network; (4) a URL for any data or dataversions not available in the
>>> Schweingruber network at WDCP. Regards, Steve McIntyre

>>

>
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> web: <http://www.cru.uea.ac.uk/~timo/>
> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
>

--

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The Pennsylvania State University email: mann@psu.edu
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<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: t.m.melvin@uea.ac.uk
Subject: Fwd: Re: Standardisation uncertainty for tree-ring series
Date: Fri Apr 28 15:34:54 2006

X-Mailer: QUALCOMM Windows Eudora Version 7.0.0.16
Date: Fri, 28 Apr 2006 15:08:05 +0100
To: philip.brohan@metoffice.gov.uk
From: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: Standardisation uncertainty for tree-ring series
Cc: Keith Briffa <k.briffa@uea.ac.uk>,simon.tett@metoffice.gov.uk

Hi Philip,

we have three "groups" of trees:

"SCAND" (which includes the Tornetrask and Finland multi-millennial chronologies, but also some shorter chronologies from the same region). These trees fall mainly within the 3 boxes centred at:

17.5E, 67.5N

22.5E, 67.5N

27.5E, 67.5N

"URALS" (which includes the Yamal and Polar Urals long chronologies, plus other shorter ones). These fall mainly within these 3 boxes:

52.5E, 67.5N

62.5E, 62.5N (note this is the only one not at 67.5N)

67.5E, 67.5N

"TAIMYR" (which includes the Taimyr long chronology, plus other shorter ones). These fall mainly within these 4 boxes:

87.5E, 67.5N

102.5E, 67.5N

112.5E, 67.5N

122.5E, 67.5N

We do some analysis at the group scale, and for this we take the JJA temperatures from each box and average to the group scale to obtain a single series from each of SCAND, URALS and TAIMY.

We do some analysis at the overall scale, and for this we take these three group temperature series and average them to get an overall NW Eurasia temperature for boxes with tree chronologies in them.

We did also try using a wider average for the region, including all LAND temperatures from grid boxes within a rectangular region from 12.5E to 127.5E and from 57.5N to 72.5N, but I don't think it correlated so well against the tree-ring width data (I can't remember the exact correlations), so we didn't pursue that.

Does that give you enough information to be going on with? I'd recommend using CRUTEM3 rather than HadCRUT3, because the correlations seem to deteriorate with the inclusion of

SST data in some cases -- though of course you can look into this yourself.

Cheers

Tim

At 16:35 27/04/2006, philip.brohan@metoffice.gov.uk wrote:

Thanks Tim.

I need to extract from the instrumental and model data the appropriate data to calibrate the tree-rings against. Presumably this is the June-July-August average land surface temperature for a particular region in NW Eurasia. Could you send me the lat and long ranges of the region?

Cheers,

Philip

On Thu, 2006-04-27 at 16:01, Tim Osborn wrote:

> Thanks for the nice precise description of methodology, Philip. It's
> good that we are all clear exactly what procedure is to be applied.

>

> On the train after our meeting last week, Keith and I discussed this
> a bit more. In the NW Eurasian case study, n is quite high and
> therefore it is likely that the bootstrap estimates will show
> relatively little variation and probably will underestimate the true
> error (due to additional errors in the assumptions underlying RCS, as
> discussed in London). We will do the calculations anyway, and then
> we will know for sure how large/small they are, rather than just speculating.

>

> It looks likely that Tom Melvin will have time to devote directly to
> this issue as he will probably be funded by our (that includes you,
> Simon) NERC RAPID project for a while. Once/if this is confirmed,
> then we'll get Tom to do the calculations outlined below and
> communicate directly with Philip over any implementation issues etc.

>

> Cheers

>

> Tim

>

> At 16:02 26/04/2006, philip.brohan@metoffice.gov.uk wrote:

> >Keith, Tim.

> >

> > At our meeting last Wednesday I agreed to specify exactly what needed
> >to be done to make uncertainty estimates for standardisation of the
> >tree-ring data.

> >

> > Suppose we are making a proxy series from n cores. From those n cores

>>we can make an RCS age correction curve, and a mean proxy series (the
>>average of the cores after applying the age correction curve to each
>>one?). These are the best-estimate values for the age-correction curve
>>and the proxy series.

>>
>> We also need bootstrap estimates of the age correction curve and the
>>mean proxy series. To make a bootstrap estimate: sample, with
>>replacement, from the n cores until you have a set of n samples. (Some
>>of the cores will be in this sample once, some several times, and some
>>not at all). From this set of n samples, make an age correction curve
>>and a mean proxy series as before. These are the bootstrap estimates.

>>
>> We need a lot of bootstrap estimates. I'd like 1000 - 100 will probably
>>do at a pinch. So please can you make these and send me the 1001 age
>>correction curves and 1001 mean proxy series.

>>
>> I will do something similar with the instrumental series, and we can
>>then make bootstrap estimates of the regression uncertainty and the
>>uncertainty in the reconstructed temperatures.

>>
>>Cheers,

>>
>> Philip

>>
>>--

>>Philip Brohan, Climate Scientist
>>Met Office Hadley Centre for Climate Prediction and Research
>>Tel: +44 (0)1392 884574 Fax: +44 (0)1392 885681
>>Global climate data sets are available from [1]<http://www.hadobs.org>

>
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> Climatic Research Unit
> School of Environmental Sciences, University of East Anglia
> Norwich NR4 7TJ, UK

>
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> phone: +44 1603 592089
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> web: [2]<http://www.cru.uea.ac.uk/~timo/>
> sunclock: [3]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

--
Philip Brohan, Climate Scientist
Met Office Hadley Centre for Climate Prediction and Research

Tel: +44 (0)1392 884574 Fax: +44 (0)1392 885681

Global climate data sets are available from [4]<http://www.hadobs.org>

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fax: +44 1603 507784
web: [5]<http://www.cru.uea.ac.uk/~timo/>
sunclock: [6]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

--

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Phone: +44-1603-593909

Fax: +44-1603-507784

[7]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.hadobs.org/>
2. <http://www.cru.uea.ac.uk/~timo/>
3. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
4. <http://www.hadobs.org/>
5. <http://www.cru.uea.ac.uk/~timo/>
6. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
7. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Eduardo Zorita <Eduardo.Zorita@gkss.de>
To: t.osborn@uea.ac.uk (Tim Osborn), k.briffa@uea.ac.uk (Keith Briffa)
Subject: Wengen meeting
Date: Wed, 03 May 2006 23:31:00 +0200 (MET DST)

Dear Tim, dear Keith,

I am writing to inform you that I have reconsidered my acceptance to attend the Wengen meeting. In the last days I have convinced myself that under the present circumstances a constructive discussion on reconstruction methods is unfortunately not possible. We have another exchange on the last Journal of Climate paper by Mann et al, which is now under review. Even the editor of J. of Climate found adequate to tell us that all inflammatory comments in their response would have to be eventually deleted. Even considering the considerable pressure that he has is exposed to in American politics, I think Michael Mann is unable of any constructive discussion.

I am very grateful for your invitation to this meeting and I hope that we can continue our collaboration in other ocasion.

Best wishes

eduardo

From: <mann@meteo.psu.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, "Phil Jones" <p.jones@uea.ac.uk>, "Michael E. Mann" <mann@psu.edu>, Scott Rutherford <srutherford@rwu.edu>
Subject: Re: FW: Ruherford et al 2005
Date: Fri, 12 May 2006 8:10:00 -0600

hi tim. personally, I don't see why you should make any concessions for this moron. By the way, our supplementary site (now on scott's computer) doesn't block any ip#s. another lie.. Mike

-----Original Message-----

From: Tim Osborn <t.osborn@uea.ac.uk>
Subj: FW: Ruherford et al 2005
Date: Fri May 12, 2006 8:10 am
Size: 4K
To: Keith Briffa <k.briffa@uea.ac.uk>, "Phil Jones" <p.jones@uea.ac.uk>, "Michael E. Mann" <mann@psu.edu>, Scott Rutherford <srutherford@rwu.edu>

Thought you might be interested in the following. I *am* going to provide the list of MXD sites requested, but honestly haven't had time to put it together this his request. Clearly the 2-week delay was too long for him! Still, at least I'm not (yet) described as "juvenile"! :-)

Tim

>From: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>
>To: "Andrew Weaver" <jclim@uvic.ca>
>Cc: "Tim Osborn" <t.osborn@uea.ac.uk>
>Subject: FW: Ruherford et al 2005
>Date: Fri, 12 May 2006 09:54:37 -0400
>
>Dear Andrew,
>Rutherford et al 2005 states that supplementary information is available at
><http://fox.rwu.edu/~rutherfo/supplements/jclim2003a>.
>
>First, in passing, Scott Rutherford has blocked the IP address of the
>computer that I regularly use from access to that site (I had previously
>been blocked from Mann's FTP site.) While I have been able to have someone
>else send me the data, I'm sure that such petty behavior is inconsistent
>with Journal of Climate access policies and I request that you ask your
>authors to stop such juvenile behavior insofar as it affects the Journal of
>Climate.
>
>Second, the referenced website does NOT contain the MXD data, but only
>includes a link to "Ask Tim Osborn". As you can see from the attached
>correspondence, Osborn has undertaken to provide the requested information,
>but the article certainly implies - and I am sure that that this was your
>understanding as editor - that the data would be readily available. In this
>case, even a simple listing of the sites has not been provided after nearly
>2 weeks. (I might add that I initially requested a listing of the sites from

>a coauthor nearly 2 years ago.)

>

>In order to comply with the apparent undertakings of Rutherford et al, I
>think that you should arrange for a less ad hoc method of providing the
>supplementary information.

>

>Regards,

>

>Steve McIntyre

>

>

>

>

>

>-----Original Message-----

>From: Tim Osborn [mailto:t.osborn@uea.ac.uk]

>Sent: April 26, 2006 10:09 AM

>To: Steve McIntyre

>Cc: Andrew Weaver; Keith Briffa

>Subject: Re: Ruherford et al 2005

>

>

>Dear Steve,

>

>I have just finished responding to Science about your latest request
>to them concerning our recent paper, so I can now turn to your
>request copied below.

>

>I can answer your first request immediately:

>

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>Schweingruber network, but aren't actually the raw site-by-site data
>values. The reason why we didn't use the latter is that the
>site-by-site MXD chronologies have only been processed using a
>"traditional" approach to standardization that removes low-frequency
>climate variations. Our age-band decomposition approach (Briffa et
>al., 2001, JGR), which retains more low-frequency variability, had
>only been applied at the regional-average level. So we gridded the
>site-by-site chronologies onto a 5x5 grid and added to each grid box
>the "missing" regional-scale low-frequency information identified by
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>regional scale.

>

>I will respond with information and/or data to your requests (2)-(4) soon.

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>Regards

>

>Tim

>

>At 19:37 18/04/2006, Steve McIntyre wrote:
>>Dear Tim, I presume that the sites used in the MXD network in
>>Rutherford et al., Journal of Climate 2005 came from the
>>Schweingruber network. Could you provide me with (1) confirmation as
>>to whether this is the case; (2) identification of the sites; (3)
>>the protocol for site selection from the larger Schweingruber
>>network; (4) a URL for any data or dataversions not available in the
>>Schweingruber network at WDCP. Regards, Steve McIntyre
>
>Dr Timothy J Osborn
>Climatic Research Unit
>School of Environmental Sciences, University of East Anglia
>Norwich NR4 7TJ, UK
>
>e-mail: t.osborn@uea.ac.uk
>phone: +44 1603 592089
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>web: <http://www.cru.uea.ac.uk/~timo/>
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From: Jonathan Overpeck <jto@u.arizona.edu>

To: "Neil Roberts" <C.N.Roberts@plymouth.ac.uk>

Subject: Re: ipcc chapter 6 draft

Date: Thu, 18 May 2006 15:58:25 -0600

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Neil - Thanks for your interest in providing feedback on the draft chap 6 Second Order Draft. Since the IPCC has very strict rules about all this, I'm going to ask them (the IPCC) to send you an official invitation to review, along with the process - formal, but highly efficient - to follow. If you could send your comments in that way it would be a great help. We've been asked to keep everything squeaky clean, and not to get comments informally.

Thanks! Peck

>Dear Jonathan

>

>Please excuse me for writing direct, but Keith Briffa suggested it
>would be simplest. I have looked through the draft chapter 6 and
>find it an impressive document. However, bullet 4 on page 6.2,
>starting "global mean cooling and warming....." strikes me as
>incorrect and misleading.

>

>Whereas the mean rate of temperature change over the Pleistocene may
>have been >10 times slower than that projected for the next century,
>there is clear evidence that for specific major climatic
>transitions, global (or at least hemispheric) temperature changes in
>the past have been at least as rapid as those projected by climate
>model simulations and incorporated in the last IPCC report. The
>most obvious case in point is the global warming at the start of the
>Holocene, ca. 11.5 ka BP. Russell Coope, more than 20 years ago,
>showed from beetles that UK temperatures rose faster than could be
>dated within the errors of 14C dating. Subsequently this was
>confirmed by Greeland ice cores based on layer counting (full
>glacial to interglacial in less than 100 years), and by the Cariacos
>basin marine record. I have worked on varved lake records from both
>the tropics (Roberts et al Nature 1993 366, 146-148) and the
>Mediterranean (Roberts et al The Holocene, 2001, 11, 719-734) where
>this climate transition was accomplished in substantially less than

>a century. In short, several independent lines of evidence show
>that the climate system has been capable of flipping from one
>meta-stable state to another, very different one over timescales
>that could be experienced by a single human lifetime. This is not
>an unimportant conclusion in terms of the potential for non-linear
>responses of future climate to GHG forcing.

>
>I also looked for supporting argument for bullet 4 later in chapter
>6, but found nothing of substance.

>
>In short, this particular bullet seems in need of critical
>reassessment before the definitive version of the next IPCC reprot
>emerges.

>
>Thanks in anticipation and best egards

>
>Neil

--

Jonathan T. Overpeck
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From: Jonathan Overpeck <jto@u.arizona.edu>

To: "Wahl, Eugene R" <wahle@alfred.edu>

Subject: RE: Wahl & Amman paper

Date: Sun, 21 May 2006 22:58:50 -0600

Cc: "Bette Otto-Bleisner" <ottobli@ncar.ucar.edu>, "Eystein Jansen" <eystein.jansen@geo.uib.no>, "Caspar Ammann" <ammann@ucar.edu>, t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi Gene - thanks for the update. If Tim/Keith/Caspar want to add anything (or Martin ask for more clarification), please cc to the entire list on this email. Sounds like the UCAR version is the one to consider "official" (right everyone?).

Thanks again, Peck

>Hello Peck, Martin, Bette, Eystein, Caspar:

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>I just double checked the UCAR website version with the pdf version

>I have, and they are identical with the exception that the

>supplemental tables (Tables 1S and 2S), and supplemental figure

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From: Jonathan Overpeck <jto@u.arizona.edu>

To: Caspar Ammann <ammann@ucar.edu>

Subject: Re: Wahl & Amman paper

Date: Mon, 22 May 2006 07:58:44 -0600

Cc: "Bette Otto-Bleisner" <ottobli@ncar.ucar.edu>, "Eystein Jansen" <eystein.jansen@geo.uib.no>, "Wahl, Eugene R" <wahl@alfred.edu>, Martin Manning <mmanning@al.noaa.gov>, t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>

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>>

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>>
>
>--
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>Climate and Global Dynamics Division - Paleoclimatology
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>Boulder, CO 80307-3000
>email: ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

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References

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Hello all:

Yes, Martin, the paper you sent today is indeed an old version, and should be replaced by the NCAR pdf version.

This old version sent today is actually older than the Feb 21 version I mentioned yesterday (see below), and has no relevance in terms of the text that is accepted/in press with Climatic Change as of February 28, 2006.

As I mentioned yesterday (see below), the text of the UCAR pdf is identical to the WORD version I sent to Peck, Keith, and Eystein on February 24.

Peace, Gene

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Alfred University

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Sent: Monday, May 22, 2006 6:49 PM
To: Wahl, Eugene R
Subject: Sent by Martin Manning -- Wahl & Amman paper --with old version

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Sent: Monday, May 22, 2006 12:19 PM
To: Jonathan Overpeck; Caspar Ammann
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In summary, the UCAR website pdf document should be considered the official one that is "accepted/in press". Formal notification of acceptance from Stephen Schneider at Climatic Change came on February 28. The article is still in this status.

Let me know if I can help clarify things futher. Please note that I will be in Boulder starting May 27, to be a visiting scholar at NCAR for a month. I will be keeping up with email from there.

Peace, Gene Dr. Eugene R. Wahl
Asst. Professor of Environmental Studies
Alfred University
607-871-2604
1 Saxon Drive
Alfred, NY 14802 _____

From: Jonathan Overpeck [[1] <mailto:jto@u.arizona.edu>]

Sent: Sat 5/20/2006 8:39 PM

To: Martin Manning

Cc: Bette Otto-Bleisner; Eystein Jansen; Caspar Ammann; Wahl, Eugene R

Subject: Re: Wahl & Amman paper

Hi Martin - We'll look into this asap. I'll cc to Caspar and Gene to see if they can clarify the situation and make sure we have the correct version. I'll also cc Bette since she may see Caspar around NCAR and make sure he know's we are trying to clarify things with his paper.

More soon, thx, Peck

Dear Eystein and Jonathan

It has been pointed out to us by a reviewer that the version of the Wahl and Amman paper (accepted by Climatic Change) on our review web site differs from the version that is available publicly from the NCAR web site at:

[

[2]http://www.cgd.ucar.edu/ccr/ammann/millennium/refs/WahlAmmann_ClimaticChange_inPress.pdf

<[3]http://www.cgd.ucar.edu/ccr/ammann/millennium/refs/WahlAmmann_ClimaticChange_inPress.pdf

>]

Although the differences are not (in my view) substantial, the paper on the NCAR web site is apparently dated Feb 24th (i.e. before the date of final submission of the SOD), it has additional figures and data, and the running header says "Feb 24, in press".

Could you please clarify which of the two versions of this paper would reflect most accurately the status of the paper as used by the Chapter 6 team when preparing the SOD. That has been our basis for deciding on which version to include on our reviewer web pages up until now, but we are now reconsidering whether to also include updated versions of unpublished papers as well. If you have any thoughts on that please let me know.

Best regards

Martin

--

Recommended Email address: mmanning@al.noaa.gov

** Please note that problems may occur with my @noaa.gov address

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--

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--

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[4]<http://www.geo.arizona.edu/>

[5]<http://www.ispe.arizona.edu/>

--

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References

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2. http://www.cgd.ucar.edu/ccr/ammann/millennium/refs/WahlAmmann_ClimaticChange_inPress.pdf

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4. <http://www.geo.arizona.edu/>

5. <http://www.ispe.arizona.edu/>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: expert review comments on AR4
Date: Thu, 25 May 2006 13:16:21 -0400
Reply-to: mann@psu.edu

<x-flowed>

Hi Keith,

here is the submitted comment by Tapio Schneider, attached. Please do not pass along or show to others. Thanks in advance,

mike

Keith Briffa wrote:

> Hi Mike
> thanks for these comments and especially thanks for your remarks on
> the effort of trying to produce a balanced picture of the current
> state of things in the IPCC Chapter 6. In fact , I know that it is
> already out of date and I am going to get particularly lambasted for
> not discussing problems with recent tree responses to warming and
> potential problems wit CO2 fertilization - I may have to add even more
> text yet .You are absolutely correct that we had unreasonable trouble
> from Susan , who was not as "hands off" as she might have been. I will
> certainly study your comments carefully - as I always do . I would
> rather reserve comment on the Crowley reconstruction til I speak
> personally to you. I really hope that we can get an atmosphere of
> constructive discussion that , I believe, must include some discussion
> of the sceptics . Look forward to those drinks and some time away from
> the mad house of teaching/exam marking etc. See you soon

>
> best wishes

> Keith

>
> At 18:08 24/05/2006, you wrote:

>
>> Hi Keith,

>>
>> I wanted you to have an advance copy of the comments I'll be
>> submitting on the final draft of the AR4. I commend you for the
>> excellent work you've done and the tough battle I know you have had

>> to fight. I don't envy it, and you know the tough battles I've been
>> through.

>>

>> Confidentially, I do have a number of specific concerns mostly in
>> the area of discussions of where things actually now stand in terms
>> of some of the earlier criticisms. I believe that the discussion is
>> still out of date, given what has been shown in recent publications,
>> including Wahl and Ammann (Science). Also, and I don't think this is
>> the only place you're going to hear this from, there are deep
>> problems w/ Hegerl et al '06, particularly the claims of what TLS can
>> do, which are egregiously incorrect. There is a comment in review in
>> Nature (not me, but I can promise you, by someone who understands the
>> statistical issues involved better than anyone else in our community)
>> that is very critical. I think its unwise for the TAR to
>> uncritically accept the claims made, particularly given that the
>> actual J. Climate paper was in limbo at least at the time the most
>> recent draft was finalized. I believe that disqualifies it for
>> consideration for AR4, no?

>>

>> Also, I think it is an absolute travesty that figure 6.10 isn't being
>> shown in the SPM. I think that is unforgiveable, and there should be
>> an effort to over-ride that decision (I would suspect that is Susan
>> Solomon's doing?),

>>

>> I hope we can discuss these things (and much else) over a few beers
>> in Switzerland. Looking forward to seeing you soon,

>>

>> mike

>>

>> --

>> Michael E. Mann
>> Associate Professor
>> Director, Earth System Science Center (ESSC)

>>

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>>

>>

>
> --
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</x-flowed>

Attachment Converted: "c:\eudora\attach\heger106_comment.pdf"

From: Keith Briffa <k.briffa@uea.ac.uk>
To: mann@psu.edu
Subject: Re: expert review comments on AR4
Date: Thu May 25 17:34:59 2006

Hi Mike

thanks for these comments and especially thanks for your remarks on the effort of trying to produce a balanced picture of the current state of things in the IPCC Chapter 6. In fact , I know that it is already out of date and I am going to get particularly lambasted for not discussing problems with recent tree responses to warming and potential problems wit CO2 fertilization - I may have to add even more text yet .You are absolutely correct that we had unreasonable trouble from Susan , who was not as "hands off" as she might have been. I will certainly study your comments carefully - as I always do . I would rather reserve comment on the Crowley reconstruction til I speak personally to you. I really hope that we can get an atmosphere of constructive discussion that , I believe, must include some discussion of the sceptics . Look forward to those drinks and some time away from the mad house of teaching/exam marking etc. See you soon

best wishes

Keith

At 18:08 24/05/2006, you wrote:

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Confidentially, I do have a number of specific concerns mostly in the area of discussions of where things actually now stand in terms of some of the earlier criticisms. I believe that the discussion is still out of date, given what has been shown in recent publications, including Wahl and Ammann (Science). Also, and I don't think this is the only place you're going to hear this from, there are deep problems w/ Hegerl et al '06, particularly the claims of what TLS can do, which are egregiously incorrect. There is a comment in review in Nature (not me, but I can promise you, by someone who understands the statistical issues involved better than anyone else in our community) that is very critical. I think its unwise for the TAR to uncritically accept the claims made, particularly given that the actual J. Climate paper was in limbo at least at the time the most recent draft was finalized. I believe that disqualifies it for consideration for AR4, no?

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I hope we can discuss these things (and much else) over a few beers in Switzerland.

Looking forward to seeing you soon,
mike

--

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References

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2. <http://www.met.psu.edu/dept/faculty/mann.htm>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: john mitchell <jfbmitchell@yahoo.co.uk>

Subject: Re: Review comments

Date: Wed, 21 Jun 2006 16:57:03 -0600

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Jouzel@lscce.saclay.cea.fr, Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk

Hi John - thanks. I'll cc to Keith and Tim too, and we'll be sure to discuss these in Bergen. I'll be on my normal email to the extent we have time to be check email - experience suggests it's tough. But... we'll try to keep an eye on email.

See you soon, best, peck

Hi Eystein, Jon,

I am in Geneva at the WMO EC meeting,so I have not had a lot of time to look at the SOD comments. I can not get to Bergen before Tuesday. I had a quick look at the comments on the hockey stick and include below the questions I think need to be addressed which I hope will help the discussions. I do tbelieve we need a clear answer to the skeptics . I have also copied these comments to Jean. Please let me know that you have received this, and what email address I can contact you at in Bergen".

With best wishes

John

1. There needs to be a clear statement of why the instrumental and proxy data are shown on the same graph. The issue of why we dont show the proxy data for the last few decades (they dont show continued warming) but assume that they are valid for early warm periods needs to be explained.

2. There are number of methodological issues which need a clear response. There are two aspects to this. First, in relation to the TAR and MBA which seems to be the obsession of certain reviewers. Secondly (and this I believe this is the main priority for us) in relation to conclusions we make in the chapter We should make it clear where our comments apply to only MBH (if that is appropriate), and where they apply to the overall findings of the chapter. Our response should consider all the issues for both MBH and the overall chapter conclusions

a. The role of bristlecone pine data

Is it reliable?

Is it necessary to include this data to arrive at the conclusion that recent warmth is unprecedented?

b. Is the PCA approach robust? Are the results statistically significant? It seems to me that in the case of MBH the answer in each is no. It is not clear how robust and significant the more recent approaches are.

3. The chapter notes that new data has been included, but we dont say how much or is this is substantial or minor. The impression I have that the amount added is minor, but I cant tell.

4. The Esper et al and Moburg et al data both show increased variance, but the temporal patterns are quite different. We need to say why the discrepancy does not undermine our conclusions of greater cooling in the Little Ice Age.

5. I have not had time to check the original chapter, but the comments give the impression that the recent 50 yr warming is unprecedented over the last 500years (seems reasonable) and elsewhere over the last 1000years (less clear)

John FB Mitchell

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john.f.mitchell@metoffice.com

Like being first? Check out the [1]all-new Yahoo! Mail today.

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<http://www.ispe.arizona.edu/>

References

1. http://us.rd.yahoo.com/mail/uk/taglines/yahoo_co_uk/nowyoucan/check_out/*http://us.rd.yahoo.com/evt=40569/*http://uk.docs.yahoo.com/nowyoucan.html

From: Tim Osborn <t.osborn@uea.ac.uk>
To: simon.tett@metoffice.gov.uk, philip.brohan@metoffice.gov.uk, Eduardo Zorita <Eduardo.Zorita@gkss.de>, Gerd Bärger <gerd.buerger@met.fu-berlin.de>
Subject: report back from PAGES/CLIVAR Wengen meeting
Date: Fri Jun 23 16:35:28 2006
Cc: Keith Briffa <k.briffa@uea.ac.uk>

Hi Simon, Philip, Eduardo & Gerd (cc Keith),

I thought you might be interested in a brief report back from the recent Wengen meeting, specifically about how SO&P-funded work on pseudo-proxies was covered and related hockey-stick issues.

****Please don't circulate this further, because it is just my personal viewpoint****

Thanks for letting me show some of your material. I skipped over some graphs I took from Philip's regression presentation at the SO&P meeting because Francis Zwiers covered forward/inverse/total least squares before me. I did show some results from Eduardo, including pseudo-proxy results from Erik-II. And I showed a figure from Gerd's "many flavours" pseudo-proxy paper.

The meeting included fairly intensive discussions about many issues, and this included some discussion of von Storch et al. (2004, 2006), Wahl et al. (2006), Mann et al. (2005), Burger and Cubasch (2005) and Burger et al. (2006). Generally the discussion was quite open, with only a few disdainful remarks made about the work of people not there -- certainly not enough to distract from useful discussions.

In general, most people accepted that the MBH method could, in some situations, result in biased reconstructions with too little low-frequency. I'm not sure how much Mike Mann accepted this, but it was reinforced by findings shown by Eugene Wahl that indicated some bias in their CSM pseudo-proxy studies, and particularly by Francis Zwiers who looked to have almost completely replicated the von Storch et al. results with respect to the MBH method (though he emphasised the preliminary nature of his work and he may not have implemented the MBH method correctly... we'll have to wait and see).

Mike showed many detailed psuedo-proxy tests of the RegEM method and these seemed quite convincing in showing little problem with that method... it does assume equal error in both instrumental and proxies, so it should show less bias than other methods that wrongly put all the error in the instrumental record (i.e., "typical" regression).

So... there was some confusion about how the MBH method can be biased but the RegEM not be biased (in pseudo-proxy tests) yet they give the same results for the real proxies.

Mike thought it might be the ECHO-G vs CSM differences, but I argued against this and was supported by Caspar Ammann and Eugene Wahl who did

not think that the character of the model runs was a big factor in explaining different results.

There was limited discussion of trend/detrend and white/red noise pseudo-proxy issues. Many seemed to think that if pseudo-proxy studies showed that detrending definitely caused a problem, then this was a reason not to detrend. The alternative of finding a method that worked with detrended data was not really discussed.

The discussion was fairly constructive and for the most part friendly. Eugene Wahl in particular seemed keen to "build bridges" within the community.

I should also mention two of the workshop outcomes.

The first is that a paper is being planned based on the things discussed at the workshop and covering many issues from proxy data, forcings, model simulations and reconstructions. I hope that the authorship of this might be wider than just the participants of the workshop, but we will have to wait and see who else is asked to contribute.

The second is that we should set up a "climate reconstruction challenge". The idea would be to use a simulation (*not* of the last 1000 years, so none of us know the expected answer) and provide some data from a "calibration period" and some "pseudo-proxies" from the full period and make these public so that anyone could attempt to make a reconstruction using their favoured method(s). The true model NH temperature series would be kept secret for 6 months or so. Thus it would be a "blind" test and after attempts had been submitted they would be evaluated against the true result to assess which methods were most successful.

Caspar Ammann will probably provide the simulation, so he wouldn't take part in making any reconstructions. He would keep the details secret from all others so that any one, including MBH, you and us, could enter the challenge.

Finally, it was asked whether the model runs that have so far been used for pseudo-proxy studies (NCAR CSM, ECHO-G Erik-I, HadCM3, maybe ECHO-G Erik-II?) might be made publicly available for shared use, so results are less model dependent. This would just be the surface air temperature fields from the runs, not all the other variables. What do you think, Simon and Eduardo? If you are happy with this then they could get them from the SO&P website, so no need for data extraction on your part.

Hope you find this summary interesting. It's just my opinions. I've cc'd this to Keith in case he wants to say anything different!

Cheers

Tim

From: Caspar Ammann <ammann@ucar.edu>
To: Christoph Kull <christoph.kull@pages.unibe.ch>
Subject: Re: climate reconstruction challenge
Date: Thu, 29 Jun 2006 06:43:40 -0600
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi Christoph,
sounds excellent. 20th is a good target with three weeks left. Let me launch one full round to solicit comments and ideas, and then I can send you what we have to build the web site. I'll check with Mike about having him fold this into the report.
Cheers
Caspar

Christoph Kull wrote:

> Dear Caspar and Tim,
> Thanks for putting this issue forward!!
> PAGES/CLIVAR may help communicating this challenge to the community.
>
> We will be able to setup the website with the data sets and the call etc.:
> - let me know what you need! It would be best for us to have first a simple
> "word document with the structure, headings and text. We will then produce a
> "hidden site" that can be updated and finalized before it will go public
> online.
>
> We will be able to announce the challenge to the community via the
> Newsletter and e-news:
> - we need a respective experiment description.
> - the next Newsletter is going to be published by end of July. Can you
> provide me this information by the 20th? This would also fit with the
> planned announcement in the workshop report for EOS...Mike will draft this
> report.
> I suggest to directly contact him for an incorporation of this call.
>
> All the best, thanks a lot and greetings from Bern,
> Christoph
>
>
> On 23.06.2006 19:23, "Caspar Ammann" <ammann@ucar.edu> wrote:
>

>
>> Hi Tim,
>>
>> just back from the various trips and meetings, most recently
>> Breckenridge and the CCSM workshop until yesterday. This coincided with
>> the release of the NRC report...
>>
>> Thanks Tim for getting in touch with Simon and Eduardo. And I would
>> think it would be excellent if you would be on the reconstruction side
>> of things here. We really need to make sure that all the reconstruction
>> groups (the ones that show up in the spaghetti-graph) also provide
>> reconstructions for the Challenge. By the way, Mike Mann is fine with
>> the participation of the german group in this as he has spoken now
>> favorably on the project.
>>
>> I think the separation you point at is absolutely crucial. So, as I
>> indicated in Wengen, I would suggest that we could organize a small
>> group of modelers to define the concepts of the experiments, and then
>> make these happen completely disconnected from standard data-centers. A
>> Pseudo-Proxy group should then develop concepts of how to generate
>> pseudo-proxy series and tell the modelers where they need what data. But
>> what they do is not communicated to the modelers. Based
>>
>> The underlying concept as well as the technical procedure of how we
>> approach the pseudo-proxies should be made public, so that everybody
>> knows what we are dealing with. We could do this under the PAGES-CLIVAR
>> intersection umbrella to better ensure that the groups are held separate
>> and to give this a more official touch. Below a quick draft, we should
>> iterate on this and then contact people for the various groups.
>>
>> So long and have a good trip to Norway,
>> Caspar
>>
>>
>>
>> Here a very quick and simple structural draft we can work from: (all
>> comments welcome, no hesitations to shoot hard!)
>>
>>
>> Primary Goals:
>>
>> - cross-verification of various emulations of same reconstruction

>> technique using same input data
>> - comparison of skill at various time scales of different techniques if
>> fed with identical pseudo-proxy data
>> - sensitivities of hemispheric estimates to noise, network density
>> - identify skill of resolving regional climate anomalies
>> - isolate forced from unforced signal
>> - identify questionable, non-consistent proxies
>> - modelers try to identify climate parameters and noise structure over
>> calibration period from pseudo-proxies
>>
>>
>> Number of experiments:
>>
>> - available published runs
>> - available unpublished, or available reordered runs
>> - CORE EXPERIMENTS OF CHALLENGE: 1-3 brand new experiments
>> ^one experiment should look technically realistic: trend in
>> calibration, and relatively reasonable past (very different phasing)
>> ^one experiment should have no trend in calibration at all, but
>> quite accentuated variations before
>> ^...one could have relatively realistic structure but contains a
>> large landuse component (we could actually do some science here...)
>>
>>
>>
>> Pseudo-Proxies and "instrumental-data":
>>
>> - provide CRU-equivalent instrumental data (incl. some noise) that is
>> degrading in time
>> - provide annually resolved network of pseudo proxies ((we could even
>> provide a small set of ~5 very low resolution records with some
>> additional uncertainty in time))
>> - 2 networks: one "high" resolution (100 records), one "low" resolution
>> (20), though only one network available for any single model experiment
>> to avoid "knowledge-tuning", or through time separation: first 500-years
>> only low-res, then second 500-years with both.
>> - pseudo-proxies vary in representation in climate (temperature, precip,
>> combination), time (annual, seasonal) and space (grid-point, small region)
>>
>>
>>
>> Organization of three separate and isolated groups, and first steps:
>>

>> - Modeler group to decide on concept of target climates, forcing series.
>> Provide only network information to Proxy-Group (People? Ammann, Zorita,
>> Tett, Schmidt, Graham, Cobb, Goosse...).

>> - Pseudo-proxy group to decide on selection of networks, and
>> representation of individual proxies to mimic somewhat real world
>> situation, but develop significant noise (blue-white-red) concepts,
>> non-stationarity, and potential "human disturbance" (People? Brohan,
>> Schweingruber, Wolff, Thompson, Overpeck/Cole, Huybers, Anderson, ...).

>> - Reconstruction group getting ready for input file structures: netCDF
>> for "instrumental", ascii-raw series for pseudo-proxy series. Decide
>> common metrics and reconstruction targets given theoretical pseudo-proxy
>> network information. (People: everybody else)

>>
>>
>>
>> Direct science from this: (important!)

>>
>> - Forced versus internal variations in climate simulations (Modelers)
>> - Review and catalog of pseudo-proxy generation: Noise and stationarity
>> in climate proxy records, problems with potential human/land use
>> influence (Proxy Group)
>> - Detection methods and systematic uncertainty estimates (Reconstruction
>> Group)

>>
>>
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>>
>>
>>
>>
>>
>>
>>
>> Tim Osborn wrote:

>>
>>> Hi Caspar and Christoph,
>>>
>>> I just wanted to let you know that:
>>>
>>> (1) I have emailed Simon Tett (for HadCM3) and Eduardo Zorita (for
>>> ECHO-G Erik-I, not sure about Erik-II) to ask if they would be
>>> prepared for surface temperature fields to be made available from
>>> their model runs and placed on a pseudo-proxy website for use in
>>> pseudo-proxy studies. I'll let you know their response.
>>>
>>> (2) In Wengen I suggested that Philip Brohan, a colleague of Simon

>>> Tett, might be interested in creating pseudo-proxies from the output
>>> of Caspar's secret model simulation, because of Philip's interest in
>>> statistical error models (e.g. in the error model he just published of
>>> the instrumental temperature record, HadCRUT3). I have emailed Philip
>>> to ask him if he would be interested. Again, I'll let you know his
>>> response.

>>>
>>> With regard to the "climate reconstruction challenge", Keith and I
>>> were wondering how it is going to be run. Obviously some kind of
>>> organising group would be useful to ensure it is designed to be as
>>> scientifically useful an experiment as possible. Yet there needs to
>>> be a clear distinction between provided experimental design advice
>>> (and things like convening EGU sessions) and having too much knowledge
>>> of the setup that would prevent such people from taking part in the
>>> challenge. Keith and I would be interested in the former, but would
>>> also like to keep our distance and take part in the challenge. I'm
>>> not sure that it was clear in Wengen exactly who is to organise this all.

>>>
>>> Cheers

>>>
>>> Tim

>>>
>>> Dr Timothy J Osborn, Academic Fellow
>>> Climatic Research Unit
>>> School of Environmental Sciences, University of East Anglia
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>>>
>>> ****Norwich -- City for Science:**
>>> ****Hosting the BA Festival 2-9 September 2006**

>>>
>>>
>>>
>
>

--
Caspar M. Ammann

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email: ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

</x-flowed>

From: Valérie Masson-Delmotte <Valerie.Masson@cea.fr>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: warning - more reviews for you
Date: Fri, 30 Jun 2006 13:46:45 +0200
Reply-to: Valerie.Masson@cea.fr

<x-flowed>

Dear Keith,

I hope that you had a good trip back from Bergen.

Some of the review comments which appeared to be relevant for the Holocene section are yours. I copy them here so that you can take there of them.

All the best,

Valérie.

6-687

A

26:18

28:19

Replace "limiting the vallue" on line 18 to "review as a" on line 19 by "which means there is no legitimate"

[VINCENT GRAY (Reviewer's comment ID #: 88-774)]

FOR KEITH

6-694

A

27:0

33:

Section 6.6.1.1 (on 2000-yr proxy reconstructions) is a little too long. It can be either shortened or reorganized into 2 or more shorter sections, say on reconstruction history, debate, and new development.

[Govt. of United States of America (Reviewer's comment ID #: 2023-407)]

6-695

A

27:0

Fig. 6.10a. Rather than showing the average of 4 European stations I suggest to plot the available averaged European mean land temperature (using much more than just 4 stations) from Luterbacher et al. 2004 and Xoplaki et al. 2005. This continental scale average would provide a more

appropriate overview for the last 250 years. The first lead author has the data or they can be obtained prepared from xoplaki@giub.unibe.ch or juerg@giub.unibe.ch. Xoplaki, E., Luterbacher, J., Paeth, H., Dietrich, D., Steiner N., Grosjean, M., and Wanner, H., 2005: European spring and autumn temperature variability and change of extremes over the last half millennium, *Geophys. Res. Lett.*, 32, L15713. Luterbacher, J., Dietrich, D., Xoplaki, E., Grosjean, M., and H. Wanner, 2004: European seasonal and annual temperature variability, trends and extremes since 1500, *Science*, 303, 1499-1503.

[Jürg Luterbacher (Reviewer's comment ID #: 151-8)]

6-696

A

27:0

Fig 6.10. I here repeat a point made in my comments on the FOD. It is statistically invalid and visually misleading to overlay the black instrumental line on this diagram. The coloured graph lines show proxy records that end at 1980. If you want a line that continues up to more recent years that then you must use the proxy records that continue past 1980, not switch to a different type of series. There are up to date proxy records available, but as I'm sure the authors of this chapter are aware, they depart from the surface instrumental record, many of them declining after 1980. By failing to show this, and including the surface temperature data in black, it constitutes a misrepresentation, since the black line is an invalid forward extrapolation of the proxy data. If the reason for not showing the updated proxies is that they are not considered to be good representatives of temperature anymore, then by what right does the Figure insinuate that they were good proxies 8-10 centuries ago? It is no defence to claim that MBH99 established a statistically skillful relationship between the proxy network and the instrumental data, since that claim has been refuted, as discussed

above. McIntyre and McKittrick (2005a,d) showed that the pre-1450 RE statistic was incorrectly benchmarked, yielding a spurious inference, and the r2 stat calculated by MB&H themselves, which showed the lack of skill, was simply not reported. The failure of the r2 and CE stats is confirmed by Wahl and Ammann. The squared correlation between the MBH long proxies and the instrumental record is nearly zero (MM05a,c). The mean correlation between the long NOAMER proxies and gridcell temperatures in the MBH98 data set (which dominate the pre-AD1450 portion) is -0.08 (McIntyre and McKittrick 2005c), and the RE significance benchmark is above the MBH98 RE score, using all available implementation of the Mann code (McIntyre and McKittrick 2005d). The surface instrumental record cannot be used as a statistically valid extrapolation for the proxies after 1980.

[Ross McKittrick (Reviewer's comment ID #: 174-35)]

</x-flowed>

From: Henry Pollack <hpollack@umich.edu>

To: Jonathan Overpeck <jto@u.arizona.edu>

Subject: Re: Borehole in the Southern Hemisphere

Date: Mon, 10 Jul 2006 16:36:08 -0400

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, Jason Smerdon <jsmerdon@ldeo.columbia.edu>

<x-flowed>

Hi Peck et al,

Thanks for your note about the Africa borehole reconstructions, along with the correspondence with Jason Smerdon. In my e-mail to you on April 18, 2006 I had indicated that the African work was unpublished. However, I had forgotten that the Nature paper by Huang, Pollack and Shen (Temperature trends over the past five centuries reconstructed from borehole temperatures, Nature 403, pp 756-758, 2000) actually showed the reconstructions for both southern Africa and Australia as bar graphs of century-long changes in Figure 3 of that paper. The figure displaying both the Africa and Australia borehole reconstructions that appears in the FAR draft (Figure 6.12? or was it 6.11?) shows temperature vs. time for five centuries, a display that differs from the bar-graphs in the Nature paper only in format, not data.

Inasmuch as there have been no additions to the datasets since that paper, it seems that we can correctly say that the reconstructions for southern Africa and Australia have both been published in the Nature (2000) paper. There is nothing "wrong" or outdated with either of those reconstructions. We have, in addition, a newer and more expansive paper about Australia alone (discussing the same reconstruction as appeared in the Nature paper), now in press in the Journal of Quaternary Science. This paper was already mentioned in the e-mail of April 18, 2006, which I will paste at the end of this message.

Other questions?

Cheers,
Henry

[\ /] Henry N. Pollack
| \ | Professor of Geophysics
[MICHIGAN] Department of Geological Sciences
University of Michigan
Ann Arbor, Michigan 48109-1005, U.S.A.

Phone: 734-763-0084 FAX: 734-763-4690

e-mail: hpollack@umich.edu

URL: www.geo.lsa.umich.edu/~hpollack/

URL: www-personal.umich.edu/~hpollack/book.html

e-mail of April 18, 2006:

Date: Tue, 18 Apr 2006 16:26:27 -0400 [04/18/2006 04:26:27 PM EDT]

From: Henry Pollack <hpollack@umich.edu>Add to Address book

(hpollack@umich.edu) United States

To: Keith Briffa <k.briffa@uea.ac.uk>

Cc: jto@u.arizona.edu, eystein.jansen@geo.uib.no

Subject: IPCC FAR draft

Headers: Show All Headers

Hi Keith (and Peck and Eystein),

I have recently been sent the current draft of the IPCC FAR by the US
Global Change

Research Program, asking for comments on the draft. This is the first
time I have seen

this product since we were feverishly exchanging e-mails in February.

Let me call to your

attention some small but not insignificant corrections to be made to
the next draft.

Page 6-33, Section 6.6.1.2, line 22. The title of this section (in
italics) should be
changed to "What do ground surface temperature reconstructions derived
from subsurface
temperature measurements tell us?"

Page 6-33, lines 49 and 52, there is a reference (Smerdon et al., in press).
This paper has now been published, so substitute "2006" for "in press",
and in the list
of references the citation should include the following:

J. Geophys. Res. 111, D07101, doi:10.1029/2004JD005578

Page 6-34, lines 43 and 44. This section is dealing with the southern
hemisphere. The
sentence "...these both indicate unusually warm conditions prevailing
in the 20th century
(Pollack and Smerdon, 2004)" , and the reference therein, are both incorrect.

The ground surface temperature changes over the last 500 years DO NOT
indicate unusually
warm conditions prevailing in the 20th century in Australia and
southern Africa. This is

because the unusually warm conditions developed late in the century, after most of the boreholes had already been logged. What the borehole reconstruction for Australia does show is very good correspondence with the Cook et al (2000) reconstruction for Tasmania and the Cook et al. (2002) recon for New Zealand. The Australia work is described in a manuscript "Five centuries of Climate Change in Australia: The View from Underground" by Pollack, Huang and Smerdon now under review in the Journal of Quaternary Science. The Africa work is unpublished.

Is this e-mail to you sufficient to activate these changes? Or should I submit these comments to the US Government Review Panel? If I am to submit to the latter, they require all comments to be filed by May 9.

Cheers,
Henry

Quoting Jonathan Overpeck <jto@u.arizona.edu>:

> Hi Henry - hope you're having a nice summer. I just got back from the
> IPCC mtg where we made plans for generating the final draft of our
> paleo chapter. One question that came up is whether we can show (in
> Fig 6.12 - southern hemisphere climate records of the last
> millennium) your borehole recon for southern Africa. As you can see
> below, Jason Smerdon has told our SH lead, Ricardo Villalba that the
> recon we've used is not yet published. The question for you is
> whether we can/should use a version that IS published, We feel your
> recon is an important one to show as it represents a region not
> represented by other good reconstructions. But, we don't want to use
> something that has proven to be wrong.
>
> We appreciate your input on this issue. Also, if there is a published
> recon that we can use, would you pls send the recon (guess it's only
> one value per century, right?) and the ref we should cite?
>
> As you can imagine, we're under a tough time constraint, so if you
> can let us know as soon as you can, that would be great.
>

> Many thanks, Peck

>

>

>> X-Sieve: CMU Sieve 2.2

>> From: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

>> To: "Keith R. Briffa" <k.briffa@uea.ac.uk>,
>> "Jonathan Overpeck" <jto@u.arizona.edu>

>> Subject: Borehole in the Southern Hemisphere

>> Date: Thu, 29 Jun 2006 06:00:20 -0300

>>

>> Hi Keith and Peck,

>> Please, find below a copy of the message that I got from Jason Smerdon,
>> regarding the South African borehole record. It looks that the record as it
>> is shown in Figure 6.12 has not been published, however former versions of
>> the South African reconstruction have been included in at least two papers.
>> Please, let me know your impressions to proceed with this matter. Cheers,
>> Ricardo

>>

>> ----- Original Message -----

>> From: "Jason Smerdon" <jsmerdon@ldeo.columbia.edu>

>> To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

>> Sent: Wednesday, June 28, 2006 8:09 PM

>> Subject: Re: Publication in JQR

>>

>>

>>> Hi Ricardo,

>>>

>>> I believe that you are referring to the reconstruction from the Southern
>>> Africa holes that we provided to Tim Osborn. That reconstruction has not
>>> been published as a time series as it is shown in Tim's figure. I
>>> believe, however, that the same reconstruction was published as a
>>> histogram in the following reference:

>>>

>>> Huang S, Pollack HN, Shen PY. 2000. Temperature trends over the last five
>>> centuries reconstructed from borehole temperatures. Nature 403: 756-758.

>>>

>>> The only thing that might be different is the number of holes that were
>>> used, but I don't think that part of the dataset has been updated since
>>> Huang's 2000 paper. To confirm this I would encourage you to contact
>> > Henry Pollack at hpollack@umich.edu. He will know for sure. A similar
>>> reconstruction using a subset of the Southern Africa holes is referenced
>>> in the Australian paper:

>>>

>>> Tyson PD, Mason SJ, Jones MQW, Cooper GRJ. 1998. Global warming and
>>> geothermal profiles: The surface rock temperature response in South
>>> Africa. Geophysical Research Letters 25: 2711-2714.

>>>

>>> But the reconstruction will of course not be exactly equal to the larger
>>> Southern African reconstruction that we provided for Tim. I hope this
>>> helps and let me know if I can be of any further assistance.

>>>

>>> Jason

>>>

>>> On Wed, 28 Jun 2006, Ricardo Villalba wrote:

>>>

>>> > Dear Jason,

>>> > Thanks for the preprint. Do you know if the South African borehole
>> records

>>> > has been published? Thanks,

>>> > Ricardo

>>> >

>>>

>>>

>

>

> --

> Jonathan T. Overpeck

> Director, Institute for the Study of Planet Earth

> Professor, Department of Geosciences

> Professor, Department of Atmospheric Sciences

>

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>

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>

>

>

</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Henry Pollack <hpollack@umich.edu>

Subject: Re: Borehole in the Southern Hemisphere

Date: Fri, 14 Jul 2006 16:46:20 -0600

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>, t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi again Henry - I've attached an 1997 paper of your's and wonder if you could shed some up-to-date insights on how to best interpret. In particular:

1) it has been pointed out to us that the result in this paper argue for a globally warm period during the middle Holocene that was warmer than today. Our assessment (i.e., Figure 6.9) indicates that there was likely no period during the Holocene that was warmer around the global than the late 20th century. Especially outside of the tropics, there were periods warmer than today during the Holocene, but these regionally warm periods were not synchronous - at least at the centennial scale we can examine with proxy data. Thus, although Huang et al. 1997, indicates greater mean annual global warmth, it was unlike the synchronous global warming of the late 20th century.

Plus, we believe the warmth of the Holocene was driven by orbital forcing, and that what we see makes sense in that regard. Huang et al, 1997 can be explained perhaps (this is a question) by the heavy borehole coverage in the Northern mid- to high-latitudes? We also know that proxy data shown in Fig 6.9 also indicate more warming (again, not synchronous) in Southern Hem mid-latitudes - where there are also many boreholes.

Obviously, another issue is that the boreholes don't give the same temporal resolution as the other proxy records we synthesized/assessed, and at least in your paper, there isn't regional information either.

So - the point is not (unless you suggest otherwise) that Huang et al 97 is wrong, but rather than within the limits of the data, it is compatible with what the higher-resolution, regionally-specific, multi-proxy data are showing in Fig 6.9, and that there was likely no period during the Holocene that was warmer synchronously around the

global than the during the late 20th century. Do you agree with this, and is our reasoning accurate and complete?

2) Huang et al 1997 also shows evidence for warmth within the last 500-1000 years that was greater than during the 20th century AND a cool minima 200 years ago. Both of these are highlighted in your abstract, and both seem incompatible with other evidence. For example, your own more recent work has shown the coolest temperatures to be about 500 years ago.

We didn't think it was within our focus to comment on these issues, but we are being asked to by reviewers, and it would be good to have your help in addressing these issues - hopefully in our responses to review comments rather than in our main text (which has to be shortened).

Many thanks for your help with this paper and the issues it raises.

Best, Peck

--

Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences

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<http://www.ispe.arizona.edu/>
</x-flowed>

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\huang1997GRLHoloceneBoreholes.pdf"

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>, Eystein Jansen <eystein.jansen@geo.uib.no>

Subject: figure issues

Date: Fri, 14 Jul 2006 17:20:26 -0600

Cc: Valerie Masson-Delmotte <Valerie.Masson@cea.fr>

<x-flowed>

Hi all - including Eystein, whom I haven't been able to talk with on these issues yet:

1) I'd like to get your status report on Fig. 6.12 - based on feedback from Henry Pollack, we will keep the borehole curves and corresponding instrumental data. I believe we are also going to add the new recon from Law Dome - Valerie was going to send. Do you have everything needed for this figure revision?

2) Since we met in Bergen, I have received feedback from many about our MWP box, and would like to float the idea that we delete the bottom (Osborn and Briffa) panel. I know this is shocking coming from me (I think O&B, 2006 is a paper of the year contender!), but I have become convinced that it will be too much of a lightning rod for what it gives us. We still show the data in the top panel, which conveys the same thing (although in a much less sophisticated way!), and we still back up with citations to O&B2006. BUT, we hopefully avoid a possible intense focus on methodological focus on the fig, and the criticism that it's LA work that hasn't been thoroughly vetted. This focus (i.e., from skeptics and those inclined to listen to them for political reasons) is stupid, but we want to keep readers focused on the science and not on the politically-generated flak. I think we can do this just as well without the O&B06 figure, assuming we still cite the findings of the O&B06 paper, but just don't show the figure. We also save space - not the reason for my suggestion, but a good thing given what Keith and Tim need to add in response to issue like divergence etc.

Obviously, was the biggest fan and pusher for the figure to be included, and I'm sorry to be suggesting otherwise now.

Does this make sense?

Thanks, Peck

--

Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
Professor, Department of Atmospheric Sciences

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</x-flowed>

From: "Smith, G. (Geoff) (SG)" <Geoff.Smith@AKZONOBEL-CHEMICALS.COM>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Re: [ITRDBFOR] Joe Barton's hockey stick hearing coming up
Date: Sat, 15 Jul 2006 10:36:57 +0800
Reply-to: ITRDB Dendrochronology Forum <ITRDBFOR@LISTSERV.ARIZONA.EDU>

Dr. Solomon,

It is not clear what makes the Wegman Committee Report in your opinion a "new low". In scientific study, one part is clearly physical (growth rates of trees, IR absorption, etc.) and a separate part is the statistical treatment of the data.

Dr. Wegman's report is clearly focused on the latter. He is well qualified to analyze statistical methods, as chair of the National Academy of Sciences' (NAS) Committee on Applied and Theoretical Statistics, and a board member of the American Statistical Association.

The conclusion of the Committee headed by Dr. Wegman is clear - the statistical methods of MBH 98/99 cannot be relied upon to support the claim that the 90's were the hottest decade of the past millennium. If one wants to argue with Dr. Wegman's conclusion, it will be necessary to show how he has misunderstood or misrepresented the statistical methods used in those studies.

Obviously this does not prove that the 90's were not the hottest decade of the past millennium, only that the MBH 98/99 analyses cannot be used to support that claim, nothing more and nothing less.

Anyone interested in paleoclimatology in general, and dendrochronology in particular, should read the recent NAS report and the Wegman Committee Report (or in fact anyone interested in the use of statistics in climatology).

Your last comment seems to reflect a belief that it is scurrilous to "question unquestioned science". Wouldn't there seem to be a long honored history of exactly this type of action, both before and after Einstein? Or perhaps I'm misinterpreting your remarks.

Geoff Smith
Singapore

-----Original Message-----

From: ITRDB Dendrochronology Forum
[mailto:ITRDBFOR@LISTSERV.ARIZONA.EDU] On Behalf Of Allen M. Solomon
Sent: Saturday, July 15, 2006 6:53 AM
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Re: Joe Barton's hockey stick hearing coming up

You also may want to look at a new "report" prepared for Barton by a group of statisticians regarding the hockey stick - this is going to be the focus of the hearing, in order to advertise it. It seems (to me) to be a new low in politics to have a "congressional report" generated specifically to question unquestioned science.

-Al
Allen M Solomon, Ph.D.
National Program Leader, Global Change Research
USDA Forest Service
4th Floor, RPC
1601 North Kent St
Arlington VA 22209
allensolomon@fs.fed.us
703 605 5251

E&ENews PM
Friday, July 14, 2006

CLIMATE: New House report sets stage for another 'hockey stick' brawl
Lauren Morello, E&ENews PM reporter
Flawed statistics underlie the controversial "hockey stick" climate analysis, according to a report released today by an ad hoc panel of scientists assembled by the House Energy and Commerce Committee.

The report contradicts a recent National Academy of Sciences study that found the hockey stick analysis -- which concluded Earth has been warmer

over the last millennium than at any other point -- is largely correct.

Published in 1998 by the journal Nature, the hockey stick reconstructs past global average temperatures using data from corals, tree rings, ice cores and bore holes deep within the Earth -- the first to draw on multiple sources of "proxy data" to sketch a picture of past climate.

The study includes a graph that shows Earth's average temperature increasing sharply during the 20th century, with an upward curve that resembles the

blade of a hockey stick. Often cited as evidence that human emissions are the dominant cause of rising global temperatures, the graph became controversial after it appeared in a 2001 Intergovernmental Panel on Climate Change report.

But the House Committee's ad hoc panel says the hockey stick's authors relied on statistics that are pre-disposed to produce the hockey-stick shape.

Claims by the hockey stick paper's authors of unprecedented global warming during the 20th century "cannot be supported by [the] analysis," the panel concluded.

The Energy and Commerce Committee -- whose chairman, Rep. Joe Barton (R-Texas), is a leading Capitol Hill critic of the hockey-stick study -- has scheduled a hearing next week on the ad hoc panel's conclusions.

In June 2005, Barton and Oversight and Investigations Subcommittee Chairman Ed Whitfield (R-Ky.) launched a probe into scientific and financial records of climatologists who created the graph -- Michael Mann of Pennsylvania State University, Raymond Bradley of the University of Massachusetts and

Malcolm Hughes of the University of Arizona (Greenwire, July 18, 2005).

That prompted a rare show of public infighting between Barton and Whitfield and House Science Committee Chairman Sherwood Boehlert (R-N.Y.), who asked the National Academy of Sciences to examine the validity of the hockey stick and similar climate reconstructions (Greenwire, June 23).

Click here to view the House panel report.

Click here to view the National Academy of Sciences report.

Click here to view the hockey stick paper [Nature subscription required].

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----- Original Message -----
From: "David M. Lawrence" <dave@FUZZO.COM>
To: <ITRDBFOR@LISTSERV.ARIZONA.EDU>
Sent: Friday, July 14, 2006 3:13 PM

Subject: Joe Barton's hockey stick hearing coming up

>I thought I'd pass this on since tree-ring data and their use in
> reconstructing past climates are central to the controversy. I wonder
if
> any attention will be paid to the recently released NRC report on
climate
> over the past 2,000 years, or in a forthcoming paper in Climate Change

> that
> finds the method used to obtain the hockey stick reasonably robust.

> Dave

> -- here's my note posted to two journalism lists --

> It looks like Joe Barton will get all the climate uncertainty sorted
out

> on
> Wednesday, June 19, at 10 a.m. He will be holding a hearing called
> "Questions Surrounding the 'Hockey Stick' Temperature Studies:
> Implications

> for Climate Change Assessments." The hearing will focus on the
notorious

> "hockey stick" graph indicating that the temperatures in the latter
part

> of
> the 20th century were higher than at any time in the last millennium.

> I doubt there will be more light than heat, but the hearing will be
> interesting to watch, if anything. The hearing can be watched live
via

> the
> Internet.

> For more information:

> http://energycommerce.house.gov/108/News/07142006_1989.htm

> [http://energycommerce.house.gov/108/Hearings/07192006hearing1987/hearing
.htm](http://energycommerce.house.gov/108/Hearings/07192006hearing1987/hearing.htm)

>

> Dave

>

> -----

> David M. Lawrence | Home: (804) 559-9786

> 7471 Brook Way Court | Fax: (804) 559-9787

> Mechanicsville, VA 23111 | Email: dave@fuzzo.com

> USA | http: <http://fuzzo.com>

> -----

>

> "We have met the enemy and he is us." -- Pogo

>

> "No trespassing

> 4/17 of a haiku" -- Richard Brautigan

>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: draft of EOS piece
Date: Mon, 17 Jul 2006 08:31:41 -0400
Reply-to: mann@psu.edu

<x-flowed>

Hi Keith,

Thanks, please comment on the attached version which incorporated all other comments received.

thanks,

mike

Keith Briffa wrote:

> Mike
> just back from holiday - can you send me latest draft and I will
> comment asap on it - somewhat confused re where we are with others -
> or should I just comment immediately on the one you sent?

> Keith

>
> At 16:36 12/07/2006, you wrote:

>
>> thanks very much Guys,

>>
>> will await comments from Keith and Heinz (?), prepare one last
>> version, and then submit...

>>
>> mike

>>
>> Caspar Ammann wrote:

>>
>>> Mike,
>>> here also a few thoughts and edits from me (in-between kids waking
>>> up, dressing, feeding, etc.)

>>> Caspar

>>>

>>>

>>>

>>>
>>> On Jul 12, 2006, at 6:18 AM, Michael E. Mann wrote:

>>>
>>>> Thanks Christoph,
>>>> Awaiting comments from others.

>>>>
>>>> Caspar: any comments on our discussion of the challenge?

>>>>
>>>> thanks,

>>>>
>>>> mike

>>>>
>>>> Christoph Kull wrote:

>>>>
>>>>>

>>>>> Dear all,
>>>>> Thanks Mike for this report.
>>>>> I made a few edits / suggestions - it's up to you to decide on them.
>>>>> Hopefully Caspar can also provide some input.
>>>>> We will be ready to communicate the weblink for the challenge by
>>>>> end of this
>>>>> week. I will let you know....

>>>>>
>>>>> All the best, thanks a lot and greetings from Bern,
>>>>> Christoph

>>>>>
>>>>>
>>>>> On 10.07.2006 19:57, "Michael E. Mann"
>>>>> <mailto:mann@meteo.psu.edu><mann@meteo.psu.edu> wrote:

>>>>>
>>>>>
>>>>>> Dear Keith/Phil/Christoph/Thorsten/Heinz,
>>>>>>
>>>>>> Attached is a draft meeting report for EOS. Rather than re-invent
>>>>>> the
>>>>>> wheel, I have followed closely the PAGES newsletter piece, but have
>>>>>> expanded on certain points as appropriate for the broader EOS
>>>>>> audience.
>>>>>> I've also included Caspar. Though not a member of the PAGES/CLIVAR
>>>>>> intersection working group, I want to get his feedback too,
>>>>>> particularly
>>>>>> on the discussion of the "PR Challenge".

>>>>>>
>>>>>> The word limit for an Eos meeting piece is 1500 words, we're
>>>>>> currently
>>>>>> about 200 words under. So there is room for small additions or
>>>>>> expansions of key points.
>>>>>>
>>>>>> Please send me any suggested changes/additions/etc. or, if you
>>>>>> have none
>>>>>> simply indicate that you are happy with it as is, and happy to
>>>>>> lend your
>>>>>> name to it.
>>>>>>
>>>>>> Thanks in advance,
>>>>>>
>>>>>> mike
>>>>>>
>>>>>>
>>>>>>
>>>>>>
>>>>>>
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> --

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>
Subject: new fig 6.14
Date: Mon, 17 Jul 2006 15:08:48 +0100
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, joos <joos@climate.unibe.ch>

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Hi Peck, Eystein and Fortunat,

I've drafted two versions of the new fig 6.14, comprising a new panel showing the forcing used in the EMIC runs, plus the old fig 6.13e panel showing the EMIC simulated NH temperatures. Keith has seen them already.

First you should know what I did, so that you (especially Fortunat) can check that what I did was appropriate:

(1) For the volcanic forcing, I simply took the volcanic RF forcing from Fortunat's file and applied the 30-year smoothing before plotting it.

(2) For the solar forcing there are 2 curves. For the first, I took the Bard 0.25% column from Fortunat's RF file. For the second, I took the Bard 0.08% column from Fortunat's RF file from 1001 to 1609, and then appended the WLS RF forcing from 1610 to 1998. Then I smoothed the combined record. NOTE that for the Bard0.25%, the line is flat from 1961 onwards which probably isn't realistic, even though that is what was used in the model runs.

(3) For the "all other forcings" there are 2 curves. For the first, I took the CO2 concentrations provided by Fortunat, then used the "standard" IPCC formula from the TAR (in fact the first of the three options for CO2 in IPCC TAR Table 6.2) to convert this to a radiative forcing. I then added this to the non-CO2 radiative forcings data from Fortunat's file, to get the total radiative forcing. For the second, I replaced all values after 1765 with the 1765 value (for the natural forcings case). Then I smoothed the combined record (as in fig 6.13c, I only applied a 10-year smoothing when plotting the "all other forcings", because it is fairly smooth anyway and using a high smoothing results in lower final values when there is a strong trend at the end of a time series).

Now, some comments on the figures themselves (please print them and refer to them when reading this):

(1) File 'chap6_f6.14_option1.pdf' is strongly preferred by Keith and me. This shows the three forcing components separately, which helps with understanding the individual causes of specific warming and cooling periods. I have managed to reduce the size of this considerably, compared to the equivalent panel in fig 6.13, because with only a few series on it I could squeeze them together more and also reduce the range of the vertical axes.

(2) Although we don't prefer it, I have also made 'chap6_f6.14_option2.pdf' which is even smaller by only showing the sum of all the forcings in the top panel.

Which version do you prefer? Please let me know so I can make final changes only to the preferred version.

Some more comments:

(1) Fig 6.14b was originally Fig 6.13e. When it was part of that figure, the colour bar showing the shades of grey used to depict the overlapping ranges of the published temperature reconstructions was only on Fig 6.13d. Do you think I should now also add it to the EMIC panel (6.14b), now that it is in a separate figure? It will be a bit of a squeeze because of the legend that is already in 6.14b.

(2) Another carry over from when 6.14b was part of 6.13, is that the time range of all panels had to match (900-2010). Now that the EMICs are in a separate figure, I could start them in year 1000, which is when the forcing and simulations begin. Unless you want 6.13 and 6.14 to remain comparable? Again please comment/decide.

(3) I wasn't sure what colours to use for the forcing series. In option 1, the volcanic and other forcings apply to all runs, so I chose black (with thick/thin used to distinguish the "all" forcings from the "natural-only" forcings (basically the thin flat line in "all other forcings)). The cyan-green-blue runs used strong solar forcing, so I used blue for that forcing. The red-orange-brown runs used weak solar forcing, so I used brown for that forcing. Sound ok?

Sorry for the long email, but I wanted to get everything explained to avoid too many iterations.

Please let me know your decisions/comments on these questions, or on

any other aspects of the new figure.

Cheers

Tim

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Attachment Converted: "c:\eudora\attach\chap6_f6.14_option2.pdf"

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****Norwich -- City for Science:**

****Hosting the BA Festival 2-9 September 2006**

</x-flowed>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Special instructions/timing adjustment
Date: Mon Jul 17 16:25:59 2006
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>, joos <joos@climate.unibe.ch>

Hi all,
I'm halfway through these changes and will get the revised figures out to you probably tomorrow, except maybe the SH one, because:
I'm not sure if the van Ommen (pers. comm.) data shown by Jones & Mann and suggested by Riccardo are the data to use or not. Is it published properly? I've seen the last 700 years of the Law Dome 18O record published, so perhaps we should show just the period since 1300 AD? That period appears in:
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Mid latitude winter climate variability in the South Indian and southwest Pacific regions since 1300 AD
CLIMATE DYNAMICS 22 (8): 783-794 JUL 2004
See below for some more comments in respect to individual figures.
At 21:36 30/06/2006, Jonathan Overpeck wrote:

Figure 6.10.

1. shade the connection between the top and middle panels

yes

2. remove the dotted (long instrumental) curve from the middle panel

yes

3. replace the red shaded region in the bottom panel with the grey-scale one used in Fig 6.13

yes

4. label only every increment of 10 in the grey-scale bar (formally color) in the bottom panel

yes

5. Increase font sizes for axis numbering and axis labeling - all are too small. You can figure out the best size by reducing figs to likely page size minus margins. We guess the captions need to be bigger by a couple increments at least.

yes

Figure 6.11.

1. This one is in pretty good shape except that Ricardo has to determine if S. African boreholes need to be removed.

I think Henry said they were published and could stay

Figure 6.12

1. again, please delete S. African borehole if Ricardo indicates it's still not published.

I think Henry said they could stay.

2. consider adding Law Dome temperature record - Ricardo is investigating, but perhaps Keith/Tim can help figure out if it's valid to include. Feel free to check with Valerie on this too, as she seems to know these data at least a little

Already discussed above.

3. also, please increase font sizes and make sure they match 6.10 - probably better to use bold fonts

You are right that I've mixed bold and non-bold. When reduced to small size, the non-bold actually read more clearly than the bold, I think, so I'll standardise on non-bold. It's not possible to completely standardise on the size, because each figure I provide might be scaled by different amounts. I don't know final figure size, so will make a good guess. Should be ok.

Figure 6.13

1. we are going to split the existing 6.13 into two figure. The first is 100% Tim's fig., and is just an upgrade of the existing 6.13 a-d, with the only changes being:
1a. delete the old ECHO-G red dashed line curve in panel d, and

Keith says this was discussed and rejected, so I should keep old ECHO-G in?

1b. please also increase font sizes and make sure they match 6.10 and 12 - please use bold fonts.

ok, as discussed above.

2. The existing 6.13e is going to become a new 6.14, with the addition of a new forcings panel "a" on top of the existing panel e (which becomes 6.14b). To make this happen, Tim and Fortunat have to coordinate, as Tim has the forcing data (and knows what we what) and Tim has the existing figure. We suspect it will be easier for Fortunat to give Tim data and layout advice, and for Tim to make a figure that matches the other figs he's doing. PLEASE NOTE that this fig can't be as large as the existing 6.13a-d, but needs to be more compact to permit its inclusion.

done.

Cheers

Tim

From: "Cooke, Barry" <bcooke@NRCAN.GC.CA>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Re: [ITRDBFOR] Joe Barton's hockey stick hearing coming up
Date: Mon, 17 Jul 2006 17:46:01 -0400
Reply-to: ITRDB Dendrochronology Forum <ITRDBFOR@LISTSERV.ARIZONA.EDU>

"Non-independence" of reconstructions and "worthlessness" of the hockey stick model were raised as separate issues.

If the worth of a model is measured by its ability to predict, then a model that explains 0.5% of the variation in some variable is fairly (but not necessarily completely) "worthless". Surely, one hopes for better. Especially where consensus is required.

The proxy data on which multi-proxy reconstructions are based may be statistically independent, but the reconstructions themselves are not. This is not because of any lack of "independence" (i.e. objectivity) among networked researchers, but a measurable fact of arithmetic. To the extent that multi-proxy reconstructions are built on the same proxy data, they are statistically non-independent (i.e. correlated).

i.e. It's not the non-independence that make the model worthless. It's the uncertainty.

On your last point of social networks, try a Google search of 'Exxon Secrets'. The difference between a ruling orthodoxy and a scientific network is not the degree of connectivity, but the mode of governance: coercion & inculcation vs. facts & reason (including statistical inference). Be wary of any science that loathes statistics or resents external investigation. That's the start of rot.

If Wegman et al. are suggesting that statisticians should be put to work to serve the interests of paleoclimatologists (which they are), then who on this list is going to argue that? I say let's put them to work!

Barry Cooke

-----Original Message-----

From: ITRDB Dendrochronology Forum
[mailto:ITRDBFOR@LISTSERV.ARIZONA.EDU]
Sent: Monday, July 17, 2006 6:43 AM
To: ITRDBFOR@LISTSERV.ARIZONA.EDU

Subject: Re: Joe Barton's hockey stick hearing coming up

>Maryanne's message further claims that the "characterization of the
>hockey stick as 'worthless' underscores what appears to be a basic lack

>of understanding of how scientific consensus is formed". Yet if a
>consensus is based on invalid statistical analysis, then the consensus
>is wrong.

To explain my point (and my apologies to those to whom this is obvious):
it would not be unprecedented for a scientific consensus to be wrong.
However, there is also ample precedent for papers containing flaws
(which virtually all do, if somebody looks hard enough, or has the
misfortune of having the resources of Congress devoted to finding them)
to have constructive influence on debate. To take an example from
history, many of Charles Darwin's observations are pure amateurish
nonsense by the standards of even the late 19th century, but no one
would doubt their value in building the consensus for evolution. The
question is not always strict veracity, but whether work provokes
fruitful questions, or leads research in a constructive direction. (By
the way, this is not to take a position on the Wegman judgement on the
MBH papers).

>Dave's message further claims that there are multiple "independent
>lines of evidence" for the hockey stick. The Wegman report discusses
>this claim. See especially p.46-47, which cite twelve different
>studies and concludes that those studies "cannot really claim to be
>independent".

This part of the report is more precious than useful. In most empirical
fields, leading primary investigators have linkages--nothing unusual
about that. We could construct similar matrices of social networks in
physics, biology, statistics. That doesn't mean the works produced in
physics, biology or statistical theory are "worthless". A similar point
can be made about different investigators using the same proxy data. In
fact, isn't it one of the recommendations of the Wegman report that the
paleoclimate community share data more effectively? Seems that if that
recommendation was followed, certain statisticians would have even more
occasion to complain of a lack of true independence. Seems these poor
climate experts can't win!

Wouldn't it be interesting to see a "social network" matrix--or a
funding matrix--between those the scientists, statisticians,

Congressional Republicans, and oil companies most passionate about
"debunking" global climate change?

Dr. Maryanne W. Newton

Research Associate

Malcolm and Carolyn Wiener Laboratory for Aegean and Near Eastern

Dendrochronology Cornell University

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: Special instructions/timing adjustment

Date: Mon, 17 Jul 2006 21:33:46 -0600

Cc: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>

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Hi Tim et al (especially Valerie) - again, sorry for the confusion, but hopefully the emails sent and forwarded from Valerie and me this evening helps figure this out. I think we're going with borehole for Law Dome, but you guys need to confirm it's the way to go. I'm cc'ing to Valerie in the hope she can try to provide more guidance in this - with a confirmation that it's the best way to go and will stand up to criticism. If we have multiple conflicting temp recons from Law Dome, and one can't be shown from the literature as being the best, then we should state that, and show neither - just an idea. BUT, I think Valerie was pretty sure the borehole was best. She should be more available in a day or so.

Thanks all, cheers, Peck

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>out to you probably tomorrow, except maybe the SH one, because:

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>sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

>

>****Norwich -- City for Science:**

>****Hosting the BA Festival 2-9 September 2006**

--

Jonathan T. Overpeck

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Professor, Department of Atmospheric Sciences

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Fortunat Joos <joos@climate.unibe.ch>

Subject: Re: new fig 6.14

Date: Tue, 18 Jul 2006 10:22:26 -0600

Cc: t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi all - Thanks for all the Euro-dialog before I even got to my computer - lots of good issues raised, and glad the misunderstanding got cleared up.

Eystein and I can't connect easily today, so I'm going to take a stab at the CLA compromise, guessing that he'll concur. If not, he can clarify.

1) We really do need to see the original forcing (spikes for volc, higher freq for solar), so that should be a given. If Tim can do his usual graphical magic and get a smoothed version in there too, that's ok, but I think Fortunat is correct that this new 6.14 gives us a chance to show data differently (and in a way that the TS team really would like). BUT, to show a smoothed curve, perhaps behind? (or whatever looks best and makes it easy to see the more raw data) the more raw data, would be a nice way to connect 6.14 with 6.13, and also make the points that Tim points out - especially highlighting the obvious link between forcing and response prior to 1900. This last point is key for the TS too. BUT, please don't make the more raw data hard to see - they are a KEY part of this fig, especially in the TS. So... go for it Tim - I suggest some annotation for those peaks that are too large to plot - perhaps an asterisk with a note in the caption that "*volcanic forcing peaks larger than XXX are truncated for plotting purposes" or something like that.

2) the normalisation reference period should be consistent between all of the associated figs, so I'd stick with with you've been doing Tim. Otherwise, it will be too confusing.

3) as to whether forcing should be proportional. As long as the scaling (y-axis labeling) is explicit we can be flexible here in order to make sure viewers can see all of the smoothed and unsmoothed forcing data clearly. That is the key, and we can relax the need to have them all proportional in this fig.

Bottom line is that the forcing data we present should have the ability to see the differences in solar clearly - as Fortunat's mock-up plot does. This is driven more from the TS, but that's ok - we get serious play in the TS.

Hope this provides enough for Tim to go with, and as always, if you want to provide some options, that's fine.

Fortunat - you'll need write the caption - hopefully keeping it as brief as possible by citing the earlier captions in the report.

thanks all! best, Peck

--

Jonathan T. Overpeck
Director, Institute for the Study of Planet Earth
Professor, Department of Geosciences
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<http://www.ispe.arizona.edu/>
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From: "Cooke, Barry" <bcooke@NRCAN.GC.CA>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: [ITRDBFOR] Wegman on calibrating response functions
Date: Tue, 18 Jul 2006 10:27:21 -0400
Reply-to: ITRDB Dendrochronology Forum <ITRDBFOR@LISTSERV.ARIZONA.EDU>

That may be "the point" that you're choosing to focus on. My point, quite apart from yours, is that (1) there were oversights in MBH98, (2) that paper appears to have been rushed to publication, (3) M&M03 appear to have been shunned by the scientific review process, (4) Wegman et al. have got a couple of good points on the statistics of tree-ring calibration worthy of discussion, (5) the issue of calibration error cuts to the core of the debate, as it is what underlies the breadth of the confidence envelope around the hockey stick during the MWP. You criticize their analysis of the MBH98 social network, but what do you make of their more substantive argument regarding errors in calibration response functions?

Barry

-----Original Message-----

From: ITRDB Dendrochronology Forum
[mailto:ITRDBFOR@LISTSERV.ARIZONA.EDU]
Sent: Tuesday, July 18, 2006 6:28 AM
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Re: Joe Barton's hockey stick hearing coming up

At 05:46 PM 7/17/2006 -0400, Barry Cooke wrote:

>The proxy data on which multi-proxy reconstructions are based may be
>statistically independent, but the reconstructions themselves are not.
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>among networked researchers, but a measurable fact of arithmetic. To
>the extent that multi-proxy reconstructions are built on the same proxy
>data, they are statistically non-independent (i.e. correlated).

Fair enough. But I believe the point (or at least the implication) is being made that these networked researchers are failing to adequately review the work of their peers. It would also be naive not to expect that Mr. Barton and the political wing of the "Climate science is bunk" crowd will use those connects to argue for the "worthlessness" of most everything produced by the network. (Note the recent public comments by

Senator Inhofe).

Dr. Maryanne W. Newton

Research Associate

Malcolm and Carolyn Wiener Laboratory for Aegean and Near Eastern

Dendrochronology Cornell University

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: Law Dome figure

Date: Tue, 18 Jul 2006 10:30:36 -0600

Cc: Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, Keith Briffa <k.briffa@uea.ac.uk>, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Tim, Ricardo and friends - your suggestion to leave the figure unchanged makes sense to me. Of course, we need to discuss the Law Dome ambiguity clearly and BRIEFLY in the text, and also in the response to "expert" review comments (sometimes, it is hard to use that term "expert"...).

Ricardo, Tim and Keith - can you take care of this please. Nice resolution, thanks.

best, Peck

>Hi all,

>

>(1) Jones/Mann showed (and Mann/Jones used in >their reconstruction) an isotope record from Law >Dome that is probably O18 (they say "oxygen >isotopes"). This has a "cold" present-day and >"warm" MWP (indeed relatively "warm" throughout >the 1000-1750 period). The review comments from >sceptics wanted us to show this for obvious >reasons. But its interpretation is ambiguous >and I think (though I'm not certain) that it has >been used to indicate atmospheric circulation >changes rather than temperature changes by some >authors (Souney et al., JGR, 2002).

>

>(2) Goosse et al. showed Deuterium excess as an >indicator of Southern Ocean SST (rather than >local temperature). Goosse et al. also showed a >composite of 4 Antarctic ice core records (3 >deuterium, 1 O18). Neither of these comes up to >the 20th century making plotting on the same >scale as observed temperature rather tricky!

>

>(3) Dahl-Jensen showed the temperatures obtained >by inverting the borehole temperature profiles. >This has a colder MWP relative to the recent >period, which shows strong recent warming.

>

>I have data from (1) and now from (3) too, but

>not from (2) though I could ask Hugues Goose
>for (2). Anyway, (1) and (2) aren't calibrated
>reconstructions like the others in the Southern
>Hemisphere figure, so plotting them would alter
>the nature of the figure.

>

>But if we show only (3) then we will be accused
>of (cherry-)picking that (and not showing (1) as
>used by Mann/Jones) because it showed what we
>wanted/expected.

>

>Can I, therefore, leave the SH figure unchanged
>and can we just discuss the Law Dome ambiguities
>in the text?

>

>Cheers

>

>Tim

>

>At 02:41 18/07/2006, Jonathan Overpeck wrote:

>>Hi Tim, Ricardo and Keith - Valerie just
>>reminded me that she sent this to us all (minus
>>Tim) back in June. There is plenty below for
>>discussion in the text, and the Law Dome
>>borehole data can be obtained at the site below
>>(http://www.nbi.ku.dk/side95613.htm). This is
>>the record that should be added to the SH
>>figure.

>>

>>Thanks, Peck

>>

>>>X-Sieve: CMU Sieve 2.2

>>>Date: Wed, 28 Jun 2006 12:44:50 +0200

>>>From: Valérie Masson-Delmotte <Valerie.Masson@cea.fr>

>>>Reply-To: Valerie.Masson@cea.fr

>>>Organization: LSCE

>>>To: Jonathan Overpeck <jto@u.arizona.edu>,

>>> Ricardo Villalba <ricardo@lab.cricyt.edu.ar>,

>>> Keith Briffa <k.briffa@uea.ac.uk>

>>>Subject: (pas de sujet)

>>>

>>>Dear Ricardo and Peck,

>>>

>>>Here are the references for the Law Dome temperature discussion :

>>>

>>>* stack of Antarctic ice cores and Law Dome

>>>deuterium excess profile (showing large

>>>changes in moisture source)

>>>

>>>Title: *A late medieval warm period in the
>>>Southern Ocean as a delayed response to
>>>external forcing?*

>>>Author(s): *Goosse H*

>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=Goosse+H&curr_doc=1/3&Form=FullRecordPage&doc=1/3>,
>>>*Masson-Delmotte V*

>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=Masson-Delmotte+V&curr_doc=1/3&Form=FullRecordPage&doc=1/3>,
>>>Renssen H

>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=Renssen+H&curr_doc=1/3&Form=FullRecordPage&doc=1/3>,
>>>Delmotte M

>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=Delmotte+M&curr_doc=1/3&Form=FullRecordPage&doc=1/3>,
>>>Fichefet T

>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=Fichefet+T&curr_doc=1/3&Form=FullRecordPage&doc=1/3>,
>>>Morgan V

>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=Morgan+V&curr_doc=1/3&Form=FullRecordPage&doc=1/3>,
>>>van Ommen T

>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=van+Ommen+T&curr_doc=1/3&Form=FullRecordPage&doc=1/3>,
>>>Khim BK

>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=Khim+BK&curr_doc=1/3&Form=FullRecordPage&doc=1/3>,
>>>Stenni B

>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=Stenni+B&curr_doc=1/3&Form=FullRecordPage&doc=1/3>

>>>Source: GEOPHYSICAL RESEARCH LETTERS 31 (6): Art. No. L06203 MAR 17 2004

>>>Document Type: Article

>>>Language: English

>>>Abstract: On the basis of long simulations
>>>performed with a three-dimensional climate
>>>model, we propose an interhemispheric climate
>>>lag mechanism, involving the long-term memory
>>>of deepwater masses. Warm anomalies, formed in
>>>the North Atlantic when warm conditions
>>>prevail at surface, are transported by the
>>>deep ocean circulation towards the Southern
>>>Ocean. There, the heat is released because of
>>>large scale upwelling, maintaining warm
>>>conditions and inducing a lagged response of
>>>about 150 years compared to the Northern
>>>Hemisphere. Model results and observations
>>>covering the first half of the second
>>>millenium suggest a delay between the
>>>temperature evolution in the Northern
>>>Hemisphere and in the Southern Ocean. The
>>>mechanism described here provides a reasonable
>>>hypothesis to explain such an interhemipsheric
>>>lag.

>>>KeyWords Plus: CLIMATE-CHANGE; ICE CORE; LAW
>>>DOME; TEMPERATURES; ANTARCTICA; PALEOCLIMATE;
>>>CIRCULATION; MILLENNIUM; RECORDS; SIGNAL
>>>

>>>* borehole temperature profile from Law Dome :

>>>Title: *Monte Carlo inverse modelling of the
>>>Law Dome (Antarctica) temperature profile*
>>>Author(s): *DahlJensen D*
>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=DahlJensen+D&curr_doc=4/15&Form=FullRecordPage&doc=4/15>,
>>>Morgan VI
>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=Morgan+VI&curr_doc=4/15&Form=FullRecordPage&doc=4/15>,
>>>Elcheikh A
>>><http://wos.isiknowledge.com/CIW.cgi?SID=X1EEf2907CO3d9dPA24&Func=OneClickSearch&field=AU&val=Elcheikh+A&curr_doc=4/15&Form=FullRecordPage&doc=4/15>
>>>Source: ANNALS OF GLACIOLOGY, VOL 29, 1999
>>>ANNALS OF GLACIOLOGY 29: 145-150 1999
>>>Document Type: Article
>>>Language: English
>>>
>>>Abstract: The temperature profile in the 1200
>>>m deep Dome Summit South (DSS) borehole near
>>>the summit of Law Dome, Antarctica, was
>>>measured in 1996, 3 years after the
>>>termination of the deep drilling.
>>>
>>>The temperature profile contains information
>>>on past surface temperature over the last 4
>>>ka. This temperature history is determined by
>>>the use of a Monte Carlo inverse method in
>>>which no constraints are placed on the unknown
>>>temperature history and no solution is assumed
>>>to be unique. The temperature history is
>>>obtained from a selection of equally
>>>well-fitting solutions by a statistical
>>>treatment.
>>>
>>>The results show that solutions covering the
>>>last 4 ka have a well-developed central value,
>>>a most likely temperature history. The
>>>temperature record has two well-developed
>>>minima at: AD 1250 and 1850. From 1850 to the
>>>present, temperatures have gradually increased
>>>by 0.7 K. The reconstructed temperatures are
>>>compared with the stable oxygen isotope
>>>(delta(18)O) from the DSS ice core.
>>>
>>>=> The inversed temperature data are available on the GFY web site at :
>>><http://www.nbi.ku.dk/side95613.htm>, go to "Dye
>>>3, GRIP, Law Dome temperature reconstructed
>>>from borehole measurements"
>>>
>>>* Regarding the calibration issue there are several publications :
>>>- seasonal calibration between 18O and T :
>>>
>>>[van Ommen and Morgan, 1997a]

>>><<http://staff.acecrc.org.au/%7Eetas/home/reprints/1997%20-%20JGR%20-%20van%20Ommen%20-%20delT.pdf>>

>>>

>>>Tas D. van Ommen and Vin Morgan. Calibrating

>>>the ice core paleothermometer using

>>>seasonality. J. Geophys. Res.,

>>>102(D8):9351-9357, 1997, [AAD Cat. Ref. 7488].

>>>

>>>[van Ommen and Morgan, 1997b]

>>><<http://staff.acecrc.org.au/%7Eetas/home/reprints/1997%20-%20JGR%20-%20van%20Ommen%20-%20delTcorr.pdf>>

>>>

>>>Tas D. van Ommen and Vin Morgan. Correction to

>>>"Calibrating the ice core paleothermometer

>>>using seasonality". J. Geophys. Res.,

>>>102(D25):30,165, 1997, [AAD Cat. Ref. 8236].

>>>

>>>- decadal calibration from a high resolution

>>>ice core (using deuterium excess)

>>>

>>>*Recent southern Indian Ocean climate

>>>variability inferred from a Law Dome ice core:

>>>new insights for the interpretation of coastal

>>>Antarctic isotopic records*

>>>V. Masson-Delmotte ^{A1}, M. Delmotte ^{A1 A4},

>>>V. Morgan ^{A2}, D. Etheridge ^{A3}, T. van

>>>Ommen ^{A2}, S. Tartarin ^{A1}, G. Hoffmann

>>>

>>>Stable isotopes in water have been measured

>>>along a very high accumulation ice core from

>>>Law Dome on the east Antarctic coast. These

>>>enable a detailed comparison of the isotopic

>>>records over sixty years (1934-1992) with

>>>local (Antarctic station data) and remote

>>>meteorological observations (atmospheric

>>>reanalyses and sea-surface temperature

>>>estimates) on a seasonal to inter-annual time

>>>scale. Using both observations and isotopic

>>>atmospheric general circulation model (GCM)

>>>results, we quantify the relationships between

>>>stable isotopes ($\delta^{18}O$, δD and deuterium

>>>excess; $\delta/ = \delta D - 8 \times \delta^{18}O$) with site and

>>>source temperature at seasonal and decadal

>>>time scales, showing the large imprint of

>>>source conditions on Law Dome isotopes. These

>>>calibrations provide new insights for the

>>>quantitative interpretation of temporal

>>>isotopic fluctuations from coastal Antarctic

>>>ice cores. An abrupt change in the local

>>>meridional atmospheric circulation is clearly

>>>identified from Law Dome deuterium excess

>>>during the 1970s and analysed using GCM
>>>simulations.

>>>

>>>

>>>

>>>Valérie.

>>

>>

>>--

>>Jonathan T. Overpeck

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>

>***Norwich -- City for Science:

>***Hosting the BA Festival 2-9 September 2006

--

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</x-flowed>

From: Fortunat Joos <joos@climate.unibe.ch>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: new fig 6.14
Date: Tue, 18 Jul 2006 16:20:16 +0200
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith,

Thanks.

My concerns comes from the following. I am not convinced that one gets the same response when forcing a model with smoothed volcanic forcing instead with the spikes. I suspect that the ocean will gain more heat in the later case due to the longer time to respond to the forcing.

However, this remains to be tested, but nobody has done this as far as I know. In other words, postprocessing the output of a model forced with high resolution data does not necessarily give the same results as forcing the model with smoothed input. There is a chance to get different results. That is why I prefer to show the real forcing, i.e. the volcanic spikes. As long as nobody has done such tests run I would prefer to be scientifically on the save side with the figure. Sorry, but this is my modellers view on this.

Forcings do not need to be on the same scale here. We know that temporarily volcanic forcing, albeit negative, is much larger than anthropogenic forcing. Why should we hide this well-know fact? Sceptics my call on this. Readers of our chapter are hopefully able to interpret the y-axis.

The TS-team (in this case neither me nor Peck) asked us to show the volcanic spikes.

A point of the figure is to show the implication of low solar forcing (WLS versus Bard) that is why I prefer to blow the solar panel somewhat up. We have varied solar forcing between the different runs. Of course the point about the natural forcing only simulation not able to get the 20th century warming is very important. Indeed, I believe that this important conclusion is underscored if we make it very clear that we have varied solar forcing over a wide range (by a factor of 3).

It would also be nice to show the 11-yr solar cycle that is in the data (sun spots, but also 14C).

As far as normalisation of the forcing is concerned. I have no strong opinion. There is a consistency issue with chapter 2 where radiative forcing is always defined relative to 1750 (1750==0). This point may especially be important for the TS. There is also the issue about agreement over recent decades. This is why I slightly prefer to normalize the forcing to be zero around 1750.

The sulfur figure will show volcanic spikes. We have agreed in Bergen that we add a sentence to the caption to point out that sulfate deposition may strongly vary regionally.

I think we have with fig 13 and 14 now the opportunity to convey to the readers the same information in two different ways. Perhaps, we should not miss this opportunity. In any case, we will find a solution and then go forward.

Cheers, Fortunat

Keith Briffa wrote:

> Fortunat et al

> My opinions were consistent with Tim's expression - we discussed his
> response. The importance of consistency between different modelling
> Figures (time response of filters and in the absolute magnitude of
> forcing scale) are the most important aspects. To start showing
> apparently different volcanic spikes (in the sulphate and EMIC Figure)
> will lead to confusion also. Ultimately we should remember that the
> point of this Figure is to show that you can not get simulated
> temperatures to match observations without anthropogenic forcing - not
> to show proportional responses to different solar or volcanic events.

> cheers

> Keith

>

> At 13:45 18/07/2006, Fortunat Joos wrote:

>

>> Dear Tim,

>>

>> Sorry, that was a very careless and a totally inappropriate choice of
>> words. I seriously apologize. Of course smoothing is not dishonest (I
>> do it also all the time). To the contrary, I very much appreciate all
>> your hard work to do these figures. I know that it is very time
>> consuming from own experience ... (that is perhaps why I did not
>> reflect on my wording when writing the e-mail). What I wanted to say
>> is that if one has the opportunity to show directly what forcing was
>> used by the model than I very much prefer to do so. I hope there
>> remains no misunderstanding. I realize now that I should have used
>> more modest wording at various places.

>>

>> Let us see what Eystein, Peck and Keith are thinking about it.

>>

>> With best wishes, Fortunat

>>

>> Tim Osborn wrote:

>>

>>> Hi all,

>>> thanks for the responses, Peck and Fortunat.

>>> I drafted the new figure 6.14 following as closely as possible the
>>> approach used for the original forcing/simulation figure (now 6.13).
>>> This is why I smoothed all series and used a common anomalisation

>>> period for all curves across all panels. It can greatly help to
>>> interpret why the simulated temperature responds in the way it does,
>>> because the zero (or "normal" level) is comparable across plots and
>>> because the strengths of different forcings can be compared *on the
>>> same timescale* as the simulated temperatures are shown. And, for
>>> 6.13, with so many different forcings and models shown, it would have
>>> been impossible to use unsmoothed series without making the
>>> individual curves indistinguishable (or indeed fitting them into such
>>> a compact figure).

>>> Now that the EMIC panels are separate from the original 6.13, we do
>>> have the opportunity to make different presentational choices. But I
>>> think, nevertheless, that some of the reasons for (i) proportional
>>> scaling, (ii) common anomalisation period; and (iii) smoothing to
>>> achieve presentation on comparable time scales, that held for 6.13
>>> probably also hold in 6.14.

>>> However, I also appreciate the points raised by Fortunat,
>>> specifically that (i) it is nice to be able to compare the magnitude
>>> of the 11-yr solar cycles with the magnitude of the low-frequency
>>> solar variations; and (ii) that using a modern reference period
>>> removes the interpretation that we don't even know the forcing today.
>>> So we have various advantages and disadvantages of different
>>> presentational choices, and no set of choices will satisfy all these
>>> competing demands.

>>> One thing that I am particularly perturbed about is Fortunat's
>>> implication that to show smoothed forcings would be scientifically
>>> dishonest. I disagree (and I was also upset by your choice of
>>> wording). If it were dishonest to show smoothed data, then
>>> presumably the same holds for 6.13 (but its impossible to distinguish
>>> all the different volcanic forcings if shown unsmoothed), but also to
>>> every other graphic... should I be showing the EMIC simulated
>>> temperatures without smoothing too, so you can see the individual
>>> yearly responses to the volcanic spikes? But annual means are formed
>>> from the temperatures simulated on the model timesteps, so we still
>>> wouldn't be showing results that had not been post-processed. Most
>>> climate models, even GCMs, respond in a quasi-linear way, such that
>>> the smoothed response to unsmooth forcing is very similar to the
>>> response to smooth forcing. So if we are interested in the
>>> temperature response on time scales of 30 years and longer, it seems
>>> entirely appropriate (and better for interpretation/comparison of
>>> forcings) to show the forcings on this time scale too, because the
>>> forcing variations on those time scales are the ones that are driving
>>> the temperature response (even though the forcing may be intermittent
>>> like volcanoes or have 11-yr cycles like solar).

>>> The choice of smoothing / no smoothing is not, therefore, anything to
>>> do with honesty/dishonesty, but is purely a presentational choice
>>> that can made accordingly to what the purpose of the figure is. Here
>>> our purpose seems to be long-term climate changes, rather than
>>> response to individual volcanoes or to the 11-yr solar cycle.

>>> So the position is:

>>> (1) smoothing or no smoothing: there are arguments for both choices,
>>> though clearly I prefer smoothing and Fortunat prefers no smoothing.
>>> I could make a figure which kept the smooth lines but put the raw

>>> annual histogram volcanic spikes underneath in pale grey, as Peck
>>> requested anyway (and possibly put the 11-yr solar cycles in pale
>>> brown underneath the smoothed brown solar series). This would be a
>>> compromise but the main problem is that the scale of the largest
>>> volcanic spikes would far exceed the scale I am using to show the
>>> smoothed series (so the panel is not large enough to do this)!

>>> (2) pre-industrial or present-day anomalisation reference period:
>>> again there are arguments for both choices. Whatever we choose, I
>>> firmly believe it should be the same for **all** curves in this figure
>>> (which can make a dramatic difference).

>>> (3) exaggeration of solar scale or proportional vertical scales: this
>>> is the one that I have the firmest opinion about. I see no reason to
>>> exaggerate the scale of the solar forcings relative to volcanic or
>>> anthropogenic forcings. The difference between the forcings looks
>>> clear enough in the version of the figure that I made. Exaggerating
>>> it will wrongly make the Bard 2.5% case look (at first glance) bigger
>>> than the anthropogenic forcing, and make it look more important than
>>> volcanic forcing.

>>> I'll hold off from making any more versions till decisions are made
>>> on these issues.

>>> Cheers

>>> Tim

>>> At 09:01 18/07/2006, Fortunat Joos wrote:

>>>

>>>> Hi Tim and co,

>>>>

>>>> Thanks for the figure. I like the figure showing the model results
>>>> and the general outline/graphic style.

>>>>

>>>> However, I am concerned about what is shown in the forcing figure.

>>>>

>>>> 1) Volcanic panel: I strongly believe that we should show what was
>>>> used by the model and not some 40 year smoothed curves for volcanic
>>>> forcing or any other forcing. So please use the original data file.
>>>> Scientific honesty demands to show what was used and not something
>>>> post-processed.

>>>>

>>>> 2) solar panel:

>>>> 2a) We must show the Wang-Lean-Shirley data on the original
>>>> resolution as used to drive the models. In this way, we also
>>>> illustrate the magnitude of the 11-yr annual cycle in comparison
>>>> with the background trend. The record being flat, apart from the
>>>> 11-yr cycle, during the last decades is a reality.

>>>> 2b) Do not apply any smoothing to the Bard data. Just use them as
>>>> they are and how they were published by Bard and used in the model.

>>>> 2c) It is fine to suppress the Bard 0.08 case after 1610 (not done in
>>>> my figure version)

>>>> 2d) the emphasis of the figure is on the solar forcing differences.

>>>> So, please show solar somewhat overproportional in comparison to
>>>> volcanic and other forcings.

>>>>

>>>> 3) other forcings: again no smoothing needed here. It would be hard

>>>> to defend a double smoothing.

>>>>
>>>> 4)- normalisation of solar forcing to some period mean. If the
>>>> different solar forcings disagree for today as in your option, we
>>>> may send the signal that we do not even know solar forcing today.
>>>> Thus, I slightly prefer to have the same mean forcing values for all
>>>> solar records during the last few decades as shown in the attached
>>>> version. However, I also can see some arguments for other
>>>> normalisations.

>>>>
>>>> To illustrate points 1 to 4, I have prepared and attached a version
>>>> of the forcing panel.

>>>>
>>>> other points

>>>>
>>>> - Your choice of colors is fine
>>>> - time range 1000-2000 AD is fine
>>>> - suggest to remove the text from the y-labels except the units W/m².

>>>>
>>>> Sorry for this additional comments coming a bit late. However, I did
>>>> not realise that you planned to smoothed the model input data in any
>>>> way.

>>>>
>>>> With best wishes,

>>>>
>>>> Fortunat

>>>>
>>>> Tim Osborn wrote:

>>>>
>>>>> Hi Peck, Eystein and Fortunat,
>>>>> I've drafted two versions of the new fig 6.14, comprising a new
>>>>> panel showing the forcing used in the EMIC runs, plus the old fig
>>>>> 6.13e panel showing the EMIC simulated NH temperatures. Keith has
>>>>> seen them already.

>>>>> First you should know what I did, so that you (especially Fortunat)
>>>>> can check that what I did was appropriate:

>>>>> (1) For the volcanic forcing, I simply took the volcanic RF forcing
>>>>> from Fortunat's file and applied the 30-year smoothing before
>>>>> plotting it.

>>>>> (2) For the solar forcing there are 2 curves. For the first, I
>>>>> took the Bard 0.25% column from Fortunat's RF file. For the
>>>>> second, I took the Bard 0.08% column from Fortunat's RF file from
>>>>> 1001 to 1609, and then appended the WLS RF forcing from 1610 to
>>>>> 1998. Then I smoothed the combined record. NOTE that for the
>>>>> Bard0.25%, the line is flat from 1961 onwards which probably isn't
>>>>> realistic, even though that is what was used in the model runs.

>>>>> (3) For the "all other forcings" there are 2 curves. For the
>>>>> first, I took the CO₂ concentrations provided by Fortunat, then
>>>>> used the "standard" IPCC formula from the TAR (in fact the first of
>>>>> the three options for CO₂ in IPCC TAR Table 6.2) to convert this to
>>>>> a radiative forcing. I then added this to the non-CO₂ radiative
>>>>> forcings data from Fortunat's file, to get the total radiative

>>>>> forcing. For the second, I replaced all values after 1765 with the
>>>>> 1765 value (for the natural forcings case). Then I smoothed the
>>>>> combined record (as in fig 6.13c, I only applied a 10-year
>>>>> smoothing when plotting the "all other forcings", because it is
>>>>> fairly smooth anyway and using a high smoothing results in lower
>>>>> final values when there is a strong trend at the end of a time
>>>>> series).

>>>>> Now, some comments on the figures themselves (please print them and
>>>>> refer to them when reading this):

>>>>> (1) File 'chap6_f6.14_option1.pdf' is strongly preferred by Keith
>>>>> and me. This shows the three forcing components separately, which
>>>>> helps with understanding the individual causes of specific warming
>>>>> and cooling periods. I have managed to reduce the size of this
>>>>> considerably, compared to the equivalent panel in fig 6.13, because
>>>>> with only a few series on it I could squeeze them together more and
>>>>> also reduce the range of the vertical axes.

>>>>> (2) Although we don't prefer it, I have also made
>>>>> 'chap6_f6.14_option2.pdf' which is even smaller by only showing the
>>>>> sum of all the forcings in the top panel.

>>>>> Which version do you prefer? Please let me know so I can make
>>>>> final changes only to the preferred version.

>>>>> Some more comments:

>>>>> (1) Fig 6.14b was originally Fig 6.13e. When it was part of that
>>>>> figure, the colour bar showing the shades of grey used to depict
>>>>> the overlapping ranges of the published temperature reconstructions
>>>>> was only on Fig 6.13d. Do you think I should now also add it to
>>>>> the EMIC panel (6.14b), now that it is in a separate figure? It
>>>>> will be a bit of a squeeze because of the legend that is already in
>>>>> 6.14b.

>>>>> (2) Another carry over from when 6.14b was part of 6.13, is that
>>>>> the time range of all panels had to match (900-2010). Now that the
>>>>> EMICs are in a separate figure, I could start them in year 1000,
>>>>> which is when the forcing and simulations begin. Unless you want
>>>>> 6.13 and 6.14 to remain comparable? Again please comment/decide.

>>>>> (3) I wasn't sure what colours to use for the forcing series. In
>>>>> option 1, the volcanic and other forcings apply to all runs, so I
>>>>> chose black (with thick/thin used to distinguish the "all" forcings
>>>>> from the "natural-only" forcings (basically the thin flat line in
>>>>> "all other forcings). The cyan-green-blue runs used strong solar
>>>>> forcing, so I used blue for that forcing. The red-orange-brown
>>>>> runs used weak solar forcing, so I used brown for that forcing.

>>>>> Sound ok?

>>>>> Sorry for the long email, but I wanted to get everything explained
>>>>> to avoid too many iterations.

>>>>> Please let me know your decisions/comments on these questions, or
>>>>> on any other aspects of the new figure.

>>>>> Cheers

>>>>> Tim

>>>

>>>

>>>

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Henry Pollack <hpollack@umich.edu>

Subject: Re: Huang, et al GRL 24, 1997

Date: Tue, 18 Jul 2006 21:50:19 -0600

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Valerie Masson-Delmotte <Valerie.Masson@cea.fr>, t.osborn@uea.ac.uk, Keith Briffa <k.briffa@uea.ac.uk>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>

Hi Henry - excellent feedback, thanks. I think it should be easy for Valerie (Holocene issues in 6.5) and Keith/Tim.Ricardo (last 2k, section 6.6) to deal with the 'expert' review issues regarding this paper. It sounds to me like that is the place for discussion of this paper, rather than in the text itself. BUT, it is important that the responses to review comments be thorough and convincing - Valerie and Keith - please update your responses in this respect.

thanks all, Peck

>Hi Peck and others,

>

>Attached is a brief discussion of the subject
>paper and the questions you have asked me to
>address. Let me know if you need additional
>clarification.

>

>Cheers,

>Henry

>

> ____ Henry N. Pollack

>[\ /] Professor of Geophysics

> | v | Department of Geological Sciences

> |MICHIGAN| University of Michigan

>[]/[] Ann Arbor, Michigan 48109-1005, U.S.A.

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>

>

>Quoting Jonathan Overpeck <jto@u.arizona.edu>:

>

>>Hi again Henry - I've attached an 1997 paper of

>>your's and wonder if you could shed some

>>up-to-date insights on how to best interpret.

>>In particular:

>>

>>1) it has been pointed out to us that the

>>result in this paper argue for a globally warm

>>period during the middle Holocene that was
>>warmer than today. Our assessment (i.e., Figure
>>6.9) indicates that there was likely no period
>>during the Holocene that was warmer around the
>>global than the late 20th century. Especially
>>outside of the tropics, there were periods
>>warmer than today during the Holocene, but
>>these regionally warm periods were not
>>synchronous - at least at the centennial scale
>>we can examine with proxy data. Thus, although
>>Huang et al. 1997, indicates greater mean
>>annual global warmth, it was unlike the
>>synchronous global warming of the late 20th
>>century.

>>
>>Plus, we believe the warmth of the Holocene was
>>driven by orbital forcing, and that what we see
>>makes sense in that regard. Huang et al, 1997
>>can be explained perhaps (this is a question)
>>by the heavy borehole coverage in the Northern
>>mid- to high-latitudes? We also know that proxy
>>data shown in Fig 6.9 also indicate more
>>warming (again, not synchronous) in Southern
>>Hem mid-latitudes - where there are also many
>>boreholes.

>>
>>Obviously, another issue is that the boreholes
>>don't give the same temporal resolution as the
>>other proxy records we synthesized/assessed,
>>and at least in your paper, there isn't
>>regional information either.

>>
>>So - the point is not (unless you suggest
>>otherwise) that Huang et al 97 is wrong, but
>>rather than within the limits of the data, it
>>is compatible with what the higher-resolution,
>>regionally-specific, multi-proxy data are
>>showing in Fig 6.9, and that there was likely
>>no period during the Holocene that was warmer
>>synchronously around the global than the during
>>the late 20th century. Do you agree with this,
>>and is our reasoning accurate and complete?

>>
>>2) Huang et al 1997 also shows evidence for
>>warmth within the last 500-1000 years that was
>>greater than during the 20th century AND a cool
>>minima 200 years ago. Both of these are
>>highlighted in your abstract, and both seem
>>incompatible with other evidence. For example,
>>your own more recent work has shown the coolest
>>temperatures to be about 500 years ago.

>>
>>We didn't think it was within our focus to

>>comment on these issues, but we are being asked
>>to by reviewers, and it would be good to have
>>your help in addressing these issues -
>>hopefully in our responses to review comments
>>rather than in our main text (which has to be
>>shortened).

>>

>>Many thanks for your help with this paper and the issues it raises.

>>

>>Best, Peck

>>

>>

>>

>>--

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>>Professor, Department of Geosciences

>>Professor, Department of Atmospheric Sciences

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>

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>

>Attachment converted: Macintosh HD:GRL 1997.doc (WDBN/«IC») (00141CBF)

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: cddhr@giss.nasa.gov
Subject: Fwd: Re: Gavin Smchmidt'comment
Date: Wed, 19 Jul 2006 09:06:29 -0600
Cc: joos <joos@climate.unibe.ch>, Ricardo Villalba
<ricardo@lab.cricyt.edu.ar>, Eystein Jansen <eystein.jansen@geo.uib.no>,
cddhr@giss.nasa.gov, Keith Briffa <k.briffa@uea.ac.uk>,
t.osborn@uea.ac.uk

<x-flowed>

David - can you comment, help? thx, Peck

>X-Sieve: CMU Sieve 2.2
>X-Virus-checked: by University of Berne
>Date: Wed, 19 Jul 2006 16:51:05 +0200
>From: Fortunat Joos <joos@climate.unibe.ch>
>Organization: University of Bern
>X-Accept-Language: en-us, en
>To: Jonathan Overpeck <jto@u.arizona.edu>
>Cc: Ricardo Villalba <ricardo@lab.cricyt.edu.ar>,
> Eystein Jansen <eystein.jansen@geo.uib.no>, cddhr@giss.nasa.gov,
> Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk
>Subject: Re: Gavin Smchmidt'comment

>

>

>

>Jonathan Overpeck wrote:

>>Hi Fortunat - Glad you're on this, and thanks for helping us get it
>>right. I agree we need assurance from Chap 2 (David, can you make
>>sure we've got it) that the deleted issues are, indeed, covered in
>>Chap 2.

>

>In particular, I am not sure that chap 2 covers the Solanki et al. issue

>

>>

>>thanks again, Peck

>>

>>>Hi,

>>>

>>>What we agreed was actually to keep line 25 to line 34 on p 6-35
>>>and not just until line 30. (As well line 50, p-36 line 2-7).

>>>

>>>The sentence on line 32/33 that there is general agreement in the
>>>evolution of the different proxies is important as there is in
>>>general much confusion about this and this is a chapter 6
>>>statement covering the whole millennium. The sentence also links
>>>nicey to the next sentence on line 50. Yes, as agreed in Bergen
>>>delete the other parts if chapter 2 indeed is going to cover it. I
>>>have not done so in my revision as I wanted to hear what chap 2 is
>>>doing before deleting.

>>>

>>>Peck, in total we will delete 22 line. Note that I have also
>>>squeezed out a few line in the sulfur section. Making progress!

>>>

>>>Regards, Fortunat
>>>
>>>David Rind wrote:
>>>
>>>>Jonathan,
>>>>
>>>>
>>>>Keith and I discussed this at the meeting; basically what we need
>>>>to keep is:
>>>>
>>>>P. 6-25, lines 25-30, first sentence on line 50, and P. 6-26 the
>>>>first paragraph (lines 2-7).
>>>>
>>>>
>>>>All the rest is discussed in one form or another in Chapter 2, pp.
55-56.
>>>>
>>>>Concerning the volcanic forcing, there isn't nearly as much
>>>>overlap, and Chapter 6 did not have very much anyway - I think it
>>>>would be useful to keep what's there, adding just a reference to
>>>>Chapter 2 (add: "see also Chapter 2", at the end of line 26).
>>>>(I'm assuming that Fig. 6-13a still includes the solar and
>>>>volcanic forcing).
>>>>
>>>>David
>>>>
>>>>
>>>>At 11:40 AM -0600 7/18/06, Jonathan Overpeck wrote:
>>>>
>>>>>Hi David - it's good to know you can get to work before someone,
>>>>>even if they live in Europe.
>>>>>
>>>>>Your plan sounds good, and is it safe to assume that you will be
>>>>>making sure Chap 2 gets the right material from chap 6, and that
>>>>>we can thus pare our discussion of past solar and volcanic
>>>>>forcing down to a minimum? Can you give us an update of what
>>>>>they will not cover that we should (i.e., looking at section
>>>>>6.6)?
>>>>>
>>>>>Many thanks, Peck
>>>>>
>>>>>>Hi All,
>>>>>>
>>>>>>>[It's a sad state of affairs if I'm the one who gets to work
>>>>>>>sooner! (regardless of the time difference).]
>>>>>>>
>>>>>>>>What is discussed below is basically what we thought in
>>>>>>>>response to Gavin's comment - that we would basically
>>>>>>>>cross-reference chap 2, where the primary discussion would
>>>>>>>>occur. It's consistent with chapter 2's general discussion of
>>>>>>>>how forcings have changed over time, and would seem odd if
>>>>>>>>chapter 2 left out past solar and volcanic forcing. Chapter 2
>>>>>>>>should feel free to utilize anything that existed in Chapter 6
>>>>>>>>on these issues to complement their discussion, if the need

>>>>>arises. Once that is finalized, Chapter 6 can then make the
>>>>>proper cross-references.
>>>>>
>>>>>David
>>>>>
>>>>>
>>>>>At 10:26 AM -0600 7/18/06, Jonathan Overpeck wrote:
>>>>>
>>>>>>Hi Ricardo - good points. We did discuss this in Bergen, and
>>>>>>David Rind (as a Chap 2 CA) was going to help make sure we
>>>>>>kept things covered in chap 2, while cutting our solar and
>>>>>>volcanic discussions in chap 6. The key will be
>>>>>>cross-referencing chap 2 carefully. So, Keith, Ricardo and
>>>>>>David - please interact to figure out how to work this
>>>>>>efficiently. Perhaps David could comment first since he's at
>>>>>>work sooner.
>>>>>>
>>>>>>Thanks... Best, Peck
>>>>>>
>>>>>>>Hi all!
>>>>>>>
>>>>>>>In comment 6-811, Gavin Schmidt points out that our sections
>>>>>>>
>>>>>>>6.6.3.1 Solar forcing
>>>>>>>
>>>>>>>6.6.3.2 Volcanic forcing
>>>>>>>
>>>>>>>largely replicate the discussion in Chap. 2 on the same
>>>>>>>topics. I checked
>>>>>>>Chap. 2, and they provide a large (almost 8 pages in the SOD)
>>>>>>>discussion
>>>>>>>mainly on solar and but also on volcanic forcings. Gavin
>>>>>>>suggests that only
>>>>>>>the implementation issues should be discussed in our chapter
>>>>>>>and leave the
>>>>>>>most general information in Chapter 2. We can substantially short
our
>>>>>>>section following his advice. Please, find below the outline of
the
>>>>>>>sections in Chap. 2 dealing with solar and volcanic forcings.
Cheers,
>>>>>>>
>>>>>>>Ricardo
>>>>>>>
>>>>>>>
>>>>>>>
>>>>>>>2.7 Natural Forcings
>>>>>>>
>>>>>>>
>>>>>>>
>>>>>>>2.7.1 Solar Variability
>>>>>>>
>>>>>>>2.7.1.1 Direct observations of solar irradiance
>>>>>>>

>>>>>>2.7.1.1.1 Satellite measurements of total solar irradiance
>>>>>>
>>>>>>2.7.1.1.2 Observed decadal trends and variability
>>>>>>
>>>>>>2.7.1.1.3 Measurements of solar spectral irradiance
>>>>>>
>>>>>>2.7.1.2 Estimating past solar radiative forcing
>>>>>>
>>>>>>2.7.1.2.1 Reconstructions of past variations in solar irradiance
>>>>>>
>>>>>>2.7.1.2.2 Implications for solar radiative forcing
>>>>>>
>>>>>>2.7.1.3 Indirect effects of solar variability
>>>>>>
>>>>>>
>>>>>>2.7.2 Explosive Volcanic Activity
>>>>>>
>>>>>>2.7.2.1 Radiative effects of volcanic aerosols
>>>>>>
>>>>>>2.7.2.2 Thermal, dynamic and chemistry perturbations forced by
volcanic
>>>>>>aerosols
>>>>>>
>>>>>>
>>>>>>
>>>>>>
>>>>>>
>>>>>>----- Original Message -----
>>>>>>From: "Tim Osborn" <t.osborn@uea.ac.uk>
>>>>>>To: "Jonathan Overpeck" <jto@u.arizona.edu>; "Keith Briffa"
>>>>>><k.briffa@uea.ac.uk>
>>>>>>Cc: "Eystein Jansen" <eystein.jansen@geo.uib.no>; "Ricardo
Villalba"
>>>>>><ricardo@lab.cricyt.edu.ar>; "joos" <joos@climate.unibe.ch>
>>>>>>Sent: Monday, July 17, 2006 12:25 PM
>>>>>>Subject: Re: Special instructions/timing adjustment
>>>>>>
>>>>>>> Hi all,
>>>>>>>
>>>>>>> I'm halfway through these changes and will get the revised
figures
>>>>>>> out to you probably tomorrow, except maybe the SH one,
because:
>>>>>>>
>>>>>>> I'm not sure if the van Ommen (pers. comm.) data shown by
Jones &
>>>>>>> Mann and suggested by Riccardo are the data to use or not. Is
it
>>>>>>> published properly? I've seen the last 700 years of the Law
Dome 180
>>>>>>> record published, so perhaps we should show just the period
since
>>>>>>> 1300 AD? That period appears in:

>>>>>>>
>>>>>>> Mayewski PA, Maasch KA, White JWC, et al.
>>>>>>> A 700 year record of Southern Hemisphere extratropical
>>>>>>> climate variability
>>>>>>> ANNALS OF GLACIOLOGY 39: 127-132 2004
>>>>>>>
>>>>>>> and
>>>>>>>
>>>>>>> Goodwin ID, van Ommen TD, Curran MAJ, et al.
>>>>>>> Mid latitude winter climate variability in the South Indian
and
>>>>>>> southwest Pacific regions since 1300 AD
>>>>>>> CLIMATE DYNAMICS 22 (8): 783-794 JUL 2004
>>>>>>>
>>>>>>> See below for some more comments in respect to individual
figures.
>>>>>>>
>>>>>>> At 21:36 30/06/2006, Jonathan Overpeck wrote:
>>>>>>> >Figure 6.10.
>>>>>>> >1. shade the connection between the top and middle panels
>>>>>>>
>>>>>>> yes
>>>>>>>
>>>>>>> >2. remove the dotted (long instrumental) curve from the
middle panel
>>>>>>>
>>>>>>> yes
>>>>>>>
>>>>>>> >3. replace the red shaded region in the bottom panel with the
>>>>>>> >grey-scale one used in Fig 6.13
>>>>>>>
>>>>>>> yes
>>>>>>>
>>>>>>> >4. label only every increment of 10 in the grey-scale bar
(formally
>>>>>>> >color) in the bottom panel
>>>>>>>
>>>>>>> yes
>>>>>>>
>>>>>>> >5. Increase font sizes for axis numbering and axis labeling -
all
>>>>>>> >are too small. You can figure out the best size by reducing
figs to
>>>>>>> >likely page size minus margins. We guess the captions need to
be
>>>>>>> >bigger by a couple increments at least.
>>>>>>>
>>>>>>> yes
>>>>>>>
>>>>>>>
>>>>>>> >Figure 6.11.
>>>>>>> >
>>>>>>> >1. This one is in pretty good shape except that Ricardo has
to

>>>>>>>
>>>>>>>
>>>>>>> >1b. please also increase font sizes and make sure they match
6.10
>>>>>>> >and 12 - please use bold fonts.
>>>>>>>
>>>>>>> ok, as discussed above.
>>>>>>>
>>>>>>> >2. The existing 6.13e is going to become a new 6.14, with the
>>>>>>> >addition of a new forcings panel "a" on top of the existing
panel e
>>>>>>> >(which becomes 6.14b). To make this happen, Tim and Fortunat
have to
>>>>>>> >coordinate, as Tim has the forcing data (and knows what we
what) and
>>>>>>> >Tim has the existing figure. We suspect it will be easier for
>>>>>>> >Fortunat to give Tim data and layout advice, and for Tim to
make a
>>>>>>> >figure that matches the other figs he's doing. PLEASE NOTE
that this
>>>>>>> >fig can't be as large as the existing 6.13a-d, but needs to
be more
>>>>>>> >compact to permit its inclusion.
>>>>>>>
>>>>>>> done.
>>>>>>>
>>>>>>> Cheers
>>>>>>>
>>>>>>> Tim
>>>>>>>
>>>>>>>
>>>>>>> Dr Timothy J Osborn, Academic Fellow
>>>>>>> Climatic Research Unit
>>>>>>> School of Environmental Sciences, University of East Anglia
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>>>>>>> sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm
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>>>>>>> **Hosting the BA Festival 2-9 September 2006
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--
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From: Tim Osborn <t.osborn@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>
Subject: new figs 6.11 and 6.12
Date: Wed Jul 19 16:04:00 2006
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>, joos <joos@climate.unibe.ch>

Here's the new 6.11 and 6.12. Very few changes necessary.
At 21:36 30/06/2006, Jonathan Overpeck wrote:

Figure 6.11.

1. This one is in pretty good shape except that Ricardo has to determine if S. African boreholes need to be removed.

It turned out that these could stay. All I've done is to add some white latitude/longitude lines.

Figure 6.12

1. again, please delete S. African borehole if Ricardo indicates it's still not published.

Not necessary.

2. consider adding Law Dome temperature record - Ricardo is investigating, but perhaps Keith/Tim can help figure out if it's valid to include. Feel free to check with Valerie on this too, as she seems to know these data at least a little

We decided not to do this, but to discuss in the text instead.

3. also, please increase font sizes and make sure they match 6.10 - probably better to use bold fonts

Fonts are bigger. Decided to standardise on non-bold fonts for all these plots.

Cheers

Tim

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: Special instructions/timing adjustment
Date: Thu, 20 Jul 2006 10:33:20 -0600
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>

Hi Tim - Thanks. If you don't mind, let's see what the new grey in panel c, and also the 5-95% range on a. Also, another alternative to the grey and red could be some other color that is just less bright - perhaps blue?

Agree there is no reason to switch the reviewed panel c uncertainty approach. It argues a bit that we leave panel a as is too. I'm unsure what is best, so maybe see what Keith thinks too - and discuss more with Phil - he is right that most are trying to go with 5-95 where possible.

Thanks again.

>Hi again,

>

>I still have the red option built into the
>program, so can easily revert to it. Of course
>the grey has the advantage of consistency with
>the model and EMIC panels, which really must be
>grey so that all the coloured lines indicating
>the simulated temperatures will show up (red
>isn't really an option for the reconstruction
>shading in those figures). I'll see if I can
>make it clearer yet keep it in grey.

>

>On a different note, Phil Jones just popped in
>and said why are we using "+-2SE" shading in the
>top instrumental panel when it has apparently
>been decided to show the smaller 5-95% range (he
>says this is only 0.8225 times the +-2SE range)
>in all IPCC WG1 figures. Shall I change this?
>If I do, then the brown and orange curves will
>fall outside this narrower range more often than
>they fall outside the current wider SE range.

>

>The grey shading in panel (c) is also computed
>from the overlap of the +-1 SE and +-2 SE ranges
>of individual reconstructions, but I guess this
>can stay unchanged, rather than needing to be
>recalculated using the overlap of the ?-?% and
>5-95% ranges?

>

>Cheers

>

>Tim

>

>At 16:05 19/07/2006, Jonathan Overpeck wrote:

>>Hi Tim - thanks! Now I can see why you went
>>with the red rather than grey in the bottom
>>panel - it's hard to see. I'd like to float the
>>idea with everyone on the email that we
>>consider going back to red, or try something
>>else. All else is good (thanks) perhaps make
>>the bottom/top axis labels bigger still? (both
>>numbers and "Year").

>>

>>Thx again, Peck

>>

>>>Hi Peck et al.,

>>>

>>>revised fig 6.10 is attached.

>>>

>>>At 21:36 30/06/2006, Jonathan Overpeck wrote:

>>>>Figure 6.10.

>>>>

>>>>1. shade the connection between the top and middle panels

>>>>

>>>>It was already shaded. Your poor old eyes must be failing you ;-)

>>>>

>>>>Ok, so it *was* rather pale! I've made it a bit darker.

>>>>

>>>>>2. remove the dotted (long instrumental) curve from the middle panel

>>>>>

>>>>>Done

>>>>>

>>>>>3. replace the red shaded region in the
>>>>>bottom panel with the grey-scale one used in
>>>>>Fig 6.13

>>>>>

>>>>>Done - how does it look now? I had to outline
>>>>>the instrumental series with a narrow white
>>>>>band to ensure it could be seen against the
>>>>>very dark grey shading.

>>>>>

>>>>>4. label only every increment of 10 in the
>>>>>grey-scale bar (formally color) in the bottom
>>>>>panel

>>>>>

>>>>>Done

>>>>>

>>>>>5. Increase font sizes for axis numbering and
>>>>>axis labeling - all are too small. You can
>>>>>figure out the best size by reducing figs to
>>>>>likely page size minus margins. We guess the
>>>>>captions need to be bigger by a couple
>>>>>increments at least.

>>>>>

>>>>>Increased the axis numbering/labelling by a couple of points.

>>>>>

>>>>>Cheers

>>>

>>>Tim

>>>

>>>

>>>

>>>Attachment converted: Macintosh HD:chap6_f6.10.pdf (PDF /«IC») (00141E77)

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>**Hosting the BA Festival 2-9 September 2006

--

Jonathan T. Overpeck

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</x-flowed>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Fwd: Re: pseudo-proxies for the climate reconstruction challenge
Date: Thu Jul 20 15:33:31 2006
Cc: philip.brohan@metoffice.gov.uk

I should also say, Caspar, that I've not forwarded any documents to Philip yet with more details about the challenge. I thought that you should do that instead, because you will have (more likely) kept track of where the latest version is.

Cheers

Tim

Hi Caspar,

I forgot to forward to you Philip Brohan's positive response to my invitation for him to be involved in the production of pseudo-proxy and pseudo-instrumental data for the climate reconstruction challenge.

It is copied below and you can find his contact details below too.

Best wishes

Tim

From: philip.brohan@metoffice.gov.uk
Subject: Re: pseudo-proxies for the climate reconstruction challenge
To: Tim Osborn <t.osborn@uea.ac.uk>
Cc: simon.tett@metoffice.gov.uk, Keith Briffa <k.briffa@uea.ac.uk>
Date: Thu, 29 Jun 2006 11:08:54 +0100

Hi Tim.

Thanks for your notes from the workshop. It sounds both interesting and very positive - I was afraid that the relations between the participants would break down completely, but you've clearly made good progress.

I think a blind test of reconstruction methods is an excellent idea, and I'm happy to support it in any capacity. I've done this before with nuclear fuel performance models, and the results were both alarming and instructive. Doing it properly won't be easy though, I think several different stretches of model simulation will be required.

So yes - volunteer me to Caspar (or the organising committee) to make pseudo-proxy and pseudo-instrumental data.

Philip

On Fri, 2006-06-23 at 16:48, Tim Osborn wrote:

> Hi Philip (cc Simon & Keith),

>

> Please read my report-back from Wengen workshop first. You'll see

> that a "climate reconstruction challenge" was suggested and that this

> would be a "blind" test where participating groups would not know
> what the real answer is.
>
> Caspar Ammann would provide and keep secret a suitable model
> simulation. But we discussed who should make the pseudo-proxy data
> from the model output. I wondered whether you (Philip) would be
> interested in this, given your experience with the instrumental error
> model and interest in statistical models for proxy error. What do
> you think of this idea, Philip? A number of proxy people, including
> us, might liaise with you about how such an error model might be
> structured, but ultimately we would not be allowed to know precise
> details about how you generated a set of pseudo-proxies otherwise we
> wouldn't be allowed to take part in the challenge ourselves.
>
> Would you be interested in participating in this "challenge" in this
> way, and have time to do so? It would preclude you from entering the
> challenge of course.
>
> Please let me know and I will liaise with whoever else is involved in
> organising this challenge (at least Caspar, but it's not yet clear who else).
>
> Cheers
>
> Tim
>
--

Philip Brohan, Climate Scientist
Met Office Hadley Centre for Climate Prediction and Research
Tel: +44 (0)1392 884574 Fax: +44 (0)1392 885681
Global climate data sets are available from [1]<http://www.hadobs.org>

References

1. <http://www.hadobs.org/>

From: "Wahl, Eugene R" <wahl@alfred.edu>
To: "Keith Briffa" <k.briffa@uea.ac.uk>
Subject: RE: confidential
Date: Fri, 21 Jul 2006 04:23:24 -0400

Hi Keith:

I hope you are well in all this!!

I have done my best this evening to digest the issues you asked me to look at, and to give perspective on them. Here is what I can offer at this point.

1) Thoughts and perspective concerning the reviewer's comments per se. These are coded in blue and are in the "Notes" column between pages 103 and 122 inclusive. It got to the point that I could not be exhaustive, given the very lengthy set of review thoughts, so I am also attaching a review article Caspar and I plan to submit to Climatic Change in the next few days. [The idea is that this would accompany the Wahl-Ammann article, to summarize and amplify on it -- given all the proper and non-proper interpretation WA has received and the need for subsequent analysis that WA only lightly touches on. Steve Schneider is aware that it is coming.] I think a read through this, especially the part on PCs and Bristlecones, can say about all I might offer additionally. It is not lengthy.

Please note that this Ammann-Wahl text is sent strictly confidentially -- it should not be cited or mentioned in any form, and MUST not be transmitted without permission. However, I am more than happy to send it for your use, because it succinctly summarizes what we have found on all the issues that have come up re: MBH. As you can see, we agree at some level with some of the criticisms raised by MM and others, but we do not find that they invalidate MBH in any substantial way.

2) I have added a brief suggested alteration to page 6-3 of the draft text you sent, to take into account the fact Wahl-Ammann decidedly settles the issue concerning how proxy PC calculations impact the MBH style reconstruction. These changes are encoded using WORD's "Track Changes" feature.

I did not get into suggesting how that paragraph might otherwise be rewritten. You can see more generally where Caspar and I have gone in the attached text, and how our work relates generally to the MM, von Storch, etc. "examinations" of MBH. Thinking further, the "Validation Thresholds and Measures of Merit" and "Amplitude Issues" sections might also be well worth a look. The former will help you see how over-strong and one-sided are the arguments Steven McIntyre puts forth in this area. (Cf. Wahl-Ammann Appendix 1 also on this topic -- McIntyre strongly avoids, or simply chastizes as ad hoc, the false negative issues at lower frequencies that we raise concerning the use of r2.) He has done with the IPCC just what he did in reviewing the Wahl-Ammann paper--and indeed in all his efforts--write volumes of very strongly worded, one-sided

critiques, which can take a lot of time to see through and then respond to. I hope what we have written can help you in this way. I note that Mike Mann, Richard Alley, and others have written response comments, which would be useful for getting perspective also.

Finally, note also that I corrected the reference to Wahl, Ritson, Ammann (Wahl et al., 2006) on page 6-6, and put the correct publication information in the reference section.

I hope this all helps. I would be happy to do my best to answer any further questions you might have.

All the best, and Peace, Gene
Dr. Eugene R. Wahl
Asst. Professor of Environmental Studies
Alfred University

607-871-2604
1 Saxon Drive
Alfred, NY 14802

From: Keith Briffa [mailto:k.briffa@uea.ac.uk]
Sent: Tue 7/18/2006 10:20 AM
To: Wahl, Eugene R
Subject: confidential

Gene

I am taking the liberty (confidentially) to send you a copy of the reviewers comments (please keep these to yourself) of the last IPCC draft chapter. I am concerned that I am not as objective as perhaps I should be and would appreciate your take on the comments from number 6-737 onwards, that relate to your reassessment of the Mann et al work. I have to consider whether the current text is fair or whether I should change things in the light of the sceptic comments. In practise this brief version has evolved and there is little scope for additional text, but I must put on record responses to these comments - any confidential help, opinions are appreciated. I have only days now to complete this revision and response.
note that the sub heading 6.6 the last 2000 years

is page 27 line35 on the original (commented) draft.
Cheers
Keith

--

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Attachment Converted: "c:\eudora\attach\AW_Editorial_July15.doc"

Attachment Converted:
"c:\eudora\attach\AR4SOR_BatchAB_Ch06_ERW_comments.doc"

Attachment Converted:
"c:\eudora\attach\Ch06_SOD_Text_TSU_FINAL_2000_12jul06_ERW_suggestions.doc"
c"

From: Keith Briffa <k.briffa@uea.ac.uk>

To: Fortunat Joos <joos@climate.unibe.ch>, David Rind <drind@giss.nasa.gov>

Subject: Re: Gavin Smchmidt'comment

Date: Fri Jul 21 07:54:29 2006

Cc: Jonathan Overpeck <jto@u.arizona.edu>, Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, Eystein Jansen <eystein.jansen@geo.uib.no>, cddhr@giss.nasa.gov, t.osborn@uea.ac.uk

I suggest only one of us - Fortunat - make these changes in his version , otherwise we are all going to do it slightly differently.

Keith

At 08:16 19/07/2006, Fortunat Joos wrote:

Hi,

What we agreed was actually to keep line 25 to line 34 on p 6-35 and not just until line 30. (As well line 50, p-36 line 2-7).

The sentence on line 32/33 that there is general agreement in the evolution of the different proxies is important as there is in general much confusion about this and this is a chapter 6 statement covering the whole millennium. The sentence also links nicely to the next sentence on line 50. Yes, as agreed in Bergen delete the other parts if chapter 2 indeed is going to cover it. I have not done so in my revision as I wanted to hear what chap 2 is doing before deleting.

Peck, in total we will delete 22 line. Note that I have also squeezed out a few line in the sulfur section. Making progress!

Regards, Fortunat

David Rind wrote:

Jonathan,

Keith and I discussed this at the meeting; basically what we need to keep is:

P. 6-25, lines 25-30, first sentence on line 50, and P. 6-26 the first paragraph (lines 2-7).

All the rest is discussed in one form or another in Chapter 2, pp. 55-56.

Concerning the volcanic forcing, there isn't nearly as much overlap, and Chapter 6 did not have very much anyway - I think it would be useful to keep what's there, adding just a reference to Chapter 2 (add: "see also

Chapter 2", at the end of line 26). (I'm assuming that Fig. 6-13a still includes the solar and volcanic forcing).

David

At 11:40 AM -0600 7/18/06, Jonathan Overpeck wrote:

Hi David - it's good to know you can get to work before someone, even if they live in Europe.

Your plan sounds good, and is it safe to assume that you will be making sure Chap 2 gets the right material from chap 6, and that we can thus pare our discussion of past solar and volcanic forcing down to a minimum? Can you give us an update of what they will not cover that we should (i.e., looking at section 6.6)?

Many thanks, Peck

Hi All,

[It's a sad state of affairs if I'm the one who gets to work sooner! (regardless of the time difference).]

What is discussed below is basically what we thought in response to Gavin's comment - that we would basically cross-reference chap 2, where the primary discussion would occur. It's consistent with chapter 2's general discussion of how forcings have changed over time, and would seem odd if chapter 2 left out past solar and volcanic forcing. Chapter 2 should feel free to utilize anything that existed in Chapter 6 on these issues to complement their discussion, if the need arises. Once that is finalized, Chapter 6 can then make the proper cross-references.

David

At 10:26 AM -0600 7/18/06, Jonathan Overpeck wrote:

Hi Ricardo - good points. We did discuss this in Bergen, and David Rind (as a Chap 2 CA) was going to help make sure we kept things covered in chap 2, while cutting our solar and volcanic discussions in chap 6. The key will be cross-referencing chap 2 carefully. So, Keith, Ricardo and David - please interact to figure out how to work this efficiently. Perhaps David could comment first since he's at work sooner. Thanks... Best, Peck

Hi all!

In comment 6-811, Gavin Schmidt points out that our sections

6.6.3.1 Solar forcing

6.6.3.2 Volcanic forcing

largely replicate the discussion in Chap. 2 on the same topics. I checked Chap. 2, and they provide a large (almost 8 pages in the SOD) discussion mainly on solar and but also on volcanic forcings. Gavin suggests that only the implementation issues should be discussed in our chapter and leave the most general information in Chapter 2. We can substantially short our section following his advice. Please, find below the outline of the sections in Chap. 2 dealing with solar and volcanic forcings. Cheers, Ricardo

2.7 Natural Forcings

2.7.1 Solar Variability

2.7.1.1 Direct observations of solar irradiance

2.7.1.1.1 Satellite measurements of total solar irradiance

2.7.1.1.2 Observed decadal trends and variability

2.7.1.1.3 Measurements of solar spectral irradiance

2.7.1.2 Estimating past solar radiative forcing

2.7.1.2.1 Reconstructions of past variations in solar irradiance

2.7.1.2.2 Implications for solar radiative forcing

2.7.1.3 Indirect effects of solar variability

2.7.2 Explosive Volcanic Activity

2.7.2.1 Radiative effects of volcanic aerosols

2.7.2.2 Thermal, dynamic and chemistry perturbations forced by volcanic aerosols

----- Original Message -----

From: "Tim Osborn" <t.osborn@uea.ac.uk>

To: "Jonathan Overpeck" <jto@u.arizona.edu>; "Keith Briffa" <k.briffa@uea.ac.uk>

Cc: "Eystein Jansen" <eystein.jansen@geo.uib.no>; "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>; "joos" <joos@climate.unibe.ch>

Sent: Monday, July 17, 2006 12:25 PM

Subject: Re: Special instructions/timing adjustment

Hi all,

I'm halfway through these changes and will get the revised figures out to you probably tomorrow, except maybe the SH one, because: I'm not sure if the van Ommen (pers. comm.) data shown by Jones & Mann and suggested by Riccardo are the data to use or not. Is it published properly? I've seen the last 700 years of the Law Dome 180 record published, so perhaps we should show just the period since 1300 AD? That period appears in:

Mayewski PA, Maasch KA, White JWC, et al.

A 700 year record of Southern Hemisphere extratropical climate variability
ANNALS OF GLACIOLOGY 39: 127-132 2004

and

Goodwin ID, van Ommen TD, Curran MAJ, et al.

Mid latitude winter climate variability in the South Indian and southwest Pacific regions since 1300 AD

CLIMATE DYNAMICS 22 (8): 783-794 JUL 2004

See below for some more comments in respect to individual figures.

At 21:36 30/06/2006, Jonathan Overpeck wrote:

>Figure 6.10.

>1. shade the connection between the top and middle panels

yes

>2. remove the dotted (long instrumental) curve from the middle panel

yes

>3. replace the red shaded region in the bottom panel with the

>grey-scale one used in Fig 6.13

yes

>4. label only every increment of 10 in the grey-scale bar (formally

>color) in the bottom panel

yes

>5. Increase font sizes for axis numbering and axis labeling - all

>are too small. You can figure out the best size by reducing figs to

>likely page size minus margins. We guess the captions need to be

>bigger by a couple increments at least.

yes

>Figure 6.11.

>

>1. This one is in pretty good shape except that Ricardo has to

>determine if S. African boreholes need to be removed.

I think Henry said they were published and could stay

>

>Figure 6.12

>

>1. again, please delete S. African borehole if Ricardo indicates

>it's still not published.

I think Henry said they could stay.

>2. consider adding Law Dome temperature record - Ricardo is

>investigating, but perhaps Keith/Tim can help figure out if it's

>valid to include. Feel free to check with Valerie on this too, as

>she seems to know these data at least a little

Already discussed above.

>3. also, please increase font sizes and make sure they match 6.10 -

>probably better to use bold fonts

You are right that I've mixed bold and non-bold. When reduced to

small size, the non-bold actually read more clearly than the bold, I

think, so I'll standardise on non-bold. It's not possible to

completely standardise on the size, because each figure I provide

might be scaled by different amounts. I don't know final figure

size, so will make a good guess. Should be ok.

>Figure 6.13

>

>1. we are going to split the existing 6.13 into two figure. The

>first is 100% Tim's fig., and is just an upgrade of the existing

>6.13 a-d, with the only changes being:

>1a. delete the old ECHO-G red dashed line curve in panel d, and

Keith says this was discussed and rejected, so I should keep old ECHO-G

in?

>1b. please also increase font sizes and make sure they match 6.10

>and 12 - please use bold fonts.

ok, as discussed above.

>2. The existing 6.13e is going to become a new 6.14, with the

>addition of a new forcings panel "a" on top of the existing panel e

>(which becomes 6.14b). To make this happen, Tim and Fortunat have to

>coordinate, as Tim has the forcing data (and knows what we what) and

>Tim has the existing figure. We suspect it will be easier for

>Fortunat to give Tim data and layout advice, and for Tim to make a

>figure that matches the other figs he's doing. PLEASE NOTE that this

>fig can't be as large as the existing 6.13a-d, but needs to be more

>compact to permit its inclusion.

done.

Cheers

Tim
Dr Timothy J Osborn, Academic Fellow
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**Norwich -- City for Science:
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[8]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
3. <http://www.geo.arizona.edu/>
4. <http://www.ispe.arizona.edu/>
5. <http://www.geo.arizona.edu/>
6. <http://www.ispe.arizona.edu/>
7. <http://www.climate.unibe.ch/~joos/>
8. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>,
Subject: where I am up to now
Date: Fri Jul 21 18:23:42 2006
Cc: Fortunat Joos <joos@climate.unibe.ch>, drind@giss.nasa.gov

Need Fortunat to check the Gavin simplification (with David) and awaiting comments from Henry - though I have had a go at the relevant ones. Still needs the paragraph on tree rings and I have to incorporate Ricardo's bit. But this gives you a near overview of where we are - the inputting of the very many comment responses nearly there.

Keith

Is any body out there - any chance of call her in next half hour - or at home later 44 1953 8510 - Peck?

Peck and Eystein

OK I am still struggling . I will not be able to get stuff to you til tuesday I reckon - masses of typing and having to re-read and consult with others (Henry will get back to me early next week) on the borehole stuff. Discussing stuff with Eugene Wahl (confidentially) and still need to check corrections and balance text. Tim still working on Figures. We are doing best to get stuff back asap - but if I have to incorporate Ricardo's stuff and put into version by Fortunat , it is getting more complicated. Fortunat should do edits relating to the rationalising of the forcing text (as per Gavin comment - or has he already?) . Best if Oyvind puts the lot together then.

Keith

--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Fortunat Joos <joos@climate.unibe.ch>
Subject: Re: solar and Law Dome GHG reference
Date: Mon, 24 Jul 2006 13:14:57 -0600
Cc: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk, Eystein Jansen <eystein.jansen@geo.uib.no>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

<x-flowed>

Hi Fortunat and Keith - thanks for keeping close track of the volcanic and solar forcing aspects of 6.6, including coordination w/ Chap 2. The more you can do at this stage, Keith, the better (i.e., mystery changes), but there will be time to update re: chap 2 later.

Thanks again! Peck

>Hi,

>

>Three points:

>

>- Reference to MacFarling Meure already changed in my revision.

>

>- solar: It will probably not be a big deal to delete a few lines,
>when we have seen what chap 2 is doing.

>

>- Note that I am away for two weeks from July 29 to August 12, but I

>have time to work on remaining issues during the second half of

>August.

>

>With best wishes, Fortunat

>

>Jonathan Overpeck wrote:

>>Hi all - we probably have to cite this one, no? Thx, Peck

>>

>>>X-Sieve: CMU Sieve 2.2

>>>Date: Fri, 21 Jul 2006 11:07:59 -0600

>>>To: eystein.jansen@geo.uib.no, jto@u.arizona.edu

>>>From: Martin Manning <mmanning@al.noaa.gov>

>>>Subject: Fwd: Law Dome GHG reference

>>>Cc: Melinda Marquis <Marquis@ucar.edu>, ipcc-wg1@al.noaa.gov

>>>

>>>Hi Eystein, Peck

>>>

>>>The following from Dave Etheridge gives the citation for the

>>>published version of the MacFarling Meure et al paper. Not sure if

>>>you are switching to citing the GRL paper in preference to

>>>MacFarling Meure's thesis - but if you are here is the right

>>>reference.

>>>

>>>>Cheers
>>>>Martin
>>>>
>>>>DomainKey-Signature: s=email; d=csiro.au; c=noaws; q=dns;
>>>>b=QFtbAVZCd84qWm9oHqL5Q+VatZDVO/wqkH4eZVeBGcwDj6LT57x2oyOdHwNvJZy8jbW0qelqAUxaZvAcwNqCdAvbK9kTL2qq3KXA2S21EvnS2a+f7LIXMAZdllfm2vAa;
>>>>X-IronPort-AV: i="4.07,164,1151848800";
>>>> d="pdf?scan'208,217"; a="103465294:sNHT485096344"
>>>>Subject: Law Dome GHG reference
>>>>Date: Fri, 21 Jul 2006 11:57:05 +1000
>>>>X-MS-Has-Attach: yes
>>>>X-MS-TNEF-Correlator:
>>>>Thread-Topic: Law Dome GHG reference
>>>>Thread-Index: AcasaPcmdL+xIxSPRpytWeF8iOx2pg==
>>>>From: <David.Etheridge@csiro.au>
>>>>To: <mmanning@al.noaa.gov>, <d.lowe@niwa.co.nz>, <piers@env.leeds.ac.uk>
>>>>X-OriginalArrivalTime: 21 Jul 2006 01:57:05.0834 (UTC)
>>>>FILETIME=[F7AA30A0:01C6AC68]
>>>>X-Rcpt-To: <mmanning@aztec.al.noaa.gov>
>>>>X-DPOP: Version number suppressed
>>>>
>>>>Some of you were asking about this paper for IPCC AR4. It is now
>>>>published (today) in GRL. A pdf is attached.
>>>>
>>>>Regards
>>>>
>>>>David
>>>>
>>>>MacFarling Meure, C., Etheridge, D., Trudinger, C., Steele, P.,
>>>>Langenfelds, R., van Ommen, T., Smith, A. and Elkins, J. (2006).
>>>>The Law Dome CO2, CH4 and N2O Ice Core Records Extended to 2000
>>>>years BP. Geophysical Research Letters, Vol. 33, No. 14, L14810
>>>>10.1029/2006GL026152.
>>>><<http://www.agu.org/journals/gl/g10614/2006GL026152/2006GL026152.pdf>>
>>>><http://www.agu.org/journals/gl/g10614/2006GL026152/2006GL026152.pdf>
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>>>><<2000yr_CO2CH4N2O_MacFarlingMeure_GRL.pdf>>
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>>>>Aspendale, Victoria 3195, Australia
>>>>phone (61) 3 9239 4590 FAX (61) 3 9239 4444
>>>>email: david.etheridge@csiro.au
>>>>
>>>>website: <<http://www>><http://www.cmar.csiro.au/>
>>>>
>>>>--
>>>>Recommended Email address: mmanning@al.noaa.gov

>>>** Please note that problems may occur with my @noaa.gov address
>>>Dr Martin R Manning, Director, IPCC WG I Support Unit
>>>NOAA Aeronomy Laboratory Phone: +1 303 497 4479
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<http://www.ispe.arizona.edu/>
</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: MWP box figure

Date: Mon, 24 Jul 2006 13:33:01 -0600

Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi again Tim et al - looks good to me. Obviously, you and Keith need to nail the divergence issue in the text, and also refer to it in the caption for this fig, but otherwise, it's looking good.

Thanks, Peck

>Hi again,

>
>attached is the new MWP box figure.

>
>We reverted back to the figure used in the FOD
>because the decision to drop the panel from
>Osborn & Briffa (2006) meant that we were able
>to show a different selection of curves in the
>remaining panel from those we used in our paper.
>This allowed us to drop the shorter series that
>didn't span the medieval period, simplifying the
>figure and also dealing with a number of review
>comments that had been made about those series.

>
>The only differences from the FOD figure are
>that the font is now consistent with the others
>figures, the composite mean series has been
>removed, and the figure has been shrunk
>vertically to save space.

>
>Cheers

>
>Tim

>
>Attachment converted: Macintosh HD:chap6_box6.4_f1.pdf (PDF /«IC») (00143489)

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>

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>**Hosting the BA Festival 2-9 September 2006

--

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</x-flowed>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Special instructions/timing adjustment
Date: Mon Jul 24 15:58:18 2006
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, Fortunat Joos <joos@climate.unibe.ch>

Hi Peck et al.,

I've increased the axis labelling font size by another pt in all plots.

I attach two versions of 6.10, one in the grey (same as before except for bigger axis labelling) and one in brown. Brown looks like some old curry stain (or worse!). Note that conversion from postscript to PDF or GIF tends to alter the colours, which alter again on different printers compared with the screen. So there's not much point in me playing around much more with the colours.

Also attached are new versions of 6.13 and 6.14. Both have the bigger axis fonts and the matching grey shading as 6.10.

6.13 has thicker lines for all models, so they show up better on the new darker grey shading.

6.14 now has the EMIC forcing shown without any smoothing. I have used a vertical scale for the volcanoes which is half that of the solar and anthropogenic forcings. Only one spike (1258) hits the bottom of the plot with this choice of scaling, and only a couple overlap the solar forcing lines. What do you think now (Fortunat too)? Oh, and I also start now at 1000 rather than 900 AD.

Cheers

Tim

At 17:06 22/07/2006, Jonathan Overpeck wrote:

Hi Tim - this looks pretty nice, and I appreciate your chugging through to make the switch to 5-95%. I'd still be keen to see what the fig looks like in some more modest color than the old red. Grey could be the final choice, but it's not too much of a hassle, could you try a color version that is a bit more sharp?

Also, it would be nice to make the x-axis labels (numbers and "Year") as large as makes sense - they still seem too small.

Sorry to be nitpicky, but this figure is going to be a major one of the whole report, so it makes sense to get it as perfect as we can.

Thanks! best, peck

Hi Peck and Eystein,

what do you think of the attached new version of 6.10? Keith and I have spent some time examining various options and think that this one looks clearer (less smudgy) while still being a good representation of the data and in grey.

I spoke with Phil and Keith and the 5-95% range seems preferable for consistency with other chapters.

So:

(1) I now use 5-95% range in panel (a).

(2) Panel (b) has no further changes to it.

(3) Panel (c) is now also based on the overlap of the 5-95% ranges of the individual reconstructions, rather than on the ± 2 standard error ranges (extra weight is still given for temperatures that fall within the ± 1 SE range). I also applied some week smoothing prior to plotting. I also now plot using just 10 grey shades, in 10% steps, rather than the 20 shades in 5% steps that I used previously (in the last version, I changed the scale bar to have 10 steps of 10%, but I had still plotted the data using 20

steps of 5%).

Hope you like it,

Tim

At 17:33 20/07/2006, Jonathan Overpeck wrote:

Hi Tim - Thanks. If you don't mind, let's see what the new grey in panel c, and also the 5-95% range on a. Also, another alternative to the grey and red could be some other color that is just less bright - perhaps blue?

Agree there is no reason to switch the reviewed panel c uncertainty approach. It argues a bit that we leave panel a as is too. I'm unsure what is best, so maybe see what Keith thinks too - and discuss more with Phil - he is right that most are trying to go with 5-95 where possible.

Thanks again.

Hi again,

I still have the red option built into the program, so can easily revert to it. Of course the grey has the advantage of consistency with the model and EMIC panels, which really must be grey so that all the coloured lines indicating the simulated temperatures will show up (red isn't really an option for the reconstruction shading in those figures). I'll see if I can make it clearer yet keep it in grey.

On a different note, Phil Jones just popped in and said why are we using "+-2SE" shading in the top instrumental panel when it has apparently been decided to show the smaller 5-95% range (he says this is only 0.8225 times the +-2SE range) in all IPCC WG1 figures. Shall I change this? If I do, then the brown and orange curves will fall outside this narrower range more often than they fall outside the current wider SE range.

The grey shading in panel (c) is also computed from the overlap of the +-1 SE and +-2 SE ranges of individual reconstructions, but I guess this can stay unchanged, rather than needing to be recalculated using the overlap of the +-?% and 5-95% ranges?

Cheers

Tim

At 16:05 19/07/2006, Jonathan Overpeck wrote:

Hi Tim - thanks! Now I can see why you went with the red rather than grey in the bottom panel - it's hard to see. I'd like to float the idea with everyone on the email that we consider going back to red, or try something else. All else is good (thanks) perhaps make the bottom/top axis labels bigger still? (both numbers and "Year").

Thx again, Peck

Hi Peck et al.,

revised fig 6.10 is attached.

At 21:36 30/06/2006, Jonathan Overpeck wrote:

Figure 6.10.

1. shade the connection between the top and middle panels

It was already shaded. Your poor old eyes must be failing you ;-)

Ok, so it **was** rather pale! I've made it a bit darker.

2. remove the dotted (long instrumental) curve from the middle panel

Done

3. replace the red shaded region in the bottom panel with the grey-scale one used in Fig 6.13

Done - how does it look now? I had to outline the instrumental series with a narrow white band to ensure it could be seen against the very dark grey shading.

4. label only every increment of 10 in the grey-scale bar (formally color) in the bottom panel

Done

5. Increase font sizes for axis numbering and axis labeling - all are too small. You can figure out the best size by reducing figs to likely page size minus margins. We guess the captions need to be bigger by a couple increments at least.

Increased the axis numbering/labelling by a couple of points.

Cheers

Tim

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References

1. <http://www.cru.uea.ac.uk/~timo/>

2. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
3. <http://www.geo.arizona.edu/>
4. <http://www.ispe.arizona.edu/>
5. <http://www.cru.uea.ac.uk/~timo/>
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10. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
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12. <http://www.ispe.arizona.edu/>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>
Subject: MWP box figure
Date: Mon, 24 Jul 2006 16:20:56 +0100
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi again,

attached is the new MWP box figure.

We reverted back to the figure used in the FOD because the decision to drop the panel from Osborn & Briffa (2006) meant that we were able to show a different selection of curves in the remaining panel from those we used in our paper. This allowed us to drop the shorter series that didn't span the medieval period, simplifying the figure and also dealing with a number of review comments that had been made about those series.

The only differences from the FOD figure are that the font is now consistent with the others figures, the composite mean series has been removed, and the figure has been shrunk vertically to save space.

Cheers

Tim

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</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: latest me, fortunat, ricardo bit
Date: Tue Jul 25 18:27:29 2006
Cc: Fortunat Joos <joos@climate.unibe.ch>, <oyvind.paasche@bjerknes.uib.no>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>

Guys

here is what I understand you want from me - revised text (only up Table of Key etc) - ie not touched refs (understand Oyvind will put them in - most are given in text)

Tim sending Table and Figure captions separately. I am sending the text with my, Fortunat's and Ricardo's changes - with minor edits of mine added to them.

I undersatand that Oyvind will sort this ou and insert in final Chapter. I am also sendoing my reponses to data to most of my comments (Findicates that Fortunat has answered that one) . I will also send my edited version of Ricardo's reponses that I tweeked - ignore if wish) . I know I have not done all comments yet but the remaining ones can be done tomorrow I hope and any changes needed put on next draft. I do not expect many - and I was not clearwhether Peck wanted to respond to the regional (US) precip related ones anyway? I have added in the rather large paragraph on the tree-ring issues in response to several comments - I know you will scream at the size but I think we need to pu it in and then get Ricardo's

--

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References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: issue from Susan
Date: Fri, 28 Jul 2006 08:37:11 -0600
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith - in our TS/SPM discussions, Susan has raised this question:

"In the TAR they spoke of 1998 being the warmest year in the millennium and the 1990s the warmest decade. I don't see that chapter 6 addresses any of these time scales. I am not saying you should do so - but are you planning to say anything about it and why you aren't doing so? and if you're not planning to say anything at all, can you please tell me what you think about it, just for my own info?"

Would you please give me your feedback on this, with enough thoughtful detail to hopefully make me/Susan fully informed (a para should be enough).

Thanks, Peck

--

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From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: Valerie.Masson@cea.fr, Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Thompson et al, 2006 paper to include
Date: Mon, 31 Jul 2006 09:52:02 +0200
Cc: Olga Solomina <olgasolomina@yandex.ru>, Keith Briffa <k.briffa@uea.ac.uk>, Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, ValÈrie Masson-Delmotte <Valerie.Masson@cea.fr>, Oyvind.Paasche@bjerknes.uib.no

<x-flowed>

Hi Olga,

I agree with Valerie that the ice core evidence is ambiguous. I would personally place more weight on the alkenone data, which is a reasonable well calibrated SST proxy. Foraminifer transfer function based SSTs and some Mg/Ca results that are available suggest a similar picture as far as I know. Of course it is possible and plausible that the tropical oceans are behaving in a non consistent manner and not all areas are showing the same signal, but a sizeable portion appear to do so in order to conclude as we do in the chapter in my opinion. Some signals may be due to changes in in trade wind induced coastal upwelling strength, but there are enough cores with alkenone data outside of these areas. If we were to say more about the uncertainties it may be the fact that proxies are seasonally skewed.

My conclusion is to let the chapter say what we say at the moment.

Cheers,
Eystein

At 09:42 +0200 31-07-06, ValÈrie Masson-Delmotte wrote:

>Thanks Olga.

>

>It seems to me that there is still a large
>uncertainty about the temperature versus
>precipitation effect on these tropical glaciers.
>Other indications from south America are related
>to lake levels with contrasted views in the low
>versus highlands.
>Several references suggest that there is the end
>of a wet period after the early Holocene in
>tropical south America ; this is expected to
>induce an increase of 18O signals.
>One review was conducted several years ago
>within the PEPI project
>(http://www.paztcn.wr.usgs.gov/pcaw/ and
>references herein).
>I think that the state of the art is that we
>have no reliable proxy record that is sensitivite
>to temperature only on the tropical lands for
>the Holocene; therefore the statement that was

>written for the Holocene was based on areas of
>the tropical oceans where SST reconstructions
>were published.
>Do we have to write more explicitly about the uncertainty?
>
>Valérie.

>Jonathan Overpeck a écrit :
>>Hi Olga - it is not too late to ask these good
>>questions. Glaciers can, of course, be affected
>>by both temp and precip changes, so the
>>question is really for Valerie (land) and
>>Eystein (ocean) - are the land and ocean data
>>from the tropics strong enough to outweigh what
>>the glaciers are saying about tropical temps
>>earlier in the Holocene? Lonnie's Figure 8
>>(see attached) presents Hauscaran and
>>Kilimanjaro data that suggest early to mid
>>warmth in tropical South America and Africa
>>that is (if the O-isotopes are temp) greater
>>than today. Personally, I'm quite unsure that
>>these are reliable temperature records, BUT if
>>we want to make that case, we have to be
>>convincing. What do terrestrial and ocean temp
>>data say?

>>
>>Thanks Olga for sending the proposed revised
>>text - I think Eystein is putting finishing
>>touches on the next draft for LA etc. review.

>>Best, Peck

>>>>

>>>>Hello everybody,

>>>> I attach here a version of glacier box and
>>>>suggestions (in red) how to include there the
>>>>reference to the new Thompson et al., 2006
>>>>paper.

>>>> In this relation - I am getting more and more
>>>>concern about our statement that the Early
>>>>Holocene was cool in the tropics - this paper
>>>>shows that it was, actually, warm - ice core
>>>>evidences+glaciers were smaller than now in
>>>>the tropical Andes. The glaciers in the
>>>>Southern Hemisphere (Porter, 2000, review
>>>>paper) were also smaller than at least in the
>>>>Neoglacial. We do not cite Porter's paper for
>>>>the reason that we actually do not know how to
>>>>explain this - orbital reason does not work
>>>>for the SH, but if we do cite it (which is
>>>>fair) we have to say that during the Early to
>>>>Mid Holocene glaciers were smaller than later
>>>>in both Northern, and Southern Hemisphere,
>>>>including the tropics, which would contradict
>>>>to our statement in the Holocene chapter and
>>>>the bullet. It is probably too late to rise

>>>these questions, but still just to draw your
>>>attention.

>>> I am going to Kamchatka tomorrow, but will be
>>>available by e-mail from time to time.

>>> All the best,
>>>olga

>>>
>>> ----- Original Message -----

>>> *From:* Jonathan Overpeck <mailto:jto@u.arizona.edu>

>>> *To:* Olga Solomina <mailto:olgasolomina@yandex.ru>

>>> *Cc:* Eystein Jansen <mailto:eystein.jansen@geo.uib.no> ;
>>> oyvind.paasche@bjerknes.uib.no
>>> <mailto:oyvind.paasche@bjerknes.uib.no>

>>> *Sent:* Sunday, July 30, 2006 3:42 AM

>>> *Subject:* Re: [Wg1-ar4-ch06] Fwd: Additional In-Press Papers

>>>
>>> Hi Olga - I agree, and hope that you and òyvind make sure you
>>> include it in the next round of edits, which will begin very
>>> soon. We have all of the new text and Eystein is assembling for
>>> authors to check. This same new draft will be the one that
>>> Eystein and I work on to achieve more consistency and the proper
>>> length. Although we've cut some text already, some received has
>>> grown too. So... think about a way to include the reference to
>>> Lonnie's work without lengthening if you can.

>>> OK? Many thanks, Peck

>>>> Hi Peck,

>>> Lonnie's paper is a very good one and suitable for the
>>> glacier box. If it is still possible I would add this reference.

>>> olga

>>> ----- Original Message -----

>>> *From:* Jonathan Overpeck <mailto:jto@u.arizona.edu>

>>> *To:* wg1-ar4-ch06@joss.ucar.edu
>>> <mailto:wg1-ar4-ch06@joss.ucar.edu>

>>> *Sent:* Friday, July 28, 2006 6:32 PM

>>> *Subject:* [Wg1-ar4-ch06] Fwd: Additional In-Press Papers

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>>--
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From: Tim Osborn <t.osborn@uea.ac.uk>
To: Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen
<eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>, Øyvind
Paasche <oyvind.paasche@bjerknes.uib.no>
Subject: latest figures, captions and tables from Keith/Tim
Date: Mon Jul 31 14:31:24 2006

Dear all,

we have now updated the figure captions for our section and these are attached as a PDF together with the figures. Unfortunately I forgot to highlight the caption changes in blue... can you just completely replace the old captions with the new ones? We worked hard to make the captions as short as possible, while retaining their accuracy.

When updating the captions, we spotted minor inconsistencies in the labelling of figures 6.10 and 6.12 and so we have corrected these figures and new versions are incorporated in the attachment. The labelling change also affects table 6.1. I have attached the tables again too, with the new change highlighted in red (simply change 'CED2004' to 'ECS2002' in Table 6.1).

Hope this is all ok and now finalised.

If you want me to send the new figures as individual files (EPS format) then please say. Also if you want the new captions/figures as Word rather than PDF, please say (the Word file is large and very slow to open on my PC).

Cheers

Tim & Keith

From: Keith Briffa <k.briffa@uea.ac.uk>

To: Eystein Jansen <Eystein.Jansen@geo.uib.no>

Subject: Re: Urgent Re: latest version of my responses

Date: Tue Aug 1 15:48:34 2006

Cc: Tim Osborn <t.osborn@uea.ac.uk>, jto@u.arizona.edu, Fortunat Joos <joos@climate.unibe.ch>, Valerie.Masson@cea.fr, Ricardo Villalba <ricardo@lab.cricyt.edu.ar>

Dear all

attached is my latest (currently definitive) version of the responses to the "sky-blue-highlighted" comments on text and Figures.

PLEASE NOTE THAT THESE HAVE CHANGED IN VARIOUS PLACES FROM WHAT I SENT EARLIER AS WELL AS BEING UPDATED. I would suggest that they be cut and pasted into the document rather than just including the new ones. Sorry , but I had to reconsider a number of responses and edit others to remove typos etc.

Even though marked in blue - a few were not relevant to me. Two have been marked with "Valerie " - (6-1072, 6-1073) . Those marked PECK (6-862 through 6-868; ie 7 comments) are best dealt with by he. The comment 6-1110 is for Stefan. The comments marked F are those I sent from Fortunat before and I also sent the edited version of Ricardo's. The two outstanding ones he marked for me/Tim are here (6-818 and 6-819)

6-818 Noted - this issue will be reviewed , though the discussion of forcings must come before that of comparison of simulation results.

6-819 Noted - the text is intended to provide examples only and will be modified to refer to Table 6.2 , where details of all simulations used are provided.

I think that should be OK as far as my stuff goes. I will send minor changes to text (separate message) that have arisen in dealing with final comments.

Cheers

Keith

At 10:37 01/08/2006, Eystein Jansen wrote:

Hi Keith,

could you send me responses to the reviewers's comments received on the figures for 6.6?

The Batch i received only had responses for the comments to the main text. This relates to comment 1074 and onwards. Only quite few comments.

We need to send the comments responses file to the TSU by the week-end so this is urgent. Hope you have time..

Cheers,

Eystein

--

Eystein Jansen

Professor/Director

Bjerknes Centre for Climate Research and

Dep. of Earth Science, Univ. of Bergen

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: response to your question

Date: Tue, 1 Aug 2006 22:05:40 -0600

Cc: "Susan Solomon" <Susan.Solomon@noaa.gov>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Keith - thanks. This makes sense to me. I'll cc Susan so she understands the issue better, and also can advise on any strategy we should adopt to make sure we communicate effectively.

thanks again

best, peck

>Peck,

>

>The TAR was, in my opinion, wrong to say
>anything about the precedence (or lack thereof)
>of the warmth of the individual year 1998.

>

>The reason is that all reconstructions have very
>wide uncertainty ranges bracketing
>individual-year estimates of part temperature.
>Given this, it is hard to dismiss the
>possibility that individual years in the past
>did exceed the measured 1998 value. These errors
>on the individual years are so wide as to make
>any comparison with the 1998 measured value very
>problematic, especially when you consider that
>most reconstructions do not include it in their
>calibration range (curtailed predictor network
>in recent times) and the usual estimates of
>uncertainty calculated from calibration (or
>verification) residual variances would not
>provide a good estimate of the likely error
>associated with it even if data did exist.

>

>I suspect that many/most reconstructions of NH
>annual mean temperature have greater fidelity at
>decadal to multidecadal timescales (based on

>examination of the covariance spectrum of the
>actual and estimated data over the calibration
>period. This is the reason many studies
>implicitly (Hegerl et al.) or explicitly (Esper
>et al.;, Cook et al.) choose to calibrate
>directly against decadal-smoothed data.
>
>The exception is the Briffa et al (tree-ring
>density network based) reconstruction back to ~
>1400. This has probably the best year-to-year
>fidelity – but for summer land only and does not
>go back anyway to the MWP.
>
>We are on much safer grounds focusing on
>decadal/multi-decadal timescales and so this is
>where we place the emphasis. As for the ‘warmest
>decade’ – this is likely to be the 1990s or the
>last 10 years – but again, the proxies do not
>cover this period, and we do anyway state that
>post 1980 is the warmest period – which I think
>is fair enough.

>
>
>--
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From: Anders Moberg <anders.moberg@natgeo.su.se>
To: Martin Juckes <m.n.juckes@rl.ac.uk>
Subject: McIntyre, McKittrick & MITRIE ...
Date: Fri, 04 Aug 2006 09:18:24 +0100
Cc: Anders <anders@misu.su.se>, Eduardo.Zorita@gkss.de, hegerl@duke.edu, esper@wsl.ch, k.briffa@uea.ac.uk, m.allen1@physics.ox.ac.uk, weber@knmi.nl, t.osborn@uea.ac.uk

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Dear Martin and all others,

Having read the new manuscript, I would like to draw the attention of all of you to the section about McIntyre&McKittrick vs Mann et al. I am not entirely happy with this section. It may be that I am not fully updated about all details on their dispute, but it appears to be some mistakes in this section of our manuscript. Therefore, I ask all of you to check how this section can be improved and clarified. This is very important! If we refer incorrectly to the MM-Mann dispute, I am convinced that all of us will be involved in lengthy frustrating e-mail discussions later on. I anticipate this from personal experience! Let's do our best to avoid this.

The problematic bit of text starts on p. 16, para 4: ("The failure of MM2003 ... is partly due to a misunderstanding of the stepwise reconstruction method") and slightly below: ("MM2003 only calculate principal components for the period when all chronologies are present").

I read through the MM2003 paper yesterday. From what is written there, on p. 763-765, it appears that they were well aware of the stepwise method. On p. 763, about at the middle of the page, they write: "Following the description of MBH98 ... our construction is done piecewise for each of the periods listed in Table 8, using the roster of proxies available through the period and the selection of TPCs for each period listed in Table 8".

This is clearly at odds to what is written in our manuscript. Has it been documented somewhere else that MM2003, despite what they wrote, really misunderstood the stepwise technique? If it is so, we need to insert a reference. If this is not the case, we need to omit the lines about the misunderstanding. We also need to explain better why the MM2003 calculations differ from MBH.

Moreover, our sentence ("MM2003 only calculate principal components for the period when all chronologies are present") imply that MM2003 only calculated PCs for the period 1820-1971, as this would be the period when all chronologies are present according to the MM2003 Table 8. Obviously, they calculated PCs beyond 1820, as their calculations actually extend back to 1400.

The problem continues in the legend to our Fig. 2. (" Each of the 212 data series is shown ... The red rectangle indicates the single block used by MM2003, neglecting all data prior to 1619"). The last sentence is inconsistent with the information in MM2003 in three ways; a) MM2003 clearly show in their Table 8 that they analysed the same blocks of data as MBH. b) The year 1619 as a starting point of a data block is inconsistent with MM Table 8. Where does the year 1619 come from? It is not mentioned anywhere in MM2003. c). The red block implies that MM2003 made calculations back only to 1619, but they did back to 1400.

Moreover, the numbers given in the graph of our Fig. 2 indicate that the total number of series is 211, whereas the text in the legend and also in the main text on p. 16 says 212. Which number is correct?

I suppose that some of you others will know this subject much better than I. I have just read the MM2003 paper, and find our reference to it

to be inconsistent with it. I hope you all can make efforts to make this bit crystal clear. If not, I fear we will get problems!

Finally, I would like to draw your attention to the related sentence in our conclusions on p. 26: ("Papers which claim to refute ... have been reviewed and found to contain serious flaws"). Are all of you happy with this statement? Would it sound better with a somewhat less offending sentence, something like:

"Papers which claim to refute ... have been reviewed and found to essentially contribute with insignificant information that does not affect the consensus, and even to include some flaws."

I attach the MM2003 paper.

I will send some comments to the other parts of the text in a separate mail.

Cheers,
Anders

Martin Juckes wrote:

```
> Hello All,  
>  
> here is another draft. I've added a new reconstruction, using 19 independent  
> proxies series from Jones et al., Mann et al., Esper et al. and Moberg et al.  
> This gives a good fit to the calibration data, such that 2 recent years exceed  
> the maximum pre-industrial estimate by 4 sigma levels. I've included this  
> because without it I found it hard to draw precise and useful conclusions  
> from the 4 partially overlapping reconstructions I had done before.  
>  
> cheers,  
> Martin  
>  
> -----  
>  
> \documentclass[cpd,11pt]{egu}  
>  
> \input macs  
> \voffset 5cm  
> \hoffset 1.5cm  
>  
> \begin{document}  
>  
> \title  
> {\bf Millennial Temperature Reconstruction Intercomparison and Evaluation  
> }  
>  
> \runningtitle{Millennial Temperature}  
> \runningauthor{M.-N.-Juckes et al}  
> \author{Martin Juckes{(1)},  
> Myles Allen{(2)},  
> Keith Briffa{(3)},  
> Jan Esper{(4)},  
> Gabi Hegerl{(5)},  
> Anders Moberg{(6)},  
> Tim Osborn{(3)},  
> Nanne Weber{(7)},  
> Eduardo Zorita{(8)}}  
> \correspondence{Martin Juckes (M.N.Juckes@rl.ac.uk)}  
> \affil{  
> British Atmospheric Data Centre, SSTD,  
> Rutherford Appleton Laboratory  
> Chilton, Didcot,
```

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> Oxfordshire, OX11 0QX,
> United Kingdom
> }
>
> \affil{1: Rutherford Appleton Laboratory,
> 2: University of Oxford,
> 3: University of East Anglia,
> 4: Swiss Federal Research Institute,
> 5: Duke University,
> 6: Stockholm University,
> 7: Royal Netherlands Meteorological Institute (KNMI),
> 8: GKSS Research Centre
> }
> \date{Manuscript version from 31 Oct 2005 }
> \msnumber{xxxxxx}
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> \pubyear{}
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> \received{}
> %\pubacpd{} % ONLY applicable to ACP
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> \firstpage{1}
>
> \maketitle
>
> \begin{abstract}
> There has been considerable recent interest in paleoclimate reconstructions of the temperature history of
> the last millennium. A wide variety of techniques have been used.
> The interrelation among the techniques is sometimes unclear, as different studies often
> use distinct data sources as well as distinct methodologies.
> Recent work is reviewed with an aim to clarifying the import of
> the different approaches.
> A range of proxy data collections used by different authors are passed
> through two reconstruction algorithms: firstly, inverse regression and,
> secondly, compositing followed by variance matching.
> It is found that the first method tends to give large weighting to
> a small number of proxies and that the second approach is more robust
> to varying proxy input.
> A reconstruction using 19 proxy records extending back to 1000AD shows a
> maximum pre-industrial temperature of 0.227K (relative to the 1866 to 1970 mean).
> The standard error on this estimate, based on the residual in the calibration
> period is 0.149K. Two recent years (1998 and 2005) have exceeded the pre-industrial
> estimated maximum by more than 4 standard errors.
> \end{abstract}
>
>
> %%\openup 1\jot
>
> \introduction\label{sec:intro}
>
> The climate of the last millennium has been the subject of much
> debate in recent years, both in the scientific literature
> and in the popular media.
> This paper reviews reconstructions of past temperature,
> on the global, hemispheric, or near-hemispheric scale, by
> \citet{jones_etal1998} [JBB1998],
> \citet{mann_etal1998a} [MBH1998],
> \citet{mann_etal1999} [MBH1999],
> \citet{huang_etal2000} [HPS2000],
> \citet{crowley_lowery2000} [CL2000],
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> \cite{briffa_etal2001} [BOS2001],
> \cite{esper_etal2002b} [ECS2002],
> \cite{mann_jones2003} [MJ2003],
> \cite{moberg_etal2005} [MSH2005],
> \cite{oerlemans2005} [OER2005],
> \cite{hegerl_etal2006+} [HCA2006].
> %%The criticism
> %%directed at them (mainly MBH1999) by \cite{mcintyre_mckitrick2003} [MM2003] and others.
>
>
> Climate variability can be partitioned into contributions from
> internal variability of the climate system and response to forcings,
> which the forcings being further partitioned in natural and
> anthropogenic.
> The dominant change in forcing in the late 20th century
> arises from human impact in the form of
> greenhouse gases \cite{primarily carbon dioxide, methane and
> chloro-fluoro carbons:} [IPCC2001].
> The changes in concentration of these gases in the atmosphere
> are well documented and their radiative properties which reduce,
> for a given temperature difference, radiative loss of heat to space
> from the mid and lower troposphere
> \cite{for carbon dioxide, this was first documented by} [arrhenius1896]
> are beyond dispute.
>
> However, there remains some uncertainty on two issues:
> firstly, how much of the observed change is due to greenhouse forcing as
> opposed to natural forcing and internal variability;
> secondly, how significant, compared to past natural changes, are the
> changes which we now observe and expect in the future?
>
> The first question is not answered by the IPCC conclusion cited above because
> that conclusion only compares the anthropogenic forcing of the late 20th century
> with the natural forcings of the same period. Further back in the past, it is
> harder to make definitive statements about the amplitude of variability in natural
> forcings. The second question reflects the uncertainty in the response of the
> climate system to a given change in forcing. In the last century both the
> variations in forcing and the variations in response have been measured with
> some detail, yet there remains uncertainty about the contribution of
> natural variability to the observed temperature fluctuations.
> In both cases, investigation is hampered by the fact that
> estimates of global mean temperature based on reliable direct measurements
> are only available from 1856 onwards \cite{jones_etal1986}.
>
> Climate models are instrumental in addressing both questions,
> but they are still burdened with
> some level of uncertainty and there is a need for more detailed knowledge
> of the behaviour of the actual climate on multi-centennial timescales
> both in order to evaluate the climate models and in order to address the
> above questions directly.
>
> The scientific basis for proxy based climate reconstructions may be stated simply: there are
> a number of physical indicators
> which contain information about the past environmental variability.
> As these are not direct measurements, the term proxy is used.
>
>
> \cite{jones_mann2004} review evidence for climate change in
> the past millennium and conclude that there had been a
> global mean cooling since the 11th century
> until the warming period initiated in the 19th century, but the issue remains
> controversial. This paper reviews recent contributions and evaluates the impact
> of different methods and different data collections used.
>
> Section 2 discusses recent contributions, which have developed a range of new

> methods to address aspects of the problem.
> Section 3 discusses the technique used by MBH1998/9
> in more detail in the context of criticism by \citet{mcintyre_mckitrick2003}
> (hereafter MM2003).
> Section 4 presents some new results using the data collections from 5 recent studies.
>
>
> \section{A survey of recent reconstructions}
>
> This section gives brief reviews of recent
> contributions, displayed in Fig.~1.
> Of these, 5 are estimates of the Northern Hemisphere mean temperature
> (MBH1999, HPS2000, CL2000, MSH2005, HCA2006),
> 2 of the Northern Hemisphere extra tropical mean temperature (BOS2001, ECS2002)
> and 3 of the global mean temperature (JBB1998, MJ2003, OER2005).
> All, except the inherently low resolution reconstructions of HPS2000 and OER2005,
> have been smoothed with a 40 year running mean.
> With the exception of HPS2000 and OER2005, the reconstructions
> use partly overlapping methods and data, so they
> cannot be viewed as independent from a statistical viewpoint.
> In addition to exploiting a range of different data sources,
> the above works also use a range of techniques.
> The subsections below cover different scientific themes,
> ordered according to the date of key publications.
> Some reconstructions which do not extend all the way
> back to 1000AD are included because of their
> importance in addressing specific issues.
> The extent to which the global, northern hemisphere and northern hemisphere
> extratropical reconstructions might be expected to agree
> is discussed in Sect.~2.10 below.
>
> \subsection{High-resolution paleoclimate records}
>
> \citet{jones_etal1998} [JBB1998] present the first annually resolved
> reconstructions of temperatures back to 1000AD, using
> a composite of standardised 10 proxies for the northern hemisphere and 7 for the southern,
> with variance damped in the early part of the series to account for the
> lower numbers of proxies present (6 series extend back to 1000AD), following \citet{osborn_etal1997}.
> The composites are
> scaled by variance matching (Appendix A) against the annual mean summer temperatures for 1931-1960.
> Climate models are also employed to investigate the temperature coherency
> between proxy sites and it is shown that there are strong large scale
> coherencies in the proxy data which are not reproduced by
> the climate model. An evaluation of each individual
> proxy series against instrumental data from 1881 to 1980
> shows that tree-rings and historical reconstructions
> are more closely related to temperature than those
> from corals and ice-cores.
>
> With regard to the temperatures of the last millennium,
> the primary conclusion of JBB1998 is that
> the twentieth century was the warmest of the millennium.
> There is clear evidence of a cool period from 1500 to 1900,
> but no strong ``Medieval Warm Period" [MWP] (though the second warmest
> century in the northern hemisphere reconstruction is
> the 11th). The MWP is discussed further in Sect.~2.4 below.
>
> JBB1998 draw attention to the limitations of some of the proxies
> on longer timescales (see Sect.~3.5 below).
> Homogeneity of the data record and
> its relation with temperature may not be guaranteed on longer timescale.
> This is an important issue, since
> many climate reconstructions assume a constant relationship between
> temperature anomalies and the proxy indicators

> (there are also problems associated with timescale-dependency in the
> relationship which are discussed further in Sect.~2.6 below).
>
> MJ2003 include some additional proxy series and extend to study period back a
> further millennium and conclude that the late 20th century warmth
> is unprecedented in the last two millennia.
>
> \subsection{Climate field reconstruction}
>
> \cite{mann_etal1999} published
> the first reconstruction of the last thousand years northern hemispheric mean
> temperature which included objective error bars,
> based on the analysis of the residuals in the calibration period.
> The authors concluded not only
> that their estimate of the temperature over the whole period 1000AD to 1860AD
> was colder than the late twentieth century, but also that 95% certainty limits
> were below the last decade of the twentieth century.
> The methods they used were presented in MBH1998
> which described a reconstruction back to 1400AD.
>
> MBH1998 use a collection of 415 proxy time indicators, many more than used in \cite{jones_etal1998},
> but many of these are too close geographically to be considered
> as independent, so they are combined into a smaller number of representative
> series.
> The number of proxies also decreases significantly with age:
> only 22 independent proxies extend back to 1400AD,
> and, in
> MBH1999, 12 extend back to 1000AD (7 in the Northern Hemisphere).
> MBH1998 and MBH1999 have been the subject of much debate since the latter was cited
> in the IPCC (2001) report, though the IPCC
> conclusions\footnote{\cite{IPCC2001} concluded that
> ``The 1990s are likely to have been the warmest decade of the millennium in
> the Northern Hemisphere, and 1998 is likely to have been the warmest
> year," where ``likely" implies a greater than 66% probability.
> Since 2001 it has been recognised that there is a need to explicitly
> distinguish between an expression of confidence, as made by the IPCC in this quote,
> which should include expert assessment of the robustness of statistical methods
> employed, and simple citation of the results of statistical test.
> In the language of
> \cite{manning_etal2004} we can say that MBH1999 carried out statistical
> tests which concluded that the 1990s have been the warmest decade of the
> millenium with 95% likelihood, while IPCC (2001), after assessing all
> available evidence had a 66% confidence in the same statement.}
> were weaker than those of MBH1999.
>
> This work also differ from Jones et al. (1998) in using spatial patterns of temperature
> variability rather than hemispheric mean temperatures. In this way the study aims
> to exploit proxies which are related to temperature indirectly: for
> instance, changes in temperature may be associated with changes in
> wind and rainfall which might affect proxies more strongly than
> temperature. Since wind and rainfall are correlated with
> changes in temperature patterns, it is argued, there may be important non-local
> correlations between proxies and temperature.
>
> Different modes of atmospheric variability are evaluated through an
> Empirical Orthogonal Function [EOF] analysis of the time period 1902 to 1980,
> expressing the global field as a sum of spatial patterns (the EOFs) multiplied by
> Principal Components (PCs -- representing the temporal evolution).
> Earlier instrumental data are too sparse to be used for this purpose:
> instead they are used in a validation calculation to determine how
> many EOFs should be included in the reconstruction.
> Time series for each mode of variability are then reconstructed from the proxy data using
> a optimal least squares inverse regression.
>
> Finally, the skill of the regression of each PC is tested using the

> 1856 to 1901 validation data.
> Prior to 1450AD it is determined that only
> one PC can be reconstructed with
> any accuracy. This means that the main advantage of the
> Climate Field Reconstruction method does not apply at earlier dates.
> The methodology will be discussed further in Sect.~3 below.
>
> The reconstructed temperature evolution (Fig.~1) is rather less variable than that of Jones et al. (1998),
> but the differences are not statistically significant.
> The overall picture is of gradual cooling until the mid 19th century,
> followed by rapid warming matching that evaluated by the earlier work.
>
> \subsection{Borehole temperatures}
>
> \cite{huang_etal2000} [HPS2000] estimate northern hemisphere temperatures
> back to 1500AD using
> measurements made in 453 boreholes (their paper also presents global and
> southern hemisphere results using an additional 163 southern hemisphere boreholes).
> The reconstruction is included here, even though it does not extend back to 1000AD,
> because it has the advantage of being completely
> independent of the other reconstructions shown.
> Temperature fluctuations at the surface propagate slowly downwards, so that measurements
> made in the boreholes at depth contain a record of past surface temperature fluctuations.
> HPS2000 used measurements down to around 300m.
> The diffuse nature of the temperature anomaly means that short time scale fluctuations
> cannot be resolved. Prior to the 20th century, the typical resolution is about 100 years.
>
> \cite{mann_etal2003} analyse the impact of changes in land use and snow cover
> on borehole temperature reconstructions and conclude that
> it results in significant errors.
> This conclusions has been refuted by
> \cite{pollack_smerdon2004} (on statistical grounds), \cite{gonzalez-rouco_etal2003}
> (using climate simulations) and \cite{huang2004} (using an expanded network of 696
> boreholes in the northern hemisphere).
>
> \subsection{Medieval Warm Period}
>
> Despite much discussion
> \cite[e.g.]{}{hughes_diaz1994, bradley_etal2003}, there is no clear quantitative
> understanding of what is meant by the ``Medieval Warm Period" [MWP].
> \cite{crowley_lowery2000}
> [CL2000] discuss the evidence for a global MWP, which they interpret as
> a period of unusual warmth in the 11th century. All the reconstructions
> of the 11th century temperature shown
> in Fig.~1 estimate that century to have been warmer than most of the
> past millennium. However, the question of practical importance is not
> whether it was warmer than the 12th to 19th centuries, which is
> generally accepted, but whether it was a period of comparable
> warmth to the late 20th century. MBH1999 concluded, with 95\% confidence, that
> this was not so. CL2000 revisit the question
> using 15 proxy records, of which 9 were not used in the studies
> described above. Several of the series used have extremely low temporal resolution.
> %%CL2000 sought to select tree ring chronologies with consistent quality
> %%throughout their length, as measured by the "sample replication"
> %%\cite{cook_etal2004}.
> %%[check usage of "sample replication" -- cook etal (QSR) is available from Jan's website]]
>
> They draw attention to the spatial localization of the MWP in their proxy series:
> it is strong in North America, North Atlantic and Western Europe, but not
> clearly present elsewhere. Periods of unusual warmth
> do occur in other regions, but these are short and asynchronous.
>
> Their estimate of northern hemispheric temperature over the past millennium is consistent
> with the works discussed above. They conclude that the occurrence of decades of

> temperatures similar to those of the late 20th century cannot be unequivocally ruled
> out, but that there is, on the other hand, no evidence to support the claims
> that such an extended period of large-scale warmth occurred.
>
> \cite{soon_baliunas2003} carry out an analysis of local climate reconstructions.
> They evaluate the number of such reconstructions which show (a) a sustained ``climate
> anomaly" during 800-1300AD, (b) a sustained ``climate
> anomaly" during 1300-1900AD and (c)
> their most anomalous 50 year period in the 20th century.
> Their definition of a ``sustained climate anomaly" is 50 years of warmth,
> wetness or dryness for (a) and (c) and 50 years of coolness, wetness
> or dryness in (b).
> It should be noted that they do not carry out evaluations which allow direct comparison between
> the 20th century and earlier times:
> they compare the number of extremes occurring in the 20th century with the
> number of anomalies occurring in periods of 3 and 4 centuries in the past.
> Both the use of sampling periods of differing length and different selection criteria make interpretation
> of their results problematic.
> They have also been criticised for interpreting
> regional extremes which occur at distinct times as being indicative of a global
> climate extremes \cite{jones_mann2004}. This issue is discussed further in
> Sect.~2.9 below.
> \cite{osborn_briffa2006} perform a systematic analysis along the lines of \cite{soon_baliunas2003}
> and conclude that the proxy records alone, by-passing the problem of proxy calibration
> against instrumental temperatures, show an unprecedented anomaly in the 20th century.
>
> \subsection{Segment length curse}
>
> \cite{briffa_etal2001} and \cite{briffa_etal2002} discuss the impact of
> the ``segment length curse" \cite{cook_etal1995a, briffa_etal1996, briffa2000} on
> temperature reconstructions from tree rings.
> Tree rings have been shown to have much greater sensitivity
> than other proxies on short timescales (JBB1998), but there is a concern that this may not
> be true on longer timescales. Tree ring chronologies are often made up of
> composites of many trees of different ages at one site.
> The width of the annual growth ring
> depends not only on environmental factors but also on the age of the
> tree. The age dependency on growth is often removed by subtracting
> a growth curve from the tree ring data for each tree. This process,
> done empirically, will not only remove age related trends but also any environmental
> trends which span the entire life of the tree.
> \cite{briffa_etal2001} use a more sophisticated method
> (Age Band Decomposition [ABD], which
> forms separate chronologies from tree rings in different age bands,
> and then averages all the age-band chronologies)
> to construct northern hemisphere
> temperatures back to 1400AD, and show that
> a greater degree of long term variability is preserved.
> The reconstruction lies between those
> of MBH1999 and JBB1998, showing the cold 17th century of the former,
> but the relatively mild 19th century of the latter.
>
> The potential impact of the segment length limitations is analysed further
> by \cite{esper_etal2002b, esper_etal2003}, using `Regional Curve Standardisation' (RCS)
> \cite{briffa_etal1992}.
> In RCS composite growth curves (different curves reflecting
> different categories of growth behaviour) are obtained from all the trees
> in a region and this, rather than a fitted curve, is subtracted
> from each individual series. Whereas ABD circumvents the need to
> subtract a growth curve, RCS seeks to evaluate a growth curve which
> is not contaminated by climate signals.
> The ECS2002 analysis agrees well with that of MBH1999 on short
> time scales, but has greater centennial variability \cite{esper_etal2004}.
> ECS2002 suggest that this may be partly due to the lack of tropical proxies
> in their work, which they suggest should be regarded as an extratropical

> Northern Hemisphere estimate. The extratropics are known to have
> greater variability than the tropics.
> % [check]:from eduardo:: Table 1 in MBH GRL 99 --add ref??
> However, it has to be also noted that among the proxies used by MBH1999
> (12 in total), just 2 of them are located in the tropics, both at one location
> (see table 1 below).
>
> \cite{cook_etal2004} study the data used by ECS2002 and pay particular attention
> to potential loss of quality in the earlier parts of tree-ring chronologies
> when a relatively small number of tree samples are available. Their analysis
> suggests that tree ring chronologies prior to 1200AD should be treated with
> caution.
>
> \subsection{Separating timescales}
>
> \cite{moberg_etal2005} follow BOS2001 and ECS2002 in trying to address
> the ``segment length curse'', but rather than trying to improve the
> tree-ring chronologies by improving the standardizations,
> they discard low frequency component of the tree-ring data,
> and replace this with low-frequency information from proxies with lower temporal resolution.
> A wavelet analysis is used to filter different temporal scales.
>
> Each individual proxy series is first scaled to unit variance and then wavelet transformed.
> Averaging of the wavelet transforms is made separately for tree ring data
> and the low-resolution data.
> The average wavelet transform of tree-ring data for timescales less than 80
> years is combined with the averaged wavelet transform of the low-resolution data for
> timescales longer than 80 years to form one single wavelet transform covering all timescales.
> This composite wavelet transform is inverted to create a dimensionless temperature
> reconstruction, which is calibrated against the instrumental record of
> northern hemisphere mean temperatures, AD 1856-1979, using a variance matching method.
>
> Unfortunately, the calibration period is too short to independently calibrate the
> low frequency component. The variance matching represents a form of cross-calibration.
> In all calibrations against instrumental data, the long period (multi-centennial)
> response is determined by a calibration which is dominated by
> sub-centennial variance. The MSH2005 approach makes this explicit and
> shows a level of centennial variability which is much larger than in
> MBH1999 reconstruction and
> similar to that in simulations of the past millennium with two
> different climate models, ECHO-G \cite{storch_etal2004} and NCAR CSM
> ("Climate System Model") \cite{mann_etal2005}.
>
> \subsection{Glacial advance and retreat}
>
> \cite{oerlemans2005} provides another independent estimate of the global mean temperature
> over the last 460 years from an analysis of glacial advance and retreat.
> As with the bore hole based estimate of HPS2000, this work uses a
> physically based model rather than an empirical calibration.
> The resulting curve lies within the
> range spanned by the high-resolution proxies, roughly midway between
> the MBH1999 Climate Field Reconstruction and the HPS2000 bore hole estimate.
>
> Unlike the borehole estimate, but consistent with most other works presented
> here, this analysis shows a cooling trend prior to 1850, related to glacial
> advances over that period.
> It should be noted that
> the technique used to generate the bore hole estimate \cite{pollack_etal1998}
> assumes a constant temperature prior to 1500AD. The
> absence of a cooling trend after this date may be influenced by this
> boundary condition.
>
> \subsection{Regression techniques}
>

> Many of the reconstructions listed above depend on empirical relationships
> between proxy records and temperature. \cite{storch_etal2004} suggest
> that the regression technique used by MBH1999
> under-represents\footnote{This is sometimes referred to as ``underestimating",
> which will mean the same thing to many people, but something slightly different
> to statisticians. Any statistical model (that is, a set of assumptions about the
> noise characteristics of the data being examined) will deliver estimates of
> an expected value and variability. The variability of the expected value is
> not generally the same as the expected value of the variability.)
> the variability of past climate.
> This conclusion is drawn after a applying a method similar to that of MBH1999 to output from a
> climate model using a set of pseudo-proxies: time series generated from
> the model output and degraded with noise which is intended to match the noise
> characteristics of actual proxies.
> \cite{mann_etal2005} use the same approach and arrive at a different conclusion:
> namely, that their regression technique is sound.
> \cite{mann_etal2005} show several implementations of their
> Climate Field Reconstruction Method in the CSM simulation, using different levels
> of white noise in their synthetic pseudo proxies.
> For a case of pseudo-proxies with a realistic signal-to-noise ratio of 0.5, they use
> a calibration period (1856-1980) which is longer than that
> used in MBH1998 and MBH1999 (1901-1980).
> It turns out that the difference in the length of the calibration period is critical
> for the skill of the method (Zorita, personal communication et al., submitted).
> % (I think you can refer to Buerger et al 2006 here. Check with Eduardo if this is OK.
> % By the way, update the reference list: Tellus, 58A, 227-235) [AM]
>
> There is some uncertainty about the true nature of noise on the proxies, and
> on the instrumental record, as will be discussed further below.
> The optimal least squares estimation technique of MBH1998 effectively
> neglects the uncertainties in the proxy data relative to uncertainties
> in the temperature.
> Instead,
> \cite{hegerl_etal2006+} use total least squares regression \cite{allen_stott2003, adcock1878}.
> This approach
> allows the partitioning of noise between instrumental temperatures
> and proxy records to be estimated, on the assumption that the instrumental
> noise is known. \cite{hegerl_etal2006+} show that this approach leads to greater variability in the reconstruction.
>
> \cite{rutherford_etal2005} take a different view. They compare reconstructions
> from 1400AD to present using a regularised expectation maximisation technique \cite{schneider2001}
> and the MBH1998 climate field reconstruction method and find only minor differences.
> Standard regression techniques assume that we have a calibration period, in which
> both sets of variables are measured, and a reconstruction (or prediction) period
> in which one variable is estimated, by regression, from the other.
> The climate reconstruction problem is more complex:
> there are hundreds of instrumental records
> which are all of different lengths, and similar numbers of proxy records,
> also of varying length. The expectation maximisation technique
> \cite{little_rubin1987}
> is well suited to deal with this: instead of imposing an
> artificial separation between a calibration period and a reconstruction
> period, it fills in the gaps in a way which exploits all data present.
> Regularised expectation maximisation is a generalisation
> developed by \cite{schneider2001} to deal with ill posed problems.
> Nevertheless, there is still a simple regression equation at the heart of the technique.
> That used by \cite{rutherford_etal2005} is similar to that used by
> %new: corrected
> MBH1998, so the issue raised by \cite{hegerl_etal2006+} is unanswered.
>
> \subsection{Natural variability and forcings}
>
> Global temperature can fluctuate through internally generated variability of
> the climate system (as in the El Ni\~no phenomenon), through
> variability in natural forcings (solar insolation, volcanic aerosols,

> natural changes to greenhouse gas concentrations) and human changes.
> Reconstructions of variations in the external forcings for the last
> millenium have been
> put forward \citep{crowley2000}, although recent studies have
> suggested a lower amplitude
> of low-frequency solar forcing \citep{lean_etal2002, foukal_etal2004}.
>
> Analysis of reconstructed temperatures of MBH1999 and CL2000 and
> simulated temperatures using reconstructed solar and volcanic forcings
> shows that changes in the forcings can explain the reconstructed long
> term cooling through most of the millenium
> and the warming in the late 19th century \citep{crowley2000}.
> The relatively cool climate in the second half of the 19th century may be
> attributable to cooling from deforestation \citep{bauer_etal2003}.
> \citet{hegerl_etal2003} analyse the correlations between four
> reconstructions (MBH1999, BOS2001, ECS2002, and a modified version of
> CL2000)
> and estimated forcings \citep{crowley2000}.
> They find that that natural forcing, particularly by
> volcanism, explains a substantial fraction of decadal variance.
> Greenhouse gas forcing is detectable
> with high significance levels in all analyzed reconstructions except
> MSH2005, which ends in 1925.
> \citet{weber2005b} carries out a similar analysis with a wider range
> of reconstructions. It is shown that the regression of reconstructed
> global temperatures on the forcings has a similar dependence on timescale
> as regressions derived from the climate model. The role of solar forcing is
> found to be larger for longer timescales, whereas volcanic forcing dominates
> for decadal timescales.
> The trend component over the period 1000 to 1850 is, however, in all
> reconstructions larger than the trend implied by the forcings.
>
> The methods employed by
> \citet{hegerl_etal2006+} attribute about a third of the early 20th
> century warming, sometimes
> more, in high-variance reconstructions to greenhouse gas forcing.
> These results indicate that enhanced variability in the past does not
> make it more difficult to detect greenhouse warming, since a large
> fraction of the variability can be attributed to external forcing.
> Quantifying the influence of external forcing on the proxy records is
> therefore more relevant to understanding climate variability and its
> causes than determining if past periods were possibly as warm as the
> 20th century.
>
> \citet{goosse_etal2005} investigate the role of internal variability using
> an ensemble of 25 climate model simulations of the last millennium
> and forcing estimates from \citet{crowley2000}.
> They conclude that internal variability dominates local and regional
> scale temperature anomalies, implying that most of the variations
> experienced by a region such as Europe over the last millennium could
> be caused by internal variability. On the hemispheric and global scale,
> however, the forcing dominates.
> This agrees with results from a long
> solar-forced model simulation by \citet{weber_etal2004}.
> %% similar This reinforces similar statements made by JOS1998. [where does this come from?]
> \citet{goosse_etal2005}
> make the new point, that noise can lead to regional temperature anomalies
> peaking at different times to the forcing, so that disagreements in
> timing between proxy series should not necessarily be interpreted as
> meaning there is no common forcing.
>
> \subsection{The long view}
>
> The past sections have drawn attention to the problems of calibrating

> temperature reconstructions using a relatively short
> period over which instrumental records are available.
> For longer reconstructions, with lower temporal resolution,
> other methods are available. Pollen
> reconstructions of climate match the ecosystem types with those
> currently occurring at different latitudes. The changes in
> ecosystem can then be mapped to the temperatures at which
> they now occur \citep[e.g.][]{}{bernabo1981, gajewski1988}.
> These reconstructions cannot resolve decadal variability,
> but they provide an independent estimate of local low-frequency
> temperature variations. The results of \cite{weber_etal2004}
> and
> \cite{goosse_etal2005} suggest that such estimates
> centennial mean temperatures can provide some information about
> global mean anomalies, as they strongly reflect the external forcings on
> centennial and longer timescales. However, there has, as yet,
> been no detailed intercomparison between the pollen based
> reconstructions and the higher resolution reconstructions.
>
>
> \section{Critics of the IPCC consensus on millennial temperatures}
>
> The temperature reconstructions described in the previous section
> represent (including their respective differences and similarities)
> the scientific consensus, based on objective analysis
> of proxy data sources which are sensitive to temperature.
> Nevertheless, there are many who are strongly attached to the view that past
> temperature variations were significantly larger and that, consequently,
> the warming trend seen in recent decades should not be considered
> as unusual.
>
>
> The criticism has been directed mainly at the \cite{mann_etal1998a, mann_etal1999}
> work.
> Therefore, this section focuses mainly on this criticism.
> %new
> Though some of the critics identify the consensus with the MBH1998 work,
> this is not the case: the consensus rests on a broader body of work, and
> as formulated by IPCC2001 is less strong than the conclusions of
> MBH1998 (Sect.~3.2).
> \cite{mcintyre_mckitrick2003} [MM2003]
> criticize MBH1998 on many counts, some related to deficiencies
> in the description of the data used and possible irregularities in the data
> themselves. These issues have been largely resolved in \cite{mann_etal2004}.
> %%\footnote{ftp://holocene.evsc.virginia.edu/pub/MANNETAL1998}.
>
> As noted above, the MBH1998 analysis is considerably more complex than others,
> and uses a greater volume of data.
> There are 3 main stages of the algorithm: (1) sub-sampling of
> regions with disproportionate numbers of proxies, (2) regression,
> (3) validation and uncertainty estimates.
>
> Stage (1) is necessary because some parts of the globe, particularly
> North America and Northern Europe, have a disproportionate number of
> proxy records. Other authors have dealt with this by using only
> a small selection of the available data or using regional
> averages \citep[BOS2001;][]{hegerl_etal2006+}. MBH1998
> use a principal component analysis to extract the common signal from the records in
> densely sampled regions.
>
> The failure of MM2003 to replicate the MBH1998 results is partly due to
> a misunderstanding of the stepwise reconstruction method. MBH1998 use
> different subsets of their proxy database for different time periods.
> This allows more data to be used for more recent periods.
>
>

> For example, Fig.~2 illustrates
> how the stepwise approach applies to the North American tree ring network.
> Of the total of 212 chronologies, only 66 extend back beyond 1400AD.
> MM2003 only calculate principal components for the period when all
> chronologies are present. Similarly, MBH1998 use one principal
> component calculated from 6 drought sensitive tree-rings chronologies from South West Mexico
> and this data is omitted in MM2003.
> % [is this clear now?? (AM)]
> %new
> % Table 7 of MM2003 indicates only 20 series for the region, as the
> % supplementary information provided with MBH2003 omitted 2
> % \citep{mann_etal2004}.
> %endnew
> \cite{mcintyre_mckitrick2005a} [MM2005] continue the criticism of the techniques
> used by MBH1998 and introduce a ``hockey stick index": defined in terms of the ratio
> of the variance at the end of a time series
> to the variance over the remainder of the series.
> MM2005 argue that the way in which
> a principal component analysis is carried out in MBH generates an artificial
> bias towards a high ``hockey-stick index" and that the statistical significance of
> the MBH results may be lower than originally estimated.
>
> The issue arises because the tree ring chronologies are standardized:
> this involves subtracting a mean and dividing by a variance.
> MBH1998 use the mean and variance of the detrended series evaluated
> over the calibration period. MM2005 are of the view that this is
> incorrect.
> They suggest that each series should be standardised with respect to the
> mean and variance its full length.
>
> The code used by MM2005 is not, at the time of writing available,
> but the code fragments included in the text imply
> that their calculation used data which had been
> centred (mean removed) but had not been normalized to unit variance (standardised).
> Figure 3 shows the effect of the changes, applied to the
> North American tree ring sub-network of the data used by MBH1998,
> using those chronologies which extend back to 1400AD.
> The calculation used here does not precisely reproduce the archived MBH1998
> result, but the differences may be due to small differences in
> mathematical library routines used to do the decomposition.
> The effect of replacing the MBH1998 approach with centering and
> standardising on the whole time series is small, the effect of
> omitting the standardisation as in MM2005 is much larger:
> this omission causes the 20th century trend to be removed from the
> first principal component.
>
> \cite{storch_zorita2005} look at some of the claims made in MM2005
> and analyses them in the context of a climate simulation.
> They find the impact of the modifications suggested by McIntyre and McKitrick to
> be minor.
> \cite{mcintyre_mckitrick2005b} clarify their original claim, stating that the
> standardisation technique used by MBH98 does not create the ``hockey-stick" structure
> but does ``steer" the selection of this structure in principal component
> analysis.
>
> \cite{mcintyre_mckitrick2005c} [MM2005c] revisit the MM2003 work and correct
> their earlier error by taking the stepwise reconstruction technique into account.
> They assert that the results of MM2003, which show a 15th century
> reconstruction 0.5K warmer than found by MBH1998,
> are reproduced with only minor changes to the MBH1998 proxy data base.
> Examination of the relevant figures, however, shows that this is not entirely
> true. The MM2005c predictions for
> the 15th century are 0.3K warmer than the MBH1998
> result: this is still significant, but, unlike the discredited MM2003 result, it

```

> would not make the 15th century the warmest on record.
>
> MM20005c and \citet{wahl_ammann2005} both find that
> excluding the north American bristlecone pine data from the proxy
> data base removes the skill from the 15th century reconstructions.
> MM2005c justify this removal on the grounds that the first principal component
> of the North American proxies, which is dominated by the
> bristlecone pines, is a statistical outlier with respect to the joint distribution
> of  $\text{SR}^2$  and the difference in mean between 1400 to 1450 and 1902 to 1980.
> %%first ref to table 1
> Table 1, which lists a range of proxies extending back to 1000,
> shows that the North American first principal component ( $\text{ITRDB}$  [pc01]" in that table)
> is not an outlier
> in terms of its coherence with northern hemispheric mean temperature from 1856 to 1980.
>
> \begin{table}[t]
> \small
> %% output from mitrie/pylib/multi_r2.py, edited
> \begin{tabular}{|p{7.0cm}|r|r||r||}
> \hline
> Name & Lat. & Lon. & Id &  $\text{SR}^2$  & Type \cr
> \hline
> GRIP: borehole temperature (degC) (Greenland) $^1$  & 73 & -38 & *,Mo & 0.67 & [IC] \cr
> China: composite (degC) $^2$  & 30 & 105 & *,Mo & 0.63 & [MC] \cr
> Taymir (Russia) & 72 & 102 & He & 0.60 & [TR C] \cr
> Eastern Asia & 35 & 110 & He & 0.58 & [TR C] \cr
> Polar Urals $^3$  & 65 & 67 & Es, Ma & 0.51 & [TR] \cr
> Tornetraesk (Sweden) $^4$  & 58 & 21 & Mo & 0.50 & [TR] \cr
> ITRDB [pc01] & 40 & -110 & Ma & 0.49 & [TR PC] \cr
> Mongolia & 50 & 100 & He & 0.46 & [TR C] \cr
> Arabian Sea: Globigerina bull $^5$  & 18 & 58 & *,Mo & 0.45 & [CL] \cr
> Western Siberia & 60 & 60 & He & 0.44 & [TR C] \cr
> Northern Norway & 65 & 15 & He & 0.44 & [TR C] \cr
> Upper Wright (USA) $^6$  & 38 & -119 & *,Es & 0.43 & [TR] \cr
> Shihua Cave: layer thickness (degC) (China) $^7$  & 40 & 116 & *,Mo & 0.42 & [SP] \cr
> Western Greenland & 75 & -45 & He & 0.40 & \cr
> Quelcaya 2 [do18] (Peru) $^8$  & -14 & -71 & *,Ma & 0.37 & [IC] \cr
> Boreal (USA) $^6$  & 35 & -118 & *,Es & 0.32 & [TR] \cr
> Tornetraesk (Sweden) $^9$  & 58 & 21 & *,Es & 0.31 & [TR] \cr
> Taymir (Russia) $^{10}$  & 72 & 102 & *,Es, Mo & 0.30 & [TR] \cr
> Fennoscandia $^{11}$  & 68 & 23 & *,Jo, Ma & 0.28 & [TR] \cr
> Yamal (Russia) $^{12}$  & 70 & 70 & *,Mo & 0.28 & [TR] \cr
> Northern Urals (Russia) $^{13}$  & 66 & 65 & *,Jo & 0.27 & [TR] \cr
> \hline
> \end{tabular}
> \caption{Continued overleaf.}
> \end{table}
>
> \renewcommand{\thetable}{\arabic{table}}
> \addtocounter{table}{-1}
> \begin{table}[t]
> \small
> \begin{tabular}{|p{7.0cm}|r|r||r||}
> \hline
> Name & Lat. & Lon. & Id &  $\text{SR}^2$  & Type \cr
> \hline
> ITRDB [pc02] & 42 & -108 & Ma & 0.21 & [TR PC] \cr
> Lenca (Chile) $^{14}$  & -41 & -72 & Jo & 0.18 & [TR] \cr
> Crete (Greenland) $^{15}$  & 71 & -36 & *,Jo & 0.16 & [IC] \cr
> Methuselah Walk (USA) & 37 & -118 & *,Mo & 0.14 & [TR] \cr
> Greenland stack $^{15}$  & 77 & -60 & Ma & 0.13 & [IC] \cr
> Morocco & 33 & -5 & *,Ma & 0.13 & [TR] \cr
> North Patagonia $^{16}$  & -38 & -68 & Ma & 0.08 & [TR] \cr
> Indian Garden (USA) & 39 & -115 & *,Mo & 0.04 & [TR] \cr
> Tasmania $^{17}$  & -43 & 148 & Ma & 0.04 & [TR] \cr

```

```

> ITRDB [pc03] & 44 & -105 & Ma & -0.03 & [TR PC] \cr
> Chesapeake Bay: Mg/Ca (degC) (USA)$^{18}$ & 38 & -76 & *.Mo & -0.07 & [SE] \cr
> Quelcaya 2 [accum] (Peru)$^{8}$ & -14 & -71 & *.Ma & -0.14 & [IC] \cr
> France & 44 & 7 & *.Ma & -0.17 & [TR] \cr
> \hline
> \end{tabular}
> \caption{(continued)
> The primary reference for each data set is indicated by the superscript in the first column as
> follows:
> 1: \citep{dahl-jensen_etal1998}, 2: \cite{yang_etal2002}, 3: \cite{shiyatov1993}, 4: \cite{grudd_etal2002}, 5: \cite{gupta_etal2003},
> 6: \cite{lloyd_graumlich1997}, 7: \cite{tan_etal2003}, 8: \cite{thompson1992},
> 9: \cite{bartholin_karlen1983}, 10: \cite{naurzbaev_vaganov1999}, 11: \cite{briffa_etal1992},
> 12: \cite{hantemirov_shiyatov2002}, 13: \cite{briffa_etal1995}, 14: \cite{lara_villalba1993},
> 15: \cite{fisher_etal1996}, 16: \cite{boninsegna1992}, 17: \cite{cook_etal1991}, 18: \cite{cronin_etal2003}.
> the "Id" in column 4 refers to the reconstructions in which the data were used.
> The type of proxy is indicated in column 6:: tree-ring [TR], tree-ring composite [TR C],
> tree-ring principle component [TR PC], coral [CL], sediment [SE], ice core [IC],
> multi-proxy composite [MC]. The 19 proxy series marked with a "*" in column 4 are used in the
> ``Union" reconstruction.
> }
> \end{table}
>
> \citep[; MM2005c]{briffa_osborn1999} suggest that
> rising CO$_2$ levels may have contributed significantly to the
> 19th and 20th century increase in growth rate in some trees,
> particularly the bristlecone pines, but such an
> effect has not been reproduced in controlled experiments with mature trees
> \citep{korner_etal2005}.
>
> Once a time series purporting to represent past temperature has been obtained,
> the final, and perhaps, most important, step is to verify its
> and estimate uncertainty limits. This is discussed further in the next section.
>
> \section{Varying methods vs. varying data}
>
> One factor which complicates the evaluation of the various reconstructions is
> that different authors have varied both method and data collections. Here we will
> run a representative set of proxy data collections through two algorithms:
> inverse regression and scaled composites. These two methods, and the different
> statistical models from which they may be derived, are explained in the
> Appendix A.
>
> Esper et al. (2005) investigated the differing calibration approaches used in the recent literature, including
> regression and scaling techniques, and concluded that the methodological differences in calibration result in differences
> in the reconstructed temperature amplitude/variance of about 0.5K.
> This magnitude is equivalent to the mean annual temperature change for the Northern Hemisphere reported in the last
> IPCC report for the 1000-1998 period.
> \cite{burger_etal2006} take another approach and investigate a family of 32 different regression algorithms
> derived by adjusting 5 binary switches, using pseudo-proxy data.
> They show that these choices, which
> have all been defended in the literature, can lead to a wide variety of different
> reconstructions given the same data.
> They also point out that the uncertainty is greater when we
> attempt to estimate the climate of periods which lie outside the range experienced
> during the calibration period. The relevance of this point to the last millennium is
> under debate: the glacier based temperature estimates of OER2005 suggest that the
> coldest northern hemisphere mean temperatures occurred close to the start of
> the instrumental record, in the 19th century. The borehole reconstructions,
> however, imply that there were colder temperatures experienced in the 16th to 18th centuries.
> For the question as to whether the warmth of the latter part of the calibration
> period has been experienced in the past, however,
> this particular issue is not directly relevant.
>
> As noted above, much of the MBH1999 algorithm is irrelevant to reconstructions

```

> prior to AD 1450, because before that date the data only suffice,
> according to estimates in that paper, to determine one degree of freedom.
> Hence, we will only look at direct evaluation of the hemispheric mean temperature.
>
> Several authors have evaluated composites and calibrated those composites
> against instrumental temperature. Many of the composites contain more samples in later
> periods, so that the calibration may be dominated by samples which do
> not extend into the distant past. Here, we will restrict attention to
> records which span the entire reconstruction period.
> The data series used are listed in table 1.
>
> \subsection{Proxy data quality issues}
>
> As noted previously, there has been especially strong criticism of
> MBH1998, 1999, partly concerning some aspects of their data collection.
> Figures 4 and 5 show reconstructions made using the MBH1999 and MBH1998 data respectively.
> Regression against northern hemispheric mean temperature from 1856 to 1980 is used
> instead of regression against principal components of
> temperature from 1902 to 1980. There are differences, but key features remain.
> MM2003 draw attention to the fact that one time series,
> ``CANA036'' in the ITRDB classification, contributed
> by Gasp\`e, appears twice in the MBH1998 database.
> This error is corrected in the red dashed curve of Fig.~5,
> which is almost identical to the green curve, which retains the duplication.
>
> \subsection{Reconstruction using a union of proxy collections}
>
> The following subsection will discuss a range of reconstructions using different
> data collections. The first 5 of these collections are defined as those proxies used by
> JBB1998, MBH1999, ECS2002, MSH2005 and HCA2006, respectively, which extend back to 1000AD.
> These will be referred to below as the JBB, MBH, ECS, MSH, HCA composites below
> to distinguish them from the composites used in the published articles, which include
> additional, shorter, proxy data series.
> Finally there is a `Union' composite made using 19 independent northern
> hemisphere proxy series marked with ``*'' in table 1. Apart from the China composite
> record, all the data used are individual series. The PCs used by MBH1999 have been
> omitted in favour of individual series used in other studies.
> Two southern hemisphere tropical series, both from the Quelcaya glacier, Peru,
> are included ensure adequate representation of tropical temperatures.
> This 'Union' collection contains 11 tree-ring series, 4 ice-cores, and one each of
> coral, speleothem, lake sediment and a composite record including historical data.
>
> \subsection{Intercomparison of proxy collections}
>
> Figure 6 shows reconstructions back to 1000AD using
> composites of proxies and variance matching [CVM] (for the proxy
> principal components in the MBH1998, MBH1999 data collections the sign
> is arbitrary: these series have, where necessary, had the sign reversed so that
> they have a positive correlation with the northern hemisphere
> temperature record).
> Surprisingly, the `Union' does not lie in the range spanned by the other reconstructions,
> and reaches colder temperatures than any of them. It does, however, fit the calibration period
> data better than any of the sub-collections.
>
> The reconstructions shown in Fig.~7 use the same data as is used: this time
> using inverse regression [INVR] (Appendix A), as used by MBH1998
> (the method used here differs from that of MBH1998 in using northern hemisphere
> temperature to calibrate against, having a longer calibration period,
> and reconstructing only a single variable instead of multiple EOFs).
> The spread of values is substantially increased relative to the CVM reconstruction.
>
> With INVR, only one reconstruction (that using the ECS2001
> data) shows temperatures warmer than the mid 20th century.
> The inverse regression technique applies weights to the
> individual proxies which are proportional to the

> correlation between the proxies and the calibration temperature
> signature.
> For this time series the 5 proxies are weighted as:
> 1.7 (Boreal); 2.9 (Polar Urals); 1.7 (Taymir); 1.8 (Tornetraesk); and 2.3 (Upper Wright).
> Firstly, it should be noted that this collection samples North America and the
> Eurasian arctic only. The bias towards the arctic is strengthened by the weights
> generated by the inverse regression algorithm, such that the reconstruction has poor geographical coverage.
>
> The MBH1999 and HPS2000 published reconstructions are shown in Fig.~6 for comparison: the MBH1999
> reconstruction lies near the centre of the spread of estimates, while the HPS2000 reconstruction
> is generally at the lower bound.
>
> Much of the current debate revolves around the level of
> centennial scale variability in the past.
> The CVM results generally suggest
> a low variance scenario comparable to MBH1999. The inverse regression
> results, however, suggest greater variability. It should be noted
> that the MBH1999 inverse regression result use greater volumes of
> data for recent centuries, so that the difference in Fig.~7 between the
> dashed red curve and the full green curve in the 17th
> century is mainly due to reduced proxy data input in the latter
> (there is also a difference because MBH1999 used inverse regression
> against temperature principle components rather than northern hemisphere
> mean temperature as here).
>
> Table 2 shows the cross correlations of the reconstructions in Fig.~6,
> for high pass (upper right) and low pass (lower left) components
> of the series, with low pass being defined by a 40 year running mean.
> The low pass components are highly correlated.
>
> \begin{table}[t]
> %% output from mitrie/pylib/pp.py
> \begin{tabular}{|l|c|c|c|c|c|}
> \hline
> & Ma & Mo & Es & Jo & He & Union\cr
> \hline
> Ma & -- & 14\% & 25\% & 60\% & 20\% & 61\% \cr
> Mo & 69\% & -- & 37\% & 11\% & 13\% & 60\% \cr
> Es & 64\% & 77\% & -- & 14\% & 36\% & 57\% \cr
> Jo & 62\% & 51\% & 46\% & -- & 11\% & 35\% \cr
> He & 72\% & 75\% & 85\% & 53\% & -- & 26\% \cr
> Union & 67\% & 71\% & 62\% & 45\% & 84\% & -- \cr
> \hline
> \end{tabular}
> \caption{Cross correlations between reconstructions from
> different proxy data bases: Mann et al (Ma), Moberg et al (Mo),
> Esper et al (Es), Jones et al (Jo), Hegerl et al (He).
> Lower left block correspond to low pass filtered series,
> upper right to high pass filtered.}
> \end{table}
>
> The significance of the correlations between these five proxy data samples
> and the instrumental temperature data during the calibration period (1856-1980)
> has been evaluated using a Monte-Carlo simulation
> with (1) a first order Markov model and (2) random time series
> which reproduces the lag correlation structure of the data samples (see Appendix A).
> Figure 8 shows the lag correlations. The instrumental record had a pronounced
> anti-correlation on the 40 year time-scale. This may be an artifact of the short
> data record, but it is retained in the significance calculation as the best available
> estimate which is independent of the proxies.
> The 'Union' composite shows multi-centennial correlations which are not present in the other data.
> The MBH and JBB composites clearly underestimate the decadal scale correlations, while
> the HCA and 'Union' composites overestimate it.
> %%first ref to table 3

> \conclusions\label{sec:end}

>
> There is general agreement that global temperatures cooled
> over the majority of the last millennium and have risen sharply
> since 1850. In this respect, the recent literature has not produced
> any change to the conclusions of JBB 1998, though there remains
> substantial uncertainty about the magnitude of centennial scale variability
> superimposed over longer term trends.

>
> The IPCC 2001 conclusion that temperatures of the past millennium
> are unlikely to have been as warm, at any time prior to the 20th
> century, as the last decades of the 20th century is supported
> by subsequent research and by the results obtained here.

>
> The greatest range of disagreement among independent
> assessments occurs during the coolest centuries, from 1500 to
> 1900, when the departure from recent climate conditions
> was strongest and may have been outside the range of
> temperatures experienced during the later
> instrumental period.

>
> There are many areas of uncertainty and disagreement within
> the broad consensus outlined above, and also some who
> dissent from that consensus. Papers which claim to refute the
> IPCC2001 conclusion on the climate of the past millennium have been
> reviewed and found to contain serious flaws.

>
> A major area of uncertainty concerns the accuracy of the long time-scale
> variability in the reconstructions. This is particularly
> so for timescale of a century and longer. There does not appear to be any
> doubt that the proxy records would capture rapid change on
> a 10 to 50 year time scale such as we have experienced in recent decades.

>
> Using two different reconstruction methods on a range of proxy data
> collections, we have found that inverse regression
> tends to give large weighting to
> a small number of proxies and that the relatively simple
> approach of compositing all the series and using variance matching to
> calibrate the result gives more robust estimates.

>
> A new reconstruction made with a composite of 19 proxies extending back
> to 1000AD fits the instrumental record to within a standard error of 0.149K.
> This reconstruction gives a maximum pre-industrial temperature of 0.227K
> relative to the 1866 to 1970AD mean. The maximum temperature from the
> instrumental record is 0.841K, over 4 standard errors larger.

>
> The reconstructions evaluated in this study show considerable disagreement
> during the 16th century. The new 19 proxy reconstruction implies 21-year mean
> temperatures close to 0.6K below the 1866 to 1970AD mean. As this reconstruction
> only used data extending back to 1000AD, there is a considerable volume of 16th century
> data which has not been used. This will be a focus if future research.

> {\bf Acknowledgments}

>
> This work was funded by the Netherlands Environment Assessment Agency (RIVM) as part of the
> Dutch Scientific Assessment and Policy Analysis (WAB) programme.
> Additional funding was provided as follows:
> from the UK Natural Environment Research Council for M.N. Jukes,
> from the Swedish Research Council for A. Moberg.

> \vfill\ject

>
> \def\thesection{A}
> {\bf Appendix A: Regression methods}

>
> Ideally, the statistical analysis method would be determined by the
> known characteristics of the problem. Unfortunately, the error
> characteristics of the proxy data are not sufficiently well
> quantified to make the choice clear.
> This appendix describes two methods and the statistical models which can be
> used to motivate them.

>
> \subsection{Inverse regression [INVR]}

>
> Suppose x_{ik} , $i=1, N_{pr}$, $k=1, L$ is a set of N_{pr}
> standardised proxy records of length L and that we are trying
> to obtain an estimate \hat{y}_i of a quantity y_i which is
> known only in a calibration period (i in C).

>
> Several "optimal" estimates of y_i can be obtained, depending on
> the hypothesised relation between the proxies and y .

>
> Inverse regression follows from the model

> \$\$

> $\beta_i y_k + \mathcal{N}$

> =

> x_{ik}

> \$\$

> where \mathcal{N} is a noise process, independent between proxies.
> It follows that optimal estimate for the coefficients β_i are

> \$\$

> $\hat{\beta}_i = \frac{\sum_{k \in C} x_{ik} y_k}{\sum_{k \in C} y_k^2}$

> .

> \$\$

> Given these coefficients, the optimal estimate of the y_k outside
> the calibration period is

> \$\$

> $\hat{y}_k = \frac{\sum_i \hat{\beta}_i x_{ik}}{\sum_i \hat{\beta}_i^2}$.

> \$\$

>
> \subsection{Composite plus variance matching [CVM]}

>
> This method is rather easier. It starts out from the hypothesis that different
> proxies represent different parts of the globe. A proxy for the global mean
> is then obtained as a simple average of the proxies:

> \$\$

> $\overline{x}_k = N_{pr}^{-1} \sum_i x_{ik}$

> .

> \$\$

>
> Suppose

> \$\$

> $\overline{x}_k = \beta y_k + \mathcal{N}$

> ,

> \$\$

> then an optimal estimate of β is easily derived as

> $\hat{\beta} = \frac{\sum_{k \in C} \overline{x}_k y_k}{\sum_{k \in C} y_k^2}$.

> However, $y_k^* = \hat{\beta}^{-1} \overline{x}_k$ is not an optimal estimate
> of y_k .

>
> Because of the added noise, \overline{x}_k is generally an overestimate
> of βy_k . To correct for this we should use:

> \$\$

> $\beta y_k^* = \overline{x}_k$

> $\sqrt{\frac{\beta^2 \sigma_y^2}{\beta^2 \sigma_y^2 + \sigma_{\mathcal{N}}^2}}$

> ,

> \$\$

> where σ_y^2 and $\sigma_{\mathcal{N}}^2$ are the expected variance of y and the
> respectively.

> This leads to an estimate:

> \$\$

> $y_k^{*} = \overline{x_k} \left(\frac{\sigma_y}{\sigma_x} \right)$

> .

> \$\$

> This is known as the variance matching method because it matches the

> variance of the reconstruction with that of observations over

> the calibration period.

>

> \def\thesection{B}

> \setcounter{subsection}{0}

> {\bf Appendix B: Statistical tests}

>

>

> \subsection{Tests for linear relationships}

>

> The simplest test for a linear relationship is the anomaly correlation

> (also known as: Pearson Correlation, Pearson's product moment correlation, R^2 ,

> product mean test):

> \be

> $R = \frac{\overline{y^{\prime} x^{\prime}}}{\sqrt{\overline{y^{\prime 2}} \overline{x^{\prime 2}}}}$

> \ee

> where the over-bar represents a mean over the data the test is being applied to,

> and a prime a departure from the mean

> \citep{pearson1896}.

>

> The significance of an anomaly correlation can be estimated using the

> t statistic:

> \be

> $t = \frac{R \sqrt{n-2}}{\sqrt{1-R^2}}$

> \ee

> where n is the sample size (for independent variables).

> Two Gaussian variables will produce a t statistics which obeys the

> Student's t -distribution of $n-2$ degrees of freedom.

>

> Ideally, if the noise affecting all the x and y values is independent,

> n is simply the number of measurements. This is unlikely to be the case,

> so an estimate of n is needed. The Monte-Carlo approach is more

> flexible: a large sample of random sequences with specified correlation

> structures is created, and the frequency with which the specified

> R coefficient is exceeded can then be used to estimate its significance.

>

> \subsection{Lag correlations}

>

> Following \cite{hosking1984}, a random time series with a specified

> lag correlation structure is obtained from the partial correlation coefficients,

> which are generated using Levinson-Durbin regression.

>

> It is, however, not possible to generate a sequence matching an arbitrarily

> specified correlation structure and there is no guarantee that an

> estimate of the correlation structure obtained from a small sample will

> be realizable. It is found that the Levinson-Durbin regression diverges

> when run with the lag correlation functions generated from the \cite{jones_etal1986}

> northern hemisphere temperature record and also that from the HCA composite.

>

> For the northern hemisphere temperature record, this is resolved by truncating the regression after $n=50$.

> The sample lag-correlation coefficients are, in any case, unreliable beyond this point.

> Truncating the regression results in a random sequence with a lag correlation fitting that

> specified up to lag 50 and then decaying.

> For the HCA composite, the sample lag-correlation, $C(n)$, is scaled by $\exp(-0.0001 n)$,

> where n is the lag in years.

>

> {\bf Appendix C: Acronyms}

```

>
> Table 4 shows a list of acronyms used in this paper.
> \begin{table}
> \begin{tabular}{|l|p{12cm}|}
> \hline
> ABD & Age Band Decomposition tree ring standardisation method \cr
> \hline
> CSM & Climate System Model: A coupled ocean-atmosphere climate model produced by NCAR,
> \http://www.cgd.ucar.edu/csm/ \cr
> \hline
> CFM & Climate Field Reconstruction: method for reconstructing spatial structures
> of past climate variables using proxy data \cr
> \hline
> CVM & Composite plus Variance Matching reconstruction method \cr
> \hline
> ECHO-G & Hamburg coupled ocean-atmosphere climate model \cr
> \hline
> EOF & Empirical Orthogonal Component \cr
> \hline
> INVR & Inverse Regression reconstruction method \cr
> \hline
> IPCC & The Intergovernmental Panel on Climate Change, established by the
> World Meteorological Organization (WMO) and the United Nations Environment Programme (UNEP)
> to assess scientific, technical and socio-economic information relevant for the understanding of climate change, its potential impacts and options for adaptation and mitigation. It is open to all Members of the UN and of WMO. \cr
> \hline
> ITRDB & International Tree-Ring Data Bank, maintained by the NOAA Paleoclimatology
> Program and World Data Center for Paleoclimatology (www.ncdc.noaa.gov/paleo) \cr
> \hline
> MWP & Medieval Warm Period \cr
> \hline
> PC & Principal Component \cr
> \hline
> RCS & Regional Curve Standardisation tree ring standardisation method \cr
> \hline
> \end{tabular}
> \caption{Acronyms used in the text}
> \end{table}
>
> \bibliographystyle{egu}%
> \bibliography{citations,extras}
>
> \vfill\ejct
> \begin{figure*}[h]
> %% produced by idl/mitrie/plot_recon.pro
> \centering{\includegraphics[width=12cm]{cpd-2006-xxxx-f01}}
> \caption{\label{fig:1}}
> Various reconstructions. With mean of 1900 to 1960 removed.
> }
> \end{figure*}
>
> \vfill\ejct
> \begin{figure*}[h]
> %% produced by idl/paleo/mbh_70.pro
> \centering{\includegraphics[width=12cm]{cpd-2006-xxxx-f02}}
> \caption{\label{fig:2}}
> Data blocks for PC calculation by MBH1998. Each of the 212 data series is shown as a horizontal
> line over the time period covered. The dashed blue rectangles indicate some of the blocks of data
> used by MBH1998 for their proxy principal component calculation, using fewer series for longer time
> periods. The red rectangle indicates the single block used by MM2003, neglecting all data prior
> to 1619.
> }
> \end{figure*}
>
>
> \vfill\ejct

```

```

>\begin{figure*}[h]
>%% produced by pylab/do_eof.py
>\centering{\includegraphics[width=12cm]{cpd-2006-xxxx-f03}}
>\caption{\label{fig:3}}
>First Principal Component of the North American proxy record collection, following MBH1998.
>The black line is the MBH1998 archived version.
>The other lines differ only in the method of standardisation of series prior to calculation of the
>principal components.
>Red: calculated following the MBH1998 method, the individual series have the mean of the calibration
>period removed and are normalised by the variance of the detrended series over that period;
>Blue: with the mean of the whole series removed, and normalised with the variance of the whole series.
>Green: mean removed but no normalisation.
>}
>\end{figure*}
>
>
>\vfill\eject
>\begin{figure*}[h]
>%% produced by pylab/plot_regc.py
>\centering{\includegraphics[width=12cm]{cpd-2006-xxxx-f13}}
>\caption{\label{fig:4}}
>Reconstruction back to 1000, calibrated on 1856 to 1980 northern hemisphere temperature,
>using the MBH1999 proxy data collection.
>The MBH1999 NH reconstruction and the Jones et al. (1986) instrumental data are shown for comparison.
>All data have been smoothed with a 21-year running mean.
>}
>\end{figure*}
>
>
>\vfill\eject
>\begin{figure*}[h]
>%% produced by pylab/plot_regc.py
>\centering{\includegraphics[width=12cm]{cpd-2006-xxxx-f12}}
>\caption{\label{fig:5}}
>As Fig.~4, but using the MBH1998 data collection back to 1400AD.
>}
>\end{figure*}
>
>
>\vfill\eject
>\begin{figure*}[h]
>%% produced by pylab/plot_regc.py
>\centering{\includegraphics[width=12cm]{cpd-2006-xxxx-f10}}
>\caption{\label{fig:6}}
>Reconstruction back to 1000AD, calibrated on 1856 to 1980 northern hemisphere temperature,
>using a composite and variance matching,
>for a variety of different data collections.
>The MBH1999 and HPS2000 NH reconstructions and the Jones et al. (1998) instrumental
>data are shown for comparison.
>Graphs have been smoothed with a 21-year running mean and centered on 1866 to 1970.
>The maximum of the 'Union' reconstruction in the pre-industrial period (0.227K, 1091AD) is shown
>by a short cyan bar, the maximum of the instrumental record (0.841K, 1998AD) is shown as a
>short purple bar.
>}
>\end{figure*}
>
>
>\vfill\eject
>\begin{figure*}[h]
>%% produced by pylab/plot_regc.py
>\centering{\includegraphics[width=12cm]{cpd-2006-xxxx-f11}}
>\caption{\label{fig:7}}
>As Fig.~6, except using inverse regression.
>}
>\end{figure*}
>

```

```
> \vfill\eject
> \begin{figure*}[h]
> %% produced by pylab/plot_regc.py
> \centering{\includegraphics[width=12cm]{cpd-2006-xxxx-f14}}
> \caption{\label{fig:8}}
> Lag correlations for proxy composites and instrumental record (gray).
> }
> \end{figure*}
>
> \vfill\eject
> \begin{figure*}[h]
> %% produced by pylab/plot_regc.py
> \centering{\includegraphics[width=12cm]{cpd-2006-xxxx-f09}}
> \caption{\label{fig:9}}
> The ``Union" reconstruction, using `composite plus variance scaling', for the
> calibration period. Also shown is the level of the maximum plus two standard errors.
> The Jones and Mann instrumental data is plotted as a dashed line.
> }
> \end{figure*}
>
> \end{document}
> diliberate bad speling
>
> \vfill\eject
>
> {\it\small
> Both these questions could be answered by a detailed knowledge of the
> climate and its forcings over the past 1000 years, but the detailed
> instrumental record only extends back to 1856. Hence ... []]
>
> %%The motivation for the study of past climate variability is twofold:
> Current projections of future climate change are still burdened with
> some level of uncertainty, even within a particular scenario of future
> greenhouse concentrations. Although all climate models simulate an
> increase of global temperatures in this century, the range of warming
> simulated by different models still covers a wide range \citep{IPCC2001}.
> A much pursued goal is to reduce this uncertainty range.
> A question is whether warming of magnitude similar to that observed in the
> 19th and 20th centuries, very likely caused at least to a large part by
> anthropogenic greenhouse gas, has also occurred in the preindustrial recent past,
> when, to a large extent, only natural forcings of the climate system were active.
>
> {\small\it Reconstructions of the climate of the past millennium can help us to
> answer the second point by describing the magnitude of
> global temperature fluctuations in the past and can address the first
> point by helping to quantify the climate sensitivity: the
> ratio of the response to the forcing.)
> Progress in both questions can be achieved through the analysis
> of reconstructions and simulations of the climate of the past millennium:
> firstly,
> we wish to know whether current high global temperatures are
> within the range of natural variability. Secondly, we wish to
> evaluate the skill and reliability of climate models.
> %%The rise in global mean temperatures since then is
> Therefore, some form of empirical reconstruction based on early-instrumental
> records, documentary evidence and proxy data is needed.
> %%On the other hand,
> %%the global warming observed in the past 2 centuries may be partly
> %%due to the recovery from an extended
> %%period of anomalously low temperatures which was reflected
> %%in a large number of indirect European records.
> %%[omit above sentence (AM)??]
> %%[justify "recovery" (JE)??]
> %%[was it really gradual (JE)]
> %% "gradual deleted": jones and mann suggest that hemispheric mean cooling trend
```

> %% is "relatively steady" in contrast to more episodic cooling in Europe,
> %% but esper etal (2002) suggests that attributing this difference to
> %% hemisphere vs. europe is wrong, it might be whole hemisphere vs. extra-tropical,
> %% or it might be failure to resolve variability.
> %[check]: copied from Gabi's email -- needs clearing up.
> %%However, some unsolved questions will remain.
> %%For instance, the climate sensitivity may depend on the nature of the external
> %%forcing (greenhouse gas, solar irradiance, etc), so that an estimation
> %%of past climate sensitivity has still to be considered with some care.
> %%There are indeed indications that climate sensitivity to changes in solar
> %%forcing is lower than to changes to greenhouse gas forcing
> %%\citep{tett_etal2005+, joshi_etal2003 }.
> %%[be more precise -- (i.e. in terms of $K W^{-1} m^2$??)]
> %::joshi etal show a 0-20% difference between sensitivity to solar forcing
> %%compared to CO2 forcing. This is much less than variability in sensitivity
> %%among models.
> %%[this is not really relevant if the difference in climate sensitivity between
> %%forcings is much less than that between, say, models]
>
> A wide range of proxy
> data sources which have been exploited for this problem
> \citep[reviewed in][]{jones_mann2004 }.
> Tree rings are a particularly important source of information
> within the time frame of the last millennium. The precise dating which
> is provided by the annual growth rings allows anomalous growth
> rates to be compared reliably with historical events.
> However, its not straightforward to retrieve the climate variability
> at timescales that exceed the typical life span of a tree (see Sect.~2.5 below).
> Statistical regression against instrumental temperature data is often used
> because the majority of proxy records cannot be directly related to temperature
> by deterministic models
> (two exceptions, reconstructions obtained from borehole temperatures
> and those based on glacial advance and retreat, are discussed below).
> Appendix A gives mathematical details of some basic statistical measures.
> The measures of skill used by MBH1998, MBH1999 are the
> R^2 test, which measures the degree of coherence between two data
> sets, and the "Reduction of Error" (RE) statistic, which measures the
> effectiveness of one series (typically a model or prediction)
> in explaining the total (i.e. including the mean) variance in another (the verification data).
>
> The statistical tests on these measures of skill are described
> in many text books, and their application is straight forward
> when all sources of noise contaminating the
> data are well characterised. The difficulty which arises
> in many applications, including climate reconstructions, is that
> the noise has significant but poorly characterised correlations.
> %%[is this true for tests of skill -- probably not for analytical tests of RE]]
> }
> \vfill\ejct
> \vfill \ejct
>
> The B\urger et al. analyses use a collection of pseudo-proxies created from
> pseudo observations of a climate simulation with added white noise.
> This is a pragmatic approach -- there is little reliable information about
> the true nature of the noise spectrum. It has been suggested that bristlecone pines
> in N. America have an anomalous growth trend in the 20th century which is
> coherent among that species. The inverse regression algorithm can give large
> weight to individual proxies and negative weight to others: this may be
> correct in some circumstances, but in others it could amplify the error.
> The composite approach, on the other hand, is robust:
> simply taking the mean of the available proxies does not rely on
> specific assumptions about the noise spectrum.
>
> \vfill\ejct

```
> \begin{figure*}[h]
> %% produced by pylab/plot_regc.py
> \centering{\includegraphics[width=12cm]{figz/c_var_nh_reconc_10_1000_c}}
> \centering{\includegraphics[width=12cm]{figs/cpd-2006-xxxx-f04}}
> \caption{\label{fig:1}}
> As Fig.~7, except
> using composite and variance matching.
> }
> \end{figure*}
>
> \vfill\eject
>
> \begin{figure*}[h]
> %% produced by pylab/plot_regc.py
> \centering{\includegraphics[width=12cm]{figz/c_var_nh_reconc_10_1000_c}}
> \centering{\includegraphics[width=12cm]{figs/cpd-2006-xxxx-f04}}
> \caption{\label{fig:1}}
> The ``Union" reconstruction, using `composite plus variance scaling', for the
> calibration period. Also shown is the level of the maximum plus two standard errors.
> The Jones and Mann instrumental data is plotted as a dashed line.
> }
> \end{figure*}
>
>
> Willmott, C.J., 1981. On the validation of models. Phys. Geog., 2, 184-194
>
>
> {\bf A2: Principal Components}
>
> Principal component analysis is a standard technique for reducing the
> volume of data while attempting to retain as much of the variability
> of the original data as possible.
>
> Stage (2) establishes an empirical link between the proxy records and
> temperature. In MBH1998 inverse least squares regression of the
> proxy network against the principal components of the measured temperature field,
> over the period 1902 to 1980, is used.
>
> Stage (3), the verification stage, determines how many, if any, of the
> reconstructed time series for the principal components can be
> considered to have some descriptive value. This is done by evaluating the
> fit of the implied fields to the observations in the verification period, 1856 to 1901.
> The northern hemisphere mean temperature is calculated from the
> The uncertainties are calculated from the residuals to the fit in the calibration period.
>
> \citet{mcintyre_mckitrick2005c} assert that the fact that omission of data
> led to a different result demonstrates that the method is unreliable.
> This would be true if the computation of a time series were the
> end point of the analysis. However, the need to verify the computed series
> was recognised by MBH1998. This is discussed further below.
>
> \subsection{Spurious metaphors}
>
> The term ``hockey-stick" has become widely used, particularly in the US
> media, to refer to the temperature history implied by the MBH1999
> temperature reconstruction. It did not originally apply to the reconstruction
> itself, which has a relatively minor temperature increase in the early
> 20th century, but rather to the combination of this series with the
> more recent observed temperature trends: the combination shows
> a dramatic increase in the 20th century, substantially greater than anything
> that occurred in the past millennium.
> The first attempt to attach any scientific meaning to the phrase
> was with the introduction of a ``hockey stick index"
> \citet{mcintyre_mckitrick2005a} (hereafter MM2005).
```

- > This index is defined in terms of the ratio of the variance at the end of a time series
- > to the variance over the remainder of the series.
- > MM2005 argue that the way in which
- > a principal component analysis is carried out in MBH generates an artificial
- > bias towards a high "hockey-stick index" and that the statistical significance of
- > the MBH results may be lower than originally estimated.
- > % and that this is responsible for the
- > % shape in the MBH temperature reconstruction.
- > % Martin: I think that what MM05 indicate is that "hockey-stick may arise from random time series more easily as previously thought, when using the decentered PCs. I am not sure if they make this decentering responsible for the final output in MBH.
- > %
- > \subsection{Validation}
- >
- > As noted above, MM2003 have shown that removing data
- > degrades the result, as might be expected.
- > Among the adjustments which they characterize as "corrections" was the
- > omission of the 3 principal components mentioned above.
- > In fact, 70% of the 90 time series extending back to
- > 1400 are omitted from their analysis.
- >
- > In principle, it would be possible to estimate the accuracy of
- > reconstructions calculated by regression from the data in the
- > calibration period. However, this calculation can easily be biased
- > by unreliable assumptions about the noise covariances within
- > the calibration period.
- > MBH1998, 1999 follow a more robust approach, using independent
- > data from a validation period (1856 to 1901) to,
- > firstly, determine whether a reconstruction has any relation to temperature
- > and, secondly, estimate the error variance.
- >
- > MM2003, however, omitted the validation phase.
- > \citep{wahl_ammann2005} have carried out a detailed investigation
- > of the robustness of the MBH1998 technique to address this
- > and many other issues. They find that the MM2003 series fails the
- > validation tests used by MBH1998.
- >
- > As an illustration of the robustness of the reconstruction,
- > figures 5 and 6 shows a reconstructions made using the MBH1999 and MBH1998 data respectively.
- > Regression against northern hemispheric mean temperature is used
- > instead of regression against principal components of
- > temperature. There are differences, but key features remain.
- > [[more details in appendix and/or supplementary materials]]
- > MM2003 draw attention to the fact that one time series,
- > "CANA036" in the ITRDB classification, contributed
- > by Gasp'le, appears twice in the MBH1998 database.
- > This error is corrected in the red dashed curve of Fig.~5,
- > which is almost identical to the green curve, which retains the duplication.
- >
- > With our simplification of the method it is possible
- > to use the entire instrumental record for calibration.
- > This leaves no data for validation, but the difference
- > between this and a reconstruction based on a shorter
- > period gives some idea of the robustness.
- > Figure 4b shows the result.
- >
- > Finally, MM question the calculation of uncertainty limits.
- > This depends on the number of degrees of freedom
- > assigned to the data. MM state that the standard method used
- > by MBH is wrong, and that a lower number of degrees of
- > freedom is appropriate because of long range correlations in
- > the data. MBH use the lag-one autocorrelation to estimate
- > the degrees of freedom.
- >
- > In all such tests it is necessary to remember the distinction between the
- > sample correlation, which one is forced to deal with, and

> the actual correlation, we cannot know exactly. For this reason
> it is generally unwise to use methods which rely on statistics
> which cannot be estimated robustly in a small sample.
>
> MM05 also confuse the auto-correlation structure of the tree-ring data,
> which are known to have an environmental signal with correlations
> on at least the decadal time-scale, with the auto-correlation of the
> residuals which should be used in estimating the noise structure.
> \vfill\eject
> \begin{figure*}[h]
> %% produced by idl/paleo/mbh_70.pro
> \centering{\includegraphics[width=12cm]{cpd-2006-xxxx-f03}}
> \caption{\label{fig:1}}
> Data blocks for PC calculation by MBH.
> }
> \end{figure*}
>
>
>
> \subsection{Natural variability and forcings}
>
> Global temperature can fluctuate through natural internal variability of
> the climate system (as in the El Ni\~no phenomenon), through
> variability in natural forcings (solar insolation, volcanic aerosols,
> natural changes to greenhouse gas concentrations) and human changes.
>
> Analysis of the physical links between the estimated temperature changes
> of the past millennium and estimated variations in the
> different forcing mechanisms can give improve our understanding of those
> mechanisms and help to validate the estimated temperature and
>
> \cite{goosse_etal2005} investigate the role of natural variability using
> an ensemble of 25 climate model simulations of the last millennium
> and forcing estimates from \cite{crowley2000}.
> They conclude that natural variability dominates local and regional
> scale temperature anomalies, implying that most of the variations
> experienced by a region such as Europe over the last millennium could
> be caused by natural variability. On the hemispheric and global scale, however, the
> external forcing dominates.
> This reinforces similar statements made by JOS1998. \cite{goosse_etal2005}
> make the new point, that noise can lead to regional temperature anomalies
> peaking at different times to the forcing, so that disagreements in
> timing between proxy series should not necessarily be interpreted as meaning there
> is no common forcing.
>
> Analysis of natural climate forcings \cite{crowley2000}
> show that changes in atmospheric aerosol content due to changes
> in volcanic activity and changes in solar irradiance
> can explain this long term cooling through most of the millenium,
> shown by paleoclimate reconstructions,
> and the observed warming in the late 19th century.
> \cite{hegerl_etal2003} analyse the correlations between four
> reconstructions (MBH1999, BOS2001, ECS2002, and a modified version of CL2000)
> and estimated forcings \cite{crowley2000}.
> They find that that natural forcing, particularly by
> volcanism, explains a substantial fraction of decadal variance, also in
> new high-variance reconstructions. Greenhouse gas forcing is detectable
> with high significance level in all analyzed reconstructions analyzed.
> \cite{weber2005b} carries out a similar analysis with a wider range
> of reconstructions.
> It is shown that the correlation between reconstructed
> global temperatures and forcings are similar to those derived from
> the ECBILT climate model \cite{opsteegh_etal1998}.
> The trend component over the period 1000 to 1850 is, however, larger in the
> reconstructions compared to the forcings.

>
> The methods employed by
> \citet{hegerl_etal2006+} attribute about a third of the early 20th century warming, sometimes
> more, in high-variance reconstructions to greenhouse gas forcing.
> These results indicate that enhanced variability in the past does not
> make it more difficult to detect greenhouse warming, since a large
> fraction of the variability can be attributed to external forcing.
> Quantifying the influence of external forcing on the proxy records is
> therefore more relevant to understanding climate variability and its
> causes than determining if past periods were possibly as warm as the
> 20th century.
>
> The dominance of volcanic forcing over solar variability found in some of the
> above studies is consistent with recent questioning of the
> magnitude of low-frequency solar forcing \citep{lean_etal2002, foukal_etal2004}.
> \subsection{Tests of skill in reconstructions}
>
> RE: Reduction of Error
>
> \be
> RE = 1. - { \overline{ (y - \hat{y}^{\prime})^2 } \over
> \overline{ y^2 } }
> \ee
>
>
--

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</x-flowed>

Attachment Converted: "c:\eudora\attach\McIntyre2003.pdf"

From: Keith Briffa <k.briffa@uea.ac.uk>

To: cbaisan@dakotacom.net

Subject: Re: help with an idea?

Date: Wed Aug 9 15:05:58 2006

Dear Chris

just wondering what became of my forwarded request (from you to Tony) ? Have not received any feedback and still anxious to follow this up

cheers

Keith

At 15:53 17/10/2003, you wrote:

Keith,

I am inclined to forward your note to Tony Caprio - any objections?

He has the best temperature sensitive foxtail pine material I am aware of.

I have some sense that there is a change in regional climate patterns prior to 1000AD in the western US. Not sure what or why...

Matt Salzer and Malcolm Hughes are working on 3k yr material from temperature sensitive upper tree-line sites in the west.

John King knows a great deal about the Sierra collections and data.

MaryBeth Keifer and Andrea Loyd-Faste collected the Sierra Foxtail you referred to.

Chris B.

> Hi Lisa and Chris and Ed

>

> The first point of this message is to ask for access to the raw data
> for the Boreal and Camp Hill Foxtail pine chronologies (Lisa) that I
> believe you and/or your students produced and similar data that you
> may have (Chris). for the area inland of the Santa Barbara Basin ,
> California. I am also trying to stimulate your interest and hopefully
> start a joint collaboration (Lisa , Chris and Ed). Please allow me to
> explain . I was reading some papers on the putative link between North
> Atlantic temperatures (oxygen isotope record from Greenland) and
> climate (bio-turbation index) in the Santa Barbara basin , on the
> 1000-year time scale (papers by Boyle and Leuschner et al. in the
> PAGES QSR Volume published in 2000). It got me to thinking whether a
> robust regional temperature chronology for North west Scandinavia
> might show any associations with any climate factors as represented in
> either high or low elevation tree-ring chronologies in Western

> California , at higher temporal resolution (perhaps decades to
> century) - and hence whether there is any evidence for a thermohaline
> link (or other more direct dynamic atmospheric connection) operating
> on various time scales. Of course there are problems with what
> specific climate response one would investigate (in terms of season
> and variable). However, as a first look I compared our Tornetrask
> temperature reconstruction (JJA in Northern Sweden) with a (very) few
> series I had for the west US - among which were the chronologies
> mentioned above from AD 800 that Jan Esper and Ed produced for their
> Science paper, using data supplied by Lisa I believe .
> Now I don't actually like the general way they applied the RCS (-
> using
> a very large scale standardisation curve based on disparate data from
> a very wide expanse of sites across the Northern Hemisphere - but as
> Ed might say " it seems to work "). However, the association between
> the Tornetrask series and the curves for Boreal/ Upper Wright have
> stimulated me to try to look deeper and solicit your interest and
> help. In my opinion, for the 600-year period between AD 1100 and 1700
> the similarity in the 5 circa 120-year cycles that make up these
> series certainly warrant serious further study. The similarity is not
> apparent before this but the two California series themselves show
> little agreement in the earlier 300 years of data that I have seen,
> implying that the common signal at the regional level may not be well
> represented in either anyway. This could be a standardisation issue
> though. By producing more robust mean series and especially by
> extending the series back before the post Christian era we could
> significantly extend the power of the comparison. I would like to
> establish well replicated series (using more-local RCS curves based
> applied to more, and longer, data) for both the Tornetrask (and
> possibly Northern Finnish) region and the combined set from Upper
> Wright and Boreal and any other nearby Foxtail data (from the region
> of the 118 degrees west 36 degrees north) . We have earlier (than
> circa AD 800) data for Tornetrask and Finland , showing good inter
> region coherence . If we can establish stronger evidence of a North
> Atlantic/Eastern Pacific link (at different time scales perhaps) we
> can look at other high resolution records to establish the nature of
> the likely forcing and the possible climate dynamic mechanisms. What
> do you think? Can I play with your data to this end ? Whatever you
> think , I would appreciate it if you would treat this as confidential
> and any thoughts on the idea , or pointers to relevant data sets are
> still welcome.
> All the very best
> Keith

>
> --
> Professor Keith Briffa,
> Climatic Research Unit
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>
> Phone: +44-1603-593909
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> [1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

>
) :))))))))))))))))))) .))
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tel: 520-621-7681
Fax: 520-621-8229
) .))))))))) .))))))))) .)))))))))

--
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References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Hans von Storch <hvonstorch@web.de>
Subject: Re: open data access?
Date: Fri Aug 11 17:57:15 2006

Hans
just too bogged down with stuff to even read their crap - but I have no intention of withholding anything. Will supply the stuff when I get five minutes!! no idea what the so-called update stuff is about

Keith
At 11:19 05/08/2006, you wrote:

Dear Keith,
I read this comment on the prometheus-weblog of Roger Pielke jr:
"Ask Briffa for site identifications for Briffa et al 2001? While you're at it, ask him for the measurement data for Taimyr, Tornetrask update and Yamal? Ask Briffa why he didn't publish the updated Polar Urals results."

The background of this inquiry seems to be the replicability of your studies. I think this is a reasonable request, but some people claim that you would "stonewall" any such attempts. ("The issue of data access was discussed in the dendro conference in Beijing - some people suggesting that withholding data was giving the trade a black eye. Industry leaders, such as presumably Briffa, said that they were going to continue stonewalling.") I can not believe this claim, and I would greatly appreciate if you would help me to diffuse any such suspicions.

As you possibly have heard, I had a chance to hear a lot what is said on Capitol Hill (see attachment) - and I am concerned if we do not apply a truly open data and algorithm-policy, our credibility will be severely damaged, not only in the US but also in Europe. "Open" means also to provide data to groups which are hostile to our work - we have done so with our ECHO-G data, which resulted in two hostile comments in "science", which were, however, useful as they helped to clarify some issues.

All the best,

Hans

--

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presently: Kaspervej 2, 4673 Rödving, Danmark
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References

1. <http://w3g.gkss.de/staff/storch>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Hans von Storch <hvonstorch@web.de>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: open data access?
Date: Fri, 11 Aug 2006 21:32:50 +0200
Cc: Hans von Storch <hans.von.storch@gkss.de>, Hans Graf
<hfg21@cam.ac.uk>

Dear Keith,

I think we have to take this talking and questioning seriously. what we do is important and we have to allow for replication. when we were confronted with such requests concering the ERIK-simulations, we were initially reluctant, but now we gove teh data to verybpody. Got us two critical comments in "science" but I think it was worth it.

Do you mind if I publish your response? Would be the prometheus weblog. I could ask what is meant with "update" - I do know not what is meant; I had just quoted a request which I find in principle not unreasonable - and I am happy to hear that you in principle agree.

Regards,
Hans

> -----Ursprⁿgliche Nachricht-----
> Von: Keith Briffa <k.briffa@uea.ac.uk>
> Gesendet: 11.08.06 18:57:25
> An: Hans von Storch <hvonstorch@web.de>
> Betreff: Re: open data access?

> Hans
> just too bogged down with stuff to even read
> their crap - but I have no intention of
> withholding anything. Will supply the stuff when
> I get five minutes!! no idea what the so-called update stuff is about
> Keith
>
> At 11:19 05/08/2006, you wrote:
> >Dear Keith,
> >
> >I read this comment on the prometheus-weblog of Roger Pielke jr:
> >
> >"Ask Briffa for site identifications for Briffa
> >et al 2001? While you're at it, ask him for the
> >measurement data for Taimyr, Tornetrask update
> >and Yamal? Ask Briffa why he didn't publish the updated Polar Urals
> >results."
> >
> >The background of this inquiry seems to be the
> >replicability of your studies. I think this is a
> >reasonable request, but some people claim that
> >you would "stonewall" any such attempts. ("The

> >issue of data access was discussed in the dendro
> >conference in Beijing - some people suggesting
> >that withholding data was giving the trade a
> >black eye. Industry leaders, such as presumably
> >Briffa, said that they were going to continue
> >stonewalling.") I can not believe this claim,
> >and I would greatly appreciate if you would help
> >me to diffuse any such suspicions.
> >
> >As you possibly have heard, I had a chance to
> >hear a lot what is said on Capitol Hill (see
> >attachment) - and I am concerned if we do not
> >apply a truly open data and algorithm-policy,
> >our credibility will be severly damaged, not
> >only in the US but also in Europe. "Open" means
> >also to provide data to groups which are hostile
> >to our work - we have done so with our ECHO-G
> >data, which resulted in two hostile comments in
> >"science", which were, however, useful as they helped to clarify some
issues.
> >
> >All the best,
> >Hans
> >
> >
> >--
> >Hans von Storch
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Attachment Converted: "c:\eudora\attach\smime2.p7s"

From: "Wahl, Eugene R" <wahl@alfred.edu>
To: "Keith Briffa" <k.briffa@uea.ac.uk>
Subject: RE: confidential
Date: Sat, 12 Aug 2006 13:02:44 -0400

Hi Keith:

Thanks so much for the chance to look over this section. I think the long section you added on pp 6-5 and 6-6 reads well, and makes good sense according to what I know. Indeed, reading the whole section is a good review for me!

I suggested addition of a phrase in lines 32-33 on page 6-3 regarding MM 2003 and analysis of it by Wahl-Ammann 2006. I also suggest a (logically useful) change from singular to plural in line 42 of that page. The changes are in RED/BOLD font.

[I should note that AW 2006 is still in "in press" status, and its exact publication date will be affected by publication of an editorial designed to go with it that Caspar and I are submitting this weekend. Thus I cannot say it is certain this article will come out in 2006, but its final acceptance for publication as of 2/28/06 remains completely solid.]

Also, I added the full information for the Wahl-Ritson-Ammann 2006 Science article in the references section, also in RED/BOLD font.

By the way, is the "AJS" NCAR-CSM model in Fig. 6-13 the one Caspar did? I couldn't tell this for sure from the information in the text. If it is, perfect. If not, is there a way to include his millenium run?

Thanks to you and all the authors for you painstaking work.

Peace, Gene
Dr. Eugene R. Wahl
Asst. Professor of Environmental Studies
Alfred University

607-871-2604
1 Saxon Drive
Alfred, NY 14802

From: Keith Briffa [mailto:k.briffa@uea.ac.uk]
Sent: Mon 7/31/2006 10:29 AM
To: Wahl, Eugene R
Subject: RE: confidential

First Gene - let me say that I never intended that you should spend

so much time on this - though I really appreciate your take on these points. The one you highlight here - correctly warns me that in succumbing to the temptation to be lazy in the sense of the brief answer that I have provided - I do give an implied endorsement of the sense of the whole comment. This is not, of course what I intended. I simply meant to agree that some reference to the "divergence" issue was necessitated . I will revise the reply to say briefly that I do not agree with the interpretation of the reviewer. I am attaching what I have done (see blue highlighting) to the section in response to comments (including the addition of the needed extra section on the "tree-ring issues" called for by several people). I have had no feedback yet on this as it has not been generally circulated , but thought you might like to see it. PLEASE REMEMBER that this is "for your eyes only " . Please do NOT feel that I am asking /expecting you to go through this in any detail - but given the trouble you have taken,I thought it reasonable to give you a private look. Cheers
Keith

At 07:16 27/07/2006, you wrote:

>Hi Keith:

>

>Here is the text with my comments. I will go over the "stolen"
>parts (highlighted in blue outline) for a final time tomorrow
>morning, but I wanted to get this to you ASAP. The main new point I
>have to make is added in bold/blue font on pp. 101-103. I question
>the way the response to the comment there is currently worded, as it
>seems to imply that the divergence issue really does invalidate any
>dendro-based reconstructions before about 1850--which I imagine is
>not what you would like to say. I give a series of arguments
>against this as a general conclusion. Maybe I got over-bold in
>doing so, as in my point (1) I'm examining issues that are at the
>very core of your expertise! Excuse me that one, but I decided to
>jump in anyway. Let me know if I got it wrong in any way!

>

>There are other quite minor suggestions (mostly focused on
>referencing other responses in a few places) that are also in
>bold/blue. These go on into the "120's" in terms of page numbers.

>

>This is really a lot of work you've taken on, and I REALLY
>appreciate what you and the others are doing!

>

>[I've also been a lot involved with helping to get a person from the
>Pew Center for Global Climate Change ready to testify in front of
>the House Energy and Environment Committee tomorrow. That is why I
>couldn't get this done and sent to you earlier today. Send Mike
>Mann and Jay Gulledge (Pew Center) all good thoughts for strength and
clarity.]

>

>

>NB -- "r" towards the end of the filename stands for my middle initial.

>

>

>Peace, Gene

>Dr. Eugene R. Wahl
>Asst. Professor of Environmental Studies
>Alfred University

>
>607-871-2604
>1 Saxon Drive
>Alfred, NY 14802

>
>_____

>From: Keith Briffa [mailto:k.briffa@uea.ac.uk]
>Sent: Mon 7/24/2006 3:16 PM
>To: Wahl, Eugene R
>Subject: RE: confidential

>
>
>
>
>

>Gene

>here is where I am up to now with my responses (still a load to do) -
>you can see that I have "borrowed (stolen)" from 2 of your responses
>in a significant degree - please assure me that this OK (and will not
>later be obvious) hopefully.

>You will get the whole text(confidentially again) soon. You could
>also see that I hope to be fair to Mike - but he can be a little
>unbalanced in his remarks sometime - and I have had to disagree with
>his interpretations of some issues also.

>
>Please do not pass these on to anyone at all.
>Keith

>
>
>

>Will pass all comments to you before they are fixed in stone- nothing
>from review article will be mentioned.

>Really grateful to you - thanks
>Keith

>
>At 05:08 22/07/2006, you wrote:

> >Hi Keith:

> >

> >Glad to help. (!)

> >

> >If I could get a chance to look over the sections of my text you
> >would post to the comments before you do, I would appreciate it. If
> >this is a burden/problem let me know and we'll work it out.

> >

> >If it is anything from the Wahl-Ammann paper, of course that is fine
> >to use at once since it is publicly available. There will only be
> >exceedingly minor/few changes in the galleys, including a footnote
> >pointing to the extended RE benchmarking analysis contained in the
> >Ammann-Wahl review article.

> >

> >What I am concerned about for the time being is that nothing in the
> >review article shows up anywhere. It is just going in, and
> >confidentiality is important. The only exception to this are the
> >points I make in my blue comments in the big review file on page
> >104, concerning the MM way of benchmarking the RE statistic. Those
> >comments are fine to repeat at this point. [Please excuse my
> >hesitance in this way.]

> >

> >Actually, all the other blue comments I made in the big review file
> >are also fine to use at once.

> >

> >

> >Again, if this request is in any way a problem, let me know and
> >we'll figure out something.

> >

> >

> >Peace, Gene

> >Dr. Eugene R. Wahl

> >Asst. Professor of Environmental Studies

> >Alfred University

> >

> >

> >

> >From: Keith Briffa [mailto:k.briffa@uea.ac.uk]

> >Sent: Fri 7/21/2006 2:00 PM

> >To: Wahl, Eugene R

> >Subject: RE: confidential

> >

> >Gene

> >your comments have been really useful and reassuring that I am not
> >doing MM a disservice. I will use some sections of your text in my
> >comments that will be eventually archived so hope this is ok with
> >you. I will keep the section in the chapter very brief - but will
> >cite all the papers to avoid claims of bias. I really would like to
> >discuss the whole issue of the reconstruction differences at a later
> >, less stressful time. I completely accept the arguments about the
> >limitation in the r2 and the value of capturing longer-term variance
> >. I think I will have to stop now as the temp and humidity are killing
here.

> >

> >Thanks a lot again

> >

> >Keith

> >

> >At 18:39 21/07/2006, you wrote:

> > >Hi Keith:

> > >

> > >I'm sorry that there is a bit to digest...although I know it is just
> > >a result of the nature of things.

> > >

> > >By the way, copied below is a synopsis that I sent this morning to a
> > >person in DC who is working on all this with regard to the House of
> > >Representative hearings. Evidently, there is to be at least one
> > >more hearing next week, and Mike Mann will go. The person I sent

> > >this to is trying to understand the importance of the proxy PC
> > >issues --especially how, no matter what way the PC extraction is
> > >done, the reconstructions converge if the structures actually
> > >present in the data are not tossed out by truncating the number
> > >retained PCs at a too low level. What I've copied is this
> > >synopsis. I think it is straightforward -- maybe a bit dense, but
> > >at least brief.

> > >

> > >Also, let me know if I can help on the issue of RE vs r^2 . I could
> > >write a few brief sentences as something for you to look at if you
> > >would like. Wahl-Ammann show very clearly that there is objectively
> > >demonstrated skill at the low-frequency level of the verification
> > >period mean for all the MBH segments, although the earlier MBH
> > >segments do have really low r^2 values (indicating very little skill
> > >at the interannual level). Our argument that to throw out the
> > >reconstruction completely based on the fastest varying frequency,
> > >when it has objectively demonstrable meaning at lower frequencies,
> > >is to me quite reasonable. That it is some how entirely ad hoc, as
> > >McIntyre claims in one (more?) of his comments, is neither logical
> > >nor factual in my perspective. The idea of frequency dependent
> > >skill/non-skill is not new to the literature, and the independent
> > >re-reviewer that Steve Schneider had look over Wahl-Ammann said s/he
> > >had experienced this issue in his/her work. G.

> > >

> > >

> > >***** COPIED TEXT

> > >

> > >What it boils down to in the end is as follows:

> > >

> > >1) The different reference periods used to calculate proxy PCs from
> > >N. America (calibration only for MBH, full period for MM) only have
> > >the effect of re-arranging how the hockey stick shape appears across
> > >the rank ordering of PCs. In MBH it is concentrated in PC1. In the
> > >full-period method, it is spread over PCs 1 and 2. If one adds PCs
> > >1 and 2 (either arithmetically or as vectors) from either
> > >convention, you get an essentially IDENTICAL time series, only the
> > >amplitudes are a bit different. [Note that the input data were
> > >centered AND standardized before being put into the PC calculation
> > >algorithm. This is important, as shown below.]

> > > WHEN ACTUALLY USED IN THE RECONSTRUCTION, THE DIFFERENCE
> > > IS MINISCULE -- MBH is colder over 1400-1449 by 0.05 degrees!

> > >

> > >2) IF the data are centered but NOT standardized and are input into
> > >in a PCA algorithm using the variance-covariance matrix and not the
> > >correlation matrix (the way MM did it), then the hockey stick shape
> > >shows up in PC4. MM in fact reported this first in their 2005
> > >Energy and Environment article. In effect, the first two PCs are
> > >ARE ACTING TO DO THE STANDARDIZING OF THE DATA not done as a
> > >pre-processing step. [When the correlation matrix is used instead
> > >in the PCA algorithm, then the standardization is in effect done by
> > >the algorithm, because all the correlations are "standardized" by
> > >construction--they all range between 0 and 1.]

> > > When 4 PCs from this calculation method are used rather

> > > than 2 PCs calculated as above, then the RECONSTRUCTION CONVERGES
> > > TO THE SAME AS ABOVE.
> > >
> > >3) Thus, all the different "flavors" for PC extraction have
> > >essentially no effect on reconstruction when one does the exercise
> > >of adding PCs sequentially from 2 to 5 for any flavor. In the case
> > >of (1), the reconstructions converge by the second PC. In the case
> > >of (2), they converge by PC4. They don't change with higher order
> > PCs added.
> > > THIS SHOULD BE EXPECTED FROM FIRST PRINCIPLES. That is,
> > > the same underlying information is there in all cases, it is only
> > > how the structures present in these data are spread across the rank
> > > order of PCs, as explained. The simple exercise of taking the
> > > reconstructions to convergence across the number of PCs used shows
> > > this clearly.
> > >
> > >4) In fact, MM essentially say all this in the 2005 EE
> > >article--INCLUDING ABOUT THE RECONSTRUCTION RESULTS -- but they
> > >strongly claim that the movement of the hockey stick shape to the
> > >4th PC shows it is not a leading pattern of variance as MBH claim,
> > >and thus should not be used. This might be logical if their
> > >analysis was an apples-apples comparison, but it is not, due to the
> > >PCA method they use and applying it on NON-standardized data.
> > > THESE TWO DIFFERENCES (which one can only fully get
> > > from their actual code, not in the articles published) DRIVE THEIR
> > > ENTIRE ARGUMENT ON THIS PARTICULAR ISSUE. What they do not say is
> > > that convergence to something like the MBH result is expectable,
> > > and indeed MUST happen given the data used, because the hockey
> > > stick shape is actually IN the data, it is NOT an artifact of PC
> > > calculation procedure.
> > >
> > >
> > >5) FINALLY, note that all of this rests on the foundation that
> > >keeping the bristlecone pine records in the data is appropriate,
> > >which Caspar and I find can be reasonable presumption. If one
> > >believes that the bristlecone data should be removed, then the
> > >1400-1449 reconstruction does not pass verification testing with the
> > >RE statistic, and the MBH reconstruction should commence from 1450
on out.
> > >
> > >Although there are a number of reasons to keep the bristlecone data
> > >in, maybe the most compelling reason they are a NON-ISSUE is that,
> > >over the common period of overlap (1450-1980), the reconstruction
> > >based on using them from 1400-1980 is very close to the
> > >reconstruction based on omitting them from 1450-1980. Since the
> > >issues about the bristlecone response to climate are primarily about
> > >1850 onwards, especially 1900 onwards [KEITH -- PLEASE LET ME KNOW
> > >IF I AM NOT ACCURATE IN THIS], there is no reason to expect that
> > >their behavior during 1400-1449 is in any way anomalous to their
> > >behavior from 1450-1850. Thus, THERE IS NO REASON TO THINK THAT THE
> > >BRISTLECONES ARE SOMEHOW MAKING THE 1400-1449 SEGMENT OF THE MBH
> > >RECONSTRUCTION BE INAPPROPRIATELY SKEWED.
> > >
> > >

> > >***** END OF COPIED TEXT

> > >

> > >Peace, Gene

> > >Dr. Eugene R. Wahl

> > >Asst. Professor of Environmental Studies

> > >Alfred University

> > >

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> > >

> > >_____

> > >

> > >From: Keith Briffa [mailto:k.briffa@uea.ac.uk]

> > >Sent: Fri 7/21/2006 4:51 AM

> > >To: Wahl, Eugene R

> > >Subject: RE: confidential

> > >

> > >

> > >

> > >Gene

> > >thanks a lot for this - I need to digest and I will come back to
you.

> > >

> > >thanks again

> > >Keith

> >

> >--

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Attachment Converted:

"c:\eudora\attach\Ch06_SOD_Text_TSU_FINAL_2000_25jul06KRB-FJ-
RV_ERW_suggestions.doc"

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, t.osborn@uea.ac.uk, Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Fwd: Tett et al. paper
Date: Sun, 13 Aug 2006 15:32:38 -0600
Cc: IPCC-WG1 <ipcc-wg1@al.noaa.gov>

Hi Mel - thanks. Since chap 6 CA Tim Osborn is an author on this paper, I'm sure he and Keith have made the right call.

Thanks again, Peck

X-Sieve: CMU Sieve 2.2
Date: Thu, 10 Aug 2006 09:44:03 -0600

From: IPCC-WG1 <ipcc-wg1@al.noaa.gov>
X-Accept-Language: en-us, en
To: Jonathan Overpeck <jto@u.arizona.edu>,
Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Tett et al. paper

Hi Peck and Eystein,
Although the deadline for additional accepted papers has now passed, this submission comes from a CLA (Gabi Hegerl) so am forwarding on. Official acceptance of the Tett et al. paper was 2 June. My understanding is that you already have a copy, but will forward the copy sent in by Simon just in case.
Cheers,
Mel

--
~~~~~

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325 Broadway DSRC CSD08  
Boulder, CO 80305, USA  
Phone: +1 303 497 7072  
Fax: +1 303 497 5686/5628  
Email: [1]ipcc-wg1@al.noaa.gov

--

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<http://www.ispe.arizona.edu/>

Attachment Converted: "c:\eudora\attach\Tett\_etal.pdf"

## References

1. <mailto:ipcc-wg1@al.noaa.gov>

From: Eduardo Zorita <Eduardo.Zorita@gkss.de>  
To: m.n.juckles@rl.ac.uk, " Moberg; Anders " <anders.moberg@natgeo.su.se>, Gabi Hegerl <hegerl@duke.edu>, esper@wsl.ch, " Briffa; Keith " <k.briffa@uea.ac.uk>, " Osborn; Tim " <t.osborn@uea.ac.uk>, m.allen1@physics.ox.ac.uk, weber@knmi.nl  
Subject: comments to mitrie manuscript  
Date: Thu, 17 Aug 2006 12:31:28 +0200

077

Due to the ongoing debate, this has turned an even more difficult manuscript. In general, I think Martin did a very good job in the review of the literature. Concerning the new reconstructions and the evaluation of McIntyre work, I would not fully agree with some of the conclusions, which I thin do not follow from the material presented in the text. I have some remarks on this which you may consider useful. But I think that I am not the one that should give the manuscript the final shape, as Martin is the person in charge of the project. Please, consider the following comments as suggestions.

eduardo

Consensus: I would tend to avoid the word 'consensus', since it is not a well defined concept.

Depending on the meaning of consensus, each would agree with it to a certain degree. I would prefer to refer to a particular IPCC conclusion, or something similar. I think this review of the literature is very well written and informative, but I am not sure that each one of us will agree with each one of the concussions of each of the papers.

Page 12, section 2.8. I think the text is somewhat vague here, and it could be misunderstood.

Mann et al (2005) tested the RegEM method, not the original MBH98 method. It is true that applied to the real proxies both methods, according to Mann, yield very similar results. But strictly speaking , Mann did not test the MBH98 method in the CSM simulation. The MBH98 method is thereby only by implication

I tested the the sensitivity of the MBH98, and not of RegEM, to the length of the calibration period. It may be the RegEM is less sensitive or not at all. Figure 4 and 5, if I understood well, support this dependency of MBH to the calibration period. Am I correct to interpret the large differences between the original MBH reconstruction (dashed red) and the black curve as due to the different calibration period (1901-1980 versus 1856-1980) and to the use of the leading PC or NHT as calibration target? At least in the period prior to 1600 I think these are the only methodological differences between both curves (?).

My interpretation of this figure is also somewhat different. If the final reconstructions differs so strongly by using a longer calibration period (in general yielding stronger decadal variability in the reconstruction)

I would tend to think that the method based on these proxies is quite unstable. What would happen if the calibration period could have been extended to 1800, for instance?.

Page 15: top. The role of forcing on the global or NH T is also recognized in the correlation between the NHT simulated by ECHO-G and CSM for the millennium. For the case of a second ECHO-G simulation (/Gonzalez-Rouco et al.) the agreement is very close at 30-year timescale.

Section 3, beginning.

In my opinion, MM05 stress the inadequacies and uncertainties in the MBH work, but they not put forward their own reconstruction implying a warmer-than-today MWP. They believe that this is true, but in their works so far, at least to my knowledge, they do not assert that the MWP was warmer than present, only that the uncertainties are too large for such a claim.

Section 3: Consensus. This paragraph may be problematic. Again what is the consensus? If we look at the recent NAS report, which again not every one would agree with, the 'consensus' is reduced to the past 400 years in comparison to IPCC, leaving ample space for speculation before this period. Does the NAS report belong to the consensus? perhaps partially, but I am not sure to what extent.

Section 3, discussion of MM05 and hockey-stick index. I have here a certain level of disagreement with these paragraphs. The issue raised by MM05 would be that the de-centering of the proxies prior to the calculations of the principal components tends to produce hockey-stick-shaped leading PC. I think this effect is true, at least with spatially uncorrelated red-noise series. It can be easily verified and it has been recognized in the NAS, the Wegman report and by Francis Zwiers. To be fair, following this issue is the problem of the truncation- just to keep the leading PC or further Pcs down the hiercharchy, and if this is done, the final differences could be probably minor. in the final reconstructions. But the paragraph implies, in my opinion, that this criticism by MM05 has no grounds, which as I said is problematic and could open the manuscript with criticisms based on these recent reports.

I think that the calculation shown in Figure 3 is very useful, as it boils down to the issue raised by MM05: how relevant is the de-centering and standardization with real proxies?. Apparently, I get a different message from Figure3 (although I may have misinterpreted the text). I see quite large differences in the 20th century between the original MBH leading PC and the 'correct' calculation (whole period centering and standarization, blue line). Only the original MBH PC shows a positive trend in the 20th century. The blue lines seems even to show a negative trend or no trend at all. If this PCs were to be used in the MBH regression model (with trend included in the calibration) the results could be quite different. I would tend to think that this figure actually supports the MM05 criticism, since the hockey-stick shape of the leading PC disappears.

Section 3, end, bristlecone pines. I am also worried by this paragraph. The recent NAS report clearly states that the bristlecone pines should not be used for reconstructions in view of their potential problems. They cite previous analysis on this issue. I think that to refer to just one study indicating no fertilization effect could not be enough. However, I am not a dendroclimatologist. This could open the door to potential problems.

Section 4, end. years 1997 and onwards were the warmest in the millennium. I see here also potential problems with this claim, and I do not see the need to make our lives more complicated. The NAS report expressed that the uncertainties are too large for this type of conclusion and certainly this conclusion would attract some attention from the reader. I see two lines of criticism on this: one is that the standard errors have been calculated with the calibration residuals and these are an underestimation of the true uncertainties. A reviewer may require that the uncertainty range be calculated by cross-calibration or bootstrapping. In the case of CVM perhaps this effect is not very important, as there is just one free parameter, but in the case of inverse regression there are much many more free parameters and the true uncertainties can be quite different from those estimated from the calibration residuals. This potential criticism could be exacerbated by the fact that the new reconstruction has not been tested in a validation period.

The other line of criticism could be that the calibration period has been, as in all reconstructions, a priori truncated -data after 1980 are not considered as the proxies are known to not follow the temperature. Strictly speaking this truncation can be only justified by a credible physical explanation about the cause of this divergence. Statistically, I think it is not correct to a priori ignore some data because they do not fit. If one does so, I think the uncertainty range should be enlarged to encompass the possibility that this divergence could have happened in the past, i.e. an additional standard deviation of the instrumental NH T in the period 1980-2000 (or perhaps more correct, the square root of the sum of the error variance and the NHT variance in 1980-2000). Alternatively, one could include the period 1980-2000 in the calibration and due to the divergence the standard errors would grow, but perhaps this is practically not possible as the proxy time series may not have been archived for the last 20 years.

Section 5, conclusions.

I share the worry of Anders Moberg about the wording 'serious flaws' in the analysis of MM05. This sentence would be based on Figure 3, if I understood properly, but as I said I think Figures 3 actually does not support this conclusion.

Finally, I think it would strategically better to avoid conflicts on the particular point of whether some particular year was the warmest of the millennium or not, and to stress the fact that all reconstructions, also the new ones presented in the manuscript (with one exception) show MWP temperatures lower than late 20th century temperatures.

Another conclusion could be, in my view, that the average temperature in the cold centuries in the millennium seems to be still quite uncertain. The new reconstructions, or the calculation of the leading PCs of the proxies, seem to be still quite sensitive to particular choices in the statistical set-up.

From: "Michael E. Mann" <mann@meteo.psu.edu>  
To: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: ECHO-G?  
Date: Wed, 23 Aug 2006 08:07:54 -0400  
Reply-to: mann@psu.edu  
Cc: Scott Rutherford <srutherford@rwu.edu>

<x-flowed>

Hi Keith,

If the offer still stands, we wanted to get from you the ECHO-G surface temperature field, so we can do some tests of RegEM with this. So far we've only tested on CSM 1.4 and it would be nice to test this on on ECHO-G, especially since other groups apparently now also have the ECHO-G output (e.g. Mark Cane's group and Francis Zwiers' group).

Thanks in advance for any help w/ this,

mike

--

Michael E. Mann  
Associate Professor  
Director, Earth System Science Center (ESSC)

Department of Meteorology            Phone: (814) 863-4075  
503 Walker Building                 FAX: (814) 865-3663  
The Pennsylvania State University    email: mann@psu.edu  
University Park, PA 16802-5013

<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
To: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Fwd: Chpt 6 - last 1000 yrs  
Date: Wed, 30 Aug 2006 21:43:25 +0200  
Cc: Jonathan Overpeck <jto@u.arizona.edu>

Hi Keith,

John should have the latest versions of the comments file and the chapter text, i.e. the ones that went out for LA review this summer. I believe he is after some more specific answers in the comments and not so much changes to the text, and has selected the bristlecone issue, the divergency issue and the verification and robustness issues. If you are unsure what comments or tetx he refers to, I think the best thing is for to ask John for the specific comments he thinks are not adequate, or the specific lines of text which he suggests changed. It seems he needs some reassurance rather than you writing much new in terms of comments and text, so the best would be to talk to him and ask what he needs you to do to the documents.

Best wishes,

Eystein

Envelope-to: Eystein.Jansen@geo.uib.no  
Date: Wed, 30 Aug 2006 15:31:12 +0100  
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>

From: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Fwd: Chpt 6 - last 1000 yrs  
X-UEA-Spam-Score: -101.6  
X-UEA-Spam-Level: -----  
X-UEA-Spam-Flag: NO  
X-checked-clean: by exiscan on noralf  
X-UiB-SpamFlag: NO UIB: -13.8 hits, 8.0 required  
X-UiB-SpamReport: spamassassin found;  
-15 From is listed in 'whitelist\_SA'  
0.1 BODY: Message is 30% to 40% HTML  
0.0 BODY: HTML included in message  
1.1 BODY: Message only has text/html MIME parts

Eystein

John sent these remarks - have not talked with him yet - but not sure what is now required

Keith

X-IronPort-AV: i="4.08,132,1154908800";  
d="scan'208,217"; a="17827006:sNHT58118592"  
Subject: Chpt 6 - last 1000 yrs  
Date: Wed, 16 Aug 2006 16:14:52 +0100  
X-MS-Has-Attach:  
X-MS-TNEF-Correlator:  
Thread-Topic: Chpt 6 - last 1000 yrs  
Thread-Index: AcbBRrj0FPNJH9bQTyCswuNw7Ln3bw==  
From: "Mitchell, John FB \((Chief Scientist)\)" <john.f.mitchell@metoffice.gov.uk>  
To: "Keith Briffa" <k.briffa@uea.ac.uk>  
Cc: "Mitchell, John FB \((Chief Scientist)\)" <john.f.mitchell@metoffice.gov.uk>  
X-UEA-Spam-Score: 2.1  
X-UEA-Spam-Level: ++  
X-UEA-Spam-Flag: NO

Hi Keith

I have tried to condense what I think the main issues for the and what the response is below. The weakest area seems to be statistical significance and by implication the likely/ very likely statements. I can't think of any easy solution - in the TAR for detection and attribution we used 95% limits on stats tests and then downrated them to allow for other uncertainties.

I am interested in your comments

John

Issues

1. Reliance on Bristlecone pine -

Response - the issues are in calibration period- they agree with other indicators for the rest of the record

2. Centring of principle components leads to "hockeysticks"-

Response - this makes only a small difference when standardised data used.

Comment - Would be useful to know which reconstructions do and donot make this assumption- this could strengthen the response

3. The divergence issue-

Response - it is only apparent in high latitudes, and only with some trees.

Comment- Do we know what happens if we eliminate those records with a divergence problem. The wider issue is whether or not it is reasonable to extend the reconstructions outside the calibration range.

4. There are different ways of verifying reconstructions and assigning significance levels( calibration period or seprate verifying period, different statistics)

Response ?

Comment- it is difficult in the text to gauge how well reconstructions are validated - eg using the calibration period to estimate errors as opposed to an independent period clearly makes a difference. This is important where "likely", "very likely"are used-

based on what statistics? I think this is the area where I think the current response is weakest

5. Robustness- Burger and Cubasch show a wide range of results using different assumptions-

Response ?

Mann makes a reasoned defence- there are other checks and tests which would rule out many of the arbitrary assumptions explored by Cubasch and Burger, but this is not clear in the response to M&M etc

--

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Phone: +47-55-583491 - Home: +47-55-910661  
Fax: +47-55-584330

From: David Rind <drind@giss.nasa.gov>  
To: Jonathan Overpeck <jto@u.arizona.edu>  
Subject: Re: urgent IPCC need  
Date: Thu, 31 Aug 2006 21:28:16 -0400  
Cc: joos <joos@climate.unibe.ch>, Eystein Jansen <eystein.jansen@geo.uib.no>, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, cddhr@giss.nasa.gov, rahmstorf@ozean-klima.de, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Jonathan,

I haven't looked at these in great detail, but I have a problem with Martin making suggestions about the TSU Exec Summary for chap 6. Weren't these decided by consensus among the Chap 6 authors? Why does Martin have any say about this? Clarification is one thing, but some of these suggestions seem to be 'leading'. I think we should be very cautious about changing anything substantive here at the last moment. [This is the expurgated version of what I really thing.]

David

At 4:55 PM -0600 8/31/06, Jonathan Overpeck wrote:

>Hi all - We need to submit our latest chap 6  
>Exec Summary to TSU tomorrow if we can. We can  
>still make changes, but I wanted to update with  
>Martin's suggestions taken into account. See the  
>attached and please comment regarding my strike  
>throughs and additions (yellow highlight).  
>Martin's comments are in yellowish text, and my  
>questions to you (especially FORTUNAT) are  
>highlighted in PURPLE.

>

>Please send by tomorrow aft if you can.

>

>Not that I've sent to those I think are on-line  
>right now. Will send to the whole team later  
>with more edited text.

>

>Thanks, Peck

>--

>Jonathan T. Overpeck  
>Director, Institute for the Study of Planet Earth  
>Professor, Department of Geosciences  
>Professor, Department of Atmospheric Sciences

>

>Mail and Fedex Address:

>

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><http://www.geo.arizona.edu/>  
><http://www.ispe.arizona.edu/>  
>  
>Attachment converted:  
>Toltec:Ch06\_FinalDraft\_ExecSumV3.doc (WDBN/«IC»)  
>(1BEA76C7)

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////////////////////////////////////

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</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>  
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
Subject: Re: urgent IPCC need  
Date: Fri, 1 Sep 2006 15:25:20 -0600

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, david.adelman@law.arizona.edu

Hi all - today has been a hectic one, with lots of good input from multiple folks. In the end, we agreed to stick with our existing bullets, which changes only where they would improve the clarity of what we were saying. Please check the attached - need Fortunat's detailed look in particular. Changes are all in yellow highlight. Two special issues:

1) There is still concern that this bullet is too vague to be as useful as it could be:

o It is very likely that the global warming of 4 to 7 °C since the Last Glacial Maximum (ca. 21,000 years ago) occurred at an average rate about ten times slower than the warming of the 20th century.

but, perhaps the safest thing would be to leave as is.

2) As for the 1998/2005 warmest in last 1000 years issue, we suggest adding nothing new to the ES, in line with our chapter policy from Bergen, BUT adding something in the chapter along the lines of: " There is currently insufficient knowledge to form a consensus on the issue of how the warmth of individual years of the last 100 years compare with individual years of the last 1000 years" Keith, would you like to make a suggestion on the wording and placement?

The reasoning expressed by Stefan on this issue is undoubtedly shared by others outside our team, and perhaps a paper be written on this key topic to help the community reach better consensus.

Thanks for your continued dialog and work! Have a good weekend.

best, Peck and Eystein

dear All, thanks for being alert.

I think we have an agreement that Martin's comments are useful, but that we should change only those sentences where they clarify. Otherwise i agree with Stefan and Keith's statements below.

Eystein

At 15:45 +0100 01-09-06, Keith Briffa wrote:

I forgot to say that I too disagree with removing the first sentence re simulations

being consistent with reconstructed NH temps. As Stefan says we need the context , and our results are independent of Chapter 9 in this regard.

Keith

At 15:37 01/09/2006, Stefan Rahmstorf wrote:

Hi Peck,

Martin as in Manning? I have found his feedback very useful so far, so we should definitely look at what he suggests - he mostly tends to look for whether our sentences are clear. Obviously, he cannot suggest real changes in meaning, only issues of clarity, but the latter I would take very seriously. Mostly I find his small rewordings good, I comment on the larger points and exceptions below.

- I am against deleting the bullet on speed of deglacial change. This point is extremely effective. Just two days ago an oil industry person told me that there have been big natural climate changes like ice ages in the past, hence we need not worry. I responded that the biggest warming in recent climate history was the end of the last Ice Age - but that warming by about 5 °C took about 5,000 years, not a hundred. "Oh" he said, "Really so long? I didn't know that." I think it is a very important point, we need to make it. Maybe not in term of "average rate", may we should just say: the warming of 4-7 °C took about 5,000 years, as compared to a future change of up to the same magnitude within a century.

- Next ice age bullet in 30k seems fine to me.

- exceptional warmth: the SPM said:

20th C T increase likely the largest in a millennium - that is strengthened (perhaps very likely now?)

1990s likely the warmest decade in a millennium - that again is strengthened

1998 likely the warmest year - I'd say this is unchanged (except for 2005 challenging it), likely is only 66%! Even though the annual proxy data may be uncertain, as a physicist I would find it unlikely that there is a mechanism to cause a big warm outlier year that beats 1998 from a much cooler background state. How would that work - where would the heat come from?

So in my view we could actually say that these past SPM statements held up or were strengthened - but in fact I also like the bullet as it is.

- Paleoclimate model simulations are broadly consistent with the reconstructed NH temperatures over the past 1000 years. The rise in surface temperatures since 1950 very likely cannot be reproduced without including anthropogenic greenhouse gases in the model forcings, and it is very unlikely that this warming was merely a recovery from the pre-20th century cold period.

On this I disagree with deleting the first sentence, as the second one needs it to follow logically. And why should the paleo chapter suddenly make a statement on post-1950 warming, if it is not in the context of the past millennium?

Cheers, Stefan

--

To reach me directly please use:

<mailto:rahmstorf@ozean-klima.de>rahmstorf@ozean-klima.de

(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)

Stefan Rahmstorf

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Attachment Converted: "c:\eudora\attach\Ch06\_FinalDraft\_ExecSumV4.doc"

From: "Mitchell, John FB \ (Chief Scientist)" <john.f.mitchell@metoffice.gov.uk>  
To: "Stefan Rahmstorf" <rahmstorf@ozean-klima.de>, "Keith Briffa" <k.briffa@uea.ac.uk>  
Subject: RE: Fwd: Re: [Wg1-ar4-ch06] NEW DRAFT FOR LA REVIEW  
Date: Tue, 5 Sep 2006 12:29:08 +0100  
Cc: "Eystein Jansen" <Eystein.Jansen@geo.uib.no>, "Jonathan Overpeck" <jto@u.arizona.edu>, "Jean Jouzel" <jouzel@dsm-mail.saclay.cea.fr>

Keith, Stefan

Its not my role as review editor to tell you what to write, just to make sure you have responded to the reviewers comments. For what its worth, I did find Keith's text quite involved. However, you do need to respond the the reviewers comments on Burger etc - if the flaws in von Storch paper cast doubt on the subsequent papers, then why not include a sentence in the chapter that says so, and list just the key papers affected.

I hope this helps

john

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Tel. +44(0)1392884604 Fax:+44 (0) 870 9005050  
E-mail: john.f.mitchell@metoffice.gov.uk [1]<http://www.metoffice.gov.uk>

---

From: Stefan Rahmstorf [mailto:rahmstorf@ozean-klima.de]  
Sent: 01 September 2006 13:02  
To: Keith Briffa  
Cc: Mitchell, John FB (Chief Scientist); Eystein Jansen; Jonathan Overpeck  
Subject: Re: Fwd: Re: [Wg1-ar4-ch06] NEW DRAFT FOR LA REVIEW

Dear Keith,  
you disagree with my proposed revision of the paragraph re. the Von Storch papers, but you do not give any reasons or arguments for that. I think there are some good reasons to shorten this discussion and to clarify it, and I would welcome to hear your reasons against it.  
Firstly, I think your original discussion was too long and complex to understand for non-specialists, and, at this level of detail, not policy-relevant. It took up a disproportionate amount of space for what we can learn from it.  
Secondly, I don't think we need to cite all those Storch-spinoff papers by Bürger/Cubasch. Most people whose judgement I value (e.g., David Ritson, who I think has no vested interest but a very detailed knowledge of the issue) think these papers are irrelevant at best and misleading at worst (he actually has used stronger wording). You may also have seen that the latest in this series, making similar points, is highly criticised by anonymous reviewers on the open discussion site of the journal *Climate of the Past*, where one reviewer (this is not the even more scathing review by Mann) recommends rejection of the Bürger/Cubasch paper because of "numerous errors and inaccuracies in the use of statistical concepts and methods".  
Third, if we cite Von Storch et al. 2004 we need to be very clear that a number of key statements are simply incorrect, which is a fact that is not in dispute and documented in the literature. They implemented the Mann et al. method incorrectly, and it is at least unclear whether in their follow-up paper they have now fixed this (Ritson, who discovered

the problem in their original paper in the first place, thinks they still have a problem, the detrending step was not the only one - and certainly in no paper have VS et al. shown any test that verifies their algorithm). Also, they were hiding a major artificial climate drift (which they must have known about, and which makes up half of their climate signal) - it is at least unclear whether you can expect a proxy method based on physical patterns of climate variability to reconstruct an unphysical drift, which has a completely different pattern. I simply think that because of this flaw, we cannot trust or cite any results from this particular ECHO-G run, which also affects several of the Bürger/Cubasch papers using the same data set. Given that the VS04 paper was used in the US Senate and other high-profile fora to discredit IPCC, I think it is imperative that we clarify this and leave our readers in no doubt about the fact that the VS04 results have proven to be incorrect in a major way.

I am aware that you authored a favorable Science Perspective on the VS04 paper at the time, but you could not have known of those errors back then, and for a long time I thought myself that it was a valid paper. Therefore, if we state clearly in our chapter what is wrong with it, I do not think this would be a loss of face for you - quite the contrary. I also think you have done a brilliant job on the rest of the very difficult discussion of the past millennium.

Best wishes, Stefan

--

To reach me directly please use: [2]rahmstorf@ozean-klima.de  
(My former addresses @pik-potsdam.de are read by my assistant Brigitta.)

Stefan Rahmstorf  
[3]www.ozean-klima.de  
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#### References

1. <http://www.metoffice.gov.uk/>
2. <mailto:rahmstorf@ozean-klima.de>
3. <http://www.ozean-klima.de/>
4. <http://www.realclimate.org/>

From: "Michael E. Mann" <mann@meteo.psu.edu>  
To: Tim Osborn <t.osborn@uea.ac.uk>  
Subject: Re: followup  
Date: Wed, 06 Sep 2006 08:34:17 -0400  
Reply-to: mann@psu.edu  
Cc: Scott Rutherford <srutherford@rwu.edu>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Tim, thanks a bunch. This all sounds very good. We're finalizing a pseudoproxy paper for JGR based on the various tests w/ the CSM simulation I showed in Wengen, and will send you a guys a copy once its finalized. A natural followup would be a similar analysis applying to the ECHO-G simulation, and we would enjoy collaborating w/ you and Keith on this. We were also thinking of doing some "mixed signal" analyses, where the pseudoproxies represent a combinatiion of temp and precip (including limiting cases of pure temp and pure precip). This might be a natural way to incorporate the ECHO-G results. We'll let you know if we have any trouble w/ format, etc.

thanks again,

mike

Tim Osborn wrote:

> Hi Mike and Scott,  
>  
> below are details about accessing the ECHO-G data from the SO&P  
> web-archive. There are time series plots of various variables and  
> regions that might be useful for a quick overview of what's going on,  
> plus the temperature fields (and fields for other variables) can be  
> accessed in netCDF format (hope that format is ok, if not I can make a  
> conversion for you but that won't be till next week).  
>  
> I'd like to add to Keith's reasons why we'd like to be involved in the  
> outcome of analysis of these data. The extra reason is that we  
> (Keith/me) are free to use these data and thus by extension you can  
> too provided we collaborate. Fidel Gonzalez-Rouco or GKSS aren't yet  
> ready to make them completely open access, preferring to consider each  
> 3rd party request and decide on that basis. I did ask Eduardo Zorita  
> about making them available for pseudo-proxy challenge after the  
> Wengen meeting, but I haven't yet followed up to find out his

> decision. The bottom line is that they might well make them available  
> for you to do your own thing with, but if you are happy to collaborate  
> with us then you can definitely use them immediately.

>  
> The data are available from here:

>  
> <http://www.cru.uea.ac.uk/cru/projects/soap/data/model/echog.htm>

>  
> Near the bottom you will find the link to the password-protected model  
> data (this includes the time series plots too). The login details for  
> this are:

>  
> soapech

> od2004

>  
> The 2m air temperature is 3rd in the list of variables. 'Erik' is the  
> simulation will all forcings, 'Enat' just has natural forcings through  
> to the present. The easiest way to get all the monthly 2m air  
> temperature fields for Erik is to use 'wget'. There is help for how  
> to use 'wget' if you aren't familiar.

>  
> The site was designed to be fairly self explanatory; hope you find it  
> so. If not, please just ask.

>  
> Best wishes

>  
> Tim

>  
> At 18:30 05/09/2006, Michael E. Mann wrote:

>  
>> sure thing Keith, thanks. and of course, we'll keep you fully in the  
>> loop on our findings. I'm copying to Scott, as he's the one who will  
>> probably obtain the data from Tim. Thanks again, got to go teach now...

>>  
>> mike

>>  
>> Keith Briffa wrote:

>>  
>>> mike

>>> simply missed the first and been away since second message -  
>>> forwarding to Tim to arrange access to these data ( I am assuming  
>>> Hans will not mind but best not say anything yet ) we wish to be  
>>> involved in this follow up please as it will be a SOAP product and

>>> Tim (especially) and I did stuff to get these data produced and in a  
>>> form for dissemination. I am rushing now to Austria for a week .

>>> cheers

>>> Keith

>>>

>>> At 13:51 28/08/2006, you wrote:

>>>

>>>> Keith, I didn't receive a response to my previous inquiry so I'm  
>>>> resending. Also copying to Phil in case you haven't been reading  
>>>> email for some reason.

>>>>

>>>> We would like to run our RegEM analysis through the ECHO-G  
>>>> simulation results. It appears that the results of that simulation  
>>>> have been widely disseminated to other groups, and yet they are not  
>>>> publically available to our knowledge.

>>>>

>>>> As per your previous suggestion, we would be grateful if we could  
>>>> acquire the surface temperature field for the simulation from you  
>>>> for some analyses we're doing.

>>>>

>>>> Thanks in advance for any help,

>>>>

>>>> mike

>>>>

>>>> --

>>>> Michael E. Mann  
>>>> Associate Professor  
>>>> Director, Earth System Science Center (ESSC)

>>>>

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> Dr Timothy J Osborn, Academic Fellow

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> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

>

> **\*\*Norwich -- City for Science:**

> **\*\*Hosting the BA Festival 2-9 September 2006**

>

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University Park, PA 16802-5013

<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re:

Date: Wed, 13 Sep 2006 09:10:59 -0600

Cc: rahmstorf@ozean-klima.de, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, cddhr@giss.nasa.gov, joos <joos@climate.unibe.ch>, Eystein Jansen <eystein.jansen@geo.uib.no>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>, t.osborn@uea.ac.uk

Keith - thanks for this and the earlier updates. Stefan is not around this week, but hopefully the others on this email can weight in. My thoughts...

1) We MUST say something about individual years (and by extension the 1998 TAR statement) - do we support it, or not, and why.

2) a paragraph would be nice, but I doubt we can do that, so..

3) I suggest putting the first sentence that Keith provides below as the last sentence, in the last (summary) para of 6.6.1.1. To make a stand alone para seems like a bad way to end the very meaty section.

4) I think the second sentence could be more controversial - I don't think our team feels it is valid to say, as they did in TAR, that "It is also likely that, in the Northern Hemisphere,... 1998 was the warmest year" in the last 1000 years. But, it you think about it for a while, Keith has come up with a clever 2nd sentence (when you insert "Northern Hemisphere" language as I suggest below). At first, my reaction was leave it out, but it grows on you, especially if you acknowledge that many readers will want more explicit prose on the 1998 (2005) issue.

Greater uncertainty associated with proxy-based temperature estimates for individual years means that it is more difficult to gauge the significance, or precedence, of the extreme warm years observed in the recent instrumental record. However, there is no new evidence to challenge the statement made in the TAR that 1998 (or the subsequent near-equivalent 2005) was likely the warmest of Northern Hemisphere year over the last 1000 years.

5) I strongly agree we can't add anything to the Exec Summary.

6) so, if no one disagrees or edits, I suggest we insert the above 2 sentences to end the last (summary) para of 6.6.1.1. Or should we make it a separate, last para - see point #3 above why I don't favor that idea as much. But, it's not a clear cut issue.

Thoughts? Thanks all, Peck

Eystein and Peck

I have thought about this and spent some time discussing it with Tim. I have come up with the following

Greater uncertainty associated with proxy-based temperature estimates for individual years means that it is more difficult to gauge the significance, or precedence, of the extreme warm years observed in the recent instrumental record. However, there is no new evidence to challenge the statement made in the TAR that 1998 (or the subsequent near-equivalent 2005) was likely the warmest in the last 1000 years.

This should best go after the paragraph that concludes section 6.6.1.1

I believe we might best omit the second sentence of the suggested new paragraph - but you might consider this too subtle (or negative) then. I think the second sentence is very subtle also though - because it does not exclude the possibility that the same old evidence that challenges the veracity of the TAR statement exists now , as then!

I think this could go in the text where suggested , but I think it best NOT to have a bullet about this point. We need to check exactly what was said in the TAR . Perhaps a reference to the Academy Report could also be inserted here?

Anyway, you asked for a straw-man statement for all to argue about so I suggest we send this to Stefan, David , Betty and whoever else you think.

cheers

Keith

--

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<http://www.geo.arizona.edu/>  
<http://www.ispe.arizona.edu/>

From: Keith Briffa <k.briffa@uea.ac.uk>  
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>  
Subject: No Subject  
Date: Wed, 13 Sep 2006 15:32:19 +0100

Eystein and Peck

I have thought about this and spent some time discussing it with Tim. I have come up with the following

Greater uncertainty associated with proxy-based temperature estimates for individual years means that it is more difficult to gauge the significance, or precedence, of the extreme warm years observed in the recent instrumental record. However, there is no new evidence to challenge the statement made in the TAR that 1998 (or the subsequent near-equivalent 2005) was likely the warmest in the last 1000 years.

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cheers

Keith

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## References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: David Rind <drind@giss.nasa.gov>  
To: Jonathan Overpeck <jto@u.arizona.edu>  
Subject: Re:

Date: Wed, 13 Sep 2006 16:43:08 -0400

Cc: Keith Briffa <k.briffa@uea.ac.uk>, rahmstorf@ozean-klima.de, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, cddhr@giss.nasa.gov, joos <joos@climate.unibe.ch>, Eystein Jansen <eystein.jansen@geo.uib.no>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>, t.osborn@uea.ac.uk

Leaving aside for the moment the resolution issue, the statement should at least be consistent with our figures. Fig. 6-10 looks like there were years around 1000 AD that could have been just as warm - if one wants to make this statement, one needs to expand the vertical scale in Fig. 6-10 to show that the current warm period is 'warmer'.

Now getting back to the resolution issue: given what we know about the ability to reconstruct global or NH temperatures in the past - could we really in good conscience say we have the precision from tree rings and the very sparse other data to make any definitive statement of this nature (let alone accuracy)? While I appreciate the cleverness of the second sentence, the problem is everybody will recognize that we are 'being clever' - at what point does one come out looking aggressively defensive?

I agree that leaving the first sentence as the only sentence suggests that one is somehow doubting the significance of the recent warm years, which is probably not something we want to do. What I would suggest is to forget about making 'one year' assessments; what Fig. 6-10 shows is that the recent warm period is highly anomalous with respect to the record of the last 1000 years. That would be what I think we can safely conclude the last 1000 years really tells us.

David

At 9:10 AM -0600 9/13/06, Jonathan Overpeck wrote:

Keith - thanks for this and the earlier updates. Stefan is not around this week, but hopefully the others on this email can weight in. My thoughts...

1) We MUST say something about individual years (and by extension the 1998 TAR statement) - do we support it, or not, and why.

2) a paragraph would be nice, but I doubt we can do that, so..

3) I suggest putting the first sentence that Keith provides below as the last sentence, in the last (summary) para of 6.6.1.1. To make a stand alone para seems like a bad way to end the very meaty section.

4) I think the second sentence could be more controversial - I don't think our team feels it is valid to say, as they did in TAR, that "It is also likely that, in the Northern Hemisphere,... 1998 was the warmest year" in the last 1000 years. But, it you think about it for a while, Keith has come up with a clever 2nd sentence (when you insert "Northern Hemisphere" language as I suggest below). At first, my reaction was leave it out, but it grows on you, especially if you acknowledge that many readers will want more explicit prose on the 1998 (2005) issue.

Greater uncertainty associated with proxy-based temperature estimates for individual years means that it is more difficult to gauge the significance, or precedence, of the extreme warm years observed in the recent instrumental record. However, there is no new evidence to challenge the statement made in the TAR that 1998 (or the subsequent near-equivalent 2005) was likely the warmest of Northern Hemisphere year over the last 1000 years.

5) I strongly agree we can't add anything to the Exec Summary.

6) so, if no one disagrees or edits, I suggest we insert the above 2 sentences to end the last (summary) para of 6.6.1.1. Or should we make it a separate, last para - see point #3 above why I don't favor that idea as much. But, it's not a clear cut issue.

Thoughts? Thanks all, Peck

Eystein and Peck

I have thought about this and spent some time discussing it with Tim. I have come up with the following

Greater uncertainty associated with proxy-based temperature estimates for individual years means that it is more difficult to gauge the significance, or precedence, of the

extreme warm years observed in the recent instrumental record. However, there is no new evidence to challenge the statement made in the TAR that 1998 (or the subsequent near-equivalent 2005) was likely the warmest in the last 1000 years.

This should best go after the paragraph that concludes section 6.6.1.1

I believe we might best omit the second sentence of the suggested new paragraph - but you might consider this too subtle (or negative) then. I think the second sentence is very subtle also though - because it does not exclude the possibility that the same old evidence that challenges the veracity of the TAR statement exists now , as then!

I think this could go in the text where suggested , but I think it best NOT to have a bullet about this point. We need to check exactly what was said in the TAR . Perhaps a reference to the Academy Report could also be inserted here?

Anyway, you asked for a straw-man statement for all to argue about so I suggest we send this to Stefan, David , Betty and whoever else you think.

cheers

Keith

--

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--

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<http://www.geo.arizona.edu/>  
<http://www.ispe.arizona.edu/>

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////////////////////////////////////  
////////////////////////////////////

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>  
To: Jonathan Overpeck <jto@u.arizona.edu>, David Rind <drind@giss.nasa.gov>  
Subject: Re:  
Date: Wed, 13 Sep 2006 23:21:13 +0200  
Cc: Keith Briffa <k.briffa@uea.ac.uk>, rahmstorf@ozean-klima.de, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, cddhr@giss.nasa.gov, joos <joos@climate.unibe.ch>, Eystein Jansen <eystein.jansen@geo.uib.no>, "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>, t.osborn@uea.ac.uk

Hi all,

My take on this is similar to what Peck wrote. My suggestion is to write:

Greater uncertainty associated with proxy-based temperature estimates for individual years means that it is more difficult to gauge the significance, or precedence, of the extreme warm individual years observed in the recent instrumental record, such as 1998 and 2005, in the context of the last millennium.

I think this is scientifically correct, and in essence means that we, as did the NAS panel say, feel the TAR statement was not what we would have said. I sympathise with those who say that it is not likely that any individual years were warmer, as Stefan has stated, but I don't think we have enough data to qualify this on the hemispheric mean.

Best wishes,

Eystein

If this is interpreted as a criticism of the TAR, then I think we

At 14:09 -0600 13-09-06, Jonathan Overpeck wrote:

thanks David - lets see what others think. I agree, that we don't want to be seen as being too clever or defensive. Note however, that all the TAR said was "likely" the warmest in the last 1000 years. Our chapter and figs (including 6.10) make it clear that it is unlikely any multi-decadal period was as warm as the last 50 years. But, that said, I do feel your are right that our team would not have said what the TAR said about 1998, and thus, we should delete that second sentence.

any other thoughts team?

thx, peck

Leaving aside for the moment the resolution issue, the statement should at least be consistent with our figures. Fig. 6-10 looks like there were years around 1000 AD that could have been just as warm - if one wants to make this statement, one needs to expand the vertical scale in Fig. 6-10 to show that the current warm period is 'warmer'.

Now getting back to the resolution issue: given what we know about the ability to reconstruct global or NH temperatures in the past - could we really in good conscience say we have the precision from tree rings and the very sparse other data to make any definitive statement of this nature (let alone accuracy)? While I appreciate the cleverness of the second sentence, the problem is everybody will recognize that we are 'being clever' - at what point does one come out looking aggressively defensive?

I agree that leaving the first sentence as the only sentence suggests that one is somehow doubting the significance of the recent warm years, which is probably not something we want to do. What I would suggest is to forget about making 'one year' assessments; what Fig. 6-10 shows is that the recent warm period is highly anomalous with respect to the record of the last 1000 years. That would be what I think we can safely conclude the last 1000 years really tells us.

David

At 9:10 AM -0600 9/13/06, Jonathan Overpeck wrote:

Keith - thanks for this and the earlier updates. Stefan is not around this week, but hopefully the others on this email can weight in. My thoughts...

1) We MUST say something about individual years (and by extension the 1998 TAR statement) - do we support it, or not, and why.

2) a paragraph would be nice, but I doubt we can do that, so..

3) I suggest putting the first sentence that Keith provides below as the last sentence, in the last (summary) para of 6.6.1.1. To make a stand alone para seems like a bad way to end the very meaty section.

4) I think the second sentence could be more controversial - I don't think our team feels it is valid to say, as they did in TAR, that "It is also likely that, in the Northern Hemisphere,... 1998 was the warmest year" in the last 1000 years. But, if you think about it for a while, Keith has come up with a clever 2nd sentence (when you insert "Northern Hemisphere" language as I suggest below). At first, my reaction was leave it out, but it grows on you, especially if you acknowledge that many readers will want more explicit prose on the 1998 (2005) issue.

Greater uncertainty associated with proxy-based temperature estimates for individual years means that it is more difficult to gauge the significance, or precedence, of the extreme warm years observed in the recent instrumental record. However, there is no new evidence to challenge the statement made in the TAR that 1998 (or the subsequent near-equivalent 2005) was likely the warmest of Northern Hemisphere year over the last 1000 years.

5) I strongly agree we can't add anything to the Exec Summary.

6) so, if no one disagrees or edits, I suggest we insert the above 2 sentences to end the last (summary) para of 6.6.1.1. Or should we make it a separate, last para - see point #3 above why I don't favor that idea as much. But, it's not a clear cut issue.

Thoughts? Thanks all, Peck

Eystein and Peck

I have thought about this and spent some time discussing it with Tim. I have come up with the following

Greater uncertainty associated with proxy-based temperature estimates for individual years means that it is more difficult to gauge the significance, or precedence, of the extreme warm years observed in the recent instrumental record. However, there is no new evidence to challenge the statement made in the TAR that 1998 (or the subsequent near-equivalent 2005) was likely the warmest in the last 1000 years.

This should best go after the paragraph that concludes section 6.6.1.1

I believe we might best omit the second sentence of the suggested new paragraph - but you might consider this too subtle (or negative) then. I think the second sentence is very subtle also though - because it does not exclude the possibility that the same old evidence that challenges the veracity of the TAR statement exists now , as then!

I think this could go in the text where suggested , but I think it best NOT to have a bullet about this point. We need to check exactly what was said in the TAR . Perhaps a reference to the Academy Report could also be inserted here?

Anyway, you asked for a straw-man statement for all to argue about so I suggest we send this to Stefan, David , Betty and whoever else you think.

cheers

Keith

--

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From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: 1988/2005

Date: Fri, 15 Sep 2006 08:55:58 -0600

Cc: David Rind <drind@giss.nasa.gov>, rahmstorf@ozean-klima.de, Bette Otto-Bleisner <ottobli@ncar.ucar.edu>, cddhr@giss.nasa.gov, Ricardo Villalba <ricardo@lab.criicyt.edu.ar>, t.osborn@uea.ac.uk, joos <joos@climate.unibe.ch>, Eystein Jansen <eystein.jansen@geo.uib.no>, <oyvind.paasche@bjerknes.uib.no>

<x-flowed>

Thanks Keith, Tim and Fortunat for your input.

We'll go with what we have then - Eystein's suggestion minus the second "individual".

Eystein and Øyvind - just want to double check that you've deleted that 2nd "individual" in the all important 1998 sentence??

Thanks, Peck

>I do not disagree either - in fact I preferred  
>not to make the "too clever" second statement in  
>my "straw man" as I said at the time. If this is  
>the consensus (and I believe it is the  
>scientifically correct one) then I would be  
>happy with Eystein's sentence. The worry is that  
>we have inserted this late with no refereeing  
>and no justification in the text. I would also  
>suggest dropping the second "individual" in the  
>sentence.

>

>At 10:50 15/09/2006, Fortunat Joos wrote:

>>Hi,

>>

>>I support Eystein's suggestion and agree with David.

>>

>>If there is not sufficient evidence to support

>>or dismiss claims whether 1998 or

>>2005 was the warmest year of the millennium than we should indeed say so.

>>It is the nature and the strenght of the IPCC

>>process that points from the TAR

>>and earlier reports get reconsidered and

>>reassessed. It is normal that earlier

>>statements get revised. Often statements can be strenghtened, but sometimes

>>statements can not be supported anymore. Our job is to present the current

>>understanding of science as balanced as possible.

>>

>>With best wishes,

>>

>>Fortunat

>>

>>Quoting Eystein Jansen <Eystein.Jansen@geo.uib.no>:

>>

>>> Hi all,

>>> My take on this is similar to what Peck wrote. My suggestion is to write:

>>>

>>> Greater uncertainty associated with proxy-based

>>> temperature estimates for individual years means

>>> that it is more difficult to gauge the

>>> significance, or precedence, of the extreme warm

>>> individual years observed in the recent

>>> instrumental record, such as 1998 and 2005, in

>>> the context of the last millennium.

>>>

>>> I think this is scientifically correct, and in

>>> essence means that we, as did the NAS panel say,

>>> feel the TAR statement was not what we would have

>>> said. I sympatise with those who say that it is

>>> not likely that any individual years were

>>> warmer, as Stefan has stated, but I don't think

>>> we have enough data to qualify this on the

>>> hemispheric mean.

>>>

>>> Best wishes,

>>> Eystein

>>>

>>>

>>> If this is interpreted as a critisim of the TAR, then I think we

>>> At 14:09 -0600 13-09-06, Jonathan Overpeck wrote:

>>> >thanks David - lets see what others think. I

>>> >agree, that we don't want to be seen as being

>>> >too clever or defensive. Note however, that all

>>> >the TAR said was "likely" the warmest in the  
>>> >last 1000 years. Our chapter and figs (including  
>>> >6.10) make it clear that it is unlikely any  
>>> >multi-decadal period was as warm as the last 50  
>>> >years. But, that said, I do feel you are right  
>>> >that our team would not have said what the TAR  
>>> >said about 1998, and thus, we should delete that  
>>> >second sentence.  
>>> >  
>>> >any other thoughts team?  
>>> >  
>>> >thx, peck  
>>> >  
>>> >  
>>> >>Leaving aside for the moment the resolution  
>>> >>issue, the statement should at least be  
>>> >>consistent with our figures. Fig. 6-10 looks  
>>> >>like there were years around 1000 AD that could  
>>> >>have been just as warm - if one wants to make  
>>> >>this statement, one needs to expand the  
>>> >>vertical scale in Fig. 6-10 to show that the  
>>> >>current warm period is 'warmer'.  
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>>> >>Now getting back to the resolution issue: given  
>>> >>what we know about the ability to reconstruct  
>>> >>global or NH temperatures in the past - could  
>>> >>we really in good conscience say we have the  
>>> >>precision from tree rings and the very sparse  
>>> >>other data to make any definitive statement of  
>>> >>this nature (let alone accuracy)? While I  
>>> >>appreciate the cleverness of the second  
>>> >>sentence, the problem is everybody will  
>>> >>recognize that we are 'being clever' - at what  
>> >>point does one come out looking aggressively  
>>> >>defensive?  
>>> >>  
>>> >> I agree that leaving the first sentence as the  
>>> >>only sentence suggests that one is somehow  
>>> >>doubting the significance of the recent warm  
>>> >>years, which is probably not something we want  
>>> >>to do. What I would suggest is to forget about  
>>> >>making 'one year' assessments; what Fig. 6-10  
>>> >>shows is that the recent warm period is highly  
>>> >>anomalous with respect to the record of the  
>>> >>last 1000 years. That would be what I think we  
>>> >>can safely conclude the last 1000 years really  
>>> >>tells us.  
>>> >>  
>>> >>David  
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>>> >>At 9:10 AM -0600 9/13/06, Jonathan Overpeck wrote:  
>>> >>>Keith - thanks for this and the earlier  
>>> >>>updates. Stefan is not around this week, but  
>>> >>>hopefully the others on this email can weight  
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>>> >>>  
>>> >>>1) We MUST say something about individual  
>>> >>>years (and by extension the 1998 TAR  
>>> >>>statement) - do we support it, or not, and why.  
>>> >>>  
>>> >>>2) a paragraph would be nice, but I doubt we can do that, so..  
>>> >>>  
>>> >>>3) I suggest putting the first sentence that  
>>> >>>Keith provides below as the last sentence, in  
>>> >>>the last (summary) para of 6.6.1.1. To make a  
>>> >>>stand alone para seems like a bad way to end  
>>> >>>the very meaty section.  
>>> >>>  
>>> >>>4) I think the second sentence could be more  
>>> >>>controversial - I don't think our team feels  
>>> >>>it is valid to say, as they did in TAR, that  
>>> >>>"It is also likely that, in the Northern  
>>> >>>Hemisphere,... 1998 was the warmest year" in  
>>> >>>the last 1000 years. But, it you think about  
>>> >>>it for a while, Keith has come up with a  
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>>> >>>that many readers will want more explicit

>>> >>>prose on the 1998 (2005) issue.  
>>> >>>  
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>>> >>>individual years means that it is more  
>>> >>>difficult to gauge the significance, or  
>>> >>>precedence, of the extreme warm years observed  
>>> >>>in the recent instrumental record. However,  
>>> >>>there is no new evidence to challenge the  
>>> >>>statement made in the TAR that 1998 (or the  
>>> >>>subsequent near-equivalent 2005) was likely  
>>> >>>the warmest of Northern Hemisphere year over  
>>> >>>the last 1000 years.  
>>> >>>  
>>> >>>5) I strongly agree we can't add anything to the Exec Summary.  
>>> >>>  
>>> >>>6) so, if no one disagrees or edits, I suggest  
>>> >>>we insert the above 2 sentences to end the  
>>> >>>last (summary) para of 6.6.1.1. Or should we  
>>> >>>make it a separate, last para - see point #3  
>>> >>>above why I don't favor that idea as much.  
>>> >>>But, it's not a clear cut issue.  
>>> >>>  
>>> >>>Thoughts? Thanks all, Peck  
>>> >>>  
>>> >>>  
>>> >>>>Eystein and Peck  
>>> >>>>I have thought about this and spent some time  
>>> >>>>discussing it with Tim. I have come up with  
>>> >>>>the following  
>>> >>>>  
>>> >>>>Greater uncertainty associated with  
>>> >>>>proxy-based temperature estimates for  
>>> >>>>individual years means that it is more  
>>> >>>>difficult to gauge the significance, or  
>>> >>>>precedence, of the extreme warm years  
>>> >>>>observed in the recent instrumental record.  
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>> >>>>>reference to the Academy Report could also be  
>>> >>>>>inserted here?  
>>> >>>>>  
>>> >>>>>Anyway, you asked for a straw-man statement  
>>> >>>>>for all to argue about so I suggest we send  
>>> >>>>>this to Stefan, David , Betty and whoever  
>>> >>>>>else you think.  
>>> >>>>>cheers  
>>> >>>>>Keith  
>>> >>>>>  
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From: Tim Osborn <t.osborn@uea.ac.uk>  
To: Øyvind Paasche <oyvind.paasche@bjerknes.uib.no>  
Subject: Re: Final checks on figures and captions Email 1 of 2  
Date: Fri Sep 15 12:01:00 2006  
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <Eystein.Jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>

Hi Oyvind,

I was off work yesterday so I couldn't check the tables until today. Keith and I have spotted some corrections that need to be made, some my mistakes and some due to other changes. Sorry! Anyway all are shown as tracked changes (plus explanation comments) in the attached file. Hope this is all clear and ok.

Cheers

Tim

At 17:02 13/09/2006, Øyvind Paasche wrote:

tim - that's very good, the minor error in box fig.1 is now corrected. I think the lines in fig.6.10 and 6.14 are sufficiently thick so if its up to me we'll leave them as they are. I believe that all the figures are incorporated into the Word file the same way and I can't see any big difference between 6.10 and 6.14. TSU have eps formats of all the figures and in the end they will probably use an entirely different program than Word to construct the report, so I guess there's nothing to worry about.

Anyway, I have cleaned the standing version of the tables (attached) and if you (and/or Keith) could go through them a last time and check that everything is as it should be that would be nice. I have one question concerning one of the refs in table 6.2: Does Bertrand et al., 2002b correspond to Bertrand, C., M.F. Loutre, M. Crucifix, and A. Berger, 2002b: Climate of the last millennium: a sensitivity study. Tellus Series a-Dynamic Meteorology and Oceanography, 54(3), 221-244.

Cheers,

Øyvind

Hi,

I've checked that the figures and captions are the final versions and they are all correct for figures 6.10-6.14.

I've also checked Box 6.4, Figure 1 and here there is an error with the caption. The caption ends with the reference period which currently states '...the period 800-1995.'

I got this wrong. The correct statement is "...the period 1001-1980."

Sorry about that (it was a mistake carried over from our earlier use of the figure from Osborn and Briffa, 2006, Science: for that paper we did use 800-1995).

Were you also after a check of the image quality? The figures I'm involved with all look pretty good, except that 6.10 is a bit blurry or lower quality. Was it inserted into Word in a different way to the others that might have degraded the image? Also, I wonder whether I should make the lines on Figure 6.10(b) and/or Figure 6.14(b) any thicker? Please let me know if you want me to do this.

Cheers

Tim

At 01:46 13/09/2006, Jonathan Overpeck wrote:

Hi all - We're editing main text, and we think we're close to the right length without having to make significant cuts beyond what you've seen already - it's mostly down to editing for consistent style and clarity. That's good news.

Øyvind recently sent (working very late over their) the attached figs and caption files (being sent in two files to keep each half under 10 Mb), and rather than Peck trying to figure out if it's all perfect, we're sending to YOU look at your figures and captions to make sure they are the most up-to-date versions. Thanks! It's a challenge to make sure we have all the most recent pieces, although we're betting that we're doing ok. Tomorrow (I hope - thanks Øyvind!) we'll send tables - hoping Tim, Keith, and Fortunat will be waiting to comment/edit if needed.

We're in much better shape than last cycles thanks to all your hard work - hope we can make this last bit easy for you. We appreciate you working fast when we send material.

Thanks, Peck and Eystein

--

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\*\*Norwich -- City for Science:  
\*\*Hosting the BA Festival 2-9 September 2006

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## References

1. <http://www.geo.arizona.edu/>
2. <http://www.ispe.arizona.edu/>
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4. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Gabi Hegerl <hegerl@duke.edu>

Subject: Re: cheers!

Date: Tue, 19 Sep 2006 11:37:49 -0600

Cc: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

<x-flowed>

Hi Gabi - we do loose quite a bit (e.g., boreholes and other proxies) back beyond 500, so that's why we drew the "very likely" line there. But, we did stay as strong as the TAR back 1300, so that was our compromise on certainty. I believe the forcing series also start to get more uncertain pretty fast back beyond even 400 years ago, but I'm pretty impressed with the match between simulated and observed NH climate back ca. 700 years (e.g., our Figs 6.13 and 6.14). Thus, I bet you are right that we know back to 700 pretty well, but not well enough to go with "very likely" in the all important chap 6 bullet.

Not sure this helps, but we do need to pay attention as we do the SPM to get the right balance.

I'll cc to Keith in case he wants to chime in, which would be appreciated.

thanks, peck

>p.s. hope you are all recovered etc!

>I have one chapter question: We were waffling back and forth if we

>SHOULD go with the chapter 6

>assessment on the last 500 being better reconstructed than say last

>700, but in the end, we stuck with

>last 700 because some results rely on using a long timehorizon to

>separate like ghg and solar signals.

>To say that very likely a substantial fraction of the variance on

>those records is externally forced (nother

>words, detectable external signals in reconstructions).

>Does this seem ok to you? In the SPM session we had some waffling

>about 5 vs 7 centuries.

>

>Gabi

>

>Jonathan Overpeck wrote:

--

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From: Gabi Hegerl <hegerl@duke.edu>  
To: Keith Briffa <k.briffa@uea.ac.uk>  
Subject: Re: 5 to 7 centuries  
Date: Wed, 20 Sep 2006 12:37:42 -0400  
Cc: Eystein Jansen <Eystein.Jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>, Francis Zwiers <Francis.Zwiers@ec.gc.ca>

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I asked Tom about it, he says (but I realize he is one sample of the volcano enthusiasts) it could have been El Chichon, the eruption seems to be huge, but there is concerns that different physics would apply to such a large eruption making it cause different climate impacts (he cites a paper for that that I promptly forgot). I am always slightly nervous about the fact that this one doesnt show up in the data, and wondering if there is a sliver of circularity, but I think results like my detection stuff and probably also EPOCH stuff (I could try) are quite robust to missing an eruption, even a biggie.

Greetings everybody!

Gabi

Keith Briffa wrote:

> Hi everyone - just been at a meeting all day so just seen this . I  
> agree with Eystein et al . so no problems . Interested to know what  
> you mean Gabi about the 1256 eruption - we have been looking at the  
> empirical evidence for a contemporaneous cooling with ambiguous results  
> cheers  
> Keith

>  
>  
>

> At 20:16 19/09/2006, Eystein Jansen wrote:

>

>> Hi Gabi,  
>> this is fine with me and does not seem to contradict Ch6.  
>> Eystein

>>  
>>  
>>  
>>

>> At 15:06 -0400 19-09-06, Gabi Hegerl wrote:

>>

>>> SOunds good - since forcing and temperature reconstrucitons are  
>>> independent,  
>>> I think it was defensible to make a statement about role of forced  
>>> response 700 yrs back in Ch9.  
>>> Is it ok to keep 700 yrs about significant externally forced  
>>> component in SPM?  
>>> Susan is finetuning that bullet right now so thats why i thought it  
>>> would be good to know if you guys are  
>>> happy.

>>> We justified ch9's assessment based on your figure 6.13 showing  
>>> model and recon agreement, and on few detection  
>>> studies and some qualitative agreement studies all saying the  
>>> agreement is not spurious.  
>>> One issue going beyond further is 1256 eruption, which is not that  
>>> well understood,  
>>> so it gets a bit dicey beyond I think!

>>>  
>>> Gabi

>>>  
>>> Jonathan Overpeck wrote:

>>>  
>>>> Hi Gabi - we do loose quite a bit (e.g., boreholes and other  
>>>> proxies) back beyond 500, so that's why we drew the "very likely"  
>>>> line there. But, we did stay as strong as the TAR back 1300, so  
>>>> that was our compromise on certainty. I believe the forcing series  
>>>> also start to get more uncertain pretty fast back beyond even 400  
>>>> years ago, but I'm pretty impressed with the match between  
>>>> simulated and observed NH climate back ca. 700 years (e.g., our  
>>>> Figs 6.13 and 6.14). Thus, I bet you are right that we know back to  
>>>> 700 pretty well, but not well enough to go with "very likely" in  
>>>> the all important chap 6 bullet.

>>>>  
>>>> Not sure this helps, but we do need to pay attention as we do the  
>>>> SPM to get the right balance.

>>>>  
>>>> I'll cc to Keith in case he wants to chime in, which would be  
>>>> appreciated.

>>>>  
>>>> thanks, peck

>>>>  
>>>>> p.s. hope you are all recovered etc!  
>>>>> I have one chapter question: We were waffling back and forth if we  
>>>>> SHOULD go with the chapter 6  
>>>>> assessment on the last 500 being better reconstructed than say  
>>>>> last 700, but in the end, we stuck with  
>>>>> last 700 because some results rely on using a long timehorizon to  
>>>>> separate like ghg and solar signals.  
>>>>> To say that very likely a substantial fraction of the variance on  
>>>>> those records is externally forced (nother  
>>>>> words, detectable external signals in reconstructions).  
>>>>> Does this seem ok to you? In the SPM session we had some waffling  
>>>>> about 5 vs 7 centuries.

>>>>>  
>>>>> Gabi

>>>>>  
>>>>> Jonathan Overpeck wrote:

>>>>  
>>>>  
>>>  
>>> --

>>> ~~~~~  
>>> Gabriele Hegerl Division of Earth and Ocean Sciences, Nicholas  
>>> School for the Environment and Earth Sciences,  
>>> Box 90227  
>>> Duke University, Durham NC 27708

>>> Ph: 919 684 6167, fax 684 5833 email: hegerl@duke.edu,  
>>> <http://www.env.duke.edu/faculty/bios/hegerl.html>

>>  
>>  
>>  
>> --  
>>

---

>> Eystein Jansen  
>> Professor/Director  
>> Bjerknes Centre for Climate Research and  
>> Dep. of Earth Science, Univ. of Bergen  
>> Allégaten 55  
>> N-5007 Bergen  
>> NORWAY  
>> e-mail: eystein.jansen@geo.uib.no  
>> Phone: +47-55-583491 - Home: +47-55-910661  
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>  
>  
> --  
> Professor Keith Briffa,  
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> Fax: +44-1603-507784  
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> <http://www.cru.uea.ac.uk/cru/people/briffa/>  
>

--  
~~~~~

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</x-flowed>

From: "Saffron O'Neill" <s.o-neill@uea.ac.uk>
To: <t.osborn@uea.ac.uk>
Subject: panel meeting and ice extent modelling
Date: Mon, 2 Oct 2006 10:46:26 +0100

<x-flowed>

Hi Tim

I've found some 'communicating cc' ref's which I've attached - nothing too hard going! Futerra's 'rules of the game' is a good intro to what climate change communicators should be working towards in terms of best practice. Sophie's poster is a summary of the main findings of her PhD research from a couple of years back in ENV, and is a message that some NGOs in particular would still do well to heed! Finally, the communicating CC document is an outline of Defra's recent initiative, as followed on from Futerra's consultancy work.

PhD stuff: at the last panel meeting, we agreed to meet again in early October. However, I think this meeting would best be delayed until we know exactly what info we can obtain for the expert elicitation as r.e. ice extent maps, time series etc.

I forwarded on the email from Xiangdong Zhang a few days ago - he's happy to give me some plots showing 2-D distribution of sea ice concentrations around 2050 and also animations from 1900-2100 under the A1B scenario.

How is the ice modelling going? Do you think you'd be able to get some plots say by w/c 9th Oct so we could talk about them in the meeting?

Cheers

Saffron

</x-flowed>

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\communicating_climate_change.pdf"

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\POSTER SNC.pdf"

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\RulesOfTheGame.pdf"

From: Jonathan Overpeck <jto@u.arizona.edu>

To: Keith Briffa <k.briffa@uea.ac.uk>, Eystein Jansen <eystein.jansen@geo.uib.no>

Subject: Re: VERY URGENT HELP NEEDED TO ADDRESS FINAL DRAFT PROBLEM

Date: Fri, 6 Oct 2006 09:11:04 -0600

Hi Keith and Eystein - thanks for the timely and helpful (very) feedback, Keith. Your suggestions for 4 and 5 seem fine, and I wonder only about 6. I too am not sure where the final clause came from, but I'll guess it was a suggestion of Stefan's that then stood the text of time. In the spirit of trying hard not to change the meaning of bullets in the ES from what the LA team agreed to in Bergen, what about changing this clause in the ES to read "natural recovery", i.e.:

and it is very unlikely that this warming was merely a natural recovery from the pre-20th century cold period."

This takes away the ambiguity, and does serve to address a widely held misconception outside of our community - or at least to phrase the issue in terms that some might find more useful.

If we keep this phrase, then I would suggest restating the entire ES sentence at the end of 6.6.3.

Is this ok? Again, I'm motivated by our team agreement - I do think we could delete this phrase since it's more repetitive than new meaning, but would rather not unless it really does not work. Personally, I like it as modified above, because it hammers the important point from a slightly different perspective - one that seems to be on the minds of the public still.

Thanks, both, for letting me know what you think fast.

best, peck

Hi Peck and Eystein

In response to Points 4-6

4. Add the following after past 1300 years. on line 13 page Y-33

"Considering the recent instrumental and longer proxy evidence together, it is very likely that average Northern Hemisphere temperatures during the second half of the 20th century were warmer than any other 50-year period in the last 500 years. "

Do not put anything in Box 6.4 which is written from the reverse perspective - evidence of medieval period not good enough to say warmer than now. Also confuses statements about 500 years and longer (1000 year) Medieval ,time.

5. The person who says this has not read the text - see lines 28-33 on Y-32 where I

think this is well covered.

6. If you read the text on lines 1-10 of PAGE Y-38 I think this meaning is clearly conveyed. It is not in the same words -but easily supports the ES statement.

HOWEVER, I do not like the last part of the statement (and not sure where this came from) because it is ambiguous and anyway implied by prior statement. I strongly urge you to remove the section

"and it is very unlikely that this warming was merely a recovery from the pre-20th century cold period."

These would sort things out I believe

cheers

Keith

At 19:26 05/10/2006, you wrote:

Hi Keith and Tim - we just got the attached consistency feedback doc from the TSU, and I've added my thoughts in red. We need your feedback on items 4-6 REALLY FAST. Tim, if Keith's not around to help, please do the job - the TSU has zero time to give us.

I think the solutions to #5 and 6 are easy as I suggested (although I don't have confirmation from Susan or Martin that we can just do as I suggest, but it seems logical to me - if you can suggest an even better solution, pls do.

I'll send the official chap 6 final draft text next - at least as it stands today.

thanks for dealing with this, perhaps before you go to sleep this evening.

Best, Peck

--

Jonathan T. Overpeck

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Professor, Department of Atmospheric Sciences

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--

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From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Christoph Kull <christoph.kull@pages.unibe.ch>
Subject: Re: [Fwd: 2006ES001559 Decision Letter]
Date: Fri, 06 Oct 2006 09:45:38 -0400
Reply-to: mann@psu.edu
Cc: Thorsten Kiefer <thorsten.kiefer@pages.unibe.ch>, Heinz Wanner <wanner@giub.unibe.ch>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>

thanks very much Christoph, that's perfect.
regards,
mike
Christoph Kull wrote:

Hi Mike,
If the EOS-piece is not already submitted...
Below a paragraph we propose to use for the short description of the second project.

"Furthermore, the participants identified the need and a major opportunity to improve the quality and value of climate reconstructions. Therefore, a workshop is planned to assess uncertainties in proxies in a coherent way and to develop strategies for future collection and integration of proxy data from key regions. The workshop will focus on climate proxies that have decadal or better temporal resolution and will involve the world data centers."

Thanks a lot! Best wishes!

Christoph

On 30.09.2006 19:56, "Michael E. Mann" [1]<mann@meteo.psu.edu> wrote:

Dear Keith/Phil/Thorsten/Christoph/Heinz,

Sorry this took Eos so long. No surprises here. A few minor revisions and it should be ready for publication. Please see attached revised version and response to reviewers. I've highlighted in yellow one place in the draft where I could use some input from someone who is better qualified to elaborate on the details of the 2nd project mentioned. Other than that, let me know if you see any need for any additional changes.

Will resubmit once I've heard back from everyone.

best regards,

mike

--

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[3]<http://www.met.psu.edu/dept/faculty/mann.htm>

References

1. <mailto:mann@meteo.psu.edu>
2. <mailto:mann@psu.edu>
3. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>
Subject: [Fwd: Re: GKSS results]
Date: Fri, 13 Oct 2006 12:04:50 -0400
Reply-to: mann@psu.edu
Cc: Caspar Ammann <ammann@ucar.edu>

<x-flowed>
Keith,

I also figured this might be what you say, and I understand where you've coming from. This represents a bit of a dilemma too, as it seems unprofessional at best that Zorita and Von Storch have not made their code public, when we of course have made ours public.

There are other sources where we could have gotten the GKSS data--I'm checking w/ Caspar for confirmation. I know that the Cane group has it, and I believe other groups have it now too. So frankly, it is effectively now 'public domain' whether VS and Zorita like it or not!

I propose, hoping that there is no loud objection, that we will include a line in our response indicating that we have confirmed that we get similar results using the GKSS Erik simulation. We'll leave it at that. We don't need to show that result necessarily, unless the editor/reviewers demand to see proof, and we certainly don't have to reveal where we got the GKSS data. As I mentioned, there are enough groups out there that now have it, that VS and Zorita would not know the source, and we would not reveal it.

We feel as if we cannot completely hide the fact that we have confirmed our result w/ GKSS, hence the "compromise" suggested above. Meanwhile, we can pursue a more thorough, official collaborative effort in the future.

Thoughts on this?

thanks,

mike

--

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<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

Return-Path: <k.briffa@uea.ac.uk>

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X-Spam-Level:

X-Spam-Status: No, score=-2.6 required=5.0 tests=AWL,BAYES_00 autolearn=ham
version=3.1.3

X-Original-To: mann@meteo.psu.edu

Delivered-To: mann@meteo.psu.edu

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by mail.meteo.psu.edu (Postfix) with ESMTP id 08C5B204B4A
for <mann@meteo.psu.edu>; Fri, 13 Oct 2006 11:51:52 -0400 (EDT)

Received: from mailgate5.uea.ac.uk (mailgate5.uea.ac.uk [139.222.130.185])
by tr12n05.aset.psu.edu (8.13.6/8.13.2) with ESMTP id k9DFpkiX2199660
for <mann@psu.edu>; Fri, 13 Oct 2006 11:51:49 -0400

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by mailgate5.uea.ac.uk with esmtp (Exim 4.50)
id 1GYP3d-0000kt-V7
for mann@psu.edu; Fri, 13 Oct 2006 16:34:50 +0100

Received: from [139.222.104.74] (helo=angara.uea.ac.uk)
by ueams2.uea.ac.uk with esmtp (Exim 4.51)
id 1GYP3d-00037Y-JU; Fri, 13 Oct 2006 16:34:45 +0100

Message-Id: <7.0.0.16.0.20061013163526.03552e98@uea.ac.uk>

X-Mailer: QUALCOMM Windows Eudora Version 7.0.0.16

Date: Fri, 13 Oct 2006 16:36:51 +0100

To: mann@psu.edu

From: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: GKSS results

Cc: Tim Osborn <t.osborn@uea.ac.uk>

In-Reply-To: <452BCB6C.1070306@meteo.psu.edu>

References: <452BCB6C.1070306@meteo.psu.edu>

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X-PSU-Spam-Flag: NO

X-PSU-Spam-Hits: -2.599

<x-flowed>

Mike

Tim and I have discussed this round and round and our response is attached

what do you think

best wishes Keith

At 17:33 10/10/2006, you wrote:

>Dear Tim/Keith,

>
>I hope all is well with both of you.

>
>We've been doing a number of sensitivity tests w/ RegEM using both
>the CSM simulation, and now more recently the GKSS simulation data
>we got from you. There are some methodological developments we'll
>describe soon, related to what is the most reliable regularization
>method in RegEM, ridge regression and truncated total least squares.
>We are now leaning towards the latter because of potential
>non-convergence problems in some cases w/ the former. More on that soon.

>
>More relevant, however, are the results. As you can see from the
>attached plot, RegEM works quite well w/ GKSS, using a short
>calibration period (1900-1980, corresponding to years 900-980 in the
>attached plot) and both white and red pseudoproxy noise (we used
> $\rho=0.5$ in the attached, but similar result for other values).

>
>The most interesting result is that while RegEM reconstructs the
>full NH series well throughout, in the case of the CSM simulation,
>it does modestly underestimate the warmth of the earliest centuries
>in the GKSS Erik simulation (it fits everything else, including the
>LIA cooling, very well). We feel that this is likely due to problem
>of correctly identifying the 'drift' pattern using CFR methods.

>
>The long and short of this is that we would like to be able to show
>this result in a (very short!) J. Climate response we need to
>finalize, to a comment on Mann et al (2005) J. Clim by Zorita and
>Von Storch. We would show you this response for comment of course,
>and would add you as co-authors. We have cleared with Andrew Weaver
>that this would be an acceptable course of action. We are hoping
>you are in agreement with this?

>
>please let us know ASAP, we have to finalize our response within days.

>
>thanks,

>
>mike

>
>--
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>Associate Professor
>Director, Earth System Science Center (ESSC)

>
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>
>
>

--

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<http://www.cru.uea.ac.uk/cru/people/briffa/>
</x-flowed>

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\letter to Mike - 131.10.06.doc"

From: Keith Briffa <k.briffa@uea.ac.uk>
To: mann@psu.edu
Subject: Re: GKSS results
Date: Fri Oct 13 16:36:51 2006
Cc: Tim Osborn <t.osborn@uea.ac.uk>

Mike

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what do you think
best wishes Keith
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The most interesting result is that while RegEM reconstructs the full NH series well throughout, in the case of the CSM simulation, it does modestly underestimate the warmth of the earliest centuries in the GKSS Erik simulation (it fits everything else, including the LIA cooling, very well). We feel that this is likely due to problem of correctly identifying the 'drift' pattern using CFR methods.

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please let us know ASAP, we have to finalize our response within days.

thanks,

mike

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References

1. <http://www.met.psu.edu/dept/faculty/mann.htm>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: [Fwd: Re: GKSS results]
Date: Thu, 19 Oct 2006 08:44:44 -0400
Reply-to: mann@psu.edu

<x-flowed>

Hi Keith,

Certainly not, and sorry for not clarifying. This is the response to the J. Climate comment by Von Storch that we're talking about here. The final draft is due this week, and so that's why I needed to check if you & Tim wanted co-authorship if we were going to show the GKSS result.

We can certainly plan to do a more detailed followup analysis jointly, I would very much enjoy that. Something we've talked about doing is a set of experiments with "mixed proxies" where the proxies have a variable combination of surface temperature and precip components--it will be very interesting to see what happens in these cases.

Perhaps this would be a good opportunity for collaboration, where we could apply this to several different models including CSM and the models you guys are working with?

let me know what you think.

thanks,

mike

Keith Briffa wrote:

> Great Mike - but hope this does not mean that you will exclude our
> possible contribution to this paper
> Keith
>
> At 13:52 18/10/2006, you wrote:
>
>> thanKs Tim. As luck would have it, zorita is providing the data to
>> Caspar anyway so this should now be a moot point. We'll keep you guys
>> updated on things,
>> Mike

>>

>> -----Original Message-----

>>

>> From: Tim Osborn <t.osborn@uea.ac.uk>

>> Subj: Re: [Fwd: Re: GKSS results]

>> Date: Wed Oct 18, 2006 3:37 am

>> Size: 6K

>> To: mann@psu.edu, Scott Rutherford <srutherford@rwu.edu>

>> cc: Keith Briffa <k.briffa@uea.ac.uk>

>>

>> Hi Mike, your suggested compromise is acceptable to both Keith and

>> me. Good luck with the J. Clim. response. Cheers, Tim

>>

>> At 17:04 13/10/2006, Michael E. Mann wrote:

>> >Keith,

>> >

>> >I also figured this might be what you say, and I understand where
>> >you've coming from. This represents a bit of a dilemma too, as it
>> >seems unprofessional at best that Zorita and Von Storch have not
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>> >confirmed our result w/ GKSS, hence the "compromise" suggested
>> >above. Meanwhile, we can pursue a more thorough, official
>> >collaborative effort in the future.

>> >

>> >Thoughts on this?

>> >

>> >thanks,
>> >
>> >mike
>> >
>> >--
>> >Michael E. Mann
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>> >X-Spam-Level:
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>> autolearn=ham
>> > version=3.1.3
>> >X-Original-To: mann@meteo.psu.edu
>> >Delivered-To: mann@meteo.psu.edu
>> >Received: from tr12n05.aset.psu.edu (tr12g05.aset.psu.edu
>> [128.118.146.135])
>> > by mail.meteo.psu.edu (Postfix) with ESMTP id 08C5B204B4A
>> > for <mann@meteo.psu.edu>; Fri, 13 Oct 2006 11:51:52 -0400
>> (EDT)
>> >Received: from mailgate5.uea.ac.uk (mailgate5.uea.ac.uk
>> [139.222.130.185])
>> > by tr12n05.aset.psu.edu (8.13.6/8.13.2) with ESMTP id
>> k9DFpkiX2199660
>> > for <mann@psu.edu>; Fri, 13 Oct 2006 11:51:49 -0400
>> >Received: from [139.222.130.167] (helo=ueams2.uea.ac.uk)
>> > by mailgate5.uea.ac.uk with esmtp (Exim 4.50)
>> > id 1GYP3d-0000kt-V7
>> > for mann@psu.edu; Fri, 13 Oct 2006 16:34:50 +0100
>> >Received: from [139.222.104.74] (helo=angara.uea.ac.uk)

>>> by ueams2.uea.ac.uk with esmtp (Exim 4.51)
>>> id 1GYP3d-00037Y-JU; Fri, 13 Oct 2006 16:34:45 +0100
>>>Message-Id: <7.0.0.16.0.20061013163526.03552e98@uea.ac.uk>
>>>X-Mailer: QUALCOMM Windows Eudora Version 7.0.0.16
>>>Date: Fri, 13 Oct 2006 16:36:51 +0100
>>>To: mann@psu.edu
>>>From: Keith Briffa <k.briffa@uea.ac.uk>
>>>Subject: Re: GKSS results
>>>Cc: Tim Osborn <t.osborn@uea.ac.uk>
>>>In-Reply-To: <452BCB6C.1070306@meteo.psu.edu>
>>>References: <452BCB6C.1070306@meteo.psu.edu>
>>>Mime-Version: 1.0
>>>Content-Type: multipart/mixed;
>>> boundary="===== _48573031==_"
>>>X-UEA-Spam-Score: -102.8
>>>X-UEA-Spam-Level: -----
>>>X-UEA-Spam-Flag: NO
>>>X-Virus-Scanned: amavisd-sophos
>>>X-PSU-Spam-Flag: NO
>>>X-PSU-Spam-Hits: -2.599
>>>
>>>Mike
>>>Tim and I have discussed this round and round and our response is
>>> attached
>>>
>>>what do you think
>>>
>>>best wishes Keith
>>>
>>>At 17:33 10/10/2006, you wrote:
>>>>Dear Tim/Keith,
>>>>
>>>>I hope all is well with both of you.
>>>>
>>>>We've been doing a number of sensitivity tests w/ RegEM using both
>>>>the CSM simulation, and now more recently the GKSS simulation data
>>>>we got from you. There are some methodological developments we'll
>>>>describe soon, related to what is the most reliable regularization
>>>>method in RegEM, ridge regression and truncated total least
>>>>squares. We are now leaning towards the latter because of potential
>>>>non-convergence problems in some cases w/ the former. More on that
>>>> soon.
>>>>

>> >>More relevant, however, are the results. As you can see from the
>> >>attached plot, RegEM works quite well w/ GKSS, using a short
>> >>calibration period (1900-1980, corresponding to years 900-980 in
>> >>the attached plot) and both white and red pseudoproxy noise (we
>> >>used $\rho=0.5$ in the attached, but similar result for other values).
>> >>
>> >>The most interesting result is that while RegEM reconstructs the
>> >>full NH series well throughout, in the case of the CSM simulation,
>> >>it does modestly underestimate the warmth of the earliest centuries
>> >>in the GKSS Erik simulation (it fits everything else, including
>> >>the LIA cooling, very well). We feel that this is likely due to
>> >>problem of correctly identifying the 'drift' pattern using CFR
>> methods.
>> >>
>> >>The long and short of this is that we would like to be able to show
>> >>this result in a (very short!) J. Climate response we need to
>> >>finalize, to a comment on Mann et al (2005) J. Clim by Zorita and
>> >>Von Storch. We would show you this response for comment of course,
>> >>and would add you as co-authors. We have cleared with Andrew Weaver
>> >>that this would be an acceptable course of action. We are hoping
>> >>you are in agreement with this?
>> >>
>> >>please let us know ASAP, we have to finalize our response within days.
>> >>
>> >>thanks,
>> >>
>> >>mike
>> >>
>> >>--
>> >>Michael E. Mann
>> >>Associate Professor
>> >>Director, Earth System Science Center (ESSC)
>> >>
>> >>Department of Meteorology Phone: (814) 863-4075
>> >>503 Walker Building FAX: (814) 865-3663
>> >>The Pennsylvania State University email: mann@psu.edu
>> >>University Park, PA 16802-5013
>> >>
>> >><http://www.met.psu.edu/dept/faculty/mann.htm>
>> >>
>> >>
>> >>
>> >>

>> >--
>> >Professor Keith Briffa,
>> >Climatic Research Unit
>> >University of East Anglia
>> >Norwich, NR4 7TJ, U.K.
>> >
>> >Phone: +44-1603-593909
>> >Fax: +44-1603-507784
>> >
>> ><http://www.cru.uea.ac.uk/cru/people/briffa/>
>>
>> Dr Timothy J Osborn, Academic Fellow
>> Climatic Research Unit
>> School of Environmental Sciences
>> University of East Anglia
>> Norwich NR4 7TJ, UK
>>
>> e-mail: t.osborn@uea.ac.uk
>> phone: +44 1603 592089
>> fax: +44 1603 507784
>> web: <http://www.cru.uea.ac.uk/~timo/>
>> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
>
>
> --
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> Climatic Research Unit
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The Pennsylvania State University email: mann@psu.edu
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<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Martin Jukes <m.n.jukes@rl.ac.uk>
Subject: Re: Mitrie: Bristlecones
Date: Thu Nov 16 17:21:25 2006

Martin

This last point is likely true (though CO₂ began to rise earlier than the 1960s and the authors of the original paper believed that the high elevation (and concomitant low CO₂ partial pressure) may have amplified the response to small concentration changes. There is also the possibility that a synergistic increase in water-use (and possibly nitrogen use) efficiency could have contributed .

However, I agree that the rapid growth increase is most likely a result of a change in the proportion of net photosynthetic production potential (ie needle mass) relative to the area of living cambium that could occur as a tree shifts from "normal" to strip bark form .If this changes suddenly , as growth occurs only along a small strip rather than around the whole circumference (I know this is oversimplified) then you could easily get this apparent change in growth rate . BUT , if this is seen synchronously in many trees it would be hard to believe that this was the cause. To look at this would require a detail examination of all the data (in relation to the precise sample geometry) . Changing precipitation trends , such as occurred pre- and post the mid 1970s will also confuse things .

Thanks Jan and Rob also for this discussion.

At 17:14 16/11/2006, Keith Briffa wrote:

To: Martin Jukes <m.n.jukes@rl.ac.uk>

From: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: Mitrie: Bristlecones

In-Reply-To: <200611161642.00377.m.n.jukes@rl.ac.uk>

References: <21885F5ACD984446A17A573C47C6D846250054@exchng2.physics.ox.ac.uk>

<p06210202c1821017d50b@[10.15.4.248]> <003701c7098e\$b8b4c850\$9d07d781@geos.ed.ac.uk>

<200611161642.00377.m.n.jukes@rl.ac.uk>

This last point is likely true (though CO₂ began to rise earlier than the 1960s and the authors of the original paper believed that the high elevation (and concomitant low CO₂ partial pressure) may have amplified the response to small concentration changes. There is also the - and I agree that the rapid growth increase is most likely a result of the proportion of net photosynthetic production potential (ie needle mass) relative to the area of living cambium .If this changes suddenly , as growth occurs only along a small strip rather than around the whole circumference (I know this is oversimplified) then you could easily get this apparent change in growth rate . To look at this would require a detail examination of all the data (in relation to the precise sample geometry) .

However, changing precipitation trends pre- and post the mid 1970s will also confuse things .At 16:41 16/11/2006, you wrote:

Thanks for all those comments.

I'm trying to avoid omitting data on the basis of circumstantial evidence, even when it is presented enthusiastically. The Bunn et al. study is interesting (attached) because they show estimated dates of the onset of strip-bark growth. It looks to me as though the growth anomaly of the strip-bark trees relative to the others is more to do with this change than anything else. The onset of a positive growth anomaly in the 1850s is certainly too early to be associated with CO2 increases.

cheers,

Martin

On Thursday 16 November 2006 14:51, Rob Wilson wrote:

> Re: Mitrie: Bristlecones Dear All,

> For the D'Arrigo et al. 2006 paper, I did indeed consider using the Bristlecone pine data.

> However, due to the issues raised by Macintyre and others, we felt that it would be unwise to use these data, especially as our data-set was biased more to higher latitudes.

>

> However, I did look at the data. I do not like ignoring potential data-sets.

>

> Of the BP data that I managed to get my hands on, I identified a significant, but relatively weak, correlation with local gridded mean summer temperatures for three sites. These three sites are: Hermit Hill (N = 38; 1048-1983) and Windy Ridge (N = 29; 1050-1985) from Colorado and Sheep Mountain (N = 71; 0 - 1990) from California.

>

> The attached figure compares the RCS chronology using these data (very similar to the STD version in actual fact) with the North American RCS composite series used in D'Arrigo et al. (2006). Both series have been normalised to the 1200-1750 period to highlight any potential differences in the 20th century.

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> In the 20th century, the BP index values are clearly UNDER the NA mean. I would interpret this as suggesting that there does not appear to be any CO2 influence in the BP data. This of course assumes that there is no

fertilisation effect in the rest of the NA data.

>
> There is also the Salzer BP based temperature reconstruction:

> [1]<http://www.ncdc.noaa.gov/paleo/pubs/salzer2005/salzer2005.html>

>
> again this does not correlate particular well with gridded temperatures - in fact it is driven more by trends, but there are some similarities with my BP chronology and NA series.

>
> I hope this helps the discussion

> best regards

> Rob

>
> ----- Original Message -----

> From: Jan Esper

> To: Keith Briffa ; Martin Jukes ; Myles Allen

> Cc: anders@misu.su.se ; Eduardo.Zorita@gkss.de ; hegerl@duke.edu ; weber@knmi.nl ; t.osborn@uea.ac.uk ; Wilson Rob

> Sent: Thursday, November 16, 2006 1:36 PM

> Subject: Re: Mitrie: Bristlecones

>
>
> ...no, no, not a lot to add from my side. This is much more than I could have said. Except, I once looked at strip bark growth trees in Central Asia, and at least there the cause for this growth form was clear to me (Esper 2000, The Holocene):

>
>
> "Strip-bark growth forms (Ferguson, 1968; Fritts, 1969; Graybill and Idso, 1993; Kelly et al., 1992; Wright and Mooney, 1965) also appear in older Juniper trees. This condition develops as the cambium is damaged locally and will no longer be overgrown. Mechanical damage by rockfall seems to be the principle stimulus for cambial dieback and unilateral growth. In extreme cases only a narrow strip on the stem is still active, creating these eccentric growth forms."

>
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> I didn't visit the Bristlecone sites yet, but the mechanism might be the same (some physical damage).

>
>
> I believe that over time the crown and root system are reduced, but not at the same rate than the reduction in circumference covered by the cambium.

This would be the key for strip bark tree rings being wider than "normal" rings.

>

>

> I am not very convinced that there are long-term fertilization effects by CO2 (but have of course no proof for this). As far as I know, (most) results from free air CO2 enrichment experiments suggest that there is no long-term effect.

>

>

> I Cc Rob Wilson to the mail, as he might have looked at Bristlecone data recently. Perhaps he wants to add something.

>

>

> Best --je

>

>

> At 11:57 Uhr +0000 16.11.2006, Keith Briffa wrote:

> Martin and all,

> I know Franco very well - but he has not worked extensively with the Bristlecones. I still believe that it would be wise to involve Malcolm Hughes in this discussion - though I recognise the point of view that says we might like to appear (and be) independent of the original Mann, Bradley and Hughes team to avoid the appearance of collusion. In my opinion (as someone who has worked with the Bristlecone data hardly at all!) there are undoubtedly problems in their use that go beyond the strip bark problem (that I will come back to later).

> The main one is an ambiguity in the nature and consistency of their sensitivity to temperature variations. It was widely believed some 2-3 decades ago, that high-elevation trees were PREDOMINANTLY responding to temperature and low elevation ones to available water supply (not always related in a simple way to measured precipitation) . However, response functions (ie sets of regression coefficients on monthly mean temperature and precipitation data derived using principal components regression applied to the tree-ring data) have always shown quite weak and temporally unstable associations between chronology and climate variations (for the high-elevations trees at least). The trouble is that these results are dominated by inter-annual (ie high-frequency) variations and apparent instability in the relationships is exacerbated by the shortness of the instrumental records that restrict analyses to short periods, and the large separation of the climate station records from the sites of the trees. Limited comparisons between tree-ring density data (which seem to display less ambiguous responses) imply that there is a reasonable decadal time scale

association and so indicate a real temperature signal , on this time scale .The bottom line though is that these trees likely represent a mixed temperature and moisture-supply response that might vary on longer timescales.

> The discussion is further complicated by the fact that the first PC of "Western US" trees used in the Mann et al. analyses is derived from a mixture of species (not just Bristlecones) and they are quite varied in their characteristics , time span, and effective variance spectra . Many show low interannual variance and a long-term declining trend , up until about 1850 , when the Bristlecones (and others) show the remarkable increasing trend up until the end of the record. The earlier negative trend could be (partly or more significantly) a consequence of the LACK of detrending to allow for age effects in the measurements (ie standardisation) - the very early sections of relative high growth were removed in their analysis, but no explicit standardisation of the data was made to account for remaining slow width changes resulting from tree aging. This is also related to the "strip bark" problem , as these types of trees will have unpredictable trends as a consequence of aging and depending on the precise nature of each tree's structure .

> Another serious issue to be considered relates to the fact that the PC1 time series in the Mann et al. analysis was adjusted to reduce the positive slope in the last 150 years (on the assumption - following an earlier paper by Lamarche et al. - that this increasing growth was evidence of carbon dioxide fertilization) , by differencing the data from another record produced by other workers in northern Alaska and Canada (which incidentally was standardised in a totally different way). This last adjustment obviously will have a large influence on the quantification of the link between these Western US trees and N.Hemisphere temperatures. At this point , it is fair to say that this adjustment was arbitrary and the link between Bristlecone pine growth and CO2 is , at the very least, arguable. Note that at least one author (Lisa Gaumlich) has stated that the recent growth of these trees could be temperature driven and not evidence of CO2 fertilisation.

>

> The point of this message is to show that that this issue is complex , and I still believe the "Western US" series and its interpretation in terms of Hemispheric mean temperature is perhaps a "Pandora's box" that we might open at our peril!

> What does Jan say about this - he is very acquainted with these issues?

>

> cheers

> Keith

> At 15:01 15/11/2006, Martin Jukes wrote:

> Hi,

>
> Concerning Bristlecones, I had a sympathetic reply from Prof. North,
but he
> deferred to the person who wrote the relevant paragraph in the NAS
report
> (Franco Biondi) who is firmly of the view that strip-bark bristlecones
should
> not be used. I've read a few of the articles cited to back up this
statement
> and I am surprised by the extreme weakness of the evidence. There is
one
> study of 27 strip-bark pines which shows that they clearly developed
> anomalous growth around 1850. Attributing this to CO2 is odd, to say
the
> least. I'm writing a brief review of the literature which I'll send
round in
> a few days time.
>
> cheers,
> Martin
>
> On Sunday 12 November 2006 22:21, Myles Allen wrote:
> > Although it probably doesn't feel like it, it seems to me you're
doing
> > rather well...
> >
> > -----Original Message-----
> > From: Martin Juckes [[2]mailto:m.n.juckes@rl.ac.uk]
> > Sent: 10 November 2006 15:24
> > To: anders@misu.su.se; Eduardo.Zorita@gkss.de; hegerl@duke.edu;
> > esper@wsl.ch; k.briffa@uea.ac.uk; Myles Allen; weber@knmi.nl;
> > t.osborn@uea.ac.uk
> > Subject: Mitrie
> >
> > Hello,
> >
> > well, I've had a few exchanges on climateaudit, and decided to leave
> > them to
> > it for a few days.
> >
> > I'm going to send an email to Prof. North of the NAS panel to ask if
he
> > really

> > meant "don't use bristlecones", as he is quoted by McIntyre. I believe
> > it
> > would be incorrect to select sites on the basis of what the data from
> > the
> > sites looks like, and this makes up a substantial part of the argument
> > in
> > Graybill and Idso (1993).
> >
> > Does anyone know where I can get hold of the categorisation of the Sheep
> >
> > Mountain trees used by Graybill and Idso (ca534.rwl from the WDC for
> > paleoclimatology I think) into "strip-bark" and "full-bark"? I've sent
> > an
> > email to the WDC query address.
> >
> > I've also sent of for a publication which is cited by co2science as
> > using
> > Sargasso Sea data with the dating shifted by 50 years (Loehle, 2004,
> > Ecological Modelling). This appears to be a source of considerable
> > confusion
> > among the climate sceptics. The shifted series fits nicely with the idea
> > that
> > the Medieval Warm Period was warmer than the 20th century, so there is a
> >
> > widespread perception that it is being ignored to fudge the results.
> >
> > Apart from a couple of oversights in the documentation of the data files
> >
> > McIntyre hasn't come up with much yet. I need to read up a bit more on
> > the
> > different Tornetraesk/Fennoscandia series. There was an interesting
> > discussion on "cherrypicking", with contributors suggesting that testing

> > the
> > effect of removing each proxy series in turn was "cherrypicking" and
> > that
> > selecting series based on subjective analysis of what the series
look
> > like
> > would be much better!
> >
> > I've had a comment from the editor saying that responses to
non-refereee
> >
> > comments are optional, especially if the comments are not relevant
to
> > the
> > paper.
> >
> > cheers,
> > Martin
> >
> >
> >
> >
> --
> Professor Keith Briffa,
> Climatic Research Unit
> University of East Anglia
> Norwich, NR4 7TJ, U.K.
>
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> Fax: +44-1603-507784
>
> [3]<http://www.cru.uea.ac.uk/cru/people/briffa/>
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>
>
> --
> PD Dr. Jan Esper
> Swiss Federal Research Institute WSL
> Zuercherstrasse 111, 8903 Birmensdorf, Switzerland
> Voice: +41-44-739 2510
> Fax: +41-44-739 2515
> [4]<http://www.wsl.ch/staff/jan.esper>

--

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References

1. <http://www.ncdc.noaa.gov/paleo/pubs/salzer2005/salzer2005.html>
2. <mailto:m.n.juckes@rl.ac.uk>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>
4. <http://www.wsl.ch/staff/jan.esper>
5. <http://www.cru.uea.ac.uk/cru/people/briffa/>
6. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "Rob Wilson" <rob.wilson@ed.ac.uk>
To: "Martin Jukes" <m.n.jukes@rl.ac.uk>
Subject: Re: Mitrie: Bristlecones
Date: Fri, 17 Nov 2006 08:54:54 -0000
Cc: "Keith Briffa" <k.briffa@uea.ac.uk>, "Myles Allen" <allen@atm.ox.ac.uk>, "Jan Esper" <esper@wsl.ch>, <anders@misu.su.se>, <Eduardo.Zorita@gkss.de>, <hegerl@duke.edu>, <weber@knmi.nl>, <t.osborn@uea.ac.uk>

ï»¿

Morning Martin,

It might be worth taking Keith's advice and contacting Malcolm Hughes.

I am not convinced that the Bunn study is fully relevant to addressing the use of BP data from Colorado and California as their study site is Montana. Malcolm gave a presentation earlier this year in Edinburgh which presented updated analyses on his BP work which played down the CO2 influence.

regards

Rob

----- Original Message -----

From: [1]Martin Jukes

To: [2]Rob Wilson

Cc: [3]Keith Briffa ; [4]Myles Allen ; [5]Jan Esper ; [6]anders@misu.su.se ; [7]Eduardo.Zorita@gkss.de ; [8]hegerl@duke.edu ; [9]weber@knmi.nl ; [10]t.osborn@uea.ac.uk

Sent: Thursday, November 16, 2006 4:41 PM

Subject: Re: Mitrie: Bristlecones

Thanks for all those comments.

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cheers,

Martin

On Thursday 16 November 2006 14:51, Rob Wilson wrote:

> Re: Mitrie: Bristlecones Dear All,

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> The attached figure compares the RCS chronology using these data (very similar to the STD version in actual fact) with the North American RCS composite series used in D'Arrigo et al. (2006). Both series have been normalised to the 1200-1750 period to highlight any potential differences in the 20th century.

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> [11]<http://www.ncdc.noaa.gov/paleo/pubs/salzer2005/salzer2005.html>

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> I hope this helps the discussion

> best regards

> Rob

>

> ----- Original Message -----

> From: Jan Esper

> To: Keith Briffa ; Martin Juckes ; Myles Allen

> Cc: [12]anders@misu.su.se ; [13]Eduardo.Zorita@gkss.de ; [14]hegerl@duke.edu ;

[15]weber@knmi.nl ; [16]t.osborn@uea.ac.uk ; Wilson Rob

> Sent: Thursday, November 16, 2006 1:36 PM

> Subject: Re: Mitrie: Bristlecones

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>

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- >
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- >
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- >
- >
- > I Cc Rob Wilson to the mail, as he might have looked at Bristlecone data recently. Perhaps he wants to add something.
- >
- >
- > Best --je
- >
- >
- > At 11:57 Uhr +0000 16.11.2006, Keith Briffa wrote:
- > Martin and all,
- > I know Franco very well - but he has not worked extensively with the Bristlecones. I still believe that it would be wise to involve Malcolm Hughes in this discussion - though I recognise the point of view that says we might like to appear (and be) independent of the original Mann, Bradley and Hughes team to avoid the appearance of collusion. In my opinion (as someone how has worked with the Bristlecone data hardly at all!) there are undoubtedly problems in their use that go beyond the strip bark problem (that I will come back to later).
- > The main one is an ambiguity in the nature and consistency of their sensitivity to temperature variations. It was widely believed some 2-3 decades ago, that high-elevation trees were PREDOMINANTLY responding to temperature and low elevation ones to available water supply (not always related in a simple way to measured precipitation) . However, response functions (ie sets of regression coefficients on monthly mean temperature and precipitation data derived using principal components regression applied to the tree-ring data) have always shown quite weak and temporally unstable associations between chronology and climate variations (for the high-elevations trees at least). The trouble is that these results are dominated by inter-annual (ie high-frequency) variations and apparent instability in the relationships is exacerbated by the shortness of the instrumental records that restrict analyses to short periods, and the large separation of the climate station records from the sites of the trees. Limited comparisons between tree-ring density data (which seem to display less ambiguous responses) imply that there is a reasonable decadal time scale association and so indicate a real temperature signal , on this time scale .The bottom line though is that these trees likely represent a mixed temperature and moisture-supply response that might vary on longer timescales.
- > The discussion is further complicated by the fact that the first PC of "Western US" trees used in the Mann et al. analyses is derived from a mixture of species (not just Bristlecones) and they are quite varied in their characteristics , time span, and effective variance spectra . Many show low

interannual variance and a long-term declining trend , up until about 1850 , when the Bristlecones (and others) show the remarkable increasing trend up until the end of the record. The earlier negative trend could be (partly or more significantly) a consequence of the LACK of detrending to allow for age effects in the measurements (ie standardisation) - the very early sections of relative high growth were removed in their analysis, but no explicit standardisation of the data was made to account for remaining slow width changes resulting from tree aging. This is also related to the "strip bark" problem , as these types of trees will have unpredictable trends as a consequence of aging and depending on the precise nature of each tree's structure .

> Another serious issue to be considered relates to the fact that the PC1 time series in the Mann et al. analysis was adjusted to reduce the positive slope in the last 150 years (on the assumption - following an earlier paper by Lamarche et al. - that this increasing growth was evidence of carbon dioxide fertilization) , by differencing the data from another record produced by other workers in northern Alaska and Canada (which incidentally was standardised in a totally different way). This last adjustment obviously will have a large influence on the quantification of the link between these Western US trees and N.Hemisphere temperatures. At this point , it is fair to say that this adjustment was arbitrary and the link between Bristlecone pine growth and CO2 is , at the very least, arguable. Note that at least one author (Lisa Gaumlich) has stated that the recent growth of these trees could be temperature driven and not evidence of CO2 fertilisation.

>

> The point of this message is to show that that this issue is complex , and I still believe the "Western US" series and its interpretation in terms of Hemispheric mean temperature is perhaps a "Pandora's box" that we might open at our peril!

> What does Jan say about this - he is very acquainted with these issues?

>

> cheers

> Keith

> At 15:01 15/11/2006, Martin Juckes wrote:

> Hi,

>

> Concerning Bristlecones, I had a sympathetic reply from Prof. North, but he

> deferred to the person who wrote the relevant paragraph in the NAS report

> (Franco Biondi) who is firmly of the view that strip-bark bristlecones should

> not be used. I've read a few of the articles cited to back up this statement

> and I am surprised by the extreme weakness of the evidence. There is one

> study of 27 strip-bark pines which shows that they clearly developed anomalous growth around 1850. Attributing this to CO2 is odd, to say

> the

> least. I'm writing a brief review of the literature which I'll send round in

> a few days time.

>

> cheers,

> Martin

>

> On Sunday 12 November 2006 22:21, Myles Allen wrote:

> > Although it probably doesn't feel like it, it seems to me you're

doing

> > rather well...
> >
> > -----Original Message-----
> > From: Martin Juckes [mailto:m.n.juckes@rl.ac.uk]
> > Sent: 10 November 2006 15:24
> > To: [17]anders@misu.su.se; [18]Eduardo.Zorita@gkss.de; [19]hegerl@duke.edu;
> > [20]esper@wsl.ch; [21]k.briffa@uea.ac.uk; Myles Allen; [22]weber@knmi.nl;
> > [23]t.osborn@uea.ac.uk
> > Subject: Mitrie
> >
> > Hello,
> >
> > well, I've had a few exchanges on climateaudit, and decided to leave
> > them to
> > it for a few days.
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> > I'm going to send an email to Prof. North of the NAS panel to ask if

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> > Mountain trees used by Graybill and Idso (ca534.rwl from the WDC for
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> > widespread perception that it is being ignored to fudge the results.

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8. <mailto:hegerl@duke.edu>
9. <mailto:weber@knmi.nl>
10. <mailto:t.osborn@uea.ac.uk>
11. <http://www.ncdc.noaa.gov/paleo/pubs/salzer2005/salzer2005.html>
12. <mailto:anders@misu.su.se>
13. <mailto:Eduardo.Zorita@gkss.de>
14. <mailto:hegerl@duke.edu>
15. <mailto:weber@knmi.nl>
16. <mailto:t.osborn@uea.ac.uk>
17. <mailto:anders@misu.su.se>
18. <mailto:Eduardo.Zorita@gkss.de>
19. <mailto:hegerl@duke.edu>
20. <mailto:esper@wsl.ch>
21. <mailto:k.briffa@uea.ac.uk>
22. <mailto:weber@knmi.nl>
23. <mailto:t.osborn@uea.ac.uk>
24. <http://www.cru.uea.ac.uk/cru/people/briffa/>
25. <http://www.wsl.ch/staff/jan.esper>

From: Malcolm Hughes <mhughes@lrr.arizona.edu>
To: Martin Jukes <m.n.jukes@rl.ac.uk>
Subject: Re: Bristlecone pines
Date: Mon, 20 Nov 2006 16:59:47 -0700
Cc: Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Martin Jukes wrote:

> Hello Prof. Hughes,
>
> I'm involved in a discussion with Stephen McIntyre about Bristlecone pines,
> which I have used as temperature proxies in a recent work
> (<http://www.copernicus.org/EGU/cp/cpd/2/1001/cpd-2-1001.htm>).
>
> I've read the NAS report section on this issue, and most of the references
> cited in the paragraph about bristlecones. I'm unimpressed by the evidence
> presented to support the idea that these valuable records of past climate
> should be discarded. In particular, the most relevant study appears to be
> that of Bunn et al., and this clearly shows anomalous strip-bark growth
> occurring well before significant atmospheric CO2 rises. Their study used
> whitebark pine, which is clearly not the same as bristlecone, but perhaps
> closer than the orange trees cited by Graybill and Idso.
>
> I'm looking for further literature and if possible data on the issue. Do you
> know of any data on anomalous growth in bristlecone strip-bark pines which is
> available for analysis?

> sincerely,
> Martin Jukes

>
Dear Dr. Jukes,

I'm afraid that, apart from the Bunn et al 2003 paper you mention, I know of no other recent literature or data directly relevant to this question. There is a graduate student here working on a dissertation related to this, but neither their data nor any publications on them are available at the moment. Two points concerning Graybill and Idso (1993):
1) I don't think the sour orange trees used in Sherwood Idso's experiments were stripbark - where did this idea come from? 2) When considering the use of upper forest border bristlecone pine (e.g. Sheep Mountain, Campito Mountain, and similar sites mainly above 3100m in the relevant region) as temperature proxies it would be a mistake to discount Figure 3 in Graybill and Idso (1993) which is a comparison of a

ufb bristlecone pine chronology with a smoothed gridpoint reconstruction from maximum latewood density in quite different trees provided by Keith Briffa, one of your co-authors. I read this graph as confirmation of LaMarche's interpretation of the ufb bcp records as having a ~bidecadal temperature signal combined with an interannual precipitation signal, at least before the 20th century. This is referred to Hughes and Funkhouser (2003).

I hope this helps, Malcolm Hughes

</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Malcolm Hughes
Subject: Fwd: Re: Mitrie: Bristlecones In confidence
Date: Tue Nov 21 09:51:52 2006

Malcolm

sorry , I should have cc'd this message sent to my coauthors some time ago(it pre-dates the message to you) , but I was sort of hoping this issue would recede . It would be useful to chat about this and other stuff if you are able to phone (afternoon my time preferably).

Cheers

Keith

Date: Thu, 16 Nov 2006 11:57:09 +0000

To: Martin Jukes <m.n.jukes@rl.ac.uk>, "Myles Allen" <allen@atm.ox.ac.uk>

From: Keith Briffa <k.briffa@uea.ac.uk>

Subject: Re: Mitrie: Bristlecones

Cc: anders@misu.su.se, Eduardo.Zorita@gkss.de, hegerl@duke.edu, esper@wsl.ch, weber@knmi.nl, t.osborn@uea.ac.uk

Martin and all,

I know Franco very well - but he has not worked extensively with the Bristlecones. I still believe that it would be wise to involve Malcolm Hughes in this discussion - though I recognise the point of view that says we might like to appear (and be) independent of the original Mann, Bradley and Hughes team to avoid the appearance of collusion. In my opinion (as someone how has worked with the Bristlecone data hardly at all!) there are undoubtedly problems in their use that go beyond the strip bark problem (that I will come back to later).

The main one is an ambiguity in the nature and consistency of their sensitivity to temperature variations. It was widely believed some 2-3 decades ago, that high-elevation trees were PREDOMINANTLY responding to temperature and low elevation ones to available water supply (not always related in a simple way to measured precipitation) . However, response functions (ie sets of regression coefficients on monthly mean temperature and precipitation data derived using principal components regression applied to the tree-ring data) have always shown quite weak and temporally unstable associations between chronology and climate variations (for the high-elevations trees at least). The trouble is that these results are dominated by inter-annual (ie high-frequency) variations and apparent instability in the relationships is exacerbated by the shortness of the instrumental records that restrict analyses to short periods, and the large separation of the climate station records from the sites of the trees. Limited comparisons between tree-ring density data (which seem to display less ambiguous responses) imply that there is a reasonable decadal time scale association and so indicate a real temperature signal , on this time scale .The bottom line though is that these trees likely represent a mixed temperature and moisture-supply response that might

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The point of this message is to show that that this issue is complex , and I still believe the "Western US" series and its interpretation in terms of Hemispheric mean temperature is perhaps a "Pandora's box" that we might open at our peril!

What does Jan say about this - he is very acquainted with these issues?

cheers

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On Sunday 12 November 2006 22:21, Myles Allen wrote:

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> rather well...

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> -----Original Message-----

> From: Martin Jukes [[1]mailto:m.n.jukes@rl.ac.uk]

> Sent: 10 November 2006 15:24

> To: anders@misu.su.se; Eduardo.Zorita@gkss.de; hegerl@duke.edu;

> esper@wsl.ch; k.briffa@uea.ac.uk; Myles Allen; weber@knmi.nl;

> t.osborn@uea.ac.uk

> Subject: Mitrie

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> the Medieval Warm Period was warmer than the 20th century, so there is a
>
> widespread perception that it is being ignored to fudge the results.
>
> Apart from a couple of oversights in the documentation of the data files
>
> McIntyre hasn't come up with much yet. I need to read up a bit more on
> the
> different Tornetraesk/Fennoscandia series. There was an interesting
> discussion on "cherrypicking", with contributors suggesting that testing
> the
> effect of removing each proxy series in turn was "cherrypicking" and
> that
> selecting series based on subjective analysis of what the series look
> like
> would be much better!
>
> I've had a comment from the editor saying that responses to non-refereee
>
> comments are optional, especially if the comments are not relevant to
> the
> paper.
>
> cheers,
> Martin
>
>
>

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784

[2]<http://www.cru.uea.ac.uk/cru/people/briffa/>

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References

1. <mailto:m.n.juckes@rl.ac.uk>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Tom Crowley <tcrowley@duke.edu>

To: mann@psu.edu

Subject: not so fast

Date: Tue, 02 Jan 2007 10:40:55 -0500

Cc: "raymond s. bradley" <rbradley@geo.umass.edu>, rahmstorf@ozean-klima.de, Eric Steig <steig@ess.washington.edu>, gschmidt@giss.nasa.gov, rasmus.benestad@physics.org, garidel@marine.rutgers.edu, Caspar Ammann <ammann@ucar.edu>, William Connelley <wmconolley@gmail.com>, d-archer@uchicago.edu, rtp1@geosci.uchicago.edu, p.jones@uea.ac.uk

we still don't have an adequate explanation as to how Jack "cooked up" that figure - I do not believe it was purely out of thin air - look at the attached - which I used in the Crowley-Lowery composite just because it was "out there" - I made no claim that it was the record of record, but just that it had been used before. the Lamb ref. is his book dated 1966. I will have to dig up the page ref later. Dansgaard et al. 1975 Nature paper on Norsemen...etc used that figure when comparing what must have been their Camp Century record - have to check that too - where the main point of that paper was that the timing of Medieval warmth was different in Greenland and England!

25 years later my provocation for writing the CL paper came from a strong statement on the MWP by Claus Hammer that the canonical idea of the MWP being warmer than the present was correct and that the 1999 Mann et al was wrong. he kept going on like that I reminded him that he was a co-author on the 1975 paper! that is also what motivated to do my "bonehead" sampling of whatever was out there just to see what happened when you added them all together - the amazing result was that it looked pretty much like Mann et al. the rest is history -- much ignored and forgotten.

I might also point out that in a 1996 Consequences article I wrote - and that Fred Singer loves to cite -- Jack (who was the editor of the journal) basically shoehorned me into re-reproducing that figure even though I didn't like it - there was not an alternative. in the figure caption it has a similar one to Zielinski except that it states "compiled by R.S. Bradley and J.A. Eddy based on J.T. Houghton...so that puts a further twist on this because it point to Houghton not Bradley/Eddy as the source. Jack must have written that part of the figure caption because I don't think I knew those details.

but we still don't know where the details of the figure came from - the MWP is clearly more schematic than the LIA (actually the details about timing of the small wiggles in the LIA are pretty good) - maybe there was a meshing of the Greenland and the England records to do the MWP part - note that the English part gets cooler. they may also have thrown in the old LaMarche record - which I also have. maybe I can schlep something together using only those old three records.

tom

Michael E. Mann wrote:

Ray, happy holidays and thanks for the (quite fascinating) background on this. It would be good material for a Realclimate article. would be even better if someone could get Chris on record confirming that this is indeed the history of this graphic...

mike

raymond s. bradley wrote:

I believe this graph originated in a (literally) grey piece of literature that Jack Eddy used to publish called "Earth Quest". It was designed for, and distributed to, high school teachers. In one issue, he had a fold-out that showed different timelines, Cenozoic, Quaternary, last 100ka, Holocene, last millennium, last century etc. The idea was to give non-specialists a perspective on the earth's climate history. I think this idea evolved from the old NRC publication edited by L. Gates, then further elaborated on by Tom Webb in the book I edited for UCAR, Global Changes of the Past. (This was an outcome of the wonderful Snowmass meeting Jack master-minded around 1990).

I may have inadvertently had a hand in this millennium graph! I recall getting a fax from Jack with a hand-drawn graph, that he asked me to review. Where he got his version from, I don't know. I think I scribbled out part of the line and amended it in some way, but have no recollection of exactly what I did to it. And whether he edited it further, I don't know. But as it was purely schematic (& appears to go through ~1950) perhaps it's not so bad. I note, however, that in the more colourful version of the much embellished graph that Stefan circulated ([1] http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirkl_1.html) the end-point has been changed to 2000, which puts quite a different spin on things. They also seem to have fabricated a scale for the purported temperature changes. In any case, the graph has no objective basis whatsoever; it is purely a "visual guess" at what happened, like something we might sketch on a napkin at a party for some overly persistent inquisitor..... (so make sure you don't leave such things on the table...). What made the last millennium graph famous (notorious!) was that Chris Folland must have seen it and reproduced it in the 1995 IPCC chapter he was editing. I don't think he gave a citation and it thus appeared to have the imprimatur of the IPCC. Having submitted a great deal of text for that chapter, I remember being really pissed off that Chris essentially ignored all the input, and wrote his own version of the paleoclimate record in that volume.

There are other examples of how Jack Eddy's grey literature publication was misused. In a paper in Science by Zielinski et al. (1994) [v.264, p.448-452]--attached-- they reproduced (in Figure 1c) a similarly schematic version of Holocene temperatures giving the following citation, "Taken from J. A. Eddy and R. S. Bradley, Earth-quest 5 (insert) (1991), as modified from J. T. Houghton, G. J. Jenkins, J. J. Ephraums, Climate Change, The IPCC Scientific Assessment (Cambridge Univ. Press, Cambridge, 1990)."

But I had nothing to do with that one!

So, that's how a crude fax from Jack Eddy became the definitive IPCC record on the last millennium!

Happy New Year to everyone

Ray

Raymond S. Bradley
Director, Climate System Research Center*
Department of Geosciences, University of Massachusetts
Morrill Science Center
611 North Pleasant Street
AMHERST, MA 01003-9297
Tel: 413-545-2120
Fax: 413-545-1200

*Climate System Research Center: 413-545-0659

<[2] <http://www.paleoclimate.org>>

Paleoclimatology Book Web Site: [3]<http://www.geo.umass.edu/climate/paleo/html>

Publications (download .pdf files):

[4]<http://www.geo.umass.edu/faculty/bradley/bradleypub.html>

--

Michael E. Mann

Associate Professor

Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075

503 Walker Building FAX: (814) 865-3663

The Pennsylvania State University email: [5]mann@psu.edu

University Park, PA 16802-5013

[6]<http://www.met.psu.edu/dept/faculty/mann.htm>

Attachment Converted: "c:\eudora\attach\Lamb_ext.pdf"

References

1. http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirkl_1.html
2. <http://www.paleoclimate.org/>
3. <http://www.geo.umass.edu/climate/paleo/html>
4. <http://www.geo.umass.edu/faculty/bradley/bradleypub.html>
5. <mailto:mann@psu.edu>
6. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: "Michael E. Mann" <mann@meteo.psu.edu>

To: Tom Crowley <tcrowley@duke.edu>

Subject: Re: not so fast

Date: Tue, 02 Jan 2007 11:18:45 -0500

Reply-to: mann@psu.edu

Cc: "raymond s. bradley" <rbradley@geo.umass.edu>, rahmstorf@ozean-klima.de, Eric Steig <steig@ess.washington.edu>, gschmidt@giss.nasa.gov, rasmus.benestad@physics.org, garidel@marine.rutgers.edu, Caspar Ammann <ammann@ucar.edu>, William Connelley <wmconolley@gmail.com>, d-archer@uchicago.edu, rtp1@geosci.uchicago.edu, p.jones@uea.ac.uk

for those who are interested, there is a paper by Gosse et al (I'm a co-author) explaining why parts of Europe such as central England would have experienced warmer summer conditions relative to present than other regions, related to early land-use change:
Gosse, H., Arzel, O., Luterbacher, J., Mann, M.E., Renssen, H., Riedwyl, N., Timmermann, A., Xoplaki, E., Wanner, H., [1]The origin of the European "Medieval Warm Period", Climate of the Past, 2, 99-113, 2006.

paper available as pdf here:
[2]<http://www.meteo.psu.edu/~mann/shared/articles/Gooseetal-CP06.pdf>
meanwhile, winter warmth could have been due to a strong AO/NAO pattern associated with decreased volcanism and high solar, as discussed in the various Shindell et al paper. this simply underscores the point that we all often make, that one needs to take into account regional factors when interpreting regional records. This is especially relevant to the extrapolation of a long record from England to the entire NH (which appears to have been tacitly done by Jack Eddy?),

mike

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we still don't have an adequate explanation as to how Jack "cooked up" that figure - I do not believe it was purely out of thin air - look at the attached - which I used in the Crowley-Lowery composite just because it was "out there" - I made no claim that it was the record of record, but just that it had been used before. The Lamb ref. is his book dated 1966. I will have to dig up the page ref later. Dansgaard et al. 1975 Nature paper on Norsemen...etc used that figure when comparing what must have been their Camp Century record - have to check that too - where the main point of that paper was that the timing of Medieval warmth was different in Greenland and England!
25 years later my provocation for writing the CL paper came from a strong statement on the MWP by Claus Hammer that the canonical idea of the MWP being warmer than the present was correct and that the 1999 Mann et al was wrong. he kept going on like that I reminded him that he was a co-author on the 1975 paper! that is also what motivated to do my "bonehead" sampling of whatever was out there just to see what happened when you added them all together - the amazing result was that it looked pretty much like Mann et al. the rest is history -- much ignored and forgotten.

I might also point out that in a 1996 Consequences article I wrote - and that Fred Singer loves to cite -- Jack (who was the editor of the journal) basically shoehorned me into re-reproducing that figure even though I didn't like it - there was not an alternative. in the figure caption it has a similar one to Zielinski except that it states "compiled by R.S. Bradley and J.A. Eddy based on J.T. Houghton...so that puts a further twist on this because it points to Houghton not Bradley/Eddy as the source. Jack must have written that part of the figure caption because I don't think I knew those details.

but we still don't know where the details of the figure came from - the MWP is clearly more schematic than the LIA (actually the details about timing of the small wiggles in the LIA are pretty good) - maybe there was a meshing of the Greenland and the England records to do the MWP part - note that the English part gets cooler. they may also have thrown in the old LaMarche record - which I also have. maybe I can schlep something together using only those old three records.

tom

Michael E. Mann wrote:

Ray, happy holidays and thanks for the (quite fascinating) background on this. It would be good material for a Realclimate article. would be even better if someone could get Chris on record confirming that this is indeed the history of this graphic...

mike

raymond s. bradley wrote:

I believe this graph originated in a (literally) grey piece of literature that Jack Eddy used to publish called "Earth Quest". It was designed for, and distributed to, high school teachers. In one issue, he had a fold-out that showed different timelines, Cenozoic, Quaternary, last 100ka, Holocene, last millennium, last century etc. The idea was to give non-specialists a perspective on the earth's climate history. I think this idea evolved from the old NRC publication edited by L. Gates, then further elaborated on by Tom Webb in the book I edited for UCAR, Global Changes of the Past. (This was an outcome of the wonderful Snowmass meeting Jack master-minded around 1990).

I may have inadvertently had a hand in this millennium graph! I recall getting a fax from Jack with a hand-drawn graph, that he asked me to review. Where he got this version from, I don't know. I think I scribbled out part of the line and amended it in some way, but have no recollection of exactly what I did to it. And whether he edited it further, I don't know. But as it was purely schematic (& appears to go through ~1950) perhaps it's not so bad. I note, however, that in the more colourful version of the much embellished graph that Stefan circulated ([3]

http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirkl_1.html
the end-point has been changed to 2000, which puts quite a different spin on things. They also seem to have fabricated a scale for the purported temperature changes. In any case, the graph has no objective basis whatsoever; it is purely a "visual guess" at what happened, like something we might sketch on a napkin at a party for some overly persistent inquisitor.... (so make sure you don't leave such things on the table...). What made the last millennium graph famous (notorious!) was that Chris Folland must have seen it and reproduced it in the 1995 IPCC chapter he was editing. I don't think he gave a citation and it thus appeared to have the imprimatur of the IPCC. Having submitted a great deal of text for that chapter, I remember being really pissed off that Chris essentially ignored all the input, and wrote his own version of the paleoclimate record in that volume.

There are other examples of how Jack Eddy's grey literature publication was misused. In a paper in Science by Zielinski et al. (1994) [v.264, p.448-452]--attached-- they

reproduced [in Figure 1c] a similarly schematic version of Holocene temperatures giving the following citation, "Taken from J. A. Eddy and R. S. Bradley, Earth-quest 5 (insert) (1991), as modified from J. T. Houghton, G. J. Jenkins, J. J. Ephraums, Climate Change, The IPCC Scientific Assessment (Cambridge Univ. Press, Cambridge, 1990)."

But I had nothing to do with that one!

So, that's how a crude fax from Jack Eddy became the definitive IPCC record on the last millennium!

Happy New Year to everyone

Ray

Raymond S. Bradley
Director, Climate System Research Center*
Department of Geosciences, University of Massachusetts
Morrill Science Center
611 North Pleasant Street
AMHERST, MA 01003-9297
Tel: 413-545-2120
Fax: 413-545-1200
*Climate System Research Center: 413-545-0659

<[4] <http://www.paleoclimate.org>>

Paleoclimatology Book Web Site: [5]<http://www.geo.umass.edu/climate/paleo/html>

Publications (download .pdf files):

[6]<http://www.geo.umass.edu/faculty/bradley/bradleypub.html>

--
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[10]<http://www.met.psu.edu/dept/faculty/mann.htm>

References

1. file://localhost/tmp/Goossetal-CP06.pdf
2. <http://www.meteo.psu.edu/~mann/shared/articles/Goossetal-CP06.pdf>
3. http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirkl_1.html
4. <http://www.paleoclimate.org/>
5. <http://www.geo.umass.edu/climate/paleo/html>
6. <http://www.geo.umass.edu/faculty/bradley/bradleypub.html>
7. <mailto:mann@psu.edu>
8. <http://www.met.psu.edu/dept/faculty/mann.htm>
9. <mailto:mann@psu.edu>
10. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: not so fast - an update
Date: Thu, 04 Jan 2007 11:40:37 -0500
Reply-to: mann@psu.edu

<x-flowed>

sounds good Phil, I agree on the forecast. I think its at least 'plausible' ;)

by the way, please remind me what input you need from me at this point on the Wengen paper. I've attached a review paper I've got in press in "AREPS". Not sure if I sent this to you before. Its mostly a re-tread of our '04 Rev Geophys review (which is getting lots of citations if you've noticed!), but a little bit of newer stuff.

talk to you later,

mike

Phil Jones wrote:

>
> Mike,
> I'm just beginning to notice this. I talked to AP about 5 hours ago.
> Our google search has noticed 150 in the last 3 hours.
> I checked one - can't recall whether it was Minneapolis or San Diego,
> but it read OK.

>
> It's a trivial forecast. GW plus ENSO.

>
> Cheers
> Phil

>
> I was hoping to put some of this background to the IPCC figure
> into the Wengen paper, but the more places the merrier.

>
> By the way - when I'll send out a reminder.

>
> Phil

>

>

> At 16:19 04/01/2007, Michael E. Mann wrote:

>

>> by the way, 2007 to be warmest year headline getting a huge amount of
>> play in the U.S. media today,

>>

>> mike

>>

>> Phil Jones wrote:

>>

>>>

>>> Dear All,

>>> The net is closing...

>>>

>>> National Research Council, US Committee for the Global Atmospheric
>>> Research Program, Understanding Climatic Change: A Program for Action,
>>> National Academy of Sciences, Washington, DC, (1975), appendix A.

>>>

>>> This book (Fig A2b) has the same figure as Imbrie/Imbrie. It is
>>> rotated.

>>> It also has the same concept of the IPCC 1990 Figure, changes on
>>> various timescales - all rotated. Loads of Lamb diagrams I have
>>> seen countless times before.

>>>

>>> This book also talks about the impending cooling.....

>>>

>>> John Mitchell also thought the figure is in a book by Gribbin
>>> called '1982 CO2 Review". Anyone recall that one. This isn't
>>> in the CRU Library nor UEA's.

>>>

>>> The direct source of the IPCC diagram is the UK Dept of Environment
>>> document from 1989 which is being posted to me. It though has
>>> a source, which isn't in the document. John and Geoff Jenkins
>>> wrote it though. It is possible that just the last millennium panel
>>> was from this source and the others from this 1975 source.

>>>

>>> Cheers

>>> Phil

>>>

>>>

>>>

>>>

>>>

>>> Dear All (Tom is off to Texas),
>>> David Warrilow has found the said report. A photocopy is being
>>> posted
>>> to me, and two others have been asked if they know more about how
>>> it was arrived at.

>>>
>>> I'll report more when I get news.

>>>
>>> Phil

>>>
>>> Tom,
>>> Here's a reply from David Warrilow (below). I still think it is
>>> in a UK Dept of the Environment report from 1988/89, as does
>>> Chris Folland, so have asked him to think a little more.

>>> I've looked at the 1979 edition, and Figure 45 is the one.
>>> It has a curve, but with the 20th century warmer than the
>>> MWP!! It is said to be based on Lamb (1969). This is a
>>> chapter in the World Survey of Climatology Series
>>> edited by Landsberg. I can't see how you can adapt anything
>>> from this. Hubert's chapter has lots of detail, many figures
>>> which have lines with the phrase 'analyst's opinion' - one
>>> of his favourite terms for things he made up. If it is an
>>> adaptation, then it comes from Hubert's ideas about
>>> England and NW Europe, because these are the curves
>>> in the 1969 chapter.

>>>
>>> Anyone have the 1986 edition, to see if this curve got changed?
>>> The 1986 date is about right for being in the document I recall
>>> seeing. Some of you who've seen my room, will be saying if I had
>>> a better filing system, then I would be able to find it. Despite
>>> keeping
>>> most things I can't find this !

>>>
>>> By the way, it is GREAT PITY, the First IPCC report didn't use
>>> Fig 45. We'd all be very happy and the skeptics wouldn't be going
>>> on about what came out in 1990.

>>>
>>> Attached is the Met Office forecast for 2007. It seems that I'm
>>> getting
>>> the credit for this in the media. All I did was talk to the
>>> Independent about
>>> what I thought 2007 had in store weatherwise. With an El Nino going
>>> on,

>>> I thought it might be a record and just trotted off the typical
>>> things that happen
>>> in El Nino years.

>>>
>>> Cheers
>>> Phil

>>>
>>>
>>> Phil,

>>>
>>> I can't be sure but I think the original diagram is from Imbrie and
>>> Imbrie :

>>> Imbrie, John and Katherine Palmer Imbrie. Ice ages: Solving the
>>> Mystery. Cambridge, Massachusetts: Harvard University Press, 1979,
>>> 1986 (reprint). ISBN 0-89490-020-X; ISBN 0-89490-015-3; ISBN
>>> 0-674-44075-7. p. 25

>>>
>>> You may have it in your library. I am afraid I don't have it to hand,

>>>
>>> David

>>>
>>>
>>>
>>> Prof. Phil Jones

>>> Climatic Research Unit Telephone +44 (0) 1603 592090
>>> School of Environmental Sciences Fax +44 (0) 1603 507784
>>> University of East Anglia
>>> Norwich Email p.jones@uea.ac.uk
>>> NR4 7TJ
>>> UK

>>> -----

>>>

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>>

>>

>> --

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>>
>
> Prof. Phil Jones
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> School of Environmental Sciences Fax +44 (0) 1603 507784
> University of East Anglia
> Norwich Email p.jones@uea.ac.uk
> NR4 7TJ
> UK
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</x-flowed>

Attachment Converted: "c:\eudora\attach\AREPS-preprint061.pdf"

From: William M Connolley <wmc@bas.ac.uk>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Re: not so fast - an update
Date: Thu, 4 Jan 2007 20:41:11 +0000 (GMT)
Cc: Phil Jones <p.jones@uea.ac.uk>, Tom Crowley <tcrowley@duke.edu>, "Michael E. Mann" <mann@psu.edu>, "raymond s. bradley" <rbradley@geo.umass.edu>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Eric Steig <steig@ess.washington.edu>, gschmidt@giss.nasa.gov, rasmus.benestad@physics.org, garidel@marine.rutgers.edu, David Archer <d-archer@uchicago.edu>, "Raymond P." <rtp1@geosci.uchicago.edu>

On Thu, 4 Jan 2007, Caspar Ammann wrote:
> check figure A9, there the 17th century is cold, and this is probably
> the curve that was used. In that case, then its Central England from
Lamb.

Ah, you mean A9(d) (I thought you meant A9(a) for a bit). Yes, that looks pretty similar to IPCC 1990. Though not identical - the scaling is different, but the timing is similar.

-W.

> Caspar
>
>
> William M Connolley wrote:
> > On Thu, 4 Jan 2007, Phil Jones wrote:
> >
> >> The net is closing...
> >>
> >> National Research Council, US Committee for the Global Atmospheric
> >> Research Program, Understanding Climatic Change: A Program for
Action,
> >> National Academy of Sciences, Washington, DC, (1975), appendix A.
> >>
> >> This book (Fig A2b) has the same figure as Imbrie/Imbrie. It is
rotated.
> >> It also has the same concept of the IPCC 1990 Figure, changes on
> >> various timescales - all rotated. Loads of Lamb diagrams I have
> >> seen countless times before.
> >>
> >
> > ? The source for IPCC can't be the 1975 NAS report. That fig is
relatively warm
> > about 1600; the IPCC '90 figure is cold then. And as noted the "MWP"
is colder
> > than 1950. But NAS 75 is the same as I+I, true (they both source to
Lamb 69).
> >
> > Incidentally my I+I says copyright 1979, seventh printing 1998.
> >
> > -W.

From: Melinda Tignor <tignor@ucar.edu>

To: Melinda Tignor <tignor@ucar.edu>, Kevin Trenberth <trenbert@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Peter Lemke <plemke@awi-bremerhaven.de>, Jurgен Willebrand <jwillebrand@ifm-geomar.de>, Nathan Bindoff <n.bindoff@utas.edu.au>, Matilde Rusticucci <mati@at.fcen.uba.ar>, Brian Hoskins <b.j.hoskins@reading.ac.uk>, zhenlin chen <cdccc@cma.gov.cn>

Subject: Re: Upcoming Observations Teleconference - Scheduling Request

Date: Fri, 05 Jan 2007 11:25:04 -0700

Cc: Susan Solomon <ssolomon@al.noaa.gov>, Martin Manning <mmanning@al.noaa.gov>

Greetings,

I have now heard back from all of you and the only date that will work for all of you will be Monday, 8 January (that's Tuesday, 9 January for Nathan & Zhenlin). A small adjustment to the time would be necessary to accommodate all of you. To ensure that we would have enough time for everyone to participate in the entire call we would need to start 30 minutes earlier. So, that would be 12:30 MST/19:30 UTC. I am going to hope that is ok and move forward with establishing the call. Please let me know ASAP if that time adjustment will NOT work for you. You will receive another email from me shortly with the details. Please also let me know if the following contact information changes for you.

| | |
|--------------------|------------------|
| Susan Solomon | +1 303 497 3483 |
| Martin Manning | +1 303 497 4479 |
| Nathan Bindoff | +61 3 62262986 |
| Kevin Trenberth | +1 303 497 1318 |
| Matilde Rusticucci | +54 11 4797 4672 |
| Phil Jones | +44 1953 605643 |
| Brian Hoskins | +44 118 98411308 |
| Peter Lemke | +49 5193 1458 |
| Jurgен Willebrand | +49 431 688475 |
| Zhenlin Chen | + 86 10 68406146 |

Cheers,

Melinda

Melinda Tignor wrote:

Greetings,

I am contacting you to schedule the upcoming teleconference. Due to the extreme variability in your time zones this will likely be a bit tricky and outside "normal" business hours for some of you.

Please let me know as soon as possible your availability for the following times for the

week of 8 Jan - 12 Jan:

Nathan - 7:00 (Hobart)

Kevin - 13:00 (MST)

Matilde - 17:00 (Buenos Aires)

Phil, Brian - 20:00 (UK)

Peter, Jurgен - 21:00 (Germany)

Again, I realize that some of you would be most likely taking this call from home due to

the early or late time.

Many thanks in advance for your prompt response.

Cheers,

Melinda

--

Melinda M.B. Tignor
Program Administrator
Intergovernmental Panel on Climate Change
Working Group I Technical Support Unit
NOAA Chemical Sciences Division
325 Broadway DSRC CSD08
Boulder, CO 80305 USA
Phone: +1 303 497 7072
Fax: +1 303 497 5686/5628
Email: [1]tignor@ucar.edu

--

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Email: [2]tignor@ucar.edu

References

1. <mailto:tignor@ucar.edu>
2. <mailto:tignor@ucar.edu>

From: Phil Jones <p.jones@uea.ac.uk>
To: William M Connolley <wmc@bas.ac.uk>, Caspar Ammann <ammann@ucar.edu>
Subject: Figure 7.1c from the 1990 IPCC Report
Date: Fri, 05 Jan 2007 13:38:40 +0000
Cc: Tom Crowley <tcrowley@duke.edu>, "Michael E. Mann" <mann@psu.edu>, "raymond s. bradley" <rbradley@geo.umass.edu>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Eric Steig <steig@ess.washington.edu>, gschmidt@giss.nasa.gov, rasmus.benestad@physics.org, garidel@marine.rutgers.edu, David Archer <d-archer@uchicago.edu>, "Raymond P." <rtp1@geosci.uchicago.edu>, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, "Mitchell, John FB \ (Chief Scientist\)" <john.f.mitchell@metoffice.gov.uk>, "Jenkins, Geoff" <geoff.jenkins@metoffice.gov.uk>, "Warrilow, David \ (GA\)" <David.Warrilow@defra.gsi.gov.uk>, Tom Wigley <wigley@cgd.ucar.edu>, mafb5@sussex.ac.uk, "Folland, Chris" <chris.folland@metoffice.gov.uk>

Dear All,

I've added a few extra names in the cc of this email list to see if we can

definitively determine where the figure in the subject title comes from. The

background is that the skeptics keep referring back to it and I'd like

to prove that it is a schematic and it isn't based on real data, but on

presumed knowledge at some point around the late 1980s. If you think it is based on something real.

What we'd like to do is show this either on 'Real Climate' or as background

in a future paper, or both.

I'm attaching a few diagrams as background (attaching in order of

introducing them) and giving some earlier thoughts. I assume you all have

a copy of the said diagram in the first IPCC report.

1. This is where the IPCC diagram came from - the top panel is also there, but the middle one from IPCC isn't. This is where Chris Folland

knows it came from. He said it was shoehorned in at a very late date.

This report comes from a UK Dept of the Environment document - where the

first edition predates 1990. David Warrilow says that this was written by

Geoff Jenkins and John Mitchell.

John said the following

I think it was based on a diagram A2 in the national Academy of Sciences

bullet "Understanding climate change" circa 1974 if rmeber correctly- I

can find out in Reading tomorrow- which I can't find in the library- it

was reproduced in one of John Gribbens books and I think a book called the "1982 CO2 review". I think there 6 diagrams and I remember Tom Wigley commenting that only the first (millions of years) and Last (instrumental record) had any credibility.

and

National Research Council, US Committee for the Global Atmospheric Research Program, Understanding Climatic Change: A Program for Action, National Academy of Sciences, Washington, DC, (1975), appendix A.

2. This 1975 book has the 3rd attachment on p130 . This is very similar to one

that David Warrilow said (also attached from Imbrie and Imbrie - second

attachment).

from David

I can't be sure but I think the original diagram is from Imbrie and Imbrie :

Imbrie, John and Katherine Palmer Imbrie. Ice ages: Solving the Mystery. Cambridge,

Massachusetts: Harvard University Press, 1979, 1986 (reprint). ISBN 0-89490-020-X; ISBN

0-89490-015-3; ISBN 0-674-44075-7. p. 25

These look the same if you invert and rotate the one from 1975, and they both

say 'winter conditions in Eastern Europe' - well Imbrie/Imbrie do. They

also say adapted from Lamb (1969). This is the World Survey of Climatology

series from Landsberg, vol2. I've been through this and I can't see much

of a plot anything like those I've attached, so some adaptation. Also I've

no idea what this Eastern European series is!

The IPCC diagram and the UK report clearly don't originate here.

3. Caspar Amman had John Gribbin's 1982 book and sent the 4th attachment. This has a warmer MWP, but is far too cool recently.

So even if this was resmoothed, it wouldn't be before the IPCC one.

4. Ray Bradley sent this text:

I believe this graph originated in a (literally) grey piece of literature that Jack Eddy

used to publish called "Earth Quest". It was designed for, and distributed to, high school

teachers. In one issue, he had a fold-out that showed different timelines, Cenozoic,

Quaternary, last 100ka, Holocene, last millennium, last century etc. The idea was to give

non-specialists a perspective on the earth's climate history. I think this idea evolved

from the old NRC publication edited by L. Gates, then further elaborated on by Tom Webb in

the book I edited for UCAR, Global Changes of the Past. (This was an outcome of the

wonderful Snowmass meeting Jack master-minded around 1990).

I may have inadvertently had a hand in this millennium graph! I recall getting a fax from

Jack with a hand-drawn graph, that he asked me to review. Where he got his version from, I don't know. I think I scribbled out part of the line and amended it in some way, but have no recollection of exactly what I did to it. And whether he edited it further, I don't know. But as it was purely schematic (& appears to go through ~1950) perhaps it's not so bad. I note, however, that in the more colourful version of the much embellished graph that Stefan circulated (

[1]http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirklich.html

the end-point has been changed to 2000, which puts quite a different spin on things. They

also seem to have fabricated a scale for the purported temperature changes. In any case,

the graph has no objective basis whatsoever; it is purely a "visual guess" at what

happened, like something we might sketch on a napkin at a party for some overly persistent

inquisitor..... (so make sure you don't leave such things on the table...).

What made the last millennium graph famous (notorious!) was that Chris Folland must have

seen it and reproduced it in the 1995 IPCC chapter he was editing. I don't think he gave a

citation and it thus appeared to have the imprimatur of the IPCC.

Having submitted a great

deal of text for that chapter, I remember being really pissed off that Chris essentially

ignored all the input, and wrote his own version of the paleoclimate record in that volume.

There are other examples of how Jack Eddy's grey literature publication was misused. In a

paper in Science by Zielinski et al. (1994) [v.264, p.448-452]-- attached-- they reproduced

[in Figure 1c] a similarly schematic version of Holocene temperatures giving the following

citation, "Taken from J. A. Eddy and R. S. Bradley, Earth-quest 5 (insert) (1991), as

modified from J. T. Houghton, G. J. Jenkins, J. J. Ephraums, Climate Change, The IPCC

Scientific Assessment (Cambridge Univ. Press, Cambridge, 1990)."

But I had nothing to do with that one!

So, that's how a crude fax from Jack Eddy became the definitive IPCC record on the last

millennium!

5. Finally, here's one from Stefan, to show how the IPCC diagram gets (first another one which appears to be the IPCC 1990 diagram).

The one I want to attach seems to be within Stefan's email so that

is the end of this email. You can also get to this by going to the link

in Ray's piece above.

It shows how you can embellish a diagram and even get Rembrandt in!

I've also seen many other embellishments mentioning Greenland, the Vikings,

Vineyards in York, frost fairs on the Thames etc. Also I've emailed over

the years for the numbers in the 1990 IPCC Figure. I even got a digitized

version once from Richard Tol and told him what he'd done was ludicrous.

6. So who put it together? Do we blame Ray? Is it a whim of his excellent imagination? I know we will all likely agree with Ray that it is based on absolutely nothing. Tom Crowley thinks it might be based on Lamb and sent the final figure. Now all of those who are or were in CRU know, you should be very careful with Lamb diagrams! This one does not stand any scrutiny and there are several more recent papers by Tom Wigley, Astrid Ogilvie and Graham Farmer that have shown that this final diagram is irreproducible and it was much cooler in the 11-13th centuries. It is also England and summer only. The galling thing is, it does look like the IPCC Figure!!!!!! When Tom sent the figure, he added this text (see below).

The figure looks like Figure 30 (I've not scanned this one), but will,

from his 1982 (reprinted in 1985 and 1995) called Climate History and the Modern World. This figure has series for the year, JJA and DJF.

Someone tell me it isn't based on a Lamb diagram, please....

Phil

Tom Crowley said

we still don't have an adequate explanation as to how Jack "cooked up" that figure - I do

not believe it was purely out of thin air - look at the attached - which I used in the

Crowley-Lowery composite just because it was "out there" - I made no claim that it was the

record of record, but just that it had been used before. the Lamb ref. is his book dated

1966. I will have to dig up the page ref later. Dansgaard et al. 1975 Nature paper on

Norsemen...etc used that figure when comparing what must have been their Camp Century

record - have to check that too - where the main point of that paper was that the timing of

Medieval warmth was different in Greenland and England!

25 years later my provocation for writing the CL paper came from a strong statement on the

MWP by Claus Hammer that the canonical idea of the MWP being warmer than the present was

correct and that the 1999 Mann et al was wrong. he kept going on like that I reminded him

that he was a co-author on the 1975 paper! that is also what motivated to do my "bonehead"

sampling of whatever was out there just to see what happened when you added them all together - the amazing result was that it looked pretty much like Mann et al. the rest is history -- much ignored and forgotten.

I might also point out that in a 1996 Consequences article I wrote - and that Fred Singer loves to cite -- Jack (who was the editor of the journal) basically shoehorned me into re-reproducing that figure even though I didn't like it - there was not an alternative. in the figure caption it has a similar one to Zielinski except that it states "compiled by R.S. Bradley and J.A. Eddy based on J.T. Houghton....so that puts a further twist on this because it point to Houghton not Bradley/Eddy as the source. Jack must have written that part of the figure caption because I don't think I knew those details. but we still don't know where the details of the figure came from - the MWP is clearly more schematic than the LIA (actually the details about timing of the small wiggles in the LIA are pretty good) - maybe there was a meshing of the Greenland and the England records to do the MWP part - note that the English part gets cooler. they may also have thrown in the old LaMarche record - which I also have. maybe I can schlep something together using only those old three records.

tom
Stefan said
the reason why I started to worry about this is the attached graph. Recognise something?

- Used in school teaching in Germany, Austria and Switzerland, is on a website with officially recommended teacher materials
- Used in university teaching in Germany
- Used in politics in Germany by people within the FDP.

Note the vertical axis label on that, by the way. The text that goes with it claims the medieval warm period was 2-4 °C warmer than today. Climate sceptics material, of course.

Cheers, Stefan
13a7140.jpg
--

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NR4 7TJ
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Embedded Content: 13a7140.jpg: 00000001,00002e31,00000000,00000000
Attachment Converted:
"c:\documents and settings\tim osborn\my
documents\eutodora\attach\Global Climate Change.pdf"
Attachment Converted: "c:\documents and settings\tim osborn\my
documents\eutodora\attach\Diagram - climate of the past 1,000 years.pdf"
Attachment
Converted: "c:\documents and settings\tim osborn\my
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trends in global climate past million years.pdf" Attachment Converted:
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"c:\documents and settings\tim osborn\my
documents\eutodora\attach\IPCC1990.jpg" Attachment
Converted: "c:\documents and settings\tim osborn\my
documents\eutodora\attach\Lamb_ext.pdf"

References

1.
http://www.politicallyincorrect.de/2006/11/klimakatastrophe_was_ist_wirkl_1.html

From: "Rasmus Benestad" <rasmus.benestad@met.no>
To: <rahmstorf@ozean-klima.de>
Subject: Re: Figure 7.1c from the 1990 IPCC Report
Date: Sat, 6 Jan 2007 17:58:46 -0000 (GMT)
Reply-to: rasmus.benestad@met.no
Cc: <p.jones@uea.ac.uk>, <ammann@ucar.edu>, <wmc@bas.ac.uk>, <tcrowley@duke.edu>, <mann@psu.edu>, <rbradley@geo.umass.edu>, <steig@ess.washington.edu>, <gschmidt@giss.nasa.gov>, <rasmus.benestad@physics.org>, <garidel@marine.rutgers.edu>, <d-archer@uchicago.edu>, <rtp1@geosci.uchicago.edu>, <k.briffa@uea.ac.uk>, <t.osborn@uea.ac.uk>, <john.f.mitchell@metoffice.gov.uk>, <geoff.jenkins@metoffice.gov.uk>, <David.Warrilow@defra.gsi.gov.uk>, <wigley@cgd.ucar.edu>, <mafb5@sussex.ac.uk>, <chris.folland@metoffice.gov.uk>

I think that this story could possible catch on and make headlines, so I agree that we should be careful. But it's important that we bring the *true* picture out, and it is best that this is done by RealClimate rather than a sceptic site. The general scientific side of the IPCC report (i.e. all the peer-reviewed papers and the scientific theories) is still sound, but to explain how *one* figure was shoe-horned into the report is harder to defend. The sceptics may argue that the IPCC reports are political after all, and this is also what it sounds like if governments 'hoisted the national flag' by having it's own figures inserted last minute. However, by providing an account of the 'evolution of the IPCC report writing', we could possibly give the story a softer landing. E.g. how many times of review the first report underwent as compared to the present report. We should also put this in perspective - the report is large and covers a wide range of topics, and most (all but our case?) is true to the science. There are sometimes a few rotten apples in a good batch, unfortunately. But the important part is that we don't accept rotten apples and that we sort it out! Forthcoming and up-front. Another important side is that this can provide a lesson for the scientific communities.

Rasmus

> Phil, I fully agree. The point is not to blame anyone at all - at least
> my point was to track down the source in order to be able to show the
> skeptics (or in my special case, the school authorities) that this old
> graph is completely superseded and should not be used any more in
> teaching! And I also see your problem: what we are finding out now
makes
> the IPCC process look somewhat unsophisticated back in 1990, so it is
a
>
> diplomatic conundrum how to be completely truthful in reporting this,
as
> we need to be as scientists, without providing the skeptics undue
> fodder for attacking IPCC. But maybe we're too concerned - the
skeptics

> can't really attack IPCC easily in this case without shooting
> themselves in the foot.
>
> Cheers, Stefan
>
> --
> Stefan Rahmstorf
> www.ozean-klima.de
> www.realclimate.org

--
Rasmus E. Benestad
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From: Kevin Trenberth <trenbert@ucar.edu>
To: Susan Solomon <Susan.Solomon@noaa.gov>
Subject: Re: Science presentation for Paris
Date: Mon, 08 Jan 2007 15:31:18 -0700
Cc: Phil Jones <p.jones@uea.ac.uk>, mmanning@al.noaa.gov

One too many 0's. 0.005.

Kevin

Susan Solomon wrote:

Phil,

Thanks. This comes up both in the presentation and in SPM language.

A suggested merge of Phil's text below with the SPM language we have implies replacing the sentence on page SPM-5, 6-7 with the following proposal:

Sites affected by the urban heat island effect are identified and excluded from these averages, so that remaining uncertainties due to this effect are negligible (less than 0.0005°C per decade).

This would address several comments asking us to explain what is done with UHI.

OK?

Susan

At 3:52 PM +0000 1/8/07, Phil Jones wrote:

Kevin, Susan,

On the UHI (slide 9) we should probably change the middle bullet. The first and third are not in dispute. May be better to spell out SSTs though, or say marine air temperatures. SSTs are used as anomalies though to approximate MATs.

Middle bullet currently says

o Major influences are identified and excluded from the records used to create the continental and global values

Perhaps we should refer directly to David Parker's paper on UHIs, where he couldn't detect any difference in trends (averaged for 200+ cities) in temperatures on calm nights (when you'd expect the biggest effect) compared to windy nights (when you'd expect the least).

There are two aspects to the major influences.

1. Some sites are removed. This isn't many as a % of the total (about 1%).
2. We include in Brohan et al (2006) an estimate of urbanization in the calculation of the errors. This is 0.0055 deg C/decade since 1900. It is a one-sided 'error'. If you look very closely the error range in this paper and in some of the Ch 3 figures is slightly one-sided. This figure comes from Jones et al. (2001) , which came from Jones et al. (1990).

Difficulty with all UHI work is that there are countless papers looking at individual sites - which generally use a site in the city centre. This site is rarely one used in the dataset - generally an airport is instead. It is made worse by then looking at individual days and not monthly averages. Only Jones et al. (1990), Parker (2005,2006) and Peterson have looked at large scales.

So

Affected site are identified and excluded from the records used to create the continental and global values (as not all sites are tested, part of the error range assumes an urban component of 0.0055 deg C/decade)

Cheers

Phil

At 22:47 07/01/2007, Kevin E Trenberth wrote:

Susan

Many thanks for the feedback. My comments and explanations follow. I'll expressly ask Phil to respond to us on the UHI issues and what we should say succinctly. I am keen to get further feedback on what to exclude. I had decided to exclude the full slide on all the regional precip trends because it is too detailed and would take too long to go through and so the zonal mean latitude-time series captures a lot of the changes. Personally I would like to have both but the issue will be time and simplicity of message, and hence my decision to drop the series: implicitly those are included of course because they are in the chapter.

> Kevin,

> Many thanks for the preview. I agree that the

> presentation has improved, thanks for that. I

> would like to offer the following suggestions:

>

> 1) Ramaswamy will cover radiative forcings, and

> will do so comprehensively including aerosols,

> ozone, etc. Calling out CO2 and N2O on your

> title slide will likely raise queries about why

> you cite those and not others. I suggest that

> you drop that bullet from your first slide.

Yes slide 1 at present is more comprehensive and perhaps more appropriate for you to use. In general with these slides that context will be desirable but perhaps not for Paris.

>

> 2) The chapter relates changes in DTR to clouds,
> and possibly aerosols and land use. The chapter
> doesn't explicitly say DTR changes are linked to
> dimming. While I personally would agree this is

> scientifically quite reasonable, your slide 8
> would be easier for people to understand and will
> avoid confusion if its language followed the
> chapter so replacing the word dimming on the
> slide with clouds, possibly linked to aerosols
> and land use, would be helpful.

I understand: indeed we did not expressly say "dimming" but in the discussion of dimming it clearly relates to clouds and aerosol. My thinking here is that some may well be aware of dimming but not of changes in clouds, so I thought that terminology might be helpful rather than add confusion. Other views appreciated.

>

> 3) Slide 9 says major influences of UHI are
> identified and excluded. Can this slide please
> be clearer as to what is meant by this and what
> exactly is done? I think it will benefit all if
> we avoid spending a lot of time explaining what
> 'major influences' are and what 'minor
> influences' aren't covered, how big those are,
> etc.

>

Let me ask Phil to suggest a couple of bullets.

> 4) A number of governments have asked for more
> clarity on where heavy precip has increased. You
> show it nicely in slide 16 but language on the
> slide will help us when the discussion of
> language comes up. In the extremes table we say
> that heavy precip has increased 'over most land
> areas' and if the title of this slide were
> 'Proportion of heavy rainfalls have increased
> over most land areas' that would be very helpful
> in laying ground for that.

Heavy precip is confusing, because some analysis are in absolute terms: and others are in terms of the percentage of precip that is heavy. The

latter change is much more universal, and the main exceptions are where precip amounts have decreased, implying a drier regional climate. Since our report there is anew report in Science on extremes in India in the monsoon increasing and there they talk about real extremes. In the slide we already say "proportion of heavy rainfalls are increasing" so the suggestion is to add "most land areas"? OK.

>

- > 5) What is the reference for slide 20? it's a
- > nice image but if it's not in the report then
- > we'll need to discuss that. Slide 19 covers
- > similar content very well, I think so the second
- > one on pdsi could be dropped.

Slide 20 is from Dai et al 2004. It is extensively discussed in the full report in section 3.3.4 and was featured in some email discussions for the TS related to the trend in the previous slide, resulting in some refinement of the FAQ 3.2. Whereas slide 19 is for all of PDSI, slide 20 separates out PDSI above and below a threshold of 3 and -3 and takes it apart to examine the precip and temperature contributions. It is quite complementary in that regard and shows more explicitly that it is the dry spells that increased first from precip decreases and second from temperature effects.

>

- > 6) The Emanuel (2007) slide is nice but that
- > paper has not been assessed in our report. If
- > you are seen by governments to be making your
- > argument for the hurricane statement based on the
- > Emanuel (2007) paper, we will almost certainly
- > have challenges to the hurricane statement on
- > procedural grounds -- which is not what we want
- > to invite. Even though it is an update, it is
- > substantially different from the published one
- > that is assessed.

No that is not true. In our discussion in section 3.8.3 we note that the original Emanuel (2005a) set of curves was revised and discussed in Emanuel (2005b) in response to the comment by Landsea. But that response did not publish the revised curve; instead it appeared on Emanuel's website. It was that curve we discuss in the report (and the main reason we did not show it was because it had changed) and we say "the PDI increasing by about 75% (versus about 100%) since the 1970s (Emanuel 2005b)." The 100% was the original finding. Now there is a further minor refinement in the 2007 paper (in response to further complaints by Landsea, the corrections to the record to make the surface p and wind estimates compatible was not done at the highest wind speeds: very small changes) but an advantage is

that it is updated to include more years: through 2005. It is standard practice for obs time series to be updated and that is mainly what the new curve does. It is not at all at odds with what we discuss already.

> You can make a similar basic

- > point using assessed material by putting one of
- > the two Webster et al panels next to the SST
- > trend in slide 27, highlighting the recent trends
- > in both SST and intense storms with your nice
- > animated ovals (and replacing the ACE figure,
- > which uses non-satellite data). While the
- > Webster figure itself wasn't explicitly in the
- > chapter, the paper was referenced so I think that
- > can be defended.

The SST curve though is for N Atlantic only and the Webster stuff is global.

We could replace the ACE curve with the numbers curve from slide 28?

With these explanations, I look forward to further suggestions.

>

- > To respond to some of your other queries: I
- > think slide 5 is better than slide 6 - showing
- > all the data is nice. I agree with the idea of
- > removing the Sahelian series.

Agree with both.

I suggest putting

- > back the large-regions rainfall trends slide for
- > several reasons (replacing the zonal mean time
- > series figure with the trends figure). It is
- > the trends figure that maps to the language in
- > the SPM which is what we are trying to explain
- > here - the zonal means are not what we explicitly
- > talk about in the SPM. If you don't explicitly
- > defend our SPM paragraph, then we certainly risk
- > losing it or at best wasting a lot of time on it.

See comments above. I'll see if I can do something else.

- > I also think the trends image is clearer for the
- > non-expert than slide 15 showing the zonal means
- > (although as you know I am a big fan of slide 15
- > personally on a scientific level).

>

- > There probably still are too many slides and it
- > will be helpful if we all think hard about which
- > of these is most needed. In cases where queries
- > are from just one or two governments, or are more

- > technical than they are likely to raise in the
- > plenary, etc., it will be better to be shorter.

>

I look forward to comments from others as to which, if any, should be excluded. Of course I love them all.

- > The comments make clear that we are going to be
- > queried on the increases in heat waves statement
- > as being too weak and only backed up in the FAQ.
- > I personally like the European example but if you
- > could also possibly put some text on that slide
- > to help back it up more broadly, that will help
- > to avoid challenges (please see the comments).

I included slide 22 which shows the shift in distribution of hot days and cold nights, and I thought this might be better than the Alexander et al maps. Again we run into too many slides. The change in hot days of course relates to heat waves, because the change in extremes relates to the whole pdf. The term heat waves is very subjective and the time scale is not always clear. There was a heat wave on east coast (New York 71F yesterday) although part of a month long warm period. The other main discussion of heat waves in our text is for Australia and I took out the slide of Australia temperatures vs precipitation in the first version (that Brian and Matilde have not seen). There is not much we can do here. The preponderance of evidence from all the statistics and studies demonstrates a clear increase in heat waves, even if there is not a definitive study just on heat waves. That is what we have to say.

Regards

Kevin

>

- > I'll probably have more comments when we talk but I hope this is helpful.

> bests,

> Susan

>

>

- > At 2:17 PM -0700 1/5/07, Kevin Trenberth wrote:

>>Hi all

>>I received some very helpful comments from
>>Jurgen and I have revamped the slides in the
>>light of the comments. I am cc'ing Matilde and
>>Brian as they are part of telecon. Please see
>>the attached. In all cases I have simplified the
>>presentation by placing the take home message at
>>the top. There are 30 slides here. At present
>>3 are hidden as possible alternates. Also some

>>should be dropped: your choice. The slides are

>>designed to address what was seen as the biggest
>>sources of misunderstanding in the comments on
>>the SPM.

>>The telecon will presumably discuss whether my
>>perceptions on that are the same as others.

>>

>>Slide 4 may now be somewhat redundant with the
>>added years on slide 2. Turns out the cleanest
>>separation is for top 8 years graphically, but
>>they do not include 1999 or 2000. Suggestions?

>>I made a new graphic of the land T vs SST
>>differences, and that is slide 6 but it could be
>>replaced by slide 5. Your choice.

>>I simplified slide 14 (on precip) and removed
>>the slide with all the time series.

>>I have cleaned up many others somewhat.

>>I would be inclined not to show the slide on the Sahel drought (21).

>>I added an extra new slide on hurricanes using
>>Kerry Emanuel's updated and corrected series.

>>So at present there are 5 slides on hurricanes
>>and at least 2 of those should be removed. The
>>Emanuel one has the advantage over the Webster
>>one of including SST. Of these only slide 27
>>includes figures from the chapter, yet I would
>>be inclined to drop that one. Your views on this?

>>

>>Slides 2 thru 12 are on aspects of temperature
>>13-16 and maybe 17 are on precipitation
>>17 to 21 are on drought

>>22 and 23 are on extremes and heat waves

>>24 and 25 deal with circulation and relations between T and precip

>>26 to 30 deal with tropical cyclones.

>>

>>To wrap up I repeated the first slide: and I
>>added a little piece to the first slide (I know
>>this will not make Susan happy, and I would not
>>include in Paris, but I thought it was funny).

>>Please view as slide show.

>>

>>That would leave about 24 slides. Some could
>>count as 1, e.g. 9 and 10 go together and would

>>take less than a minute. But I would guess a
>>minute average: order 25 minutes here.
>>Please do not use these slides at least until after the report is
>> approved.

>>
>>Regards
>>Kevin

>>
>>--
>>*****

>>Kevin E. Trenberth e-mail: [1]trenbert@ucar.edu
>>Climate Analysis Section, [2]www.cgd.ucar.edu/cas/trenbert.html
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>>Attachment converted: Discovery:C3IPCCParis.ppt (SLD3/«IC») (00377B45)

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>
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References

1. <mailto:trenbert@ucar.edu>
2. <http://www.cgd.ucar.edu/cas/trenbert.html>
3. <http://www.cgd.ucar.edu/cas/trenbert.html>
4. <mailto:p.jones@uea.ac.uk>
5. <mailto:trenbert@ucar.edu>
6. <http://www.cgd.ucar.edu/cas/trenbert.html>

From: Keith Briffa <k.briffa@uea.ac.uk>

To: Melinda Marquis <marquis@ucar.edu>, Jonathan Overpeck <jto@u.arizona.edu>, Eystein Jansen <eystein.jansen@geo.uib.no>

Subject: Re: AR4 Paleoclimate Teleconference

Date: Tue Jan 9 09:32:35 2007

Cc: chen zhenlin <chenzhenlin@hotmail.com>, czl <cdccc@cma.gov.cn>, Susan Solomon <ssolomon@al.noaa.gov>, Martin Manning <mmanning@al.noaa.gov>

THis time is fine for me and the number you have is correct. Cheers

Keith

At 18:38 08/01/2007, Melinda Marquis wrote:

Dear Peck, Eystein and Keith,

Thank you for agreeing to meet this week (Thurs., Jan. 11) to discuss paleoclimate items. Martin will send you a follow-up email with an agenda to focus the teleconference discussion.

In the meantime, if you would please confirm or correct the phone numbers where you can be reached, I would be grateful.

Jonathan Overpeck

Tucson, AZ, U.S.

9:00 a.m., Jan. 11 (Thurs.)

1 520 622 9065

Eystein Jansen

Bergen, Norway (Oslo-time)

5:00 p.m., Jan. 11 (Thurs.)

47 5558 3491

Keith Briffa

Norwich, U.K. (London-time)

4:00 p.m., Jan. 11 (Thurs.)

44 1603 593 909

Chen Zhenlin

Beijin, China [Please send phone for a midnight call.]

12 midnight Thurs.-Fri.

Cheers,

Melinda

--

Dr Melinda Marquis, Deputy Director, IPCC WG I Support Unit

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--

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[1]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Caspar Ammann <ammann@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: That darned diagram
Date: Tue, 09 Jan 2007 10:31:44 -0700

Content-Type: text/plain; charset=ISO-8859-1; format=flowed
X-MIME-Autoconverted: from 8bit to quoted-printable by routt.cgd.ucar.edu id 109HVngh027823

<x-flowed>

Phil,
here the graphs from the Brooks 1949 (2nd edition) that we have at NCAR.
One is temperature the other precip reconstructions.
Caspar

Phil Jones wrote:

>
> Tom, Caspar,
> Keep the attached to yourself. I wrote this yesterday,
> but still need to do a lot more. I added in a section
> about post-Lamb work in CRU, but need to check out
> the references I've added and look at the extra one
> from 1981 that you've sent. This may take me a little
> time as I'm away Weds/Thurs this week. I see my name
> on an abstract, by the way, that I have no recollection of !
> I presume this has something in about instrumental global
> temps. This abstract isn't in my CV!!!!
>
> So your point (3) needs to document that we knew the
> diagram wasn't any good, as well as how far back it goes.
> Knowing Hubert on some of his other 'breakthroughs!'
> it is clearly possible it goes back to Brooks !
>
> On the post-Lamb work in CRU, I recall talking to Graham
> (maybe mid-1980s) when he was comparing recent CRU work
> with Lamb - correlations etc. Did that ever see the light of day
> in these pubs or elsewhere? I will look. It isn't in the chapter
> Astrid and he wrote in the CRU book from 1997. I recall some
> very low correlations - for periods from 1100 to 1500.
>
> This is all getting quite complex. It clearly isn't something that
> should be discussed online on RC - at least till we know all

> the detail and have got the history right as best we can. A lot
> of this history is likely best left buried, but I hope to summarise
> enough to avoid all the skeptics wanting copies of these
> non-mainstream papers. Finding them in CRU may be difficult!
>
> As for who put the curve in - I think I know who did it. Chris may
> be ignorant of the subject, but I think all he did was use the
> DoE curve. This is likely bad enough.
> I don't think it is going to help getting the real culprit to
> admit putting it together, so I reckon Chris is going to get the blame.
> I have a long email from him - just arrived. Just read that and he
> seems to changing his story from last December, but I still
> think he just used the diagram. Something else happened on
> Friday - that I think put me onto a different track. This is all like
> a mystery whodunit.
>
> In the meantime - any thoughts on the attached welcome. Getting the
> level of detail required is the key.
>
> I need to do a better diagram - better scanning etc.

> Cheers
> Phil

> At 18:02 06/01/2007, Tom Wigley wrote:

>> Phil,
>>
>> I see the problems with this in terms of history, IPCC image,
>> skeptix, etc. I'm sure you can handle it. In doing so, you might
>> consider (or not) some of these points.
>>
>> (1) I think Chris Folland is to blame for this. The issue is not
>> our collective ignorance of paleoclimate in 1989/90, but
>> Chris's ignorance. The text that was in the 1990 report (thanks
>> for reminding us of this, Caspar) ameliorates the problem
>> considerably.
>>
>> (2) Nevertheless, 'we' (IPCC) could have done better even then.
>> The Rothlisberger data were available then -- and could/should
>> have been used.
>>
>> (3) We also already knew that the Lamb UK record was flawed.

>> We published a revision of this -- but never in a mainstream
>> journal because we did not want to offend Hubert. I don't have
>> the paper to hand, but I think it is ...
>>
>> Wigley, T.M.L., Huckstep, N.J., Mortimer, R., Farmer, G., Jones, P.D.,
>> Salinger, M.J. and Ogilvie, A.E.J., 1981: The reconstruction of European
>> climate on decadal and shorter time scales. (In) Extended Abstracts,
>> First Meeting, Reconstruction of Past Climates Contact Group, EEC
>> Directorate-General for Science, Research and Development, Brussels,
>> Belgium, 83-84.
>>
>> It could be ...
>>
>> Wigley, T.M.L., Farmer, G. and Ogilvie, A.E.J., 1986: Climate
>> reconstruction using historical sources. (In) Current Issues in Climate
>> Research (eds. A. Ghazi and R. Fantechi), D. Reidel Publishing
>> Company, Dordrecht, Netherlands, 97-100.
>>
>> The point of this paper (whichever one it is) is that it covers only
>> the decadal variation -- but it shows that Lamb was out to lunch
>> even on these time scales. As you know, this arose from his uncritical
>> use of historical sources -- a problem exposed in a number of CRU
>> papers in the 1980s, starting with Bell and Ogilvie in Climatic Change.
>>
>> So part of the issue is where did Hubert get the century time scale
>> changes in that diagram? The answer is, mainly from his own fertile
>> imagination. For this he tried to synthesize both his flawed historical
>> record for England (and records for Europe, equally flawed) and
>> proxy data from many sources, again accepted uncritically. Still,
>> there almost certainly was a LIA in Europe in the 17th/18th
>> centuries (but not in Iceland -- at least not in the 17th century).
>> Whether or not there was a significant centuries-long MWE is
>> doubtful in my view.
>>
>> On another historical note, Hubert got many of his ideas from
>> C.E.P. Brooks -- possibly Brooks's work is what inspired Hubert
>> to pursue his climate interests. Of course, he went a lot further
>> (too far) because he had a lot more information to work with.
>> However, it is interesting that Fig. 33 in Brooks (1928) looks a
>> lot like the IPCC90/Lamb Figure -- in Brooks the record goes
>> back further, and there is a very warm period from about 500
>> to 950AD.
>>

>> You should be careful about using "recovery from the LIA" to
>> explain warming after the Maunder Minimum. It is easy to show
>> with (e.g.) MAGICC that there is no such thing -- especially if
>> you accept the view on low-frequency solar forcing espoused
>> in the recent Foukal et al. paper in Nature. If you want some
>> support for this (i.e., the spurious recovery idea) I can send you
>> a diagram.

>>

>> Tom.

>>

>> C.E.P. Brooks, 1928: Climate through the ages. A study of the
>> climatic factors and their variations. Yale Univ. Press, New Haven,
>> 439 pp.

>>

>> [There is a cute item in this book that one never sees any more.
>> At the end of the last page it actually say "THE END".]

>

> Prof. Phil Jones

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> -----

--

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</x-flowed>

Attachment Converted: "c:\eudora\attach\Brooks1949_TempEurope.jpg"

Attachment Converted: "c:\eudora\attach\Brooks1949_PrecEurope.jpg"

From: P.Jones@uea.ac.uk
To: "Brian Hoskins" <b.j.hoskins@reading.ac.uk>
Subject: Re: IPCC WGI Observations Conference Call
Date: Wed, 10 Jan 2007 17:25:07 -0000 (GMT)
Cc: "Susan Solomon" <susan.solomon@noaa.gov>, "Kevin Trenberth" <trenbert@ucar.edu>, "Brian Hoskins" <b.j.hoskins@reading.ac.uk>, martin.manning@noaa.gov, "Matilde Rusticucci" <mati@at.fcen.uba.ar>, "Phil Jones" <p.jones@uea.ac.uk>, "Peter Lemke" <plemke@awi-bremerhaven.de>, "Jurgen Willebrand" <jwillebrand@ifm-geomar.de>, "Nathan Bindoff" <n.bindoff@utas.edu.au>, "zhenlin chen" <cdccc@cma.gov.cn>, "Melinda Marquis" <marquis@ucar.edu>

Dear All,

Agree with Brian's new bullet. I still think we will get comments about what changes with storms. If this is going to lead somewhere we don't want it and cause problems, then the final part is likely best removed.

Reading it again, better if we say .. since the 1960s. About is a little vague.

Back in CRU on Friday. I may be able to get this hotel link to work tomorrow morning.

Cheers
Phil

> Dear All
>
> To me a headline should be kept simple with the detail in the bullets
> below, so I prefer the simple version with "aspects of extreme weather"
> but I guess I am outvoted on that!
>
> For the first part of the bullet on the westerlies I should prefer to
> revert to including the shift and also using the word strengthen rather
> than increase (a number, such as the speed, increases):
>
> Mid-latitude westerly winds have shifted polewards and strengthened
since
> about the 1960s.
>
> The next part on the storms is problematic. I agree with Kevin that we
> should steer clear of the causal language Susan had used. However
> Kevin's words seemed to link a shift in the storm tracks with an
> increase in the winds. Also, as reviewed in 3.5.3, some papers suggest
> that, in addition to a poleward shift in the storm tracks and an
> increase in their average intensity, there is a decrease in the number
> of storms . This is probably too much for the bullet, so that a less
> specific version may be required.
>
> I think the whole bullet could be:
>

> Mid-latitude westerly winds have shifted polewards and strengthened since
> about the 1960s, with associated changes in storms. (3.5)
>
> Brian
>
>
> Susan Solomon wrote:
>
>> Thanks Brian and Kevin for the help.
>>
>> I agree with Brian about reversing the order in the headline sentence
>> but agree with Kevin that a separate bullet is most helpful. I
>> suggest we keep the headline short and simple and just leave the
>> language we have about wind patterns being one of several things
>> changing there. Otherwise it could be read as putting the circulation
>> change into a very high prominence in the headline which isn't quite
>> the emphasis we were discussing, I think.
>>
>> I tried to combine the suggestions and to keep things clear enough
>> that governments won't complain about lack of specifics. If you look
>> over the comments, you will have seen that above all they will not
>> tolerate vague language. Anybody who was in Shanghai (or any other
>> IPCC meeting) can attest to that so please please everybody help make
>> things as specific as we can.
>>
>> So my suggestion for the wind pattern bullet is:
>>
>> Mid-latitude westerly wind speeds have increased in both hemispheres
>> since about the 1960s. This has caused storm tracks to move towards
>> higher latitudes. {3.6}
>>
>> Regarding the headline that proceeds it, can we consider something
>> like this:
>>
>> At continental or ocean basin scales, numerous changes in climate have
>> been observed. These include sea ice extent, precipitation amounts,
>> ocean salinity, wind patterns, and [aspects of extreme weather] OR
>> [the frequency of heavy precipitation and of heat waves, the intensity
>> and duration of drought, and the intensity of hurricanes and
>> typhoons.]
>>
>> The ice sheets have been taken out of the above because they are
>> moving to a consolidated sea level subsection, to deal with several
>> requests for that.
>>
>> Is the new option after wind patterns too specific? I am a little
>> concerned that we will be challenged on that. We could keep what we
>> have: 'aspects of extreme weather'. Equally, I am worried that they
>> will challenge the vagueness of 'extreme weather' so that is why you
>> see two alternatives here.
>>
>> Thoughts?
>> Susan

>>
>>
>> At 8:54 AM -0700 1/9/07, Kevin Trenberth wrote:
>>
>>> Hi Brian
>>> Do you need the first part? Are you rewriting the headline on SPM p
>>> 5 lines 35-37 or are you adding an extra bullet on circulation?
>>> I thought we agreed on the latter, but your piece seems more like the
>>> former.
>>>
>>> If we left the headline alone and added:
>>
>>> * Changes in large-scale atmospheric circulation are apparent
>>> and, in particular, the mid-latitude westerly winds have
>>> shifted polewards and strengthened, altering storm tracks.
>>>
>>> would be an alternative approach. I think it is helpful to mention
>>> storm tracks but not be specific about how they have changed.
>>> What do you think?
>>> Kevin
>>>
>>> Brian Hoskins wrote:
>>>
>>>> Susan
>>>>
>>>> Headline 2
>>>>
>>>> I suggest the following:
>>>>
>>>> At continental or ocean basin scale, numerous changes in climate
>>>> have been observed. Mid-latitude westerly winds (and the associated
>>>> storms) have shifted polewards and strengthened. Other climate
>>>> changes include precipitation,.....
>>>>
>>>> I have taken the suggestion form SPM_327 to reverse the order of the
>>>> first sentence.
>>>>
>>>> The westerly winds sentence is essentially that in a headline in the
>>>> TS.
>>>>
>>>> I should much prefer not to include the bracketed italicised phrase
>>>> on storms. The evidence is less strong. There is some evidence for
>>>> reduced numbers of storms also but no room to say that. It was not
>>>> headlined in the chapter or the TS.
>>>>
>>>> Best wishes
>>>>
>>>> Brian
>>>>
>>>>
>>>> --
>>> *****
>>> Kevin E. Trenberth e-mail: trenbert@ucar.edu
>>> <mailto:trenbert@ucar.edu>

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>>
>>> Boulder, CO 80307 (303) 497 1333 (fax)
>>>
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>>
>>
>
>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: EGU
Date: Mon, 15 Jan 2007 12:45:46 -0500
Reply-to: mann@psu.edu
Cc: raymond s bradley <rbradley@geo.umass.edu>

thanks Phil,

not suggestion you not cite Wegman report, just suggesting you make sure the citation makes clear what the report is...

mike

p.s. where/when did Tom Crowley use it?

Phil Jones wrote:

Mike,

Thanks.

On 1) Putting the last few years in zooms the CET curve much higher.

Tim took out the last few years. I need to make this clearer in the caption.

Padding is an issue with a 50-year smoother.

2) I agree Wegman isn't a formal publication. This was the highest profile example I could come up to show abuse of the curve. if you know of any others then let me know.

Even Tom Crowley shouldn't have used it. There is a belief in the UK, that a curve of UK/CET past temperatures (by summer and winter) exists. It doesn't, but the winter curve from Lamb is probably a lot better than the summer one.

I'll let you know on time-frame when I hear from a few more I've sent the piece to.

Cheers

Phil

At 14:10 15/01/2007, Michael E. Mann wrote:

Phil,

The attached piece is very good, impressive in the detail you've been able to dig up on this. Won't pass this along.

A couple minor comments:

1. I understand the point of the 50 year smoothing, but I think it would still be very useful to show were the most recent decade is on this scale. a lot of the recent warming is washed out by the padding at the end. People will look at this and say "see medieval peak was warmer than present". but that doesn't follow because so much of the warming has been over past two decades.

2. I would not reference Wegman report as if it is a publication, i.e. a legitimate

piece of scientific literature. Its a piece of something else! It should be cited in such a way as to indicate it is not a formal publication, wasn't peer-reviewed, i.e. could be references as a "criticism commissioned by Joe Barton (R, Exxon).

3. I think that Stefan/Gavin were hoping to do something on RC sooner than the timeline you mention. What do you think about this? Do you want to forward the message to them and tell them the timeline you have in mind?

talk to you later,

mike

p.s. thanks very much for the 'nomination' :), but you flatter me. I think that someone farther along in their career such as Keith is more deserving at this time.

Phil Jones wrote:

Ray,

I have been nominating you for several years, as has Andre and Jean - I think. Not sure how much the last two have been involved recently. I haven't been for a few years.

So, congratulations ! If as in previous years, you get asked about future awards, then consider nominating Keith and/or Mike. In the past it has alternated between ice cores and others.

As for a presentation, something on the lines of where we stand etc. will be great.

Gerard seems to be very flexible with the date for CL28. I've no idea how many abstracts there are yet. Haven't done anything on publicity for the session. Later in the week I'll check how many we have. So suggest the session day you want. Avoid Friday - people leave, also a bit on Thursday. Tuesday and Weds tend to have the most people there. I'll likely put you first in a session - not the early morning, but after coffee or lunch. I'll liaise with Gerard. I have to organize everything by next Monday as I'm at the IPCC in Paris from Jan 23 till Feb 2.

Can you two give me your thoughts on the attached? I think this is best in the Wengen meeting summary. Certainly after IPCC has met and likely after June when the chapters come out. Don't pass on to anyone and don't use in Vienna.

Cheers

Phil

PS Are you two getting loads of press cuttings from Mike Schlesinger?

At 18:25 13/01/2007, Michael E. Mann wrote:

Ray, I hadn't heard the announcement. This is wonderful news. You (like Phil) couldn't be more deserving for this.

I'm sorry that I won't be there (EGU comes at a bad time of the Penn State semester). I owe you a drink when next we meet.

Congratulations again!

mike

raymond s bradley wrote:

I was totally surprised to learn I was selected for the EGU's Oeschger medal this year--so if you had anything to do with that, many, many thanks. I knew Hans quite well and so this is especially meaningful for me. Phil got the first Oeschger Medal so I know I am following in his big shoes. But I can't help feeling it's all a clerical error somehow and a correction letter will appear any day now....

But, assuming this is not so...I was asked to give a talk aimed at a non-specialist audience in one of the sessions. I think your session on the last millennium is the obvious session in which to do this, so I will prepare something along the lines of "climate of the last millennium: status and prospect" so I can briefly summarise where we are at and what seems to be needed.

I'll submit an abstract on-line this weekend.

Ray

Raymond S. Bradley

University Distinguished Professor

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< [1]<http://www.paleoclimate.org>>

Paleoclimatology Book Web Site: [2]<http://www.geo.umass.edu/climate/paleo/html>

--

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References

1. <http://www.paleoclimate.org/>
2. <http://www.geo.umass.edu/climate/paleo/html>
3. <mailto:mann@psu.edu>
4. <http://www.met.psu.edu/dept/faculty/mann.htm>
5. <mailto:p.jones@uea.ac.uk>
6. <mailto:mann@psu.edu>
7. <http://www.met.psu.edu/dept/faculty/mann.htm>
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9. <mailto:mann@psu.edu>
10. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Nathan Bindoff <n.bindoff@utas.edu.au>
To: Susan Solomon <ssolomon@al.noaa.gov>
Subject: Re: IPCC WG1 Observations ppt
Date: Mon, 15 Jan 2007 23:17:30 +1100
Cc: Kevin Trenberth <trenbert@ucar.edu>, Peter Lemke <Peter.Lemke@awi.de>, jwillebrand@ifm-geomar.de, Brian Hoskins <b.j.hoskins@reading.ac.uk>, Martin.Manning@noaa.gov, Matilde Rusticucci <mati@at.fcen.uba.ar>, Phil Jones <p.jones@uea.ac.uk>, zhenlin chen <cdccc@cmagov.cn>, Melinda Marquis <Marquis@ucar.edu>, Nathan Bindoff <n.bindoff@utas.edu.au>

G'day Folks

Just to pick up on Susan's comment below, that I am interested in, and perhaps also richard alley in using parts of Peter's presentation for the sea-level rise issues....

Hope to have a new version by the close of tomorrow.

Cheers Nathan

On Fri, 2007-01-12 at 11:26 -0700, Susan Solomon wrote:

> Dear All,
> Thanks for looking and thinking about this.
>
> I should clarify that some of what Peter kindly put into his
> presentation may link to the sea level presentation, so may be better
> moved there. We should consider that carefully. I suspect that
> Peter was trying to avoid undue emphasis on Larsen B alone - because
> other places are showing similar things. So we should evaluate that
> too. While none of the figures themselves are explicitly shown in
> Figure 4 (including the Larsen B one), the material referenced is
> assessed there and Peter has carefully given the papers - so if we
> believe this is needed, it could be considered.

>
> I do like Figure 4.13 but think it would be clearer for this audience
> if it showed just the volume changes rather than the two panels. I
> understand why the technical expert likes both but for this audience
> perhaps just something showing the changes in glacier volume (SLR)
> would be clearer.

>
> bests,
> Susan

>
> At 9:49 AM -0700 1/12/07, Kevin Trenberth wrote:

>>Hi Peter
>>I am a bit alarmed about all of these slides as being too complex
>>and not using material from the chapters enough.

>>
>>For instance Fig 4.13 I found easy to understand but your first
>>slide is not easy: why is Europe in blue going up in a and level in
>>b when the glaciers are retreating? The reason is because this
>>shows the rate of change not the result of the change isn't it?

>>
>>In your second slide I do like the Larsen B ice shelf picture and
>>that provides a nice back drop for some explanation of the new
>>bullet (which is good). But why include the 3 panels on the left?
>>What do they add?

>>
>>I am not sure the next two are needed especially in their current
>>form. None of these are in the chapter. They add too much new
>>material. In my last ppt version I added some place holders taking
>>some figures from the chapter as they are part of the picture that
>>"global warming is unequivocal". I would urge you to include the
>>first two I had, plus one of yours based on the Larsen B slide but
>>with the message from the bullet added, or something like that.

>>
>>Regards
>>Kevin

>>
>>
>>
>>

>>Peter Lemke wrote:
>>>Dear Colleagues,
>>>please find enclosed a ppt-file addressing issues of Chapter 4.
>>>Slide 1: addresses SPM-312 and 314. I suggest to accept 312. The
>>>figure (4.15 from the chapter) indicates an increased rate of
>>>change after about 1990. But I do not think that we have an
>>>indication of an acceleration (continuously increasing rate of
>>>change).
>>>Slides 2,3 and 4: address the increased flow speed of tributary
>>>glaciers after retreat/thinning/loss of ice shelves or floating
>>>glacier tongues in Antarctica and Greenland (comments SPM-349 to
>>>353)

>>>
>>>I did not find any critical comments concerning snow, sea ice and
>>>frozen ground. Therefore I did not prepare any slides for theses
>>>topics.

>>>Best regards,
>>>Peter

>>>
>>>*****
>>>Please note my new e-mail address:

>>>
>>>Peter.Lemke@awi.de

>>>
>>>*****

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From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: See the attached
Date: Wed, 17 Jan 2007 11:17:58 -0500
Reply-to: mann@psu.edu

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Phil,

I've seen this junk already. Look at the co-authors! DeFrietas, Bob Carter: a couple of frauds. I dont' think anyone will take this seriously...

Do you have any advance knowledge you could pass along that would help us gear up to do something on RealClimate? I assume that there will be no surprises in the paleoclimate chapter, but I haven't seen the final draft. Any hints you can drop would be great...

thanks,

mike

Phil Jones wrote:

>
>> Mike,
>
> You've probably seen this. We are slated about p189/190.
> I hope this doesn't come up at the final IPCC meeting in
> Paris. I've nothing to worry about anyway. I wish they
> wouldn't keep going on about it.
>
> The press release after Paris from WG1, by the way will be Feb 2.
> You might like to gear up Real Climate for the week after. Only the
> SPM will be available then. The chapters come later as you'll know -
> I've heard June mentioned. CUP are doing them again.
>
> Cheers
> Phil

>
>
>
>

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</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Susan Solomon <ssolomon@al.noaa.gov>, Susan Solomon <Susan.Solomon@noaa.gov>, Isaac Held <Isaac.Held@noaa.gov>, Ronald Stouffer <Ronald.Stouffer@noaa.gov>, peter lemke <plemke@awi-bremerhaven.de>
Subject: Re: Fwd: [Wg1-ar4-clas] Shorter presentations at Paris
Date: Fri Jan 19 15:36:09 2007
Cc: Melinda_Tignor <tignor@ucar.edu>, Martin Manning <mmanning@al.noaa.gov>, Melinda.Marquis@noaa.gov

Susan

This is very clear and very useful Thanks

Keith

At 15:21 19/01/2007, Susan Solomon wrote:

Keith, Peter, Isaac, Ron,

Thanks to all of you for helping out.

Keith, the audience for the presentations is the policy makers who will be present in Paris. As you have already seen from the comments, many of them are not scientists. The presentations need to be pitched at a non-scientist level. A number of the policy people will be lawyers, and a number will be legalistically looking to find anything that can advance their position. Most of them will however just be looking to ask questions and to better understand, and many will be constructive in how they use the information provided. So it is quite a mix. They should not be given input that distracts from the job at hand. Therefore, these presentations should not bring in new issues not raised in the comments, figures from material outside the report, etc.

I hasten to say that all of us hope there will not be big problems in going through the presentations. The presentations are being carefully prepared by excellent people, so my expectation would be for quite minor changes.

All of the above has been discussed with those preparing the presentations, so a primary role in co-chairing this session is to lend a constructively critical eye, seeking to advance the goal of clarity, conciseness, and sticking to the report rather than straying, if needed. The outcome is not a formal approval statement of the presentation. The outcome is to guide the collective subgroup to a *clear* consensus on what should be changed before the presentation is passed in to the TSU. If there are things that a majority of the group wants to see changed but others do not, you will have a chairman's job to do in finding a solution everyone can live with. It would probably be helpful if you could keep some notes on the agreed changes, since that will help you ensure that you have been clear enough in stating the conclusion. Too often there is a thrash and no closure. A good chair gets agreement with the group.

Thanks again,

Susan

At 1:00 PM +0000 1/19/07, Keith Briffa wrote:

Hi Susan et al

sorry for delayed response - just back from Paris (or so I originally thought as the meeting I was at turned out to be 3 hours away by train). I too am happy to act as you request, though I am still uncertain as to who the specific audience will be and more particularly, what you expect as an outcome of the session (a formal approval statement or recommendation for amendments?).

cheers

Keith

At 00:31 18/01/2007, Susan Solomon wrote:

Dear Peter, Isaac, Ron, and Keith

I am writing to let you know that the agenda for our C/LA meeting to take place in Paris on Saturday and Sunday Jan 27/28 will have your names listed for a proposed role, and I hope you will be able to accept.

At the end of the second day of the meeting, we will go over the set of longer 'science presentations' that will be given informally during the lunchtime sessions. There will

be two parallel sessions from 4-6 pm on Sunday, and I am hoping that Peter/Keith can chair one dealing with drivers, obs, and paleo, while Ron and Isaac can chair one on attribution/sea level/projections.

Earlier on Sat/Sun we will also have gone over the shorter formal presentations that will be used to start each section of the SPM during the meeting.

See below for some more information CLAs requested for preparation of the shorter presentations.

An important point is that the short and long presentations should be consistent and should strongly support the SPM approval process (see below).

We are seeking tough chairmen who could a) keep to a strict time schedule and avoid slippage; b) ensure that a clear statement is made about what the group conclusion is (e.g., if the group feels that a particular presentation should be changed, that needs to be made clear to the person who will hand in the final presentation to the TSU); and c) helps the group to focus on the need for these presentations to communicate with policy people (not overly technical) and help address the comments received (not to digress). In short, to be tough, fair, constructive, and well organized.

Thanks in advance for considering helping with this. If you feel you cannot do it, let me know but I will assume silence is agreement to serve.

best regards,

Susan

Date: Mon, 15 Jan 2007 17:08:01 -0700

From: Susan Solomon <Susan.Solomon@noaa.gov>

To: wg1-ar4-clas@joss.ucar.edu

Cc: zhenlin chen <cdccc@cma.gov.cn>, Martin.Manning@noaa.gov

Subject: [Wg1-ar4-clas] Shorter presentations at Paris

X-BeenThere: wg1-ar4-clas@joss.ucar.edu

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Dear CLAs,

We are writing to address the two types of presentations (shorter and longer) that are to be given in Paris. A number of you have asked about the shorter presentations in particular and we want to clarify that here.

We would like to ask the people who served as section coordinators for each section in our TS/SPM meetings to coordinate pulling together the shorter presentations of not more than 10 slides (Ramaswamy on drivers; Bindoff on observations; Hegerl on attribution, Stocker on projections).

Many of you have kindly already sent around draft material for the longer science presentations, and that has been very helpful. These will occur informally during lunch breaks, or before the morning sessions at the plenary and will not be subject to simultaneous translation. The most interested delegates will typically find these very helpful, and will want to use them to ask you questions.

In addition, during the regular formal sessions and prior to presentation of each of the major sections of the report (drivers, observations, attribution, and projections), we will benefit from a very short presentation that introduces the section. The speaker's words will be subject to simultaneous translation. We suggest that the paleo ice core material be covered as part of the drivers, that the paleo observations be covered as

part of the observations, etc, to speed things up (we can switch speakers but keep slides in the same file).

These shorter presentations are extremely important in setting the stage. They must be very short. We will have an absolute limit of not more than 10 minutes, preferably 5 minutes for the shorter sections of the report namely drivers and attribution). Please do not include more than a maximum of 10 slides. Questions will be strictly limited by the session chair (Susan or Dahe) to matters of clarity (e.g., if an axis isn't clear). We will go over both the shorter and the longer presentations jointly at our preparatory meeting at the UNESCO center on Sat/Sun Jan 27/28 so please come prepared to do that. An agenda for the preparatory meeting will be circulated to you shortly.

The shorter presentations can largely be derived from the longer ones. They will be most helpful if:

- they do seek to provide a general sense of how the section is meant to fit together and some key highlights.
 - they present the figures and tables used in the SPM section to follow, but do not include figures from the chapters unless absolutely essential. Including figures from outside the report could create problems and should be avoided.
 - they avoid raising new issues or suggesting changes from the distributed SPM.
- As some of us have seen in the heated discussions via email about the MOC, sticking to the agreed consensus obtained in the chapter teams is something our colleagues who will not be in Paris would appreciate our doing as much as possible. We will need to agree to all changes to be presented by us to delegates as a team in our preparatory meeting on Jan 27-28. They will choose to seek more and that is what we will have to jointly manage.
- they have very little text on them, as simple as possible.
 - they do not try to cover each bullet.

You may wish to consider whether it is helpful to alternate speakers between your science presentation and these short presentations, so that more of you get a chance to speak.

Some of you asked for sample presentations. You are probably aware that we completed a special report on HFCs/ozone in 2005. The short presentation on our section (section 2) at that session worked extremely well and is appended here as an example in case you want to glance at it, along with the SPM itself. We had much less material to cover of course and more time to do it (this is more than 10 slides but don't be tempted as that was a different situation) but we hope this is still helpful.

We look forward to seeing you and discussing all of the presentations on Jan 27-28.

Best regards,

Susan, Martin, and Dahe

Wg1-ar4-clas mailing list

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References

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From: Kevin Trenberth <trenbert@ucar.edu>

To: david.parker@metoffice.gov.uk

Subject: Re: 2006

Date: Wed, 24 Jan 2007 10:49:21 -0700

Cc: "Kennedy, John" <john.kennedy@metoffice.gov.uk>, Phil Jones <p.jones@uea.ac.uk>

<x-flowed>

Ok that explains several things, I am so glad to know this before going to Paris tomorrow. I made another minor tweak.

Kevin

david.parker@metoffice.gov.uk wrote:

> Kevin

>

> Thanks. The averages of the values in Fig 3.6 over 1961-1990 turned out
> not to be exactly 0.000 owing to missing data in the reference period (a
> perennial problem Phil is well aware of). But Susan (?) wanted the SPM
> curve to average exactly 0.000 in 1961-1990 so the values were shifted
> by somewhere between 0.02 and 0.03.

>

> Regards

>

> David

>

>

> On Wed, 2007-01-24 at 10:09 -0700, Kevin Trenberth wrote:

>

>> John and David

>> Thanks, I have updated the figure using your new low frequency curves,
>> and so I think 3.6 is now redone.

>> However I do not understand the other figure: the global value for T for
>> 2006 seems to be 0.46 not 0.42: it lies above half way between the
>> ticks. Again I have copied the low frequency curve and replaced the one
>> on our figure, but I don't understand the last point.

>> How do these look?

>> Kevin

>>

>> Kennedy, John wrote:

>>

>>> Kevin,

>>>

>>> I have attached updated versions of the diagrams so that you can see

>>> where the 2006 bars and dots should be moved to.

>>>

>>> John

>>>

>>> On Tue, 2007-01-23 at 14:48 -0700, Kevin Trenberth wrote:

>>>

>>>

>>>> David et al

>>>> For Fig 3.6 we need values for globe, NH and SH. I guessed at NH as

>>>> 0.55 and SH as 0.28. But not sure what the new error bars are. I

>>>> reduced them a bit from old ones but not as much as for last year.

>>>> Anyway, take a look at the attached. I also made a teeny extension of

>>>> the blue in each plot. Should I have done that or did the decadal curve

>>>> already include 2006?

>>>> This is what I can do. If you give me the correct error bars I can

>>>> refine a bit more.

>>>> Let me know

>>>> Kevin

>>>>

>>>> david.parker@metoffice.gov.uk wrote:

>>>>

>>>>

>>>>> Phil, Kevin

>>>>>

>>>>> The 2006 global annual average surface temperature anomaly wrt 1961-1990

>>>>> including December data is 0.42+-0.06C (1 sigma) and 2006 remains 6th.

>>>>> Slight upgrades to November and December land data are expected in due

>>>>> course, but this is the final number so far as IPCC is concerned.

>>>>>

>>>>> Regards

>>>>>

>>>>> David

>>>>>

>>>>>

>>>>>

>>>>>

>>>>>

>>>>>

--

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</x-flowed>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Stefan Rahmstorf <rahmstorf@ocean.klima.de>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Caspar Ammann <ammann@ucar.edu>, Ben Santer <santer1@lnl.gov>, "Raymond S. Bradley" <rbradley@geo.umass.edu>, Malcolm Hughes <mhughes@ltrr.arizona.edu>, Phil Jones <p.jones@uca.ac.uk>, James Hansen <jhansen@giss.nasa.gov>
Subject: [Fwd: IPCC and sea level rise, hi-res paleodata, etc.]
Date: Mon, 05 Feb 2007 20:13:54 -0500
Reply-to: mann@psu.edu

Curt, I can't believe the nonsense you are spouting, and I furthermore cannot imagine why you would be so presumptuous as to entrain me into an exchange with these charlatans. What is earth are you thinking? You're not even remotely correct in your reading of the report, first of all. The AR4 came to stronger conclusions than IPCC(2001) on the paleoclimate conclusions, finding that the recent warmth is likely anomalous in the last 1300 years, not just the last 1000 years. The AR4 SPM very much backed up the key findings of the TAR. The Jones et al reconstruction which you refer to actually looks very much like ours, and the statement about more variability referred to the 3 reconstructions (Jones et al, Mann et al, Briffa et al) shown in the TAR, not just Mann et al. The statement also does not commit to whether or not those that show more variability are correct or not. Some of those that do (for example, Moberg et al and Esper et al) show no similarity to each other. I find it terribly irresponsible for you to be sending messages like this to Singer and Monckton. You are speaking from ignorance here, and you must further know how your statements are going to be used. You could have sought some feedback from others who would have told you that you are speaking out of your depth on this. By instead simply blurring all of this nonsense out in an email to these sorts of charlatans you've done some irreversible damage. Shame on you for such irresponsible behavior! Mike Mann -- Michael E. Mann Associate Professor Director, Earth System Science Center (ESSC) Department of Meteorology Phone: (814) 863-4075 503 Walker Building FAX: (814) 865-3663 The Pennsylvania State University email: mann@psu.edu University Park, PA 16802-5013 http://www.met.psu.edu/dept/faculty/mann.htm Return-Path: X-Original-To: mann@meteo.psu.edu Delivered-To: mann@meteo.psu.edu Received: from tr12n04.aset.psu.edu (tr12g04.aset.psu.edu [128.118.146.130]) by mail.meteo.psu.edu (Postfix) with ESMTP id 160CA2D00B0 for ; Mon, 5 Feb 2007 19:53:22 -0500 (EST) Received: from web60817.mail.yahoo.com (web60817.mail.yahoo.com [209.73.178.225]) by tr12n04.aset.psu.edu (8.13.6/8.13.2) with SMTP id 11600Ccf2019402 for ; Mon, 5 Feb 2007 19:53:12 -0500 Received: (qmail 49251 invoked by uid 60001); 6 Feb 2007 00:53:08 -0000 DomainKey-Signature: a=rsa-sha1; q=dns; c=no; s=s1024; d=yahoo.com; h=X-YMail-OSG:Received:Date:From:Subject:To:Cc:In-Reply-To:MIME-Version:Content-Type:Content-Transfer-Encoding:Message-ID; b=folyoWjSumv93mmwscECLmtGDEGDd6Y3/mv2WavTLrekb/5qH8lhkAvbh8+QfRCFOALVKIAXeGEmhPVbFkhVMOGOT Yk4oF2q6wyDIVXV1+BSd06v8o6hJskJ/M+li1R05sH7KOixpNoxvSdjQNCdt1US3zQl3bmCWA4epZNw8=; X-YMail-OSG: gSuRbqAVM1nhqat8Z4GNlp5xY8qoAOh_P_TmtEgvuaLnZ0ixbR.Ev2V_eFEhTn CZQ-- Received: from [128.115.27.11] by web60817.mail.yahoo.com via HTTP; Mon, 05 Feb 2007 16:53:07 PST Date: Mon, 5 Feb 2007 16:53:07 -0800 (PST) From: Curt Covey Subject: IPCC and sea level rise, hi-res paleodata, etc. To: Christopher Monckton , Fred Singer Cc: Jim Hansen , mann@psu.edu, Clifford Lee In-Reply-To: <20061229145211.611FC1CE304@wsl-6.us4.outblaze.com> MIME-Version: 1.0 Content-Type: multipart/mixed; boundary="0-1893172854-1170723187-47787" Content-Transfer-Encoding: 8bit Message-ID: <805971.47787.qm@web60817.mail.yahoo.com> X-Virus-Scanned: amavisd-sophos X-PSU-Spam-Flag: NO X-PSU-Spam-Hits: 0 Christopher and Fred,

Now that the latest IPCC WG1 SPM is published, I can venture more opinions on the above-referenced subjects.

It is indeed striking that IPCC's estimate of maximum plausible 21st century sea-level rise has decreased over time. The latest estimate is 0.5 meters for the A2 emissions scenario (not much higher from the 0.4 meter estimate for the A1B emissions scenario, which the Wall Street Journal editorial page has made much of). On the other hand, the IPCC seems to have taken a pass on Hansen's argument. The IPCC says their estimates are "excluding future rapid dynamical changes in ice flow . . . because a basis in published literature is lacking."

In this one respect (sea level rise) I agree with today's Journal editorial that the science is not yet settled. Unfortunately, the editorial runs completely off the tracks thereafter by (1) comparing 2006 vs. 2001 surface temperatures, among all the 150 or so years on record, and (2) asserting a "significant cooling the oceans have undergone since 2003" based apparently on one published data-set that contradicts all the others. It is not appropriate to cherry-pick data points this way. It's like trying to figure out long-term trends in the stock market by comparing today's value of the Dow with last Tuesday's value.

Re high-resolution paleodata, I never liked it that the 2001 IPCC report pictured Mann's without showing alternates. Phil's Jones' data was also available at the time. Focusing so exclusively on Mann was unfair in particular to Mann himself, who thereby became the sole target of criticism in the Wall Street Journal etc.

It now seems clear from looking at all the different analyses (e.g. as summarized in last year's NRC review by North et al.) that Mann is an outlier though not egregiously so. Of course, like any good scientist Mann argues that his methods get you closer to the truth than anyone else. But the bottom line for me is simply that all the different studies find that the rate of warming over the last 50-100 years is unusually high compared with previous centuries.

Summarizing all this, the latest IPCC does back off a bit from the previous one. It says on Page 8, "Some recent studies indicate greater variability [than Mann] in [pre-industrial] Northern Hemisphere temperatures than suggested in the TAR . . ." The wording is perhaps insufficiently apologetic, but I find it hard to object strenuously to it in light of the main point noted in the last paragraph.

If you want to discuss any of this further, let me know. I attach my latest presentation -- and would appreciate seeing both Christopher's report mentioned in the Journal editorial and Fred's comment on Rahmstorf's article published in Science last week.

Best regards,
Curt

Christopher Monckton <monckton@mail.com> wrote:

Dear Mr. Covey - Many thanks for coming back to me so quickly. You mention Hansen's recent

papers. I have recently been looking at an (attached) earlier projection of his - the projection of temperature increase which he made to the US Congress in 1988, effectively starting the "global-warming" scare. Updating his graph shows that annual global mean land and sea surface air temperature is not rising anything like as fast as his attention-grabbing but now manifestly-misconceived Scenario A suggested. Indeed, it is beginning to look as though temperature is beginning to fall below his estimate based on CO2 having been stabilized in 1988. Morner, the world's leading authority on sea level, has been very clear in saying there is very little evidence to justify the IPCC's sea-level projections. The IPCC itself forecast up to 0.94m sea level rise in a century in its 1996 report; up to 0.88m in its 2001 report; and now 0.43m in its 2007 report. If one loosely defines whatever the IPCC says as the "consensus", then not only does the "consensus" not agree with itself: it is galloping in the direction of the formerly-derided sceptics.

As to future world population, I did some research on this several years ago, because the UN was making alarmist noises and this alerted me to the likelihood that we were being fed political propaganda masquerading as science. I learned that the prime determinant of dP in any population is the general level of prosperity in that population. As prosperity increases, dP tends to zero. The prosperity factor is many times more potent as an influence on dP than even enforced, artificial contraception or child-killing. Since I expect world prosperity to increase in the coming century, I regard it as near-certain that dP will tend to zero in the next half-century. The reason for the plummet thereafter is the widespread availability and use of artificial methods of birth-control. The combined effects of rising general prosperity and the general availability of artificial birth-control on depressing indigenous population are already discernible in all those Western European populations not having to cope with mass immigration from poorer countries. In Russia, the indigenous population is falling so fast that Muslims will soon form more than half the population.

As to the "hockey-stick" problem, the NAS report does state very clearly that, though the conclusion of Mann et al. is "plausible", evidence going back more than 400 years before the present is increasingly unreliable, and that very few reliable conclusions can be drawn if one goes back more than 900 years. This illustrates one of the problems bedevilling the climate-change question: too much of the data and processes on the basis of which we are trying to draw conclusions are unreliable, incomplete or very poorly understood. This should not deter scientists from trying to make increasingly intelligent guesses: but anyone with diplomatic knowledge of the fast-emerging, fast-growing fast-polluters such as China, India, Indonesia and Brazil will tell you that the ruling regimes in these countries will not try to prevent their people from enjoying the fossil-fuelled economic growth we have already enjoyed unless and until the science is honest, the uncertainties are admitted and the case is strengthened by the accumulation of measurements and the improvement of analytical techniques in the coming years.

Finally, you are right to take me to task for using words such as "rubbish" and "useless". I apologize. That said, a validation skill not significantly different from zero indicates that no valid scientific conclusion may be drawn from the "hockey-stick" graph.

----- Original Message -----

From: "Curt Covey"

To: "Christopher Monckton"

Subject: Sea level rise, hi-res paleodata, etc.

Date: Wed, 27 Dec 2006 15:05:51 -0800 (PST)

Dear Dr. Monckton,

Thanks for copying me on your correspondence with Fred and prompting me to look again at IPCC sea level rise estimates for 2100. I agree you are comparing like-for-like. The 2001 report has an upper limit of 0.7 meters for the A1B scenario. If the 2007 report lowers this to 0.43 meters (or if the number gets raised again before the report is made final) it will certainly be appropriate to ask why. After reading Hansen's recent papers, I don't see how to justify such small upper limits.

It also seems obvious to me (and apparently to you but not to Fred) that the A2 scenario would entail more sea level rise than A1B. Regarding the relative likelihoods of scenarios, I don't agree with you that it's "almost certain" that world population will "plummet" in the second half of this century.

Regarding the issue of recent vs. earlier global warming, when I look at the totality of data compiled by North et al. this year for their NAS / NRC report (see attached graphic), it seems clear that most of the warming since about 1850 (or 1900) occurred in recent decades. Going farther back in time, the data are of course more uncertain and estimates vary, but it appears that the warming rate for the 20th century was unusually high compared with the past 2000 years. This conclusion follows whether or not one includes Mike Mann's data.

For the record, I must add that I do not share your characterization of Mann's work as "rubbish" or "useless." Nor do I see a situation of "flagrant dishonesty in which the UN and the scientific journals persist long after the falsity of their absurd and extreme claims has been properly demonstrated."

Sincerely,

Curt Covey

Christopher Monckton <monckton@mail.com> wrote:

Dear Fred, - Many thanks for sending me this exchange. Some comments:

Temperature: This question, like so many others to do with supposed "climate change", is bedevilled by the recency of reliable, instrument-based observations. Nevertheless, some conclusions can be attempted. The Dalton Minimum is generally considered to have come to an end in 1910. The five-year mean global land and sea surface air temperature anomaly for 1908-1912, calculated from NCDC annual figures, was -0.3579K. By 1940 there had been a rapid increase of 0.4700K to +1121K. By 2004 (again taking the five-year average, including

2006) there had been a further increase of +0.4413K to +0.5534. The mean annual increase in the 30 years 1010-1940 was thus 0.0157K more than two and a quarter times greater than the 0.0069K mean annual increase in the 64 years to 2004. Mean global temperature has hardly risen at all in the five years since the IPCC's last report. And the fact of the 20th-century temperature increase tells us nothing of the cause. It is interesting, for instance, that the polar icecaps on Mars are receding, inferentially in response to increased solar activity. At any rate, it is certain that anthropogenic planetary warming is not responsible. It is possible, therefore, that most of the warming both before and after 1940 was heliogenic.

Sea level: Your correspondent does not disagree with my statement that the IPCC has revised its upper-bound estimate of sea level rise to 17 inches (0.43m). He says, however, that this upper bound is based on the A1 scenario, by which world population will peak in mid-century at ~9bn and fall thereafter. So was the 2001 report's upper bound of 0.88m. I was correctly comparing like for like. The Sunday Telegraph, which reported these figures, has been told that the revisions arise from "better data" now available to the IPCC, supporting skeptics' conclusions that the IPCC's figures are little better than exaggerated guesses. Morner (2004) concludes firmly that there is little evidence for sea level rising any faster now than it has in geologically-recent times. Your correspondent says that the A2 scenario is "business-as-usual": in fact, it is an extreme scenario regarded by very nearly all serious demographers as absurdly unrealistic, in that it posits an increase in world population to 15bn by 2100, when it is now almost certain that rising prosperity and the consequent decrease in birth rates will cause population to peak somewhere between 9bn and 10bn in mid-century, and plummet thereafter.

Reliability of the IPCC's reports: I understand that the IPCC's 2007 draft does not contain an apology for the defective "hockey-stick" graph, which the US National Academy of Sciences has described as having "a validation skill not significantly different from zero". In plain English, this means the graph was rubbish. It is difficult to have confidence in a body which, after its principal conclusion is demonstrated in the peer-reviewed, scientific literature and in numerous independent reports as having been useless, fails to make the appropriate withdrawal and apology. Worse, the UN continues to use the defective graph. This failure of basic academic honesty on the IPCC's part was the main reason why I began my investigation of the supposed climate-change "consensus".

The supposed scientific "consensus": Your correspondent seems unaware of the letter written by 61 Canadian and other scientists in climate and related fields to the Canadian Prime Minister. At the end of the attached commentary on Al Gore's recent attempt to rebut my articles on climate change in the Sunday Telegraph, beneath the references, I have appended the full text of the letter and the names, qualifications and then-current affiliations of all 61 scientists. Al gore and others tend to lean rather more heavily than is wise upon a single, rather bad one-page essay in Science for their contention that there is a scientific consensus to the effect that most of the warming in the past half-century was anthropogenic. The essay was by Oreskes (2004), who said that she had analyzed 928 abstracts mentioning "climate change" published in peer-reviewed journals on the Thomson ISI database between 1993 and 2003, and that none of the 928 had expressed dissent from the "consensus". Dr. Benny Peiser of Liverpool John Moores University subsequently made a more careful enquiry. Science had been compelled to publish an erratum to the effect that the search term used by Oreskes had not been the neutral "climate change" - which returned some 12,000 articles, but the more loaded "global climate change", which returned 1,117 articles. Of these, Dr. Peiser found that only 1% had explicitly endorsed the "consensus" as defined by Oreskes"; that almost three times as many had explicitly expressed doubt or outright disagreement; and that less than one-third had expressed explicit or implicit agreement with the "consensus". He wrote a paper for Science pointing out these serious defects, which pointed to a conclusion diametrically opposite to that of Oreskes. Science at first asked him to shorten his paper, and then said that, because conclusions like his had been widely reported on the internet, his paper would not be published. As far as I can discover, Science has not published any corrigendum to this day, providing further confirmation of what I have long suspected: that the leading peer-reviewed journals, having unwisely taken strongly-political editorial positions on the question of climate change, are no longer objective.

The need for honest science: It was only after years of increasingly-public pressure that Nature was induced to oblige Mann et al., the authors of the useless "hockey-stick" graph that starred in the IPCC's 2001 report, to publish a mealy-mouthed, partial and unsatisfactory corrigendum. In such an environment of flagrant dishonesty in which the UN and the scientific journals persist long after the falsity of their absurd and extreme claims has been properly demonstrated, it is in my view unreasonable to expect China, India, Indonesia, Brazil and other fast-polluting countries to deny to themselves the fossil-fuelled economic growth which we in the West have been fortunate enough to enjoy. Until there is honest science, no one will believe either the UN or the journals to the extent of adopting the expensive and (on my calculations) probably futile remedial measures which they and their supporters so stridently advocate. - Christopher

----- Original Message -----

From: "S. Fred Singer"

To: "Curt Covey"

Subject: Re: Belated response to "Say You're Sorry"

Date: Tue, 26 Dec 2006 08:37:25 -0500

At 07:15 PM 12/18/2006, Curt Covey wrote:

Received your 5 May 2006 e-mail via Andy Revkin last week. Regarding the Wall Street Journal and "other forums that substitute quips, showmanship, hyperbole, and conjecture for substantial discussion," the following recent quips from their Letters to the Editor may interest you:

Fred Singer's claim (13 December) that "more than 70% of the warming observed since the end of the Little Ice Age in 1850 occurred before 1940, and thus before much human-emitted CO₂." Fred has been saying this for a long time. I think it was true 20 years ago. Up-to-date records (e.g. this year's NAS report from North et al.) show that much more than half the warming since c.1850 has occurred after 1940.

Dear Curt, I am sure you are aware of the fact that such ratios depend entirely on the choice of time intervals. I don't want to quibble but surely the relevant fact is that most agree (incl IPCC -- but not Tom Wigley) that the pre-1940 warming was mostly due to natural causes.

Lord Monckton's claim (13 December) that "The U.N. [presumably IPCC] is about to cut its high-end estimate of sea-level rise in 2100 from three feet to just 17 inches." We are not supposed to discuss IPCC reports before they become final, but the last draft I saw does indeed project 17 inches (0.43 meters) of sea-level rise as the high-end climate model estimate from Emissions Scenario A1B. The scenario itself, however, is one in which (to quote IPCC) "global population peaks in mid-century and declines thereafter, and the rapid introduction of new and more efficient technologies" has atmospheric CO₂ leveling off by the end of the century. A business-as-usual scenario (like A2) would give much higher sea-level rise by 2100.

I don't think so. But you will have to read my forthcoming response to Rahmstorf (in ScienceExpress). Meanwhile, peruse the attached.

Senator Inhofe's comment today (18 December) that "60 scientists" together with "Claude Allegre, a leading French scientist who is a member of both the U.S. and French National Academies of Sciences" have concluded that agreements like Kyoto are "unnecessary" because "the cause of global warming is 'unknown.'" Presumably true, but so what? Allegre is an award-winning geochemist; the other 60 scientists are unidentified. There are tens of thousands of members of the American Geophysical Union alone (many of whom are petroleum geologists). I'm sure you can find a few hundred to support any claim you want to make about global warming.

I am one of the 60 -- and I am sure you know most of the other 59.
Best for 2007! Fred

S. Fred Singer, President
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<singer@SEPP.org>

Read about what is really causing warming
Unstoppable Global Warming : Every 1500 Years
(Natural climate cycles as seen in the geological record)
by S. Fred Singer and Dennis T. Avery
Rowman & Littlefield (2007) 260 pp. \$25.00 plus \$5 S&H
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--

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--

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"c:\eudora\attach\covey_glwarm_Feb07.pdf"

References

Visible links

1. <http://www.sepp.org/>
2. <http://www.sepp.org/>
3. <http://us.rd.yahoo.com/evt=43909/#http://mobile.yahoo.com/services?promote=mail>

Hidden links:

4. http://a8-asy.a8ww.net/a8-ads/adftrclick?redirectid=en-mail_a_01
5. http://a8-asy.a8ww.net/a8-ads/adftrclick?redirectid=en-mail_a_01

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: Re: EJ on hockey stick
Date: Thu Feb 15 09:37:48 2007

Thanks Eystein
the sceptic troupe are fading away
At 07:58 15/02/2007, you wrote:

Hi Keith,
I was asked about AR4 and the Hockey stick by a journalist. This was picked up by McIntyre's blog.
You can see the issue here: [1]<http://www.climateaudit.org/?p=1131>
The last comment gives an Ok translation from Norwegian of what i said.
Eystein

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--
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[4]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.climateaudit.org/?p=1131>
2. <mailto:eystein.jansen@geo.uib.no>
3. <http://www.bjerknes.uib.no/>
4. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: "thomas.c.peterson" <Thomas.C.Peterson@noaa.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: [Fwd: Marooned?]
Date: Mon, 19 Feb 2007 11:10:02 -0500

Hi, Phil,

I thought you might enjoy the forwarded picture and related commentary below.

I read some of the USHCN/GISS/CRU brouhaha on web site you sent us. It is both interesting and sad. It reminds me of a talk that Fred Singer gave in which he impugned the climate record by saying he didn't know how different parts were put together. During the question part, Bob Livzey said, if you don't know how it is done you should read the papers that describe it in detail. So many of the comments on that web page could be completely addressed by pointing people to different papers. Ah well, you can lead a horse to water but you can't make it think.

Warm regards,
Tom

<http://www.nature.com/nature/journal/v445/n7128/full/445567a.html>

Nature 445, 567 (8 February 2007) | doi:10.1038/445567a

Editorial

"The IPCC report has served a useful purpose in removing the last ground from under the sceptics' feet, leaving them looking marooned and ridiculous."

--
Thomas C. Peterson, Ph.D.
NOAA's National Climatic Data Center
151 Patton Avenue
Asheville, NC 28801
Voice: +1-828-271-4287
Fax: +1-828-271-4328

Attachment Converted: "c:\eudora\attach\marooned.jpg"

From: Kevin Trenberth <trenbert@ucar.edu>
To: Melinda Marquis <marquis@ucar.edu>, Kristen Averyt <averyt@ucar.edu>
Subject: Re: Copy-edited Ch. 3 files
Date: Wed, 21 Feb 2007 08:18:03 -0700
Cc: Phil Jones <p.jones@uea.ac.uk>, Martin Manning <mmanning@al.noaa.gov>, Susan Solomon <ssolomon@al.noaa.gov>

Hi all
I have ftp'd the updated cleaned up files from chapter 3 back onto your ftp site. The notes accompanying these are attached and are unchanged from yesterday. There are two references that may not be quite final. These are from Global and Planetary Change and we have doi's for them as they are published online, but no page numbers as they do not seem to have appeared yet in print. By the way, there was one notable error in the copy editing which was confusion over significance and confidence levels. I removed all the references to confidence levels when it was about significance (of trends etc). I suspect this could affect other chapters though, so you may want to check that carefully. The main concerns we have are with the figures, please see the comments on the figure files and the brief comments in the attached. If you would like me to make any of these changes (Kristen) or assemble the panels, please let me know.

Regards

Kevin

Phil Jones wrote:

Melinda et al,

I'm happy with the chapter once all the mods - mainly to the figures - are undertaken. I won't get a chance this weekend, nor the next two days as I'm away. I might have some more time next week, but I too have spent about 6 hours on Sunday and another 2-3 hours on Monday. So Kevin can send back the accepted/tracked version of the chapter, the captions and Appendix 3.B.

On the figures, will we get a chance to see the Chapter mocked-up with figures in their final positions and sizes - as we would do with journal papers? There are a number, which we'd like to check to make sure the colours are OK.

I think by the way that you have caught all the spellings correctly. I noted 'fall' changing to 'autumn' and the doubling up of letters in words like 'modelling'. I hear also from Keith Briffa that Ch 6 now spells the word palaeoclimatic, although we normally drop the extra 'a' even in English journals.

Cheers

Phil

At 23:14 20/02/2007, Kevin Trenberth wrote:

Melinda

Thanks

Phil and I have made a preliminary pass through the material. As Kristen is now considering the figures, I have attached a preliminary list of the problems. This also includes some material for you: acronyms. More detail is given on the full figure file. We have left USA as is in the main text, but I note that the Appendix B was not copy edited and we have left "United States" there. We have accepted most other changes even though I would not do them this way! We can send the material back now but I will

wait for a last check by Phil. (I spent over 12 hours on this over the weekend).

Kevin

Melinda Marquis wrote:

Hi, Kevin,

Thank you for reviewing your copy-edited chapter files -- thoroughly and promptly. I'll try to answer each of your questions.

About the convention for referring to the United States: As this document is published under the auspices of the United Nations, we are required to use official country names; the United States of America is to be abbreviated as "USA" for such publications.

Regarding the lower case "antarctica": We have capitalized "Arctic" and "Antarctic" when they are nouns, and have used lower case "arctic" and "antarctic" when they are adjectives. We used the AMS Word List

(http://www.ametsoc.org/PUBS/Authorsguide/pdf_vs/authguide.pdf) to supplement our style guide. The AMS list cites "arctic flow" (adj.) and "Arctic Circle" (noun). We thought it appropriate to treat "antarctic" analogously to "arctic" (the adjectival form).

About suggested revisions that seem pedantic: If you feel that inserting "the period" before things like 1961 to 1990 would decrease clarity or change the meaning from what is intended, then you may of course reject such changes.

Thank you for your careful review. Kristen will be replying to you about the figures.

Please let us know if you have further concerns. We want everything to be correct.

Cheers,

Melinda

Kevin Trenberth wrote:

Melinda

There appear to be changes that I do not agree with. For instance, everywhere we had "United States" it has been changed to USA. That is not the practice in AMS or AGU journals. I have also found several instances of Antarctic changed to lower case which is surely not right!!!! Some changes are very pedantic: inserting "the period" before things like 1961 to 1990.

Kevin

Melinda Marquis wrote:

Dear CLAs,

Thank you very much for your invaluable assistance during the recent SPM plenary meeting. As you will realise there are a few remaining steps that need to be completed before final completion of the WG1-AR4 but these should now be straightforward. This is to ask for your help in the next of these steps which is to check the copy-edited version of your chapter.

A professional copy-editor has reviewed all chapters of the AR4 and made some revisions. In most cases, her suggestions implement our style guide (see attached) for consistency in punctuation, spelling, grammar and language style across all chapters, points at which acronyms are spelled out, etc, etc. In a few cases, she has suggested revised wording for the sake of clarity, improved grammar or such. All these changes that might have some effect on the meaning of a sentence are shown in track-changes mode.

We would be grateful if you would now go through these edited chapter files and either accept, reject, or modify the copy-editor's tracked revisions and return "cleaned up" files to the TSU. During this step you should also:

- * make any remaining necessary and minor corrections to text or tables;
- * ensure that any corrections or updates provided to the TSU since the distribution of the final draft in October 2006, have been included;
- * update references that have been published recently by inserting volume and page numbers, etc;
- * add any adjustments to your chapter that arose from the SPM approval process in Paris.

Please return a checked file to us with all tracked changes removed.

Please also remember to check your figures and figure captions carefully including the axis labels, units used, etc. Annotated text should already have been edited to follow the styles used in the text where appropriate. In some cases we will be doing further improvements to the text fonts used in figures but this is your last chance to ensure that the wording is correct in all places. If you wish to make any small revisions to figures, please contact Kristen Averyt ([2]averyt@ucar.edu) as soon as possible.

Please remember that no substantive changes, or new references, can be made to your chapter at this stage.

The time line for delivering the camera-ready copy to the publisher is quite tight. We ask that you please return your final text and figures files to the TSU by Friday, March 9.

You may access your chapter files at the following ftp site.

server: [3]ftp.joss.ucar.edu

account: wgl_gnrl

password: EQ0KW0WG (Please note that these are zeros - not letters.)

directory: pub/AR4_CopyEditFinal/ChXX

The file names currently contain "_TSU." We ask that you change these characters to "_CLA" in the files you return to us. Finally please notify us at

[4]ipcc-wgl@al.noaa.gov when you have uploaded the checked files.

Best regards,

Melinda Marquis

--

Dr Melinda Marquis, Deputy Director, IPCC WG I Support Unit

NOAA/ESRL Phone:

+1 303 497 4487

325 Broadway, DSRC

R/CSD08 Fax:

+1 303 497 5628

Boulder, CO 80305, USA

--

Kevin E.

Trenberth

e-mail: [5]trenbert@ucar.edu

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Street address: 1850 Table Mesa Drive, Boulder, CO 80305

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References

1. http://www.ametsoc.org/PUBS/Authorsguide/pdf_vs/authguide.pdf
2. <mailto:averyt@ucar.edu>
3. <ftp://ftp.joss.ucar.edu/>
4. <mailto:ipcc-wg1@al.noaa.gov>
5. <mailto:trenbert@ucar.edu>
6. <http://www.cgd.ucar.edu/cas/trenbert.html>
7. <mailto:trenbert@ucar.edu>
8. <http://www.cgd.ucar.edu/cas/trenbert.html>
9. <mailto:p.jones@uea.ac.uk>
10. <mailto:trenbert@ucar.edu>
11. <http://www.cgd.ucar.edu/cas/trenbert.html>

From: "Tim Osborn" <t.osborn@uea.ac.uk>
To: "Keith Briffa" <k.briffa@uea.ac.uk>
Subject: Re: ppt
Date: Thu, 1 Mar 2007 14:14:23 -0000 (GMT)
Reply-to: t.osborn@uea.ac.uk
Cc: t.osborn@uea.ac.uk

Here is the old version for you to compare with... the only noticeable difference is for the URALS/YAMAL region, which previously had a higher peak near 1000 AD. Although that was quite a big change, once you average it with the other two series, the overall mean series shows very little difference.

Cheers

Tim

On Thu, March 1, 2007 1:57 pm, Keith Briffa wrote:

> Tim
> am back and looking at this now
> thanks
> Keith
> At 12:23 01/03/2007, you wrote:
>>Hi again,
>>
>>please see the attached PDF file. I've not yet put it into powerpoint,
>>because I wanted to check whether it matches what you want, or if you
>> want
>>fewer lines on it etc.
>>
>>Each page is identical layout, for the 3 regions and then the 4th page is
>>for the average across all the data.
>>
>>On each page you have the scatter graphs (and correlation) between the
>>unfiltered and the 10-year smoothed TRW and summer temperature. Plus the
>>3 calibration lines (our normal regression in black, variance matching in
>>orange, and inverting the regression of TRW onto temperature in brown),
>>thin lines between unfiltered data and thick lines between 10-year
>>smoothed data. The solid blue scatter plot points are those used in the
>>1900-1990 calibration period, the blue circles with a cross in are from
>>outside the calibration period.
>>

>>The top panels show the full 2000-yr reconstructions, with the line
>> colour
>>and thickness coordinated to match the calibration lines in the bottom
>>panels. The only exception is that I have omitted the inverse regression
>>between unfiltered data (the line is shown dotted on the bottom left
>>panels), because this resulted in such huge variance that the curves went
>>way off the vertical scale!

>>
>>In this top panel, all series, including the instrumental (blue), are
>>50-year smoothed. In the Scandinavian panel, there's also the longer
>>Tornedalen summer temperatures overlaid in green.

>>
>>So... I can put each of these into a powerpoint slide.

>>
>>Easily, I could also repeat them for a shorter period and less smoothing
>>(e.g. 1500-present with decadal smoothing, or 1800-present with no
>>smoothing).

>>
>>I could also omit some of the curves if you think 5 reconstruction
>>alternatives per panel is too many.

>>
>>With slightly more time, I could make it so that the powerpoint built up
>>with 1 alternative reconstruction at a time, until all 5 were there.

>>
>>I'll call you soon and we can talk about it.

>>
>>Cheers

>>
>>Tim

>>
>>On Thu, March 1, 2007 10:17 am, Keith Briffa wrote:

>> > Hi Tim

>> > thanks

>> > I would be happy with only the usual regression but the plots with
>> > different timescales shown - for each and the average series would be

>> > great

>> > cheers

>> > Keith

>> >

>> >

>> > At 09:51 01/03/2007, you wrote:

>> >>Hi Keith -- I forgot to describe the contents of the PPT file I sent

>> >>yesterday. Basically it starts with a few comparisons of the modern

>> >>period between the MXD-based recons and the instrumental data.
>> >>
>> >>First 3 show data only up to 1960.
>> >>(1) Full MXD reconstruction
>> >>(2) Masked MXD reconstruction (masked by availability of instrumental
>> >> temps)
>> >>(3) Masked temperatures (masked by availability of MXD)
>> >>All with 5-year filter
>> >>
>> >>Then the same as above, except the next 3 show data up to 1995 to
>> >>illustrate the decline.
>> >>
>> >>Then a couple more repeating the above, masked MXD then masked
>> >>temperature, but this time without any time-filtering, so you can see
>> >>individual warm and cold years.
>> >>
>> >>Then finally the full MXD reconstruction back to 1400, but only up to
>> >> 1960.
>> >>
>> >>I'm working from home today. I'll redo the calibrated northern
>> Eurasian
>> >>stuff -- do you want all the options again (i.e. forward and inverse
>> >>regression, variance matching, pre-/post-calibration averaging of the
>> >>regions, low and high pass filtering?).
>> >>
>> >>Then we can make any final slides Friday morning if that's ok with
>> you!
>> >>
>> >>Cheers
>> >>
>> >>Tim
>> >
>> > --
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>> > <http://www.cru.uea.ac.uk/cru/people/briffa/>
>> >

>> >

>>

>>

>>

>

> --

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> University of East Anglia

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>

> <http://www.cru.uea.ac.uk/cru/people/briffa/>

>

>

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From: Valérie Masson-Delmotte <Valerie.Masson@cea.fr>

To: masson@dsm-mail.saclay.cea.fr

Subject: Re: IPCC final text

Date: Fri, 02 Mar 2007 16:46:12 +0100

Reply-to: Valerie.Masson@cea.fr

Cc: tordis.leroen@bjerknes.uib.no, Eystein Jansen <eystein.jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>, David Rind <drind@giss.nasa.gov>, Bette Otto-Bliesner <ottobli@ucar.edu>, joos <joos@climate.unibe.ch>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

now for the Figures (this file is crashing my Word software systematically!)

Figure 6.3

OK with suggestion

Someone has to check the many comments on Figure 6.7 and 6.15 (Fortunat?)

Figure 6.9 : I cannot generate S and N latitudes, can someone of you edit the figure to generate positive latitudes?

Same for Figure Box 6.1, Figure 1 : they suggest to label the RH vertical axes but they have the same unit as the LH vertical axis. How should I proceed?

For Figure Box 6.3, 1 : should Olga reprocess it? (they ask for a change in caption)

FAQ should refer to Figure 1, Faq 6.1

APPENDIX

The definition of O-isotopes is partly false.

The isotopic composition of ice depends on temperature not because the fractionation coefficients depend on temperature but due to the progressive distillation of water masses en route for the poles. Even if fractionation coefficients were to be independent of temperature would one see a temperature / isotopic composition relationship.

All the best,

Valérie.

</x-flowed>

From: ottobli@cgd.ucar.edu
To: "Eystein Jansen" <eystein.jansen@geo.uib.no>
Subject: Re: AR4 Final Input Please check this mail
Date: Sat, 3 Mar 2007 18:14:19 -0700 (MST)
Cc: drind@giss.nasa.gov, "Bette Otto-Bliesner" <ottobli@ucar.edu>, "Fortunat Joos" <joos@climate.unibe.ch>, Valérie Masson-Delmotte <valerie.masson@cea.fr>, "Keith Briffa" <k.briffa@uea.ac.uk>, "Tim Osborn" <t.osborn@uea.ac.uk>, "Jonathan Overpeck" <jto@u.arizona.edu>, Øyvind Paasche <oyvind.paasche@bjerkes.uib.no>

Dear all,

Below are my comments addressing issues to Section 6.4 and associated figures. It would be good if Fortunat can also check especially Box 6.2, 6.4 intro, 6.4.1.1, and 6.4.1.5 written by Dominique and Fortunat.

Bette

Figures:

- * Figure 6.3, Valerie has checked comments.
- * Figure 6.4 and 6.7, Fortunat(?) should check the figures, legends, and comments.
- * Figure 6.5, line 8: generally feedbacks in glacial-interglacial ...
line 19: Simon Laplace Climate System Model (IPSL-CM) ...
line 21: ECBilt-CLIO is not an acronym as far as I can tell.
- * Figure 6.6, line 5: minimum ice thickness and extent ...
line 6: Delete "at approximately 130 to 125 ka".
line 9: ... and the ECHAM4 HOPE-G (ECHO-G) model ...
- * Figure 6.8, Dick should check that the legend is revised correctly and that color code in this figure is consistent with text.

Text 6.4:

- * Page 6-11, line 9: corresponding to other orbital periods ...
- * Page 6-11, line 13: adopt Valerie's wording of last sentence.
- * Page 6-11, line 37: the SOD has ~180 ppm and ~265 ppm. Is the change to < intentional in response to a review comment?
- * Page 6-11, line 38: adopt Valerie's wording of sentence.
- * Page 6-11, line 49: OK to delete redundant sentence.
- * Box 6.1: See Valerie comments.
- * Box 6.2: Changes look OK. Fortunat should check.
- * Page 6-15, lines 49-50: ... to the very different conditions at the LGM.
- * Page 6-16, line 2: PMIP-2 simulations ...
- * Page 6-16, line 31: Change does not make sense. The PMIP2 models do not simulate changes of greenhouse gases or ice sheets. These are prescribed. This sentence could be revised to read: The PMIP-2 AOGCM simulations using glacial-interglacial ...
- * Page 6-17, line 23: I am fine with Last Interglacial. Peck may also want to comment. The SPM uses the last interglacial period.
- * Page 6-17, line 41: ... warming over Eurasia and in the Baffin Island/northern Greenland region ... I am OK with taking out "with sea ice retreat" at the end of the sentence if that is awkward.
- * Page 6-17, line 43: Kaspar and Cubasch, 2006.
- * Page 6-18, line 1: Models and data now show ...
- * Page 6-18, line 6: adopt Valerie's wording of sentence.
- * Page 6-18, line 38: D-O is one of the abbreviations in the literature so I am fine with this change. Need to be consistent and change Page 6-19, line 49 to D-O.
- * Page 6-18, line 43: adopt Valerie's wording of sentence.
- * Page 6-21, line 12: Dick can advise if ICE-4G, ICE-5G, and VM2 are acronyms.
- * Page 6-21, line 20: Dick should be consulted to make sure sentence

meaning is OK with changes.

* Page 6-21, line 43: Dick can advise if J stands for Joseph.

* Page 6-21, line 51: Dick should be consulted on color code in Figure 6.8 and consistency with text.

* Page 6-22, line 2: Replace "longer" with "older". Valerie please comment if this is a more correct wording. Note that neither Landais et al., 2003 or Suwa et al., 2006 are currently in the reference list. Eystein/Peck, please advise if it is possible to add new references. Valerie, could we use Landais et al, 2006, which is already in the reference list, here?

* Page 6-22, line 10: OK to spell out GIS

* Page 6-22, line 11: the growth ...

* Page 6-22, line 16: Peck, can you comment "if sea level rise during the LIG" is an OK edit.

* Page 6-22, lines 25-29: Possible combination of last two sentences. Peck, please check that this conveys your original meaning: Overpeck et al. (2006) argued ... significant retreat of the Greenland Ice Sheet (and perhaps also parts of the Antarctic Ice Sheet) can be expected to occur under this future condition (see also Scherer et al. ...).

References:

Kaspar and Cubasch: published in 2006. editor order should be Sirocko, Claussen, Litt, and Sanchez-Goni. I couldn't find the location or page numbers for this publication.

NRC: OK

Otto-Bliesner: reversal of a and b OK

Peltier and Fairbanks, 2006: 25(23-24), 3322-3337.

Sarnthein: reversal of a and b OK

Taylor: OK

FAQ 6.1 and 6.2 need to be revised to refer to the correct sections of Chapter 6. Most of the references are incorrect and look to be based on an old outline of the chapter.

Appendix:

Need consistency with changes in chapter: Palaeocene, palaeosols(?)

--
Bette L. Otto-Bliesner
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Fax: 303-497-1348
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From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Hugues Goosse <hgs@astr.ucl.ac.be>
Subject: Re: 7RP / Environment (incl. Climate Change)
Date: Wed, 7 Mar 2007 15:52:11 +0100
Cc: k.briffa@uea.ac.uk

Dear Hugues,

I agree and what Damien said echoes what Keith is concerned about. We need to expand the timescale of Millennium AND focus much more on sensitivity and predictability.

best wishes

Eystein

Den 7. mar. 2007 kl. 11.22 skrev Hugues Goosse:

Hi Eystein,

Thanks a lot for the information. I agree with you that it is very important that the topic "Earth system dynamics: Palaeoenvironmental analysis" includes explicitly our area of interest. By the way, I have briefly discussed with Damien Cardinal after the meeting yesterday. He told me that the EU has already funded recently a very big project over the last Millennium, so they will be reluctant to make a new call covering this subject but we can certainly sell our science in something more general like 'natural variability and climate predictability'.

All the best

Hugues

Le 15:00 06/03/2007, vous avez écrit:

Hi Keith and Hugues,

Here are two documents re. our discussion of FP7 topics.

As you will see the plan is to have the following topic out in 2008 or later: Â·

Earth system dynamics: Palaeoenvironmental analysis

I think it will be important that the topic really comes in 2008 and that it includes the terms natural variability and climate predictability when it is described in the call. If possible our national program committee members should be contacted to propose this. As far as I know there will be a meeting later this spring to discuss the next calls.

Cheers

Eystein

i;¼i;¼

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[1]eystein.jansen@geo.uib.no
[2]www.bjerknes.uib.no

Hi Keith and Hugues,

Here are two documents re. our discussion of FP7 topics.

As you will see the plan is to have the following topic out in 2008 or later: ·

Earth system dynamics: Palaeoenvironmental analysis

I think it will be important that the topic really comes in 2008 and that it includes the terms natural variability and climate predictability when it is described in the call. If possible our national program committee members should be contacted to propose this. As far as I know there will be a meeting later this spring to discuss the next calls.

Cheers

Eystein

Content-Type: application/msword;
x-unix-mode=0644;
name=Articulating sub-activity 6 4 2.doc

Content-Disposition: attachment;
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x-unix-mode=0644;
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Content-Disposition: attachment;
filename="wp topics 2008.doc"

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[3]eystein.jansen@geo.uib.no
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References

1. <mailto:eystein.jansen@geo.uib.no>
2. <http://www.bjerknes.uib.no/>
3. <mailto:eystein.jansen@geo.uib.no>
4. <http://www.bjerknes.uib.no/>
5. <http://www.astr.ucl.ac.be/users/hgs/index.html>
6. <mailto:hgs@astr.ucl.ac.be>
7. <mailto:eystein.jansen@geo.uib.no>

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Richard Somerville <rsomerville@ucsd.edu>
Subject: Re: [Wg1-ar4-clas] Responding to an attack on IPCC and ourselves
Date: Thu, 8 Mar 2007 08:16:33 +0100
Cc: wg1-ar4-clas@joss.ucar.edu

Hi,

just a quick reply. I am in on this, and will respond to a draft letter, in the hope that you will make the first, Richard? I agree that it can be short. It is strange to see this, knowing that the delegations I spoke to in/after Paris clearly said that the CLAs got it their way, and that I believe this is the strong common perception we also had as CLAs about the outcome.

Best wishes,

Eystein

Den 8. mar. 2007 kl. 03.11 skrev Richard Somerville:

Dear Fellow CLAs,

The British magazine **New Scientist** is apparently about to publish several items critical of the IPCC AR4 WGI SPM and the process by which it was written. There is an editorial, a column by Pearce, and a longer piece by Wasdell which is on the internet and referenced by Pearce.

I think that this attack on us deserves a response from the CLAs. Our competence and integrity has been called into question. Susan Solomon is mentioned by name in unflattering terms. We ought not to get caught up in responding in detail to the many scientific errors in the Wasdell piece, in my opinion, but I would like to see us refute the main allegations against us and against the IPCC.

We need to make the case that this is shoddy and prejudiced journalism. Wasdell is not a climate scientist, was not involved in writing AR4, was not in Paris, and is grossly ignorant of both the science and the IPCC process. His account of what went on is factually incorrect in many important respects.

New Scientist inexplicably violates basic journalistic standards by publicizing and editorially agreeing with a vicious attack by an uncredentialed source without checking facts or hearing from the people attacked. The editorial and Pearce column, which I regard as packed with distortions and innuendo and error, are pasted below, and the Wasdell piece

is attached.

My suggestion is that a strongly worded letter to New Scientist, signed by as many CLAs as possible, would be an appropriate response. I think we ought to say that the science was absolutely not compromised or watered down by the review process or by political pressure of any kind or by the Paris plenary. I think it would be a mistake to attempt a detailed point-by-point discussion, which would provoke further criticism; that process would never converge.

Please send us all your opinions and suggestions for what we should do, using the email list [1]wg1-ar4-clas@joss.ucar.edu

I am traveling and checking email occasionally, so if enough of us agree that we should respond, I hope one or more of you (not me) will volunteer to coordinate the effort and submit the result to New Scientist.

Best regards to all,

Richard

Richard C. J. Somerville

Distinguished Professor
Scripps Institution of Oceanography
University of California, San Diego
9500 Gilman Drive, Dept. 0224

La Jolla, CA 92093-0224, USA

--

Here's the editorial that will appear in New Scientist on March 10.

Editorial: Carbon omissions

IT IS a case of the dog that didn't bark. The dog in this instance was the Intergovernmental Panel on Climate Change.

For several years, climate scientists have grown increasingly anxious about "positive feedbacks" that could accelerate climate change, such as methane bubbling up as permafrost melts. That concern found focus at an international conference organised by the British government two years ago, and many people expected it to emerge strongly in the latest IPCC report, whose summary for policy-makers was published in Paris last

month.

It didn't happen. The IPCC summary was notably guarded. We put that down to scientific caution and the desire to convey as much certainty as possible (New Scientist, 9 February, p 3), but this week we hear that an earlier version of the summary contained a number of explicit references to positive feedbacks and the dangers of accelerating climate change. A critique of the report now argues that the references were removed in a systematic fashion (see "Climate report 'was watered down'").

This is worrying. The version containing the warnings was the last for which scientists alone were responsible. After that it went out to review by governments. The IPCC is a governmental body as well as a scientific one. Both sides have to sign off on the report.

The scientists involved adamantly deny that there was undue pressure, or that the scientific integrity of their report was compromised. We do know there were political agendas, and that the scientists had to fight them. As one of the report's 33 authors put it: "A lot of us devoted a lot of time to ensuring that the changes requested by national delegates did not affect the scientific content." Yet small changes in language which individually may not amount to much can, cumulatively, change the tone and message of a report. Deliberately or not, this is what seems to have happened.

Senior IPCC scientists are not willing to discuss the changes, beyond denying that there was political interference. They regard the drafting process as private. This is an understandable reservation, but the case raises serious doubts about the IPCC process. A little more transparency would go a long way to removing those qualms.

--

Here's the Pearce column:

Climate report 'was watered down'

* 10 March 2007

* From New Scientist Print Edition. [2]Subscribe and get 4 free issues.

* Fred Pearce

BRITISH researchers who have seen drafts of last month's report by the Intergovernmental Panel on Climate Change claim it was significantly watered down when governments became involved in writing it.

David Wasdell, an independent analyst of climate change who acted as an accredited reviewer of the report, says the preliminary version produced by scientists in April

2006 contained many references to the potential for climate to change faster than expected because of "positive feedbacks" in the climate system. Most of these references were absent from the final version.

His assertion is based on a line-by-line analysis of the scientists' report and the final version, which was agreed last month at a week-long meeting of representatives of more than 100 governments. Wasdell told New Scientist: "I was astounded at the alterations that were imposed by government agents during the final stage of review. The evidence of collusional suppression of well-established and world-leading scientific material is overwhelming."

He has prepared a critique, "Political Corruption of the IPCC Report?", which claims: "Political and economic interests have influenced the presented scientific material." He plans to publish the document online this week at [3]www.meridian.org.uk/whats.htm.

Wasdell is not a climatologist, but his analysis was supported this week by two leading UK climate scientists and policy analysts. Ocean physicist Peter Wadhams of the University of Cambridge, who made the discovery that Arctic ice has thinned by 40 per cent over the past 25 years and also acted as a referee on the IPCC report, told New Scientist: "The public needs to know that the policy-makers' summary, presented as the united words of the IPCC, has actually been watered down in subtle but vital ways by governmental agents before the public was allowed to see it."

"The public needs to know that the summary has been watered down in subtle but vital ways by governmental agents"

Crispin Tickell, a long-standing UK government adviser on climate and a former ambassador to the UN, says: "I think David Wasdell's analysis is very useful, and unique of its kind. Others have made comparable points but not in such analytic detail."

Wasdell's central charge is that "reference to possible acceleration of climate change [was] consistently removed" from the final report. This happened both in its treatment of potential positive feedbacks from global warming in the future and in its discussion of recent observations of collapsing ice sheets and an accelerating rise in sea levels.

For instance, the scientists' draft report warned that natural systems such as rainforests, soils and the oceans would in future be less able to absorb greenhouse gas emissions. It said: "This positive feedback could lead to as much as 1.2 °C of added warming by 2100." The final version does not include this figure. It acknowledges that the feedback could exist but says: "The magnitude of this feedback is uncertain."

Similarly, the draft warned that warming will increase atmospheric levels of water vapour, which acts as a greenhouse gas. "Water vapour increases lead to a strong

positive feedback," it said. "New evidence estimates a 40 to 50 per cent amplification of global mean warming." This was absent from the published version, replaced elsewhere with the much milder observation "Water vapour changes represent the largest feedback."

The final edit also removed references to growing fears that global warming is accelerating the discharge of ice from major ice sheets such as the Greenland sheet. This would dramatically speed up rises in sea levels and may already be doing so. The 2006 draft said: "Recent observations show rapid changes in ice sheet flows," and referred to an "accelerating trend" in sea-level rise. Neither detail made the final version, which observed that "ice flow from Greenland and Antarctica... could increase or decrease in future". Wasdell points out recent findings which show that the rate of loss from ice sheets is doubling every six years, making the suggestion of a future decrease "highly unlikely".

Some of the changes were made at the meeting of government invigilators that finalised the report last month in Paris. But others were made earlier, after the draft report was first distributed to governments in mid-2006.

Senior IPCC scientists contacted by New Scientist have not been willing to discuss how any changes took place but they deny any political interference. However, "if it is true, it's disappointing", says Mike Mann, director of the Earth System Science Center at Pennsylvania State University in University Park and a past lead author for the IPCC. "Allowing governmental delegations to ride into town at the last minute and water down conclusions after they were painstakingly arrived at in an objective scientific assessment does not serve society well."

From issue 2594 of New Scientist magazine, 10 March 2007, page 10

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<Wasdell_IPCC.pdf>

Wg1-ar4-clas mailing list

[4]Wg1-ar4-clas@joss.ucar.edu

[5]<http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas>

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References

1. <mailto:wg1-ar4-clas@joss.ucar.edu>
2. <file://localhost/tmp/convertmbox32286.html>
3. <http://www.meridian.org.uk/whats.htm>
4. <mailto:Wg1-ar4-clas@joss.ucar.edu>
5. <http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas>
6. <mailto:eystein.jansen@geo.uib.no>

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Ken Denman <ken.denman@ec.gc.ca>
Subject: Re: [Wg1-ar4-clas] draft to sign
Date: Fri, 9 Mar 2007 01:05:19 +0100
Cc: wg1-ar4-clas@joss.ucar.edu

Hi all,

it is in the middle of the night here, and I cannot provide much input to writing. Just wished to say that I would be willing to sign on the draft as it is, but hope those writing would consider the input from Susan and Kevin before submitting the final letter.

Eystein

Den 8. mar. 2007 kl. 22.56 skrev Ken Denman:

Hi Piers et al,

I have taken the liberty to suggest a few changes (with change tracker turned on) - while you Europeans (oops, and Brits) at least are sleeping. And Piers and Richard, thanks a lot for getting this moving quickly.

Regards, Ken

ps. Piers - my salary is paid by Fisheries and Oceans Canada. They are VERY uneasy when I speak or write letters to the press, but they get really upset when I don't credit them appropriately. C'est la vie.

[1]piers@env.leeds.ac.uk wrote:

Hi all

This is the latest draft with Jerry's and Ken's edits. However, in addition I've deleted the para on the Paris meeting - as it was essentially repeated within the last paragraph, and slightly reordered the other paragraphs

Again please make further

edits. Also please could people approve the attachment of their name to such a

letter. Non highlighted names are people who appear to have already given approval for their name to be used. If you are a yellow highlighted name I think you are likely (or very likely) to sign!

If we could have a relaxed attitude and sign a letter that is still in the process of being drafted it would save someone (me) a bunch of work at the end collecting approvals

Cheers

Wg1-ar4-clas mailing list

[2]Wg1-ar4-clas@joss.ucar.edu

[3]<http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas>

--

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<NewScientist_2_Ken.doc>

Wg1-ar4-clas mailing list

[6]Wg1-ar4-clas@joss.ucar.edu

[7]<http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas>

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References

1. <mailto:piers@env.leeds.ac.uk>
2. <mailto:Wg1-ar4-clas@joss.ucar.edu>
3. <http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas>
4. <mailto:ken.denman@ec.gc.ca>
5. <http://www.cccma.bc.ec.gc.ca/~kdenman>
6. <mailto:Wg1-ar4-clas@joss.ucar.edu>
7. <http://lists.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas>
8. <mailto:eystein.jansen@geo.uib.no>

From: Jonathan Overpeck <jto@u.arizona.edu>
To: Stefan Rahmstorf <rahmstorf@ozean-klima.de>
Subject: Re: urgent help re Augusto Mangini
Date: Sat, 7 Apr 2007 09:35:51 -0600
Cc: Valerie Masson-Delmotte <Valerie.Masson@cea.fr>, Eystein Jansen <eystein.jansen@geo.uib.no>, Keith Briffa <k.briffa@uea.ac.uk>

<x-flowed>

Hi Stefan - Valerie was the lead on the Holocene section, so I'll cc her. I agree that your approach is the smart one - it's easy to show proxy records (e.g., speleothems) from a few sites that suggest greater warmth than present at times in the past, but our assessment was that there wasn't a period of GLOBAL warmth comparable to present. We used the term likely, however, since there still is a good deal of work to do on this topic - we need a better global network of sites.

Keith can comment on the last 1300 years, but again, I think there is no published evidence to refute what we assessed in the chapter. Again, one or two records does not hemispheric or global make.

I think Keith or Valerie could comment further if they're not Eastering. Eystein, likewise might have something, but I think it is his national responsibility to hit the glaciers over Easter.

Best, Peck

>Dear Peck and IPCC coauthors,

>

>- I know it's Easter, but I'm having to deal with Augusto Mangini, a
>German colleague who has just written an article calling the IPCC
>paleo chapter "wrong", claiming it has been warmer in the Holocene
>than now, and stalagmites show much larger temperature variations
>than tree rings but IPCC ignores them. What should I answer?

>

>One of my points is that IPCC shows all published large-scale proxy
>reconstructions but there simply is none using stalagmites - so
>please tell me if this is true?! My main point will be the local
>vs hemispheric issue, saying that Mangini only provides local
>examples, while the IPCC statement is about hemispheric or global
>averages.

>

>But how about local variations - do stalagmites show much larger
>ones than tree rings? Any suggestions what other counter-arguments I
>could write? Do we have a stalagmite expert on the author team,
>other than contributing
>author Dominik Fleitmann, whom I've already identified?
>I have to submit my response to the newspaper tomorrow.

>

>Thanks, Stefan

>

>--

>Stefan Rahmstorf
>www.ozean-klima.de
>www.realclimate.org
>

>

>

>

>--

>Stefan Rahmstorf

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>www.realclimate.org

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Jonathan T. Overpeck

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Professor, Department of Geosciences

Professor, Department of Atmospheric Sciences

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<http://www.ispe.arizona.edu/>

</x-flowed>

From: Susan Solomon <ssolomon@al.noaa.gov>
To: P.Jones@uea.ac.uk
Subject: Re: urban heat island - since 1950? or since 1900
Date: Tue, 10 Apr 2007 13:23:13 -0600
Cc: trenbert@ucar.edu, "Phil Jones" <p.jones@uea.ac.uk>

<x-flowed>

Phil

Thanks for your reply. I have removed the 'since 1950' from the TS. That was taken from your ES but in view of this discussion I think the reader needs to go to the chapter.

Please note that 'Since 1950' is not (and never was) in the SPM, so there is no interplay at all between the issues being discussed in this series of emails and anything that occurred in Paris or prior to Paris.

It was, of course, for you to decide what you wanted in your ES and how to mesh that with the main text of your chapter. It is entirely a 'within chapter' issue.

best regards,
Susan

At 4:30 PM +0100 4/10/07, P.Jones@uea.ac.uk wrote:
> Susan, Kevin,
> See attachment, I realise this is an important issue,
> as this will be one of the areas the skeptics will go over
> with a fine toothcomb. I'm happy either way - either
> with the since 1950 or without. I've explained why it is
> there.
>
> I'm back in CRU tomorrow am. I'm also
> away on Sunday for the next 2 weeks, so if there is more
> to resolve, we need to do this by Friday.
>

> Cheers

> Phil

>

>

>> Kevin,

>> Thanks for thinking about this. Based on the chapter referencing
>> Brohan and explicitly saying 1900 regarding the 0.006/decade figure
>> which is what is used as the bottom line, I wonder if this is a typo
>> and since 1950 should perhaps be since 1900 in your ES.

>>

>> The same thing occurs in the TS, and I am checking page proofs for
>> that which is why I got to wondering and checked back in chapter 3,
>> where I found this conundrum. If it is correct as 1950, fine, but
>> it doesn't look like that to me.

>>

>> I'll wait to hear from Phil, hopefully tomorrow.

>> bests,

>> Susan

>>

>>

>> At 5:28 PM -0600 4/9/07, Kevin Trenberth wrote:

>>> Susan

>>> This is Phil's territory so I'll leave to him to follow up further. Are
>>> you suggesting that something should change? Seems to me that maybe
>>> removing the "(since 1950)" from ES might help? I am on travel rest of
>>> the week.

>>> Kevin

>>>

>>>> Kevin

>>>> Thanks for your reply.

>>>>

>>>> I am referring to the final distributed draft chapter, which was
>>>> before
>>>> Paris.

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>>>> Your ES pre-Paris (and post-Paris) says 1950 but this seems
>>>> inconsistent with the text of your pre-Paris chapter, where the
>>>> hemispheric and global values are given, and post-1900 is stated at
>>>> that point. The value of 0.006 is clearly associated with post-1900
>>>> in the text.

>>>>

>>>> I don't think that this has anything to do with the clarifications to
>>>> what was meant regarding UHI that were made in the SPM at Paris. The

>>>> question is a lack of consistency in the pre-Paris chapter's ES and
>>>> main text.

>>>>

>>>> Please consult your final draft chapter and let me know.

>>>>

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>>>> Susan

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>>>>

>>>>

>>>>

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>>>> At 3:18 PM -0600 4/9/07, Kevin Trenberth wrote:

>>>>> Susan

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>>>>> after Paris and the values cited from 1900 were inserted at that
>>>>> stage based on one study. Earlier in the text you will see that
>>>>> most studies are from 1950 on: including those of Parker 2004, 2006,
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>>>>> this could be changed: certainly, with current wording it explicitly
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>>>>> appropriate to change to 1900.

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>>>>> My sense is that the awkwardness comes from the late edit.

>>>>> Kevin

>>>>>

>>>>> Susan Solomon wrote:

>>>>>> Kevin and Phil,

>>>>>>

>>>>>> In checking over some text, I noted a statement in your ES that UHI
>>>>>> effects are negligible, where since 1950 is indicated as the
>>>>>> temporal period of application. In the text of the chapter, it
>>>>>> looks more like 1900 to me. Should this be 1950, or 1900? or
> >>>>>> something else?

>>>>>>

>>>>>> Thanks,

>>>>>> Susan

>>>>>>

>>>>>>--

>>>>>*****

>>>>>Kevin E. Trenberth e-mail: trenbert@ucar.edu
>>>>>Climate Analysis Section, www.cgd.ucar.edu/cas/trenbert.html
>>>>>NCAR
>>>>>P. O. Box 3000, (303) 497 1318
>>>>>Boulder, CO 80307 (303) 497 1333 (fax)

>>>>>
>>>>>Street address: 1850 Table Mesa Drive, Boulder, CO 80305

>>>>>
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>>>>_____

>>>>Kevin Trenberth
>>>>Climate Analysis Section, NCAR
>>>>PO Box 3000
>>>>Boulder CO 80307
>>>>ph 303 497 1318
>>>>http://www.cgd.ucar.edu/cas/trenbert.html

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>>
>
>Attachment converted: Junior:urbanizationESTS.doc (WDBN/«IC») (00167B2F)

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From: "Kevin Trenberth" <trenbert@ucar.edu>
To: p.jones@uea.ac.uk
Subject: Re: urban heat island - since 1950? or since 1900
Date: Tue, 10 Apr 2007 20:24:35 -0600 (MDT)
Reply-to: trenbert@ucar.edu

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Kevin

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>> Cheers
>> Phil

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>>>>>>> effects are negligible, where since 1950 is indicated as the
>>>>>>> temporal period of application. In the text of the chapter, it

>>>>>>looks more like 1900 to me. Should this be 1950, or 1900? or
>> >>>>>something else?

>>>>>>

>>>>>>Thanks,

>>>>>>Susan

>>>>>>

>>>>>>--

>>>>>>*****

>>>>>>Kevin E. Trenberth e-mail: trenbert@ucar.edu

>>>>>>Climate Analysis Section,

>>>>>> www.cgd.ucar.edu/cas/trenbert.html

>>>>>>NCAR

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>>>>>>

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>>>>>>

>>>>>

>>>>>

>>>>>_____

>>>>>Kevin Trenberth

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>>>>

>>>>

>>>

>>Attachment converted: Junior:urbanizationESTS.doc (WDBN/«IC») (00167B2F)

>

>

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http://www.cgd.ucar.edu/cas/trenbert.html

From: P.Jones@uea.ac.uk

To: "C G Kilsby" <c.g.kilsby@newcastle.ac.uk>

Subject: RE: Outputs from WG

Date: Mon, 16 Apr 2007 13:55:37 +0100 (BST)

Cc: david.sexton@metoffice.gov.uk, "Phil Jones" <p.jones@uea.ac.uk>, "Colin Harpham" <c.harpham@uea.ac.uk>, "H J Fowler" <h.j.fowler@newcastle.ac.uk>

Chris et al,

I'll sedn some more thoughts on Thursday when back from the EGU. It is too hot in Vienna to sit through too many talks !

I suspect we need a subset of indices. The program will calculate all those recommended in various programs. One possibility is to keep them all and let users decide. We do need to make a series of checks though at some stage to make sure they are OK.

I think you'll have some fruitful discussions on some of these on April 24. I hope you can come to closure on a few things.

Cheers

Phil

> All:

>

> Indices

>

> I had a session with UKCIP last week, and we did get on to discussing
> what outputs might come out of WG (as well as DDP etc.) and the issue of
> indices derived from daily data (i.e. requiring time series) came up,
> with the distinct possibility of confusion/inconsistency as David
> mentions!

>

> I would be happy to produce indices only from WG, as long as we can
> check they are sensible first of course!
> E.g. heatwave duration (various thresholds), drought duration, various
> accumulations of rainfall ?

- > Less clear cut might be gale days (definition?), snow days, proportion
- > of days above temp threshold etc.
- >
- > I think we will need to consider the list in detail, as far as what is
- > included (STARDEX list?), how they are calculated/validated and also
- > whether they can be calculated from some other source and found to be
- > inconsistent.
- > E.g. is it planned to take the (17?) RCM runs and analyse/release these
- > indices as well ?
- >
- >
- > Rainfall stats - pdfs
- >
- > I think (hope?) lag1-ac and skewness will actually be quite well behaved
- > (if not realistic) even when you convert/downscale. The more
- > validation/analysis we do of these fields the better anyway.
- >
- >
- > Separate topic: measures of reliability
- >
- > May be a can of worms, but I think we need to address it sooner rather
- > than later: UKCIP02 had subjective measures of reliability attached to
- > different variables/predicted changes. We must do better, and a case in
- > point is the WG where we sidestep the bias issue by using change
- > factors. We therefore need to provide some measure (per grid square, per
- > variable?) of reliability.
- >
- > For example: if control annual rainfall is more than (say) 10% biased,
- > reduce reliability measure and inform the user when generating.
- > Problem 1: which model runs to use for this check?
- > Problem 2: how to assess more complex measures e.g. annual cycle in
- > rainfall/temperature?
- > Problem 3: need a common, easily understood scale of reliability
- > Furthermore - WG procedure introduces more uncertainty, e.g. for wind
- >
- > Thoughts?
- >
- > Cheers, Chris
- >
- >
- >
- >>-----Original Message-----

>>From: david.sexton@metoffice.gov.uk
>>[mailto:david.sexton@metoffice.gov.uk]
>>Sent: 16 April 2007 08:07
>>To: Phil Jones
>>Cc: david.sexton@metoffice.gov.uk; C G Kilsby; Colin Harpham
>>Subject: RE: Outputs from WG

>>

>>Hi,

>>

>>we will try for lag-1 correlation and skewness but an issue
>>for us is whether something doesn't work when we convert the
>>equilibrium pdfs to time-dependent ones or we downscale to 25km.

>>

>>As Phil has said that you can do all the derived indices
>>except gale days, if we could get a decision from the project
>>management team to cut those variables from MOHC list of
>>outputs without making any extra work for you, then that would
>>free up some time for us to investigate this further.

>>

>>Looking forward to seeing Colin's results on 24th.

>>

>>Cheers, David

>>

>>

>>

>>

>>On Fri, 2007-04-13 at 17:16 +0100, Phil Jones wrote:
>>> Some more thoughts - keep in on the loop in case i get a chance
>>> to respond from Vienna or next Thursday.

>>>

>>> Phil

>>>

>>>

>>> At 16:32 13/04/2007, david.sexton@metoffice.gov.uk wrote:

>>> >Hi,

>>> >

>>> >On Fri, 2007-04-13 at 16:00 +0100, C G Kilsby wrote:

>>> > > Phil, David

>>> > >

>>> > > Briefly, and can respond fully next week when I have
>>some more time!

>>> > >

>>> > > Some crucial points here,
>>> > > 1. the one re 90%ile of one variable not same as for
>>other variables.
>>> > > Some simple restrictions need considering before diving off into
>>> > > full joint pdfs etc.
>>> > > Also, another dimension emerges with seasons, e.g. 90%ile winter
>>> > > rainfall, or 90%ile summer rainfall?
>>> >
>>> >Joint pdfs are just an issue for me in that I am giving you several
>>> >inputs to WG and they have to be consistent. For example, we are
>>> >finding we only get wetter summers for lower end of temperature
>>> >increases. Plus we already intend to provide sets of sampled values
>>> >for lots of variables that are consistent for any given point in
>>> >model parameter space.
>>>
>>> The joint pdfs are an issue for the WG as well. Not so much for
>>> Chris, but for us we have to reproduce the statistics for
>>> the other variables. Colin
>>> has solved the double counting issue for the means (for T etc),
>>> but we've yet to look at the variance.
>>>
>>> Colin should be able to show some of the results on the 24th
>>> as to how well the WG works. This fits the WG (with our rainfall
>>> component) to HadRM3 and then applies our modification
>>> technique to an A2 future (for comparison with the true RCM
>>> future for the 2070s). Sunshine is the only real problem.
>>>
>>> I don't think we need to repeat this with the NS rainfall,
>>> but discuss that once you've seen some preliminary results
>>> on the 24tjh.
>>>
>>>
>>> > >
>>> > > 2. Bit concerned to hear David talking of some precip
>>stats being
>>> > > secondary or optional - I would say mean, var and pdry days are
>>> > > all
>>> > > essential: from our experience autocorrelation and skewness are
>>> > > also pretty well behaved and we would rather have them
>>if at all possible!
>>> >
>>> >

>>> >Good. This discussion is throwing up a few discrepancies which need
>>> >clarifying. That some precip stats are of secondary
>>importance, is an
>>> >impression I was getting from Phil's earlier emails last month.

>>>

>>>

>>> I think there is some misunderstanding here. What I said earlier
>>> confirms what Chris has said - if they are available then Chris
>>> would like them. Chris will need to consider is they may be
>>> fully relevant due to the scale issue (25km squares vs points).
>>> Could be an issue for skew and r1.

>>>

>>> Checking this out a la fitting directly to HadRCM3 control
>>> data might be useful here. See Colin's plots though before
>>> deciding.

>>>

>>>

>>>

>>> >I look forward to the fuller response next week. I will be mainly
>>> >away then which is why I raise these issues now. It would
>>be good to
>>> >have a good chat about them on the 24th.

>>> >

>>> >Cheers, David

>>> >

>>> >>

>>> >>

>>> >> Cheers,

>>> >> Chris

>>> >>

>>> >> >-----Original Message-----

>>> >> >From: Phil Jones [mailto:p.jones@uea.ac.uk]

>>> >> >Sent: 13 April 2007 15:46

>>> >> >To: david.sexton@metoffice.gov.uk

>>> >> >Cc: david.sexton@metoffice.gov.uk; C G Kilsby; Colin Harpham

>>> >> >Subject: Re: Outputs from WG

>>> >> >

>>> >> >

>>> >> > David,

>>> >> > More thoughts embedded.

>>> >> >

>>> >> > Phil

>>> > >

>>> > > >At 15:12 13/04/2007, david.sexton@metoffice.gov.uk wrote:

>>> > > >Hi,

>>> > > >

>>> > > >I think we have clarified or converged on most of my points. I

>>> > > >have some comments on points 2 and 4.

>>> > > >

>>> > > >Cheers, David

>>> > > >

>>> > > >

>>> > > >

>>> > > >

>>> > > >

>>> > > >On Fri, 2007-04-13 at 14:42 +0100, Phil Jones wrote:

>>> > > > >2. WG will produce 100 versions of 30-yr sequences for

>>> > > > >all (or just

>>> > > > >one?) WG variables for all months for a given combination

>>> > > > >of 30-yr

>>> > > > >period, emissions scenario and location.

>>> > > > >

>>> > > > >I am still not clear how to generate the 100.

>>> > > > >Percentiles of

>>> > > > >PDFs is confusing me. I think Ag needs a clear procedure

>>> > > > >outlined by us

>>> > > > >for 24th. I think the easiest way to make WG

>>> > > > >consistent with

>>> > > > >MOHC pdfs is the following (assuming I am correct so far):

>>> > > > >

>>> > > > > a. User selects WG, 30-yr period, emissions

>>> > > > >scenario and

>>> > > > >location

>>> > > > >(up

>>> > > > >to 1000km²).

>>> > > > > b. Work out which 25km x 25km box over UK is closest to

>>> > > > >this

>>> > > > >multi-

>>> > > > >site location.

>>> > > > > c. For the 30-yr period, emissions scenario and

>>> > > > >location

>>> > > > >in b),

>>> > > > >DDP

>>> > > > >internally produces a table of changes in mean T, %

>>> > > > changes in mean
>>> > > > > P,
>>> > > > > > and changes in variance of P for each month for
>> 100 randomly
>>> > > > > > sampled different model variants. DDP ALREADY needs this
>>> > > > > > capability.
>>> > > > > > d. So we have an internal matrix with $3*12=36$
>> columns and
>>> > > > > > 100
>>> > > > > > rows. WG
>>> > > > > > loops through 100 rows, using each set of 36
>> numbers to drive WG.
>>> > > > > > User
>>> > > > > > gets 100 WG's. Does what they like with it.
>>> > > > > >
>>> > > > > >
>>> > > > > > Sort of. The 100 versions of the WG I was talking
>>> > > > > > about will all
>>> > > > > > have
>>> > > > > > the same statistics. I thought these 100 would be from
>>> > > > > > one point
>>> > > > > > within
>>> > > > > > the pdf (or the joint pdf) - say the 10, 50 or 90th
>>> > > > > > percentile. We
>>> > > > > > could make
>>> > > > > > this percentile selectable.
>>> > > > > >
>>> > > > > > The 100 (or 1 or whatever) are representative of some
>>> > > > > > future 30-year period.
>>> > > > > > Your a) and b) are fine.
>>> > > > > >
>>> > > > > > Another option is like yours. There is a pdf
>> (or joint pdf).
>>> > > > > > The 100 could be
>>> > > > > > from each of the 100 percentiles? Does this make sense?
>>> > > > > > Or the 100
>>> > > > > > could
>>> > > > > > come from sampling the percentile space assuming a normal
>>> > > > > > distribution?
>>> > > > > >
>>> > > > > > Your 2) is an important aspect to sort out on the 24th.
>>> > > > > >

>>> > > >>
>>> > > >>I agree that we need to discuss this but it would be good to
>>> > > >thrash it
>>> > > >>out a bit more before 24th. UKCIP08 needs the WG pdf to be
>>> > > >>consistent with the MOHC pdf. Your solution tries to
>>> > > >>do this but
>>> > > >>a problem with selecting a percentile is that a model variant
>>> > > >>that is the 90th percentile for temperature is not
>>> > > >>90th percentile for other variables.
>>> > > >>There is also a related issue about how you chose a model
>>> > > >>variant near
>>> > > >>a given percentile. The solution I propose means these are
>>> > > >>not issues.
>>> > > >>So we could sample M model variants and run N WGs for
>>> > > >>each model
>>> > > >>variant. M has to be a good size to make sample
>>> > > >>representative of MOHC
>>> > > >>pdf but N does not have to be large as internal variability
>>> > > >>is already
>>> > > >>generated by using a different set of parameters and a
>>> > > >>different seed for each WG.
>>> > > >>I think this solution is simpler than the percentile-based
>>> > > >>solution. Do
>>> > > >>you agree?
>>> > > >>
>>> > > >>
>>> > > >> Sounds OK. Let's see what Chris thinks.
>>> > > >>
>>> > > >>
>>> > > >>
>>> > > >>
>>> > > >>
>>> > > >> >>4. Phil has mentioned in the past that EARWIG produces some
>>> > > >> >>diagnostics
>>> > > >> >>e.g. consecutive dry days, frost days etc. from WG. Will
>>> > > >> >>this be done for UKCIP08?
>>> > > >> >>
>>> > > >> >>
>>> > > >> >> The plan is yes for this. Colin has the software
>>> > > >> >>for this.
>>> > > >> >> It just needs to be set
>>> > > >> >> up carefully, as the base for all the diagnostics
>>> > > >> >>(for the

>>> > >> > future
>>> > >> > runs) has to be
>>> > >> > based on median run of the WG for the present (61-90).
>>> > > > >We shouldn't
>>> > >> > allow users to change the 61-90 base period (or the
>>> > > > >choice of the
>>> > >> > median).
>>> > >> >
>>> > >> >
>>> > >> >
>>> > >> >
>>> > >> > >Good. I would like your opinion on a problem I am having with
>>> > >> > some of the variables we are providing pdfs for. Some
>> quantities
>>> > >> > are indices derived from daily model data e.g frost days but I
>>> > >> > think
>>> > > > >there are two
>>> > >> > problems with this:
>>> > >> >
>>> > >> > 1. Model bias e.g. a model that is too warm may have very few
>>> > >> > frost days and therefore the change looks small.
>> Effect will be
>>> > >> > a nonlinear function of bias based on shape of
>> distribution of daily data.
>>> > >> >
>>> > >> > 2. WG and pdfs could provide two alternative routes to same
>>> > > > answer and
>>> > >> > they will obviously conflict for reasons we understand e.g.
>>> > > > model bias
>>> > >> > but the users won't understand.
>>> > >> >
>>> > >> > To avoid confusing user and potentially reducing their
>>> > >> > confidence in UKCIP products, I think it makes sense
>> for WGs to
>>> > >> > be the sole route towards a prediction of derived
>> indices. BTW,
>>> > >> > I have a handful of derived indices to do (hot days, wet days,
>>> > >> > gale days, heating and cooling degree days and frost
>> days) and I
>>> > >> > think you cover
>>> > > > some of these
>>> > >> > already. What do you think?

>>> > > >

>>> > > > Geoff wants to discuss issues connected to the three strands

>>> > > > of output

>>> > > > (pdfs, WG, RCM) on the 24th.

>>> > > >

>>> > > >

>>> > > > Model biases will only be a problem with their data

>>> > > > used directly.

>>> > > > So this could be a problem with the larger regions

>>> > > > where the WG

>>> > > > won't work well. The WG won't have biases as it is based on

>>> > > > 61-90 as the base period. We will be perturbing these

>>> > > > with the

>>> > > > RCM-based pdfs.

>>> > > >

>>> > > > Maybe we need to show that the following will/should/must be

>>> > > > the same

>>> > > >

>>> > > > Model-based scenario for 2070s minus model present

>>> > > > (61-90) equals

>>> > > > WG scenarios for the 2070s minus WG present (61-90).

>>> > > >

>>> > > > Geoff will need to get this across as this is how the three

>>> > > > strands will

>>> > > > produce the same answers.

>>> > > >

>>> > > > The WG and the extremes software will do all the temp/precip

>>> > > > indices but won't do gale days.

>>> > > >

>>> > > >

>>> > > > Cheers, David

>>> > > >

>>> > > >

>>> > > >

>>> > > >

>>> > > >

>>> > > >--

>>> > > >

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>>> > >>

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>>> >--

>>> >

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>>

>

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: mann@psu.edu
Subject: Re: FYI
Date: Sat, 21 Apr 2007 08:24:12 -0600 (MDT)
Reply-to: trenbert@ucar.edu
Cc: "Phil Jones" <p.jones@uea.ac.uk>, "Ben Santer" <santer1@llnl.gov>

Hi Phil

I am sure you know that this is not about the science. It is an attack to undermine the science in some way. In that regard I don't think you can ignore it all, as Mike suggests as one option, but the response should try to somehow label these guys and lazy and incompetent and unable to do the huge amount of work it takes to construct such a database. Indeed technology and data handling capabilities have evolved and not everything was saved. So my feeble suggestion is to indeed cast aspersions on their motives and throw in some counter rhetoric. Labeling them as lazy with nothing better to do seems like a good thing to do.

How about "I tried to get some data from McIntyre from his 1990 paper, but I was unable because he doesn't have such a paper because he has not done any constructive work!"

There is no basis for retracting a paper given in Keenan's message. One may have to offer a correction that a particular sentence was not correct if it claimed something that indeed was not so. But some old instrumental data are like paleo data, and can only be used with caution as the metadata do not exist. It doesn't mean they are worthless and can not be used. Offering to make a correction to a few words in a paper in a trivial manner will undermine his case.

Kevin

> Hi Phil,
>
> This is all too predictable. This crowd of charlatans is always looking
> for one thing they can harp on, where people w/ little knowledge of the
> facts might be able to be convinced that there is a controversy. They
> can't take on the whole of the science, so they look for one little
> thing they can say is wrong, and thus generalize that the science is
> entirely compromised. Of course, as nicely shown in the SPM, every
> landmass is independently warming, and much as the models predict. So

> they can harp all they want on one Chinese data set, it couldn't
> possibly change the big picture (let alone even the trends for China). The
>
> So they are simply hoping to blow this up to something that looks like a
> legitimate controversy. The last thing you want to do is help them by
> feeding the fire. Best thing is to ignore them completely. They no
> longer have their friends in power here in the U.S., and the media has
> become entirely unsympathetic to the rants of the contrarians at least
> in the U.S.--the Wall Street Journal editorial page are about the only
> place they can broadcast their disinformation. So in other words, for
> contrarians the environment appears to have become very unfavorable for
> development. I would advise Wang the same way. Keenan may or may not be
> bluffing, but if he tries this I believe that British law would make it
> easy for Wang to win a defamation suit against him (the burden is much
> tougher in the states),

>
> mike

>
> Phil Jones wrote:

>>
>> Kevin,
>> Have a look at this web site. I see you're away.
>> The websites can wait, but scroll down to the letter below
>> from Keenan - the last sentence.

>>
>> <http://www.climateaudit.org/?p=1471#comments>

>>
>> and

>>
>> <http://www.climateaudit.org/?p=1479#more-1479>

>>
>> One is about data from a paper 17 years ago (Jones et al. 1990)

>>
>> Also there is this email (below) sent to Wei-Chyung Wang, who was
>> one of the co-authors on the 1990 paper. Wei-Chyung is in
>> China, and may not yet have seen this. When he's back in
>> Albany, I've suggested he talks to someone there. It is
>> all malicious. I've cc'd this to Ben and Mike as well, to get
>> any thoughts from their experiences.

>>
>> If it gets worse I will bring Susan in as well, but I'm talking
>> to some people at UEA first. Susan has enough to do
>> with getting the AR4 WG1 volume out.

>>
>> On the 1990 paper, I have put the locations and the data for
>> the rural stations used in the paper on the CRU website. All
>> the language is about me not being able to send them the
>> station data used for the grids (as used in 1990!). I don't
>> have this information, as we have much more data now
>> (much more in Australia and China than then) and probably
>> more stations in western USSR are as well.
>>
>> As for the other request, I don't have the information on
>> the sources of all the sites used in the CRUTEM3 database.
>> We are adding in new datasets regularly (all of NZ from
>> Jim Renwick recently) , but we don't keep a source code
>> for each station. Almost all sites have multiple sources and
>> only a few sites have single sources. I know things roughly
>> by country and could reconstruct it, but it would take a while.
>>
>> GHCN and NCAR don't have source codes either. It does
>> all come from the NMSs - well mostly, but some from
>> scientists.
>>
>> A lot of the issues are in various papers, but they never
>> read these. Also certainly no use talking to them.
>>
>> In Geneva all week. David Parker and Tom Peterson will
>> be there. I can live with the web site abuse, but the Keenan
>> letter knocked me back a bit.
>>
>> I seem to be the marked man now !
>>
>> Cheers
>> Phil
>>
>>
>>
>>
>> From: "D.J. Keenan" <doug.keenan@informath.org>
>> To: "Wei-Chyung Wang" <>wang@climate.cestm.albany.edu>
>> Cc: "Phil Jones" <p.jones@uea.ac.uk>
>> Subject: retraction request
>> Date: Fri, 20 Apr 2007 13:31:15 +0100
>> X-Mailer: Microsoft Outlook Express 6.00.2900.3028
>> X-UEA-Spam-Score: 0.0

>> X-UEA-Spam-Level: /
>> X-UEA-Spam-Flag: NO
>>
>> Dear Dr. Wang,
>> Regarding the Chinese meteorological data analyzed by Wang et al.
>> [GRL, 1990] and Jones et al. [Nature, 1990], it now seems clear that
>> there are severe problems. In particular, the data was obtained from
>> 84 meteorological stations that can be classified as follows.
>> 49 have no histories 08 have inconsistent histories 18 have
>> substantial relocations 02 have single-year relocations 07 have
>> no relocations Furthermore, some of the relocations are very
>> distant--over 20 km.
>> Others are to greatly different environments, as illustrated here:
>> <http://www.climateaudit.org/?p=1323#comment-102970>
>>
>> The above contradicts the published claim to have considered the
>> histories of the stations, especially for the 49 stations that have no
>> histories. Yet the claim is crucial for the research conclusions.
>>
>> I e-mailed you about this on April 11th. I also phoned you on April
>> 13th: you said that you were in a meeting and would get back to me. I
>> have received no response.
>>
>> I ask you to retract your GRL paper, in full, and to retract the
>> claims made in Nature about the Chinese data. If you do not do so, I
>> intend to publicly submit an allegation of research misconduct to your
>> university at Albany.
>>
>>
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>>
>>
>>
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>> -----

>>

>

>

> --

> Michael E. Mann

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>

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>

>

Kevin Trenberth

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<http://www.cgd.ucar.edu/cas/trenbert.html>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: FYI
Date: Sat, 21 Apr 2007 09:45:50 -0400
Reply-to: mann@psu.edu
Cc: trenbert@ucar.edu, Ben Santer <santer1@llnl.gov>

<x-flowed>

Hi Phil,

This is all too predictable. This crowd of charlatans is always looking for one thing they can harp on, where people w/ little knowledge of the facts might be able to be convinced that there is a controversy. They can't take on the whole of the science, so they look for one little thing they can say is wrong, and thus generalize that the science is entirely compromised. Of course, as nicely shown in the SPM, every landmass is independently warming, and much as the models predict. So they can harp all they want on one Chinese data set, it couldn't possibly change the big picture (let alone even the trends for China). The

So they are simply hoping to blow this up to something that looks like a legitimate controversy. The last thing you want to do is help them by feeding the fire. Best thing is to ignore them completely. They no longer have their friends in power here in the U.S., and the media has become entirely unsympathetic to the rants of the contrarians at least in the U.S.--the Wall Street Journal editorial page are about the only place they can broadcast their disinformation. So in other words, for contrarians the environment appears to have become very unfavorable for development. I would advise Wang the same way. Keenan may or may not be bluffing, but if he tries this I believe that British law would make it easy for Wang to win a defamation suit against him (the burden is much tougher in the states),

mike

Phil Jones wrote:

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> Kevin,
> Have a look at this web site. I see you're away.
> The websites can wait, but scroll down to the letter below
> from Keenan - the last sentence.
>

> <http://www.climateaudit.org/?p=1471#comments>
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> One is about data from a paper 17 years ago (Jones et al. 1990)
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> Also there is this email (below) sent to Wei-Chyung Wang, who was
> one of the co-authors on the 1990 paper. Wei-Chyung is in
> China, and may not yet have seen this. When he's back in
> Albany, I've suggested he talks to someone there. It is
> all malicious. I've cc'd this to Ben and Mike as well, to get
> any thoughts from their experiences.
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> If it gets worse I will bring Susan in as well, but I'm talking
> to some people at UEA first. Susan has enough to do
> with getting the AR4 WG1 volume out.
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> On the 1990 paper, I have put the locations and the data for
> the rural stations used in the paper on the CRU website. All
> the language is about me not being able to send them the
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> have this information, as we have much more data now
> (much more in Australia and China than then) and probably
> more stations in western USSR are as well.
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> the sources of all the sites used in the CRUTEM3 database.
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> Jim Renwick recently) , but we don't keep a source code
> for each station. Almost all sites have multiple sources and
> only a few sites have single sources. I know things roughly
> by country and could reconstruct it, but it would take a while.
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> GHCN and NCAR don't have source codes either. It does
> all come from the NMSs - well mostly, but some from
> scientists.
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> A lot of the issues are in various papers, but they never
> read these. Also certainly no use talking to them.
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> In Geneva all week. David Parker and Tom Peterson will

> be there. I can live with the web site abuse, but the Keenan
> letter knocked me back a bit.
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> I seem to be the marked man now !
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> Cheers
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>
> From: "D.J. Keenan" <doug.keenan@informath.org>
> To: "Wei-Chyung Wang" <wang@climate.cestm.albany.edu>
> Cc: "Phil Jones" <p.jones@uea.ac.uk>
> Subject: retraction request
> Date: Fri, 20 Apr 2007 13:31:15 +0100
> X-Mailer: Microsoft Outlook Express 6.00.2900.3028
> X-UEA-Spam-Score: 0.0
> X-UEA-Spam-Level: /
> X-UEA-Spam-Flag: NO
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> Dear Dr. Wang,
> Regarding the Chinese meteorological data analyzed by Wang et al.
> [GRL, 1990] and Jones et al. [Nature, 1990], it now seems clear that
> there are severe problems. In particular, the data was obtained from
> 84 meteorological stations that can be classified as follows.
> 49 have no histories 08 have inconsistent histories 18 have
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> distant--over 20 km.
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> <http://www.climateaudit.org/?p=1323#comment-102970>
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> The above contradicts the published claim to have considered the
> histories of the stations, especially for the 49 stations that have no
> histories. Yet the claim is crucial for the research conclusions.
>
> I e-mailed you about this on April 11th. I also phoned you on April
> 13th: you said that you were in a meeting and would get back to me. I
> have received no response.
>
> I ask you to retract your GRL paper, in full, and to retract the
> claims made in Nature about the Chinese data. If you do not do so, I

> intend to publicly submit an allegation of research misconduct to your
> university at Albany.

>

>

> Douglas J. Keenan

> <http://www.informath.org>

> phone + 44 20 7537 4122

> The Limehouse Cut, London E14 6N, UK

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> Prof. Phil Jones

> Climatic Research Unit Telephone +44 (0) 1603 592090

> School of Environmental Sciences Fax +44 (0) 1603 507784

> University of East Anglia

> Norwich Email p.jones@uea.ac.uk

> NR4 7TJ

> UK

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>

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Michael E. Mann

Associate Professor

Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075

503 Walker Building FAX: (814) 865-3663

The Pennsylvania State University email: mann@psu.edu

University Park, PA 16802-5013

<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: FYI
Date: Tue, 24 Apr 2007 09:57:34 -0700
Reply-to: santer1@llnl.gov
Cc: trenbert@ucar.edu, mann@psu.edu

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Dear Phil,

Sorry about the delay in replying to your email - I've been out of my office for a few days.

This is really nasty stuff, and I'm sorry that it's happened to you. The irony in this is that you are one of the most careful and thorough scientists I know.

Keenan's allegations of research misconduct, although malicious and completely unfounded, clearly require some response. The bottom line is that there are uncertainties inherent in measuring ANY properties of the real-world climate system. You've probably delved deeper than anyone else on the planet into uncertainties in observed surface temperature records. This would be well worth pointing out to Mr. Keenan. The whole tenor of the web-site stuff and Keenan's garbage is that these folks are scrupulously careful data analysts, and you are not. They conveniently ignore all the pioneering work that you've done on identification of inhomogeneities in surface temperature records. The response should mention that you've spent much of your scientific career trying to quantify the effects of such inhomogeneities, changing spatial coverage, etc. on observed estimates of global-scale surface temperature change.

The bottom line here is that observational data are frequently "messy". They are not the neat, tidy beasts Mr. Keenan would like them to be. This holds not only for surface temperature measurements. It also holds - in spades - for measurements of tropospheric temperature from MSU and radiosondes, and for measurements of ocean temperatures from XBTs, profiling floats, etc. We would like observing systems to be more accurate, more stable, and better-suited for monitoring decadal-scale changes in climate. You and Kevin and many other are actively working towards that goal. The key message here is that, despite uncertainties in the surface temperature record - uncertainties which you and others in the field are well aware of, and have worked hard to quantify - it is

now unequivocal that surface temperatures have warmed markedly over the past 100 years. Uncertainties in the station histories do not negate this basic message.

Hope some of these random musings might be useful, Phil. Let me know if there's anything else I can do to help. Will you be at the Hadley Centre Science Review Group meeting in May?

With best regards,

Ben

P.Jones@uea.ac.uk wrote:

> All,

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>>> Douglas J. Keenan <http://www.informath.org> phone + 44 20 7537 4122 The
>>> Limehouse Cut, London E14 6N, UK Prof. Phil Jones Climatic Research
>>> Unit Telephone +44 (0) 1603 592090 School of Environmental
>>> Sciences Fax +44 (0) 1603 507784 University of East Anglia Norwich
>>> Email p.jones@uea.ac.uk NR4 7TJ UK
>>> -----
>>> -- Michael E. Mann Associate Professor Director, Earth
>>> System Science Center (ESSC) Department of Meteorology
>>> Phone: (814) 863-4075 503 Walker Building FAX:
>>> (814)
>>> 865-3663 The Pennsylvania State University email: mann@psu.edu
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>>> <http://www.met.psu.edu/dept/faculty/mann.htm>
>>> _____ Kevin Trenberth Climate Analysis Section, NCAR PO
>>> Box

>>> 3000 Boulder CO 80307 ph 303 497 1318
>>> <http://www.cgd.ucar.edu/cas/trenbert.html>
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Benjamin D. Santer
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Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: FYI
Date: Wed, 25 Apr 2007 16:58:29 -0700
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

I looked at some of the stuff on the Climate Audit web site. I'd really like to talk to a few of these "Auditors" in a dark alley. They seem to have no understanding of how science is actually done - no appreciation of the fact that uncertainty is an integral part of what we do. Once again, just let me know how I can help....

It will be good to see you in Exeter. I'm looking forward to that. I'll have two nights in London after the meeting, and am hoping to spend some time wandering around the British Museum.

I met a very nice lady (Stephanie) while I was giving a series of climate change lectures in Puerto Rico back in January. She's a Professor at the University of San Francisco, and (fortuitously), specializes in the policy implications of climate change, risk assessment, etc. She also likes hiking and climbing. It's fun to "have a life" again (as they say over here).

Best wishes to you and Ruth,

Ben

P.Jones@uea.ac.uk wrote:

- > Ben,
- > Thanks for the thoughts. I'm in Geneva at the moment,
- > so have a bit of time to think. Possibly I'll
- > get the raw data from GHCN and do some work to replace
- > our adjusted data with these, then make the Raw
- > (i.e. as transmitted by the NMSs). This will annoy them
- > more, so may inflame the situation.
- >
- > Got some ideas/thoughts from Mike, Kevin and Gavin Schmidt.
- >
- > Some of the stuff on the Climat Audit web site is awful.
- >

> Will also be talking to someone at UEA, is they have
> anything useful to say.

> Also talking to Wei-Chyung about how he'll respond.

> I will be in Exeter. Get back from Tarragona on the
> Weds am, so should be there for dinner on the first day.

> Lots of odd things going on at the HC by the way.

> See you in Exeter.

> Cheers

> Phil

>> Dear Phil,

>> Sorry about the delay in replying to your email - I've been out of my
>> office for a few days.

>> This is really nasty stuff, and I'm sorry that it's happened to you. The
>> irony in this is that you are one of the most careful and thorough
>> scientists I know.

>> Keenan's allegations of research misconduct, although malicious and
>> completely unfounded, clearly require some response. The bottom line is
>> that there are uncertainties inherent in measuring ANY properties of the
>> real-world climate system. You've probably delved deeper than anyone
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>> records. This would be well worth pointing out to Mr. Keenan. The whole
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>> mention that you've spent much of your scientific career trying to
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>> They are not the neat, tidy beasts Mr. Keenan would like them to be.
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>> past 100 years. Uncertainties in the station histories do not negate
>> this basic message.

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>> With best regards,

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>>>> the
>>>> NMSs - well mostly, but some from scientists. A lot of the issues
>>>> are
>>>> in various papers, but they never read these. Also certainly no use
>>>> talking to them. In Geneva all week. David Parker and Tom Peterson
>>>> will be there. I can live with the web site abuse, but the Keenan
>>>> letter knocked me back a bit. I seem to be the marked man !
>>>> Cheers Phil From: "D.J. Keenan" To: "Wei-Chyung Wang" Cc:
>>>> "Phil
>>>> Jones" Subject: retraction request Date: Fri, 20 Apr 2007 13:31:15
>>>> +0100
>>>> X-Mailer: Microsoft Outlook Express 6.00.2900.3028 X-UEA-Spam-Score:
>>>> 0.0
>>>> X-UEA-Spam-Level: / X-UEA-Spam-Flag: NO Dear Dr. Wang, Regarding the
>>>> Chinese meteorological data analyzed by Wang et al. [GRL, 1990] and
>>>> Jones
>>>> et al. [Nature, 1990], it now seems clear that there are severe

>>>>> problems.
>>>>> In particular, the data was obtained from 84 meteorological stations
>>>>> that can be classified as follows. 49 have no histories 08 have
>>>>> inconsistent histories 18 have substantial relocations 02 have
>>>>> single-year relocations 07 have no relocations Furthermore, some of
>>>>> the relocations are very distant--over 20 km. Others are to greatly
>>>>> different environments, as illustrated here:
>>>>> <http://www.climateaudit.org/?p=1323#comment-102970> The above
>>>>> contradicts
>>>>> the published claim to have considered the histories of the stations,
>>>>> especially for the 49 stations that have no histories. Yet the claim
>>>>> is
>>>>> crucial for the research conclusions. I e-mailed you about this on
>>>>> April
>>>>> 11th. I also phoned you on April 13th: you said that you were in a
>>>>> meeting and would get back to me. I have received no response. I ask
>>>>> you to retract your GRL paper, in full, and to retract the claims made
>>>>> in
>>>>> Nature about the Chinese data. If you do not do so, I intend to
>>>>> publicly
>>>>> submit an allegation of research misconduct to your university at
>>>>> Albany.
>>>>> Douglas J. Keenan <http://www.informath.org> phone + 44 20 7537 4122
>>>>> The
>>>>> Limehouse Cut, London E14 6N, UK Prof. Phil Jones Climatic
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>>>>> _____ Kevin Trenberth Climate Analysis Section, NCAR PO
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>>>>> 3000 Boulder CO 80307 ph 303 497 1318
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>>>>>

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</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>

To: mann@psu.edu

Subject: Re: quick note on TAR

Date: Sun Apr 29 19:53:16 2007

Mike

your words are a real boost to me at the moment. I found myself questioning the whole process and being often frustrated at the formulaic way things had to be done - often wasting time and going down dead ends. I really thank you for taking the time to say these kind words . I tried hard to balance the needs of the science and the IPCC , which were not always the same. I worried that you might think I gave the impression of not supporting you well enough while trying to report on the issues and uncertainties . Much had to be removed and I was particularly unhappy that I could not get the statement into the SPM regarding the AR4 reinforcement of the results and conclusions of the TAR. I tried my best but we were basically railroaded by Susan. I am happy to pass the mantle on to someone else next time. I feel I have basically produced nothing original or substantive of my own since this whole process started. I am at this moment , having to work on the ENV submission to the forthcoming UK Research Assessment exercise , again instead of actually doing some useful research ! Anyway thanks again Mike.... really appreciated when it comes from you
very best wishes

Keith

Keith

At 18:14 29/04/2007, you wrote:

Keith, just a quick note to let you know I've had a chance to read over the key bits on last millennium in the final version of the chapter, and I think you did a great job. obviously, this was one of the most (if not the most) contentious areas in the entire report, and you found a way to (in my view) convey the the science accurately, but in a way that I believe will be immune to criticisms of bias or neglect--you dealt w/ all of the controversies, but in a very even-handed and fair way. bravo!
I hope you have an opportunity to relax a bit now. looking forward to buying you a beer next time we have an opportunity :)

mike

--

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[2]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.met.psu.edu/dept/faculty/mann.htm>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Multi-model SST detection results
Date: Wed, 02 May 2007 08:10:38 -0700
Reply-to: santer1@llnl.gov
Cc: Nathan Gillett <n.gillett@uea.ac.uk>, peter gleckler <gleckler1@llnl.gov>, i.harris@uea.ac.uk

<x-flowed>

Dear Phil,

Thanks very much for the quick reply. It would be nice to get hold of CRU TS 3.0, even at the 0.5 x 0.5 degree resolution.

For the SST detection and attribution analysis that I described yesterday, I reduced the spatial dimensionality (to get better estimates of covariance matrices, EOFs, etc.) by regridding all model and observational SST data to a common 10 x 10 lat/long grid. I think it would make sense to do the detection and attribution analysis involving the land 2m temperature changes at the same 10 x 10 resolution. So it isn't essential for me to get the CRU TS 3.0 data at 5 x 5 resolution - we might as well have just one regridding step (from 0.5 x 0.5 to 10 x 10) rather than two. As in the SST case, the primary focus would be on land 2m temperature changes over 1950 to 2006. I'm hopeful that the changing coverage/variance issues won't be that severe over this period.

Let me back up a little and outline why I want to look at CRU TS 3.0.

I've always thought that it would be fun to contrast the S/N behavior of SST and land 2m temperature. Based purely on the amplitude of unforced variability, one might expect S/N ratios to be more more favorable for SST changes than for land 2m temperature changes. But it's not that simple! Due to land/ocean differences in specific and total heat capacity, we expect the GHG-induced surface temperature signal to be larger over land than over oceans. And then there's the issue of the spatial heterogeneity of the forcings. Arguably, anthropogenic forcings over land are more spatially heterogeneous than over oceans (e.g., no changes in land surface properties over oceans!). Such land/ocean forcing differences must also influence the S/N behavior of temperature changes over land and oceans.

So I suspect, based on S/N arguments, that it's better to search for an anthropogenic surface temperature signal over the oceans rather than the

land. Actually showing this might be useful.

Cheers,

Ben

Phil Jones wrote:

>

> Ben,

> CRU doesn't have an infilled land database at the 5 by 5 degree
> resolution.

> We do at the 0.5 by 0.5 degree resolution though. It would take a
> bit of work to average these together to the coarser resolution, but it
> ought to be possible.

> We have a new version of this (CRU TS 3.0) that Ian Harris (Harry)
> is finishing off. It runs from 1900 to 2006. It doesn't take care of
> variance issues, so will have problems when in regions with poor data
> earlier in the 20th century. Should be OK though from 1950, if you
> want to start then.

> Harry is i.harris@uea.ac.uk. I think the temperature is finished, but
> Nathan could check. I'm away now till the HC meeting in Sweden
> and Spain.

> Another option is to use the infilled 5 by 5 dataset that Tom Smith
> has put together at NCDC. All infilling has the problem that when there
> is little data it tends to revert to the 1961-90 average of zero. All
> infilling techniques do this - alluded to countless times by Kevin
> Trenberth and this is in Ch 3 of AR4. This infilling is in the current
> monitoring version of NCDC's product. The infilling is partly the reason
> they got 2005 so warm, by extrapolating across the Arctic from the
> coastal stations. I think NCDC and the HC regard the permanent
> sea ice as 'land', as it effectively is.

> As a side issue, the disappearance of sea ice in the Arctic is going
> to cause loads of problems monitoring temps there as when SST data
> have come in from the areas that have been mostly sea ice, it is always
> warm as the 61-90 means are close to -1.8C. Been talking to Nick
> Rayner about this. It isn't serious yet, but it's getting to be a problem.
> In the AR4 chapter, we had to exclude the SST from the Arctic plot
> as the Arctic (north of 65N) from 1950 was above the 61-90 average
> for most of the years that had enough data to estimate a value.

>

> See you in Exeter in a week's time.

>

> Cheers

> Phil

>
>
>

> At 01:40 02/05/2007, Ben Santer wrote:

>> Dear Nathan,

>>
>> I'm now in the process of transferring SST data from the AR4
>> pre-industrial control runs. I'm hoping that the data transfer will be
>> finished by tomorrow. As described in the Supporting Text of our PNAS
>> water vapor paper, I've changed the time model of all control runs.
>> The time model is the same as in the 20c3m runs - i.e., "months since
>> 1800". This slightly complicates life if you want to subtract a
>> model's instantaneous control run drift from its 20c3m run. You then
>> have to figure out the time (in the new "months since 1800" time
>> model) at which the 20c3m run was spawned from the pre-industrial
>> control. I find, however, that the advantages of using a uniform time
>> model far outweigh the disadvantages.

>>
>> With some help from Peter, I managed to obtain some preliminary
>> results for the detection of an anthropogenic fingerprint in observed
>> SST data. To my knowledge, most formal pattern-based D&A work that has
>> dealt with temperature changes close to Earth's surface has used
>> combined SSTs and land 2m temperatures. I'm not aware of any
>> pattern-based work (other than your work with SST changes in the
>> Atlantic and Pacific tropical cyclogenesis regions) that has focused
>> on SST changes alone. I'm assuming that the dearth of "SST only"
>> fingerprint work arises in part from pesky masking and regriding
>> problems (the same problems we had to address in the PNAS water vapor
>> paper).

>>
>> As I mentioned several days ago, I essentially replicated all of the
>> data "pre-processing" we had done for the water vapor paper: i.e., the
>> same procedures were used for masking and regriding SST data to a
>> uniform 10 x 10 lat/long grid, calculation of the V and No-V SST
>> fingerprints, and concatenation of SST data from the V and No-V
>> control runs. I also employed the same spatial domain that we used for
>> the PW analysis (all oceans, 50N-50S).

>>
>> One of the choices I have to make in estimating detection time is the
>> selection of a "start date" for calculation of trends in the signal
>> time series $Z(t)$ and $Z^*(t)$ (the projections of the observed data onto
>> the raw and optimized fingerprints, respectively). For the water vapor
>> paper, the start date was dictated by the start date of the SSM/I PW

>> data (1988). Here, however, we are using NOAA ERSST data, which are
>> available from 1880 onwards. I chose a start date in 1950. I think
>> this is a defensible choice, partly because the spatial coverage of
>> SST data is more stable over time in the second half of the 20th
>> century than in the first. Furthermore, a 1950 start date is a
>> somewhat conservative choice in view of the "flattening" of the
>> observed global-scale SST increase in the 1960s and 1970s. A start
>> date in the mid-1970s would probably yield shorter detection times.

>>
>> The detection time results are encouraging. In the "spatial mean
>> included" case, we invariably obtain robust detection of the V and
>> No-V model fingerprints in the NOAA ERSST data. As you pointed out
>> previously, Nathan, the fingerprint estimated from the No-V 20c3m runs
>> is basically an "ANTHRO-ONLY" fingerprint. For a 1950 start date, the
>> detection times are all with +/- 5 years of 1980, irrespective of
>> whether the V or No-V models are used to estimate fingerprints,
>> optimize fingerprints, or assess statistical significance. This means
>> that, if we had begun monitoring observed SST changes in 1950, we
>> would have been able to identify an anthropogenic fingerprint roughly
>> 30 years later. I should point out that (as in the vapor paper), we've
>> tried to be conservative in our significance testing procedure, and
>> have intentionally retained residual control run drift.

>>
>> Results are more ambiguous in the "spatial mean removed" case. In that
>> setting, whether we can or cannot detect an anthropogenic fingerprint
>> is much more sensitive to V/No-V dataset choices. Why might that be? A
>> preliminary hypothesis is that in the "mean removed" case, greater
>> attention is focused on differential SST changes in the western and
>> eastern Pacific. The recent GRL paper by Soden and Vecchia provides
>> some model-based evidence that such differential SST changes may be
>> forced, and are accompanied by changes in the Walker circulation. I
>> suspect that these differential west/east SST changes may evolve in a
>> complex way over time, and that in the "mean removed" case, we might
>> have more luck detecting an "ANTHRO" fingerprint if go to full
>> space-time optimal detection. But that's only a guess on my part, and
>> my intuition has often been wrong!

>>
>> In the next few days, I'll fool around with several different "start
>> dates", and will also start looking at the spatial patterns of the raw
>> and optimized fingerprints, the dominant noise modes, etc. As I
>> mentioned previously, it would be nice to contrast the "SST-only" D&A
>> results with "land-only" D&A results. Does CRU have "land-only"
>> temperature data in which missing land 2m temperatures have been

>> statistically infilled? In other words, is there a land 2m temperature
>> counterpart to the HadISST product? (I've copied this email to Phil,
>> who I'm sure will be able to answer my last question.)

>>
>> Anyway, looks like this work is worth pursuing. It will be very
>> interesting to compare your space-time results with the results we've
>> obtained thus far.

>>
>> With best regards,

>>
>> Ben

>> -----

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From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: More Rubbish
Date: Thu, 17 May 2007 11:46:30 -0400
Reply-to: mann@psu.edu

<x-flowed>

yep, I'm watching the changing of the guard live on TV here!

New Scientist was good. Gavin and I both had some input into that. They are nicely dismissive of the contrarians on just about every point, including the HS!

Heard anything back from IUGG yet? I thought Mike's email was helpful, if that doesn't do the trick I don't know what will,

mike

Phil Jones wrote:

>

> Mike,

>

> Apparently there is a lot in New Scientist this week. As usual
> our copy has gone walkabout!

>

> Blair is out on June 27 - Gordon Brown then !

>

> Phil

>

>

> At 16:33 17/05/2007, you wrote:

>> as I was looking at this, I had CNN on in the background. Live

>> conference, with Bush and Blair both agreeing about the importance of

>> significantly cutting greenhouse gas emissions.

>>

>> jokes like Carter have become completely irrelevant. they are a sad

>> anachronism...

>>

>> mike

>>

>> Phil Jones wrote:

>>>

>>>> Just in case you've not seen it. Another piece of bad science.

>>>>

>>>> It is the same old stuff, so not worth doing anything at Real

>>>> Climate,

>>>> but might be worth doing something on Figure 5.

>>>>

>>>> Cheers

>>>> Phil

>>>>

>>>>

>>>>

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Subject: RE: Invitation to review IPCC Technical Paper on Climate Change and Water

Date: Mon, 21 May 2007 12:45:15 +0100

Dear colleague,

Please find attached the spreadsheet needed for submitting your review comments on the IPCC

Technical Paper on Climate Change and Water. This was accidentally omitted from the email

below.

The Technical Paper and supporting review documents are also available online at:

[1]www.ipcc-wg2.org/review/index.html

username: GEreview

password: water08

Regards,

Paul

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At 12:00 2007.05.21, you wrote:

Dear colleague,
The First-Order Draft of the IPCC Technical Paper on Water
The IPCC requested the preparation of a Technical Paper on Water, to be based primarily on the results of the Fourth Assessment (AR4), and to involve all three Working Groups.
Organization of the process is in the charge of Working Group II.
The Expert Review for the First-Order Draft of the Technical Paper on Water will begin on May 21st, and will run for four weeks until June 17th. It is essential for the success of the process that we involve the widest community of internationally-recognized researchers in the review. We have identified you as someone whose reputation and contribution to the science is such that your participation is important. Therefore, we are sending you a First-Order Draft, with a request to review the Technical Paper. We would be most grateful if you can find time from your busy

schedule to review the Technical Paper. If you can only find time to review those

sections that are most close to your professional interests, we would still be pleased

to receive your comments, although of course we also need reviews which take a broader view of the coherence and completeness of the document as a whole.

We attach the following:

1. The draft Technical Paper on Water. This is in PDF format, because it is important to preserve the page and line numbers.

2. Background information on the Technical Paper, in the form of a Scoping Note.

3. A spreadsheet for you to use to make your comments. Instructions on how to use this spreadsheet are provided at the beginning.

The deadline for the submission of review comments is June 17th. Comments should be

submitted, using the spreadsheet, to [2]ipcc-wg2@metoffice.gov.uk .

Please note that, if you have been nominated by your government for any role in the

Fourth Assessment other than Review Editor, you may receive a separate invitation from

us inviting you to be an Expert Reviewer in that capacity.

We do hope that you will be able to find time to comment on this draft. In advance, we

would like to express our deep gratitude for any contribution you can make. In

recognition of the importance of the reviewing process, reviewers' names will be listed

in the final published Technical Paper.

Yours sincerely,

Oswaldo Canziani

Co-Chair, Working Group II

Martin Parry

Co-Chair, Working Group II

Jean Palutikof

Head, WGII TSU

<<Useful Information for Review.pdf>> <<IPCC_TP_Water.pdf>>

<<Invitation letter for

expert reviewers.pdf>>

Attachment Converted: "c:\documents and settings\tim osborn\my documents\eudora\attach\rev.xls"

References

1. <http://www.ipcc-wg2.org/review/index.html>
2. <mailto:ipccwg2@metoffice.gov.uk>

From: Gavin Schmidt <gschmidt@giss.nasa.gov>
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Cc: mann@psu.edu, Caspar Ammann <ammann@ucar.edu>

<x-flowed>

Hi Phil, sorry for the long delay. But here is a first draft of the forcings and models section I was supposed to take the lead on. Hopefully, we can merge that with whatever Caspar has.

Thanks

Gavin

=====

4 Forcing (GS/CA/EZ) 4-5pp

Histories (CA)

How models see the forcings, especially wrt aerosols/ozone and increasing model complexities (GS)

An important reason for improving climate reconstructions of the past few millenia is that these reconstructions can help us both evaluate climate model responses and sharpen our understanding of important mechanisms and feedbacks. Therefore, a parallel task to improving climate reconstructions is to assess and independently constrain forcings on the climate system over that period.

Forcings can generically be described as external effects on a specific system. Responses within that system that also themselves have an impact on its internal state are described as feebacks. For the atmosphere, sea surface temperature changes could therefore be considered a forcing, but in a coupled ocean-atmosphere model they could be a feedback to another external factor or be intrinsic to the coupled system. Thus the distinction between forcings and feedbacks is not defined a priori, but is a function of the scope of the modelled system. This becomes especially important when dealing with the bio-geo-chemical processes in climate that effect the

trace gas concentrations (CO₂ and CH₄) or aerosols. For example, if a model

contains a carbon cycle, then the CO₂ variations as a function of climate will be a feedback, but for a simpler physical model, CO₂ is often imposed directly as a forcing from observations, regardless of whether in the real world it was a feedback to another change, or a result of human industrial activity.

It is useful to consider the pre-industrial period (pre-1850 or so) separately from the more recent past, since the human influence on many aspects of atmospheric composition has increased dramatically in the 20th Century. In particular, aerosol and land use changes are poorly constrained prior to the late 20th Century and have large uncertainties. Note however, there may conceivably be a role for human activities even prior to the 19th Century due to early agricultural activity (Ruddiman, 2003; Goosse et al, 2005).

In pre-industrial periods, forcings can be usefully separated into purely external changes (variations of solar activity, volcanic eruptions, orbital variation), and those which are intrinsic to the Earth system (greenhouse gases, aerosols, vegetation etc.). Those changes in Earth system elements will occur predominantly as feedbacks to other changes (whether externally forced or simply as a function of internal climate 'noise'). In the more recent past, the human role in affecting atmospheric composition (trace gases and aerosols) and land use have dominated over natural processes and so these changes can, to large extent, be considered external forcings as well.

Traditionally, the 'system' that is most usually implied when talking about forcings and feedbacks are the 'fast' components atmosphere-land surface-upper ocean system that, not coincidentally, corresponds to the physics contained within atmospheric general circulation models (AGCMs) coupled to a slab ocean. What is not included (and therefore considered as a forcing according to our previous definition) are 'slow' changes in vegetation, ice sheets or the carbon cycle. In the real world these features will change as a function of other climate changes, and in fact may do so on relatively 'fast' (i.e multi-decadal) timescales. Our choice then of the appropriate 'climate system' is thus slightly arbitrary and does not give a complete picture of the long term sensitivity of the real climate.

These distinctions become important because the records available for atmospheric composition do not record the distinction between feedback or forcing, they simply give, for instance, the history of CO₂ and CH₄. Depending on the modelled system, those records will either be a modelling input, or a modelling target.

While there are good records for some factors (particularly the well mixed greenhouse gases such as CO₂ and CH₄), records for others are either hopelessly incomplete (dust, vegetation) due to poor spatial or temporal resolution or non-existent (e.g. ozone). Thus estimates of the magnitude of these forcings can only be made using a model-based approach. This can be done using GCMs that include more Earth system components (interactive aerosols, chemistry, dynamic vegetation, carbon cycles etc.), but these models are still very much a work in progress and have not been used extensively for paleo-climatic purposes. Some initial attempts have been made for select feedbacks and forcings (Gerber et al, 2003; Goosse et al 2006) but a comprehensive assessment over the millennia prior to the pre-industrial does not yet exist.

Even for those forcings for which good records exist, there is a question of they are represented within the models. This is not so much of an issue for the well-mixed greenhouse gases (CO₂, N₂O, CH₄) since there is a sophisticated literature and history of including them within models (IPCC, 2001) though some aspects, such as minor short-wave absorption effects for CH₄ and N₂O are still not universally included (Collins et al, 2006). However, solar effects have been treated in quite varied ways.

The most straightforward way of including solar irradiance effects on climate is to change the solar 'constant' (preferably described as total solar irradiance - TSI). However, observations show that solar variability is highly dependent on wavelength with UV bands having about 10 times as much amplitude of change than TSI over a solar cycle (Lean, 2000). Thus including this spectral variation for all solar changes allows for a slightly different behaviour (larger solar-induced changes in the stratosphere where the UV is mostly absorbed for instance). Additionally, the changes in UV affect ozone production in both the stratosphere and troposphere, and this mechanism has been shown to affect both the total radiative forcing and dynamical responses (Haigh 1996, Shindell et al 2001; 2006). Within a chemistry climate model this effect would potentially

modify the radiative impact of the original solar forcing, but could also be included as an additional (parameterised) forcing in standard GCMs.

There is also a potential effect from the indirect effect of solar magnetic variability on the shielding of cosmic rays, which have been theorised to affect the production of cloud condensation nuclei (Dickinson, 1975). However, there have been no quantitative calculations of the magnitude of this effect (which would require a full study of the relevant aerosol and cloud microphysics), and so its impact on climate is not (yet) been included.

Large volcanic eruptions produce significant amounts of sulphur dioxide (SO₂). If this is injected into the tropical stratosphere during a particularly explosive eruption, the resulting sulphate can persist in the atmosphere for a number of years (e.g. Pinatubo in 1991). Less explosive, but more persistent eruptions (e.g. Laki in 1789??) can still affect climate though in a more regional way and for a shorter term (Oman et al, 2005). These aerosols have both a shortwave (reflective) and longwave (absorbing) impact on the radiation and their local impact on stratospheric heating can have important dynamical effects. It is therefore better to include the aerosol absorber directly in the radiative transfer code. However, in less sophisticated models, the impact of the aerosols has been parameterised as the equivalent decrease in TSI. For extreme eruptions it has been hypothesised that sulphate production might saturate the oxidative capacity of the stratosphere leaving significant amounts of residual SO₂. This gas is a greenhouse gas and would have an opposite effect to the cooling aerosols. This effect however has not yet been quantified.

Land cover changes have occurred both due to deliberate modification by humans (deforestation, imposed fire regimes, agriculture) as well as a feedback to climate change (the desertification of the Sahara ca. 5500 yrs ago). Changing vegetation in a standard model affects the seasonal cycle of albedo, the surface roughness, the impact of snow, evapotranspiration (through different rooting depths) etc. However, modelling of the yearly cycle of crops, or incorporating the effects of large scale irrigation are still very much a work in progress.

Aerosol changes over the last few millennia are very poorly constrained (if at all). These might have arisen from climatically or human driven changes in dust emissions, ocean biology feedbacks on circulation change,

or climate impacts on the emission volatile organics from plants (which also have an impact on ozone chemistry). Some work on modelling a subset of those effects has been done for the last glacial maximum or the 8.2 kyr event (LeGrande et al, 2006), but there have been no quantitative estimates for the late Holocene (prior to the industrial period).

Due to the relative expense of doing millennial simulations with state-of-the-art GCMs, existing simulations have generally done the minimum required to include relevant solar, GHG and volcanic forcings. Progress can be expected relatively soon on more sophisticated treatments of those forcings and the first quantitative estimates of additional effects.

=====

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| 2880 Broadway |
| Tel: (212) 678 5627 New York, NY 10025 |
| |
| gschmidt@giss.nasa.gov <http://www.giss.nasa.gov/~gavin> |

</x-flowed>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Past Millennia Climate Variability - Review Paper - reminder
Date: Wed, 30 May 2007 10:49:34 -0400
Reply-to: mann@psu.edu

Hi Phil,

Off travelling again, will check in when I return next week on status of Perugia (arggh!). Papers is looking good. I've attached draft of Mann et al (2007) which should have the references you're looking for. Please don't distribute, we'd like to wait until galleys are available to begin distributing the paper.

One small thing, this statement at end of 1st paragraph on page 18 in the draft didn't seem appropriate:

The question of whether the proxies used by MBH98 were themselves subject to amplitude limitations is not the focus of this section, and is examined in Section 2 above.

These issues are implicit in section 2, but have nothing to do w/ MBH98 specifically. As written this is misleading/confusing, and I don't think it adds anything.

Phil Jones wrote:

Dear All,

There has been some progress. I have contributions from Gene and Gavin. Keith (2.3) and Tim (3) here in CRU tell me they are working on their parts. Francis (5) also tells me he has also started. Tas told me about 6 weeks ago he would finish the ice core part (section 2.3) shortly.

So we are getting there. I still need input from Caspar (section 4), Nick (section 2.6), Peck (section 2.5). I have added in the section names of the missing sections to help you all along.

Also need people to begin reading through the whole paper, but this is premature yet.

I saw Thorsten at the EGU and he emailed recently saying that Larry (EPRI) is keen to see this submitted soon. Remember it was through PAGES and EPRI support that we had such a great few days in Wengen almost a year ago!

If we all put some effort in over June we could be there.

Can Gene and Gavin send me their references when they have a few minutes. I suspect most will be in Mann et al. (2007), so if I can get that I can add them in. I won't pass this on to any others.

Cheers

Phil

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich Email [1]p.jones@uea.ac.uk
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Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [2]mann@psu.edu
University Park, PA 16802-5013

[3]<http://www.met.psu.edu/dept/faculty/mann.htm>

Attachment Converted: "c:\eudora\attach\MRWAJGR06-revisedfinal.doc"

References

1. <mailto:p.jones@uea.ac.uk>
2. <mailto:mann@psu.edu>
3. <http://www.met.psu.edu/dept/faculty/mann.htm>

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To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Past Millennia Climate Variability - Review Paper - reminder
Date: Wed, 30 May 2007 11:36:16 -0400
Reply-to: mann@psu.edu

thanks Phil,

yeah, I figured we might as well wait until all contributions have been received before going over the full text and making necessary revisions...

off to Oregon now. talk to you later,

mike

Phil Jones wrote:

Mike,

Thanks for the paper. Gene wrote that bit. I'll flag it for modifying at my next draft - when I get a chance to add the refs in. Likely the weekend. May have got some other responses by then.

Cheers

Phil

At 15:49 30/05/2007, you wrote:

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Cheers

Phil

Prof. Phil Jones

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--

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[3]

<http://www.met.psu.edu/dept/faculty/mann.htm>

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References

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4. <mailto:p.jones@uea.ac.uk>
5. <mailto:mann@psu.edu>
6. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Humphrey, Kathryn (CESA)" <kathryn.humphrey@DEFRA.GSI.GOV.UK>
Subject: Fwd: RE: Outstanding comms plan issues
Date: Mon, 18 Jun 2007 11:10:59 +0100
Cc: "Roger Street" <roger.street@ukcip.org.uk>, "Clare Goodess" <C.Goodess@uea.ac.uk>, <david.sexton@metoffice.gov.uk>, "Winter, Guy (SEERAD)" <Guy.Winter@scotland.gsi.gov.uk>, "Vicky Pope" <vicky.pope@metoffice.gov.uk>, "Steven Wilson" <stwi@nerc.ac.uk>, "Sear, Chris (CESA)" <chris.sear@DEFRA.GSI.GOV.UK>, "Rob Wilby" <rob.wilby@environment-agency.gov.uk>, "Rachel Warren" <r.warren@uea.ac.uk>, "Prosser, Havard (WAG-EPC)" <Havard.Prosser@Wales.GSI.Gov.UK>, "Phil Newton" <ppn@nerc.ac.uk>, "Phil Jones" <p.jones@uea.ac.uk>, "Phil James" <philip.james@ncl.ac.uk>, "Marguerite Gascoine" <m.b.gascoine@reading.ac.uk>, "Linda Livingston" <linda.livingston@metoffice.gov.uk>, "Geoff Jenkins" <geoff.jenkins@metoffice.gov.uk>, "geoff jenkins at home" <geoff.jenkins@ic24.net>, "David Sexton" <david.sexton@metoffice.gov.uk>, "Chris Kilsby" <C.G.Kilsby@newcastle.ac.uk>, "Butt, Adrian (CESA)" <adrian.butt@DEFRA.GSI.GOV.UK>, "Bryan Lawrence" <b.n.lawrence@rl.ac.uk>, "Brian Hoskins" <b.j.hoskins@reading.ac.uk>, "Barry McAuley" <barry.mcauley@doeni.gsi.gov.uk>, "Ag Stephens" <A.Stephens@rl.ac.uk>

<x-flowed>

Kathryn,

Made some slight mods to the WG definition. Maybe Chris should check

this and then we'll be there on this definition.

Cheers
Phil

>X-VirusChecked: Checked
>X-Env-Sender: kathryn.humphrey@DEFRA.GSI.GOV.UK
>X-Msg-Ref: server-13.tower-67.messagelabs.com!1182153653!16925857!1
>X-StarScan-Version: 5.5.12.11; banners=-,-,-
>X-Originating-IP: [195.92.40.48]
>X-IronPort-AV: E=Sophos;i="4.16,434,1175468400";
> d="doc'32?scan'32,208,32";a="3997439"
>Subject: RE: Outstanding comms plan issues
>Date: Mon, 18 Jun 2007 09:00:44 +0100
>X-MS-Has-Attach: yes
>X-MS-TNEF-Correlator:
>Thread-Topic: Outstanding comms plan issues
>Thread-Index: AcewxUEWmbycgv6dRPW5zHVRv1IoJQAuHs8g
>From: "Humphrey, Kathryn (CESA)" <kathryn.humphrey@DEFRA.GSI.GOV.UK>
>To:

>X-OriginalArrivalTime: 18 Jun 2007 08:02:06.0823 (UTC)

>FILETIME=[F6D0E770:01C7B17E]

>X-UEA-Spam-Score: 0.0

>X-UEA-Spam-Level: /

>X-UEA-Spam-Flag: NO

>

>I'm very happy to send this to the users' panel for recommendation to
>the SG, if those suggested below (Geoff, David S, Roger, Chris K, Phil
>Jones) are happy to work up definitions based on the latest version we
>have, attached.

>

>Kathryn

>

>PS congratulations on your Gong, Brian!

>

>-----Original Message-----

>From: Roger Street [mailto:roger.street@ukcip.org.uk]

>Sent: 17 June 2007 10:51

>To: Clare Goodess; Humphrey, Kathryn (CESA);

>david.sexton@metoffice.gov.uk

>Cc: Winter, Guy (SEERAD); Vicky Pope; Steven Wilson; Sear, Chris (CESA);

>Rob Wilby; Rachel Warren; Prosser, Havard (WAG-EPC); Phil Newton; Phil

>Jones; Phil James; Marguerite Gascoine; Linda Livingston; Geoff Jenkins;

>geoff.jenkins at home; David Sexton; Chris Kilsby; Butt, Adrian (CESA);

>Bryan Lawrence; Brian Hoskins; Barry McAuley; Ag Stephens

>Subject: Re: Outstanding comms plan issues

>

>With respect to the changes suggested by Clare (green inserts within the

>

>text) I am comfortable with the suggested changes. I am, however,

>somewhat

>concerned with the definition for weather generator but this relates to

>a

>personal perception and my concerns as to how this would be interpreted

>by

>users. I would prefer not suggesting that the weather generator

>generates

>weather data but that it generates weather variables at the daily and

>sub-daily level consistent with the projected climate. As such, I would

>

>prefer something along the lines of the following definition:

>

>Weather generators are statistically-based computer programs that use

>existing weather records and random number sampling to produce long

>timeseries of synthetic daily and sub-daily variables. The statistical

>properties of the generated weather-like variables are expect to be

>similar

>to those of the existing weather record. The UKCIP08 weather generator

>bases its daily and sub-daily variables for future time periods on the

>statistical nature of the PDF data chosen to drive it. The variables

>generated are those required by many applications: precipitation,

>maximum

>and minimum temperature, rainfall, solar radiation and wind speed, as

>well

>as measures of atmospheric water vapour and evapotranspiration.

>

>In terms of the definitions for scenarios and projections, those

>ascribed to

>me are actually those developed through the deliberations within Chapter
>2
>of the IPCC WGII for which Tim Carter was one of the Lead Authors. My
>understanding after talking with Tim was that these definitions, which
>are
>the result of considerable discussion within the IPCC impacts,
>vulnerability
>and adaptation community, will be included with the WGII publication. I
>suggest that the definitions to be included and used within UKCIP08 do
>need
>further consideration to ensure that they are clearly identifying what
>UKCIP08 will be delivering - probabilistic projections and scenarios.
>The
>definitions within UKCIP08 should be informed not constrained by the
>IPCC
>deliberations and should be directed at informing the user community
>(client
>focused).
>
>I also agree with Clare that we should be providing a definition of what
>is
>meant by probabilistic within the context of UKCIP08.
>
>In terms of a way forward, would it be reasonable to ask the following
>to
>develop for the specified terms definitions for approval by the SG
>(after
>seeking views of the Users' Panel):
>MOHC - baseline period, climate, climate change, climate model,
>deterministic, and probability/probabilistic density function;
>Newcastle - weather generator; and
>UKCIP - scenarios and projections.
>
>These could be done over the next couple of weeks with a single request
>for
>views going out to the Users' Panel in July.
>
>Roger
>
>
>----- Original Message -----
>From: "Clare Goodess" <C.Goodess@uea.ac.uk>
>To: <david.sexton@metoffice.gov.uk>; "Humphrey, Kathryn (GA)"
><kathryn.humphrey@DEFRA.GSI.GOV.UK>
>Cc: "Roger Street" <roger.street@ukcip.org.uk>; "Ag Stephens"
><A.Stephens@rl.ac.uk>; "Barry McAuley" <barry.mcauley@doeni.gsi.gov.uk>;
>
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>"David Sexton" <david.sexton@metoffice.gov.uk>; "geoff jenkins at home"
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><geoff.jenkins@metoffice.gov.uk>;

>"Linda Livingston" <linda.livingston@metoffice.gov.uk>; "Marguerite
>Gascoine" <m.b.gascoine@reading.ac.uk>; "Phil James"
><philip.james@ncl.ac.uk>; "Phil Jones" <p.jones@uea.ac.uk>; "Phil
>Newton"
><ppn@nerc.ac.uk>; "Prosser, Havard (WAG-EPC)"
><Havard.Prosser@Wales.GSI.Gov.UK>; "Rachel Warren" <r.warren@uea.ac.uk>;
>
>"Rob Wilby" <rob.wilby@environment-agency.gov.uk>; "Sear, Chris (CESA)"
><chris.sear@DEFRA.GSI.GOV.UK>; "Steven Wilson" <stwi@nerc.ac.uk>; "Vicky"
>
>Pope" <vicky.pope@metoffice.gov.uk>; "Winter, Guy (SEERAD)"
><Guy.Winter@scotland.gsi.gov.uk>
>Sent: Friday, June 15, 2007 6:59 PM
>Subject: RE: Outstanding comms plan issues
>
>

> > Dear all

> >

> > I was looking at this glossary on the train yesterday and have a few
> > relatively minor comments on some of the entries - added in green to
> > Kathryn's latest draft.

> >

> > But I found the definitions of projections and scenarios very
> > confusing, with problems in both the IPCC and Roger's wording which I
> > couldn't think how to resolve - so it was interesting to see this
> > email discussion. There do seem to be some fundamental differences
> > and still confusion, so I'm afraid that some more discussion is
> > needed (sorry Kathryn!).

> >

> > We agreed at the last meeting to add deterministic - and following
> > this logic through, I think that we should also have added
> > probabilistic.

> >

> > According to the key messages, UKCIP08 will be providing
> > 'probabilistic projections'. It therefore seems rather confusing to
> > read that 'projections are generally less comprehensive than
> > scenarios'. This implies to the user that the UKCIP08 probabilistic
> > projections are less comprehensive than the UKCIP02 scenarios. Which
> > is not the intended message - though it depends what you mean by
> > 'less comprehensive'.

> >

> > Over the last few months, I have been persuaded (by discussions with
> > people like Tim Carter) that we should avoid talking about
> > 'probabilistic scenarios'.

> >

> > I agree with David that it makes no sense to say that scenarios
> > include projections - when our definition of the latter includes
> > uncertainties/probabilities. Perhaps the solution is to make a clear
> > distinction between 'projections' - which can be deterministic or
> > probabilistic - and 'probabilistic projections'.

> >

> > At least we all seem agreed on not using 'prediction'!

> >

> > I hope that this has not further muddied the waters, best wishes,

>Clare
> >
> >
> >
> > At 15:23 14/06/2007, david.sexton@metoffice.gov.uk wrote:
> >>Hi,
> >>
> >>I am off for a week and half now and have a few things to sort out
>here
> >>so I won't be able to give you any text for PDFs. I think that might
>be
> >>best left until the report is written because it depends a lot on
what
> >>the report writers think. Other comments in the text...
> >>
> >>On Thu, 2007-06-14 at 11:03 +0100, Humphrey, Kathryn (CESA) wrote:
> >> > All,
> >> >
> >> > You seem to have all more or less agreed on the key messages which
>is
> >> > great. However, the glossary is continuing to bring up a range of
> >> > divergent views!
> >> >
> >> > I've had more comments and have got amended definitions in the
> >> > attached. David and Chris, who couldn't make last week's meeting,
> >> > have questioned the use of the AR4 definitions (Chris- too
>technical
> >> > for the layperson, see comments in the attached) and the
> >> > projections/scenarios definition (David- not in agreement with
MOHC
> >> > definitions). David, I am keen not to open up the debate again on
>the
> >> > differences between scenarios, projections and predictions (the
>latter
> >> > of which we're not using at all) as we've already had an
>astonishingly
> >> > long conversation on this one and I thought had come to agreement.
> >>
> >>>For the time being I think we should remove any reference to "climate
> >>>predictions" in the AR4 definition of projections because we haven't
>got
> >>>a glossary term for "climate prediction". So "...climate models.
>Climate
> >>>projections depend upon the emission/conce..." would be better.
> >>
> >>
> >>
> >> > However if you can find support from the rest of the SG then I'll
> >> > open this one up again; otherwise, I'd like to stick with the
> >> > definitions we have which are consistent with the AR4 WG2 ones,
> >> > defining projections as the bit that includes uncertainty and
> >> > scenarios not.
> >>
> >>>I must be missing something here but where does AR4 say "projections

> >> > deterministic pls let me have it as the AR4 doesn't give one.
>You'll
> >> > also want to check the other definitions as I've either cut them
>down
> >> > from those presented in the AR4, or added sections to make them
> >> > UKCIP08 specific. Also the only definition I can find of a
weather
> >> > generator is very old!
> >> >
> >> > Comments back to me by close Friday would be v helpful.
> >> >
> >> > Kathryn
> >> >
> >> > <<2007-06-13 comms plan Key Messages and glossary.doc>>
> >> >
> >> >
> >> >
> >> > Department for Environment, Food and Rural Affairs (Defra)
> >> >
> >> > This email and any attachments is intended for the named recipient
> >> > only.
> >> > If you have received it in error you have no authority to use,
> >> > disclose,
> >> > store or copy any of its contents and you should destroy it and
>inform
> >> > the sender.
> >> > Whilst this email and associated attachments will have been
checked
> >> > for known viruses whilst within Defra systems we can accept no
> >> > responsibility once it has left our systems.
> >> > Communications on Defra's computer systems may be monitored and/or
> >> > recorded to secure the effective operation of the system and for
>other
> >> > lawful purposes.
> >> > email message attachment
> >> > On Thu, 2007-06-14 at 11:03 +0100, Humphrey, Kathryn (CESA) wrote:
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> >> > > <barry.mcauley@doeni.gsi.gov.uk>, Brian Hoskins
> >> > > <b.j.hoskins@reading.ac.uk>, Bryan Lawrence
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><C.Goodess@uea.ac.uk>,
> >> > > Chris Kilsby <C.G.Kilsby@newcastle.ac.uk>, David Sexton
> >> > > <david.sexton@metoffice.gov.uk>, geoff jenkins at home
> >> > > <geoff.jenkins@ic24.net>, Geoff Jenkins
> >> > > <geoff.jenkins@metoffice.gov.uk>, Linda Livingston
> >> > > <linda.livingston@metoffice.gov.uk>, Marguerite Gascoine
> >> > > <m.b.gascoine@reading.ac.uk>, Phil James
><philip.james@ncl.ac.uk>,
> >> > > Phil Jones <p.jones@uea.ac.uk>, Phil Newton <ppn@nerc.ac.uk>,
> >> > > "Prosser, Havard (WAG-EPC)" <Havard.Prosser@Wales.GSI.Gov.UK>,
> >> > > Rachel Warren <r.warren@uea.ac.uk>, Rob Wilby
> >> > > <rob.wilby@environment-agency.gov.uk>, Roger Street
> >> > > <roger.street@ukcip.org.uk>, "Sear, Chris (CESA)"

> >> > > <chris.sear@DEFRA.GSI.GOV.UK>, Steven Wilson <stwi@nerc.ac.uk>,
> >> > > Vicky Pope <vicky.pope@metoffice.gov.uk>, "Winter, Guy (SEERAD)"
> >> > > <Guy.Winter@scotland.gsi.gov.uk>, "Murphy, James"
> >> > > <james.murphy@metoffice.gov.uk>
> >> > > In-Reply-To:
> >> > >
><65D9B941E291E141821FEC1AB608D203210AC9@SAMC2V1T.DEMETER.ZEUS.GSI.GOV.UK
> >
> >> > > References:
> >> > >
> >> > >
><65D9B941E291E141821FEC1AB608D203210AC9@SAMC2V1T.DEMETER.ZEUS.GSI.GOV.UK
> >
> >> > > Content-Type: text/plain
> >> > > Date: Thu, 14 Jun 2007 10:05:52 +0100
> >> > > Message-Id:
> >> > > <1181811953.5610.55.camel@eld432.desktop.frd.metoffice.com>
> >> > > Mime-Version: 1.0
> >> > > X-Mailer: Evolution 2.0.2 (2.0.2-27.rhel4.6)
> >> > > Content-Transfer-Encoding: 7bit
> >> > > X-OriginalArrivalTime: 14 Jun 2007 09:05:53.0499 (UTC) FILETIME=
> >> > > [360A52B0:01C7AE63]
> >> > > Return-Path: david.sexton@metoffice.gov.uk
> >> > >
> >> > > Hi,
> >> > >
> >> > > here are some quick comments. I probably made some similar ones
> >> > > a
> >> > > while
> >> > > back.
> >> > >
> >> > > General comment on glossary:
> >> > >
> >> > >
> >> > > A general comment is that I can see the point of having a
> >> > > glossary
> >> > > early
> >> > > on so that terms are consistent across different communications.
> >> > > But
> >> > > I
> >> > > really feel that a lot of these are scientific and that they
> >> > > need
> >> > > to
> >> > > be
> >> > > correct for the report and consistent with the ideas of the
> >> > > report
> >> > > writers (Geoff and James and to a lesser extent me, Phil and
> >> > > Chris
> >> > > and
> >> > > Stephen Dye). These ideas will develop as the report is written
> >> > > so I
> >> > > don't think it helps the report writers to set in stone these
> >> > > terms.
> >> > >

> >> > > Also, I think the glossary has several inconsistencies in it
>which
> >> > > will
> >> > > cause confusion. So here are my comments:
> >> > >
> >> > > Finally, we have to be really careful with the terms
>"prediction"
> >> > > and
> >> > > "uncertainty" because both have connotations to the lay person
>which
> >> > > are
> >> > > different to the scientist - scientific predictions should
always
> >> > > have
> >> > > an estimate of uncertainty associated with them, where a
>prediction
> >> > > to a
> >> > > lay person might mean a one-off value. "Error" is another good
> >> > > example.
> >> > > I would try to avoid these terms in the glossary and the report.
> >> > >
> >> > >
> >> > > Specific comments:
> >> > >
> >> > > PROJECTIONS, SCENARIOS and "predictions":
> >> > > At MOHC we see a climate projection as some plausible climate
>that
> >> > > is an
> >> > > outcome of some inputs e.g. emission scenario. It has no
>likelihood
> >> > > assigned to it. Here, we see "climate predictions" as a set of
> >> > > projections which have been calibrated by the observations and
> >> > > therefore
> >> > > have an assigned likelihood. It seems this is more like the AR4
> >> > > definition of SCENARIO as AR4 use observed data (see AR4 defn)
>and
> >> > > therefore scenarios DO ascribe likelihoods. This seems to
>contradict
> >> > > Roger's last line on "projections" which says scenarios do not
> >> > > ascribe
> >> > > likelihoods. Also, the product has always been referred to as
the
> >> > > "UKCIP08 scenarios" and they definitely assign likelihoods. I
>also
> >> > > disagree with Roger's last sentence on "PROJECTIONS" - I'd say
> >> > > projections are not probabilistic.
> >> > >
> >> > > So a temporary suggestion would be to use the AR4 definition of
> >> > > "PROJECTION" but delete the confusing bit relating it to
> >> > > "predictions"
> >> > > which haven't been defined in the glossary i.e. delete
> >> > > "distinguished...projections".
> >> > >
> >> > >

> >> > >
> >> > > PDF: I would use "Probability Distribution Function" cos it has
> >> > > an
> >> > > element of subjective uncertainty in it. Probability Density
> >> > > functions
> >> > > are to me more analytical e.g. Gaussian, exponential. Also, the
> >> > > definition does describe what a PDF is, but it doesn't convey
how
> >> > > the
> >> > > PDF should be viewed because it doesn't convey what
"probability"
> is
> >> > > measuring. For UKCIP08, probability is measuring the degree to
> >> > > which
> >> > > future climates are consistent with the information used to
> >> > > construct
> >> > > the scenarios (climate model data, and observations) and the
> >> > > assumptions
> >> > > and methods used in constructing them i.e. they are a convenient
> >> > > summary
> >> > > statement of all that data given some assumptions, which are
more
> >> > > usable
> >> > > than the data itself in helping planners make decisions. This is
> >> > > different to the definition learnt at school where probability
of
> >> > > say
> >> > > rolling a dice can be measured by a repeated experiment. Climate
> is
> >> > > a
> >> > > one-off so there is no repeated experiment and so the schoolboy
> >> > > definition doesn't apply and this needs to be explained. A
> >> > > consequence
> >> > > of this is the PDF will change in UKCIPnext because better
> models,
> >> > > methods and more observations will change it.
> >> > >
> >> > > Deterministic: means the output (i.e. from a single run of a
> typical
> >> > > climate model) is based solely on the inputs (here the model,
its
> >> > > input
> >> > > parameter values, and the initial conditions). What word are you
> >> > > contrasting this against. It should be contrasted against
> "random"
> >> > > or
> >> > > "stochastic" where there is a random element involved that can
> >> > > change
> >> > > the system. Hopefully, this is not be contrasted against
> >> > > "probabilistic".
> >> > >
> >> > >
> >> > > Cheers, David
> >> > >

> >> > >
> >> > >
> >> > >
> >> > >
> >> > > On Wed, 2007-06-13 at 16:32 +0100, Humphrey, Kathryn (CESA)
>wrote:
> >> > > > All,
> >> > > >
> >> > > > Attached is an updated set of key messages and glossary for
>the
> >> > > > UKCIP08 comms plan.
> >> > > >
> >> > > > For the glossary, the AR4 definitions for projections and
> >> > > > scenarios
> >> > > > differ to those Roger has from the co-author of the WGII
>report.
> >> > > > Which do you want to use? Also if anyone has a better
>definition
> >> > > > of
> >> > > > deterministic pls let me have it as the AR4 doesn't give one.
> >> > > > You'll
> >> > > > also want to check the other definitions as I've either cut
>them
> >> > > > down
> >> > > > from those presented in the AR4, or added sections to make
>them
> >> > > > UKCIP08 specific. Also the only definition I can find of a
> >> > > > weather
> >> > > > generator is very old!
> >> > > >
> >> > > > Comments back to me by close Friday would be v helpful.
> >> > > >
> >> > > > Kathryn
> >> > > >
> >> > > > <<2007-06-13 comms plan Key Messages and glossary.doc>>
> >> > > >
> >> > > >
> >> > > > Department for Environment, Food and Rural Affairs (Defra)
> >> > > >
> >> > > > This email and any attachments is intended for the named
>recipient
> >> > > > only.
> >> > > > If you have received it in error you have no authority to use,
> >> > > > disclose,
> >> > > > store or copy any of its contents and you should destroy it
>and
> >> > > > inform
> >> > > > the sender.
> >> > > > Whilst this email and associated attachments will have been
> >> > > > checked
> >> > > > for known viruses whilst within Defra systems we can accept no
> >> > > > responsibility once it has left our systems.
> >> > > > Communications on Defra's computer systems may be monitored
>and/or

> >> > > recorded to secure the effective operation of the system and
>for
> >> > > other
> >> > > lawful purposes.
> >> > > --
> >> > > _____
> >> > > David Sexton PhD Climate Research Scientist
> >> > > Met Office Hadley Centre FitzRoy Road Exeter EX1 3PB UK
> >> > > Tel: +44 (0)1392 886524 Fax: +44 (0)1392 885681
> >> > > E-mail: david.sexton@metoffice.gov.uk
>http://www.metoffice.gov.uk
> >> > >
> >> > >
> >> > email message attachment
> >> > On Thu, 2007-06-14 at 11:03 +0100, Humphrey, Kathryn (CESA) wrote:
> >> > > <<2007-06-13 comms plan Key Messages and glossary.doc>> Some
>initial
> >> > > suggestions and comments
> >> > > I think UKCIP needs its own defs. AR4 too complex and
>'scientific'
> >> > > for lay users.
> >> > > Chris
> >> > >
> >>--
> >> _____
> >>David Sexton PhD Climate Research Scientist
> >>Met Office Hadley Centre FitzRoy Road Exeter EX1 3PB UK
> >>Tel: +44 (0)1392 886524 Fax: +44 (0)1392 885681
> >>E-mail: david.sexton@metoffice.gov.uk http://www.metoffice.gov.uk
> >
>
>
>-----
>-----
>
>
> > Dr Clare Goodess
> > Climatic Research Unit
> > School of Environmental Sciences
> > University of East Anglia
> > Norwich
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> > UK
> >
> > Tel: +44 -1603 592875
> > Fax: +44 -1603 507784
> > Web: http://www.cru.uea.ac.uk/
> > http://www.cru.uea.ac.uk/~clareg/clare.htm
> >
> >
> >
>
>

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
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University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

</x-flowed>

Attachment Converted: "c:\eudora\attach\2007-06-14 comms plan Key
Messages and glossary_goodess11.doc"

From: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: FW: retraction request
Date: Tue, 19 Jun 2007 08:21:57 -0400
Cc: Wei-Chyung Wang <wang@climate.cestm.albany.edu>

Thanks Phil,

We R now responding to a former TV weather forecaster who has got press, He has a web site of 40 of the USHCN stations

showing less than ideal exposure. He claims he can show urban biases and exposure biases.

We are writing a response for our Public Affairs. Not sure how it will play out.

Regards, TOM

Phil Jones said the following on 6/19/2007 4:22 AM:

Wei-Chyung and Tom,

The Climate Audit web site has a new thread on the Jones et al. (1990) paper, with lots of quotes from Keenan. So they may not be going to submit something to Albany. Well may be?!?

Just agreed to review a paper by Ren et al. for JGR. This refers to a paper on urbanization effects in China, which may be in press in J. Climate. I say 'may be' as Ren isn't that clear about this in the text, references and responses to earlier reviews. Have requested JGR get a copy a copy of this in order to do the review.

In the meantime attaching this paper by Ren et al. on urbanization at two sites in China.

Nothing much else to say except:

1. Think I've managed to persuade UEA to ignore all further FOIA requests if the people have anything to do with Climate Audit.
2. Had an email from David Jones of BMRC, Melbourne. He said they are ignoring anybody who has dealings with CA, as there are threads on it about Australian sites.
3. CA is in dispute with IPCC (Susan Solomon and Martin Manning) about the availability of the responses to reviewer's at the various stages of the AR4 drafts. They are most interested here re Ch 6 on paleo.

Cheers

Phil

At 16:48 12/06/2007, Wei-Chyung Wang wrote:

FYI. WCW

PS I am flying out to Norway this afternoon. Keep in touch.

-----Original Message-----

From: Wei-Chyung Wang [[1]mailto:wang@climate.cestm.albany.edu]
Sent: Tuesday, June 12, 2007 11:46 AM
To: [2]doug.keenan@informath.org
Cc: 'WCW'; [3]Kld@Asrc.Cestm.Albany.Edu'
Subject: RE: retraction request
Date: June 12, 2007
To: D. J. Keenan
Cc: K. Demerjian, Director, ASRC/SUNY-Albany

Dr. Keenan,

The only valid scientific issue described in your June 11, 2007 e-mailed pdf file (attached here as reference) concerning our 1990 GRL paper is the "station histories", while others are strictly your own opinions and therefore irrelevant to your inquiry. So let me elaborate further on this issue.

Digitization of the hard copies of "station histories" was prepared in 1989-90 by Ms. Zhao-Mei Zeng (IAP/CAS) only for the 60-station network, while the "station histories" of other stations, including those we used in 1990 urban warming study, were available in paper form, as I have already indicated in my 4/30/07 e-mail to you. Therefore, the use of the word "fabrication" in your document is totally absurd.

Concerning the current status of these hard copies of "station histories", Ms. Zeng told me when I was in Beijing in April 2007, that she no longer has the access to these information because it has been a long time (since 1990) and also IAP has moved office. But if you are interested, you can make an inquiry to the China Meteorological Administration using the web site:

[4]<http://211.147.16.25/ywwz/about/cma.php>.

I believe that I have made it very clear what we had done with regard to the "station histories" in 1990 urban warming study. What and how you are going to proceed from now on is entirely your decision.

WCW

Dr. Wei-Chyung Wang
Professor of Applied Sciences
Atmospheric Sciences Research Center
State University of New York
251 Fuller Road
Albany, New York 12203
Tel: 518-437-8708
Fax: 518-437-8713
E-mail: [5]wang@climate.cestm.albany.edu

-----Original Message-----

From: D.J. Keenan [[6]mailto:doug.keenan@informath.org]

Sent: Monday, June 11, 2007 8:43 AM

To: Wei-Chyung Wang

Subject: Re: retraction request

Dear Dr. Wang,

I had something urgent arise, and so had to leave this matter for a while.

Please find attached a rough draft report. If you believe the report to be inaccurate or misrepresentative, kindly let me know.

I hope that you will reconsider. If you decide to publish retractions, I will cease to bring this forward.

Sincerely,

Douglas Keenan

----- Original Message -----

From: [7]<wang@climate.cestm.albany.edu>

To: "D.J. Keenan" [8]<doug.keenan@informath.org>

Cc: "Phil Jones" [9]<p.jones@uea.ac.uk>; [10]<Thomas.R.Karl@noaa.gov>;

"Wei-Chyung Wang" [11]<wang@climate.cestm.albany.edu>; "Zeng Zhaomei"

[12]<zzm@tea.ac.cn>

Sent: Monday, 30 April, 2007 6:14

Subject: Re: retraction request

> Dr. Keenan,

>

> The discussion with Ms. Zeng last week in Beijing have re-affirmed

> that she used the hard copies of station histories to make sure that

> the selected stations for the study of urban warming in China have

> relatively few, if any, changes in instrumentation, location, or

> observation times over the study period (1954-1983).

>

> Regards,

>

> WCW

>

> -----4/22/2007 4:46 PM e-mail Wang to Keenan-----

> Dear Dr. Keenan,

>

> I was really surprised to see your e-mail (below) after I logged into

> SUNYA webmail in Nanjing/China, after several days of disconnection

> (from internet) while travelling in central China.

>

> I flew to China early morning on 4/14, the day after your call to my

> office when I was in a meeting. My understanding was that you are

> going to call me again, but you never did.

>

> In any case, because of 4/14 trip to China, I originally plan to
> respond to your 4/11 e-mailed questions when I return to Albany the
> end of this month. To answer your questions more accurately, I need
> to look into the file (if I can find it since it has been a long
> time), and also contact the co-author, Ms. Zeng, who brought the data
> and visited SUNYA as a visiting scientist from the Institute of
> Atmospheric Physics, Chinese Academy of Sciences, during that time.

>
> Regards,

>
> WCW

>
> ----- Original Message -----

> From: "D.J. Keenan" [13]<doug.keenan@informath.org>

> Date: Friday, April 20, 2007 8:31 am

> Subject: retraction request

>
>> Dear Dr. Wang,

>>
>> Regarding the Chinese meteorological data analyzed by Wang et al.
>> [GRL, 1990] and Jones et al. [Nature, 1990], it now seems clear that
>> there are severe problems. In particular, the data was obtained
>> from 84 meteorological stations that can be classified as follows.

>> 49 have no histories

>> 08 have inconsistent histories

>> 18 have substantial relocations

>> 02 have single-year relocations

>> 07 have no relocations

>> Furthermore, some of the relocations are very distant--over 20 km.

>>
>> Others are to greatly different environments, as illustrated here:

>> [14]<http://www.climateaudit.org/?p=1323#comment-102970>

>>
>> The above contradicts the published claim to have considered the
>> histories of the stations, especially for the 49 stations that have
>> no histories. Yet the claim is crucial for the research conclusions.

>>
>> I e-mailed you about this on April 11th. I also phoned you on April
>> 13th: you said that you were in a meeting and would get back to me.
>> I have received no response.

>>
>> I ask you to retract your GRL paper, in full, and to retract the
>> claims made in Nature about the Chinese data. If you do not do so, I

>> intend to publicly submit an allegation of research misconduct to
>> your university at Albany.
>>
>>
>> Douglas J. Keenan
>> [15]<http://www.informath.org>
>> phone + 44 20 7537 4122
>> The Limehouse Cut, London E14 6N, UK
>>

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email [16]p.jones@uea.ac.uk
NR4 7TJ
UK

--

Dr. Thomas R. Karl, L.H.D.

Director

NOAA's National Climatic Data Center

Veach-Baley Federal Building

151 Patton Avenue

Asheville, NC 28801-5001

Tel: (828) 271-4476

Fax: (828) 271-4246

[17]Thomas.R.Karl@noaa.gov

References

1. <mailto:wang@climate.cestm.albany.edu>

2. <mailto:doug.keenan@informath.org>
3. <mailto:Kld@Asrc.Cestm.Albany.Edu>
4. <http://211.147.16.25/ywwz/about/cma.php>
5. <mailto:wang@climate.cestm.albany.edu>
6. <mailto:doug.keenan@informath.org>
7. <mailto:wang@climate.cestm.albany.edu>
8. <mailto:doug.keenan@informath.org>
9. <mailto:p.jones@uea.ac.uk>
10. <mailto:Thomas.R.Karl@noaa.gov>
11. <mailto:wang@climate.cestm.albany.edu>
12. <mailto:zzm@tea.ac.cn>
13. <mailto:doug.keenan@informath.org>
14. <http://www.climateaudit.org/?p=1323#comment-102970>
15. <http://www.informath.org/>
16. <mailto:p.jones@uea.ac.uk>
17. <mailto:Thomas.R.Karl@noaa.gov>

From: Thomas C Peterson <Thomas.C.Peterson@noaa.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: Jones et al 1990
Date: Wed, 20 Jun 2007 08:27:50 -0400

Fascinating. Thanks for keeping me in the loop, Phil. I won't pass it on but I will keep it in the back of my mind when/if Russ asks about appropriate responses to CA requests. Russ' view is that you can never satisfy them so why bother to try?

It seems to me that what they are saying is the equivalent of accusing a doctor of malpractice for not seeing a broken bone in a Chinese x-ray taken in 1985 when the break is clearly visible in a state of the art 2005 Canadian MRI scan examined while wearing their special problem finding glasses.

They also don't seem to understand the collaborative nature of the work, equivalent to accusing you of faulty reading of metadata at the USHCN station in Reno because you quoted a general USHCN statement that wasn't fully applicable to Reno.

Good luck.

Tom

Phil Jones said the following on 6/20/2007 3:59 AM:

Tom P.

Just for interest. Don't pass on.

Might be a precedent for your paper to J. Climate when it comes out.

There are a few interesting comments on the CA web site.

One says it is up to me to prove the paper from 1990 was correct, not for Keenan to prove we're wrong. Interesting logic.

Cheers

Phil

Wei-Chyung, Tom,

I won't be replying to either of the emails below, nor to any of the accusations on the Climate Audit website.

I've sent them on to someone here at UEA to see if we should be discussing anything with our legal staff.

The second letter seems an attempt to be nice to me, and somehow split up the original author team.

I do now wish I'd never sent them the data after their FOIA request!

Cheers

Phil

X-YMail-OSG: wrT8WAEVM1myBGklj9hAiLvnYW9GqqFcbArMYvXDn17EHo1e0Vf5eSQ4WIGJljnsEw--

From: "Steve McIntyre" [1]<stephen.mcintyre@utoronto.ca>

To: "Phil Jones" [2]<p.jones@uea.ac.uk>

Subject: Jones et al 1990

Date: Tue, 19 Jun 2007 13:44:58 -0400

X-Mailer: Microsoft Outlook, Build 10.0.2627

X-UEA-Spam-Score: 0.0

X-UEA-Spam-Level: /

X-UEA-Spam-Flag: NO

Dear Phil,

Jones et al 1990 cited a 260-station temperature set jointly collected by the US Department of Energy and the PRC Academy of Sciences, stating in respect to the Chinese stations:

The stations were selected on the basis of station history: we chose those with few, if any, changes in instrumentation, location or observation times.

This data set was later published as NDP-039

[3]<http://cdiac.ornl.gov/epubs/ndp/ndp039/ndp039.html> , coauthored by Zeng Zhaomei, providing station histories only for their 65-station network, stating that station histories for their 205-station network (which includes many of the sites in Jones et al 1990) were not available:

(s. 5) Unfortunately, station histories are not currently available for any of the stations in the 205-station network; therefore, details regarding instrumentation, collection methods, changes in station location or observing times, and official data sources are not known.

(s. 7) Few station records included in the PRC data sets can be considered truly homogeneous. Even the best stations were subject to minor relocations or changes in observing times, and many have undoubtedly experienced large increases in urbanization. Fortunately, for 59 of the stations in the 65-station network, station histories (see Table 1) are available to assist in proper interpretation of trends or jumps in the data; however, station histories for the 205-station network are not available. In addition, examination of the data from the 65-station data set has uncovered evidence of several undocumented station moves (Sects. 6 and 10). Users should therefore exercise caution when using the data.

Accordingly, it appears that the quality control claim made in Jones et al 1990 was incorrect. I presume that you did not verify whether this claim was correct at the time and have been unaware of the incorrectness of this representation. Since the study continues to be relied on, most recently in AR4, I would encourage you to promptly issue an appropriate correction.

Regards, Steve McIntyre

From: "D.J. Keenan" [4]<doug.keenan@informath.org>

To: "Steve McIntyre" [5]<stephen.mcintyre@utoronto.ca>

Cc: "Phil Jones" [6]<p.jones@uea.ac.uk>

Subject: Wang fabrications

Date: Tue, 19 Jun 2007 20:45:15 +0100

X-Mailer: Microsoft Outlook Express 6.00.2900.3138

X-UEA-Spam-Score: 0.0

X-UEA-Spam-Level: /

X-UEA-Spam-Flag: NO

Steve,

I thought that I should summarize what has happened with the Wang case.

First, I concluded that the claims made about Chinese stations by Jones et al. [Nature, 1990] and Wang et al. [GRL, 1990] were very probably fabricated. (You very likely came to the same conclusion.)

Second, some investigation showed that Phil Jones was wholly blameless and that responsibility almost certainly lay with Wang.

Third, I contacted Wang, told him that I had caught him, and asked him to retract his fabricated claims. My e-mails were addressed to him only, and I told no one about them. In Wang's reply, though, Jones, Karl, Zeng, etc. were Cc'd.

Fourth, I explained to Wang that I would publicly accuse him of fraud if he did not retract. Wang seemed to not take me seriously. So I drafted what would be the text of a formal accusation and sent it to him. Wang replied that if I wanted to make the accusation, that was up to me.

Fifth, I put a draft on my web site--

[7] <http://www.informath.org/apprise/a5620.htm>

--and e-mailed a few people, asking if they had any recommendations for improvement.

I intend to send the final version to Wang's university, and to demand a formal investigation into fraud. I will also notify the media. Separately, I have had a preliminary discussion with the FBI--because Wang likely used government funds to commit his fraud; it seems that it might be possible to prosecute Wang under the same statute as was used in the Eric Poehlman case. The simplicity of the case makes this easier--no scientific knowledge is required to understand things.

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There could be a couple problems with that. One problem is that it would be difficult for Phil to publish anything without the agreement of Wang and the other co-authors (Nature would simply say "no").

Another problem is that your e-mail says that you presume Phil was "unaware of the incorrectness" of Wang's work. I do not see how that could be true. Although the evidence that Phil was innocent in 1990 seems entirely conclusive, there is also the paper of Yan et al. [Advances in Atmospheric Sciences, 18: 309 (2001)], which is cited on my web page. Phil is a co-author of that paper.

Phil, this proves that you knew there were serious problems with Wang's claims back in 2001; yet some of your work since then has continued to rely on those claims, most notably in the latest report from the IPCC. It would be nice to hear the explanation for this. Phil?

Kind wishes, Doug

* * * * *

Douglas J. Keenan

[9]<http://www.informath.org>

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The Limehouse Cut, London E14 6N, UK

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090
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University of East Anglia
Norwich Email [10]p.jones@uea.ac.uk
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--

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NOAA's National Climatic Data Center
151 Patton Avenue
Asheville, NC 28801
Voice: +1-828-271-4287
Fax: +1-828-271-4328

References

1. <mailto:stephen.mcintyre@utoronto.ca>
2. <mailto:p.jones@uea.ac.uk>
3. <http://cdiac.ornl.gov/epubs/ndp/ndp039/ndp039.html>
4. <mailto:doug.keenan@informath.org>
5. <mailto:stephen.mcintyre@utoronto.ca>
6. <mailto:p.jones@uea.ac.uk>
7. <http://www.informath.org/apprise/a5620.htm>
8. <http://www.climateaudit.org/?p=1741#comment-115879>
9. <http://www.informath.org/>
10. <mailto:p.jones@uea.ac.uk>

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Re: Fwd: Jones et al 1990
Date: Wed, 20 Jun 2007 09:31:39 -0600 (MDT)
Reply-to: trenbert@ucar.edu

Phil

Hang in there. I went thru this on the hurricane stuff and it was hard to take. But responding to these guys unless they write papers is not the thing to do.

Kevin

>

> Kevin,

> My problem is that I don't know the best course of action.

> Just sitting tight at the moment taking soundings.

> I'd be far happier if they would write some papers and act

> in the normal way. I'd know how to respond to that. In

> a way this all seems a different form of attack from that on Ben and

> Mike in previous IPCCs.

> I know I'm on the right side and honest, but I seem to be

> telling myself this more often recently! I also know that 99.9%

> of my fellow climatologists know the attacks are groundless.

>

> Cheers

> Phil

>

>

> At 14:54 20/06/2007, you wrote:

>>Phil

>>It is nasty. It is also very inappropriate. Even were some problems to

>>emerge over time, those should be addressed in a new paper by these guys.

>>Unfortunately all they do is criticise.

>>Kevin

>>

>>

>>>

>>> Kevin,

>>> Have also forwarded these emails to Susan and Martin, just

>>> so they are aware of what is going on. The second email

>>> is particularly nasty.

>>>

>>> I'm not worried and stand by the original paper and also

>>> Wei-Chyung. I do plan to do some more work on urban-related
>>> issues. I also think there is some urban influence in more recent
>>> Chinese series from the 1980s onwards. I've seen some Chinese
>>> papers on this. They are not that well written though.

>>>
>>> The CA web site has also had a go at David Parker's paper in
>>> J. Climate (2006). David sent them the site locations and where
>>> the data came from at NCDC. There are also threads on CA about
>>> US HCN (Tom Karl and Peterson aware of these) and also about
>>> IPCC and our responses to the various drafts.

>>>
>>> Apologies for sharing these with you. It is useful to send to a
>>> very small group, as it enables me to get on with some real work.

>>>
>>> Cheers

>>> Phil

>>>
>>> Wei-Chyung, Tom,

>>> I won't be replying to either of the emails below, nor to any
>>> of the accusations on the Climate Audit website.

>>>
>>> I've sent them on to someone here at UEA to see if we
>>> should be discussing anything with our legal staff.

>>>
>>> The second letter seems an attempt to be nice to me,
>>> and somehow split up the original author team.

>>>
>>> I do now wish I'd never sent them the data after their FOIA
>>> request!

>>>
>>> Cheers

>>> Phil

>>>
>>>>X-YMail-OSG:

>>>>wrT8WAEVM1myBGklj9hAiLvnYW9GqqFcbArMYvXDn17EHo1e0Vf5eSQ4WIGJljsEw--

>>>>From: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>

>>>>To: "Phil Jones" <p.jones@uea.ac.uk>

>>>>Subject: Jones et al 1990

>>>>Date: Tue, 19 Jun 2007 13:44:58 -0400

>>>>X-Mailer: Microsoft Outlook, Build 10.0.2627

>>>>X-UEA-Spam-Score: 0.0

>>>>X-UEA-Spam-Level: /

>>>>X-UEA-Spam-Flag: NO

>> >>
>> >>Dear Phil,
>> >>
>> >>Jones et al 1990 cited a 260-station temperature set jointly
>> >>collected by the US Department of Energy and the PRC Academy of
>> >>Sciences, stating in respect to the Chinese stations:
>> >>
>> >>The stations were selected on the basis of station history: we chose
>> >>those with few, if any, changes in instrumentation, location or
>> >>observation times.
>> >>
>> >>This data set was later published as NDP-039
>> >><<http://cdiac.ornl.gov/epubs/ndp/ndp039/ndp039.html>><http://cdiac.ornl.gov/epubs/ndp/ndp039/ndp039.html>
>> >>, coauthored by Zeng Zhaomei, providing station histories only for
>> >>their 65-station network, stating that station histories for their
>> >>205-station network (which includes many of the sites in Jones et al
>> >>1990) were not available:
>> >>
>> >>(s. 5) Unfortunately, station histories are not currently available
>> >>for any of the stations in the 205-station network; therefore,
>> >>details regarding instrumentation, collection methods, changes in
>> >>station location or observing times, and official data sources are not
>> >> known.
>> >>
>> >>(s. 7) Few station records included in the PRC data sets can be
>> >>considered truly homogeneous. Even the best stations were subject to
>> >>minor relocations or changes in observing times, and many have
>> >>undoubtedly experienced large increases in urbanization.
>> >>Fortunately, for 59 of the stations in the 65-station network,
>> >>station histories (see Table 1) are available to assist in proper
>> >>interpretation of trends or jumps in the data; however, station
>> >>histories for the 205-station network are not available. In
>> >>addition, examination of the data from the 65-station data set has
>> >>uncovered evidence of several undocumented station moves (Sects. 6
>> >>and 10). Users should therefore exercise caution when using the data.
>> >>
>> >>Accordingly, it appears that the quality control claim made in Jones
>> >>et al 1990 was incorrect. I presume that you did not verify whether
>> >>this claim was correct at the time and have been unaware of the
>> >>incorrectness of this representation. Since the study continues to
>> >>be relied on, most recently in AR4, I would encourage you to
>> >>promptly issue an appropriate correction.

>> >>

>> >>Regards, Steve McIntyre

>> >>

>> >>

>> > From: "D.J. Keenan" <doug.keenan@informath.org>

>> > To: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>

>> > Cc: "Phil Jones" <p.jones@uea.ac.uk>

>> > Subject: Wang fabrications

>> > Date: Tue, 19 Jun 2007 20:45:15 +0100

>> > X-Mailer: Microsoft Outlook Express 6.00.2900.3138

>> > X-UEA-Spam-Score: 0.0

>> > X-UEA-Spam-Level: /

>> > X-UEA-Spam-Flag: NO

>> >

>> > Steve,

>> >

>> > I thought that I should summarize what has happened with the Wang
>> case.

>> >

>> > First, I concluded that the claims made about Chinese stations by
>> > Jones et al. [Nature, 1990] and Wang et al. [GRL, 1990] were very
>> > probably fabricated. (You very likely came to the same conclusion.)

>> >

>> > Second, some investigation showed that Phil Jones was wholly
>> > blameless and that responsibility almost certainly lay with Wang.

>> >

>> > Third, I contacted Wang, told him that I had caught him, and asked
>> > him to retract his fabricated claims. My e-mails were addressed to
>> > him only, and I told no one about them. In Wang's reply, though,
>> > Jones, Karl, Zeng, etc. were Cc'd.

>> >

>> > Fourth, I explained to Wang that I would publicly accuse him of fraud
>> > if he did not retract. Wang seemed to not take me seriously. So I
>> > drafted what would be the text of a formal accusation and sent it to
>> > him. Wang replied that if I wanted to make the accusation, that was
>> up to
>> > me.

>> >

>> > Fifth, I put a draft on my web site--

>> > <http://www.informath.org/apprise/a5620.htm>

>> > --and e-mailed a few people, asking if they had any recommendations
>> > for improvement.

>> >

>>> I intend to send the final version to Wang's university, and to
>>> demand a formal investigation into fraud. I will also notify the
>>> media. Separately, I have had a preliminary discussion with the
>>> FBI--because Wang likely used government funds to commit his fraud;
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>>> statute as was used in the Eric Poehlman case. The simplicity of the
>>> case makes this easier--no scientific knowledge is required to
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>>> I saw that you have now e-mailed Phil (Cc'd above), asking Phil to
>>> publish a retraction of Wang's
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>>> There could be a couple problems with that. One problem is that it
>>> would be difficult for Phil to publish anything without the agreement
>>> of Wang and the other co-authors (Nature would simply say "no").

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>>> Another problem is that your e-mail says that you presume Phil was
>>> "unaware of the incorrectness" of Wang's work. I do not see how that
>>> could be true. Although the evidence that Phil was innocent in 1990
>>> seems entirely conclusive, there is also the paper of Yan et al.
>>> [Advances in Atmospheric Sciences, 18: 309 (2001)], which is cited on
>>> my web page. Phil is a co-author of that paper.

>>>
>>> Phil, this proves that you knew there were serious problems with
>>> Wang's claims back in 2001; yet some of your work since then has
>>> continued to rely on those claims, most notably in the latest report
>>> from the IPCC. It would be nice to hear the explanation for this.

>> Phil?

>>>
>>> Kind wishes, Doug

>>> * * * * *

>>> Douglas J. Keenan
>>> <http://www.informath.org>
>>> phone + 44 20 7537 4122
>>> The Limehouse Cut, London E14 6N, UK

>>>
>>> Prof. Phil Jones
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>> > NR4 7TJ

>> > UK

>> >

>> -----

>>

>>

>> _____

>>Kevin Trenberth

>>Climate Analysis Section, NCAR

>>PO Box 3000

>>Boulder CO 80307

>>ph 303 497 1318

>><http://www.cgd.ucar.edu/cas/trenbert.html>

>

> Prof. Phil Jones

> Climatic Research Unit Telephone +44 (0) 1603 592090

> School of Environmental Sciences Fax +44 (0) 1603 507784

> University of East Anglia

> Norwich Email p.jones@uea.ac.uk

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> UK

> -----

>

Kevin Trenberth

Climate Analysis Section, NCAR

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ph 303 497 1318

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From: "Wahl, Eugene R" <wahl@alfred.edu>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: RE: personal
Date: Wed, 20 Jun 2007 13:37:38 -0400

Hi Phil:

Glad I can help, even if quite indirectly. I know what you mean about the need for community when under duress. The individual quality of being a scientist works against us in this way. Attached are the original letter and the official UCAR response. I don't know what the lawyers might have written, other than their input to the official response letter. I do know they sought information from Caspar (and myself, but less so). I don't recall if we made available to them our correspondence with Steve Schneider about our responses to the review of WA that McIntyre did, which had a lot of information in it that debunked his claims about withholding contrary results, etc, etc.. In fact, we have never mentioned this to Steve, to make sure that he was in the situation to make editorial decisions as focused solely on the science as possible.

I was wondering if there is any way we as the scientific community can seek some kind of "cease and desist" action with these people. They are making all kinds of claims, all over the community, and we act in relatively disempowered ways. Note that UCAR did send the response letter to the presidents of the two academic institutions with which MM are associated, although this seems to have had no impact. Seeking the help of the attorneys you speak about would be useful, I should think. I know that Mike has said he looked into slander action with the attorneys with whom he spoke, but they said it is hard to do since Mike is, in effect, a "public" person -- and to do so would take a LOT of his time (assuming that the legal time could somehow be supported financially). If I might ask, if you do get legal advice, could you inquire into the possibility of acting proactively in response via the British system? Maybe the "public" person situation does not hold there, or less so. I only ask you to consider this question on my part; obviously, please do what you deem best for your situation.

Finally, I have shared the MM letter and UCAR response before only with one other scientist, a now retired eminent person here in the US whom I asked to look over all the materials and give me his frank opinion if he felt we had done anything inappropriate. He came back with a solid "NO", and said that what MM were attempting was "unspeakable". Caspar has mentioned that UCAR said to him they did not want to disseminate these materials publically, and I have kept to that, other than the case mentioned. It seems clear to me that providing them to you is appropriate; I have not contacted Caspar to think about it at this point, and don't feel I need to. Anyway, this is just to give you the context on that side of things. I would imagine that sharing the doc's with legal persons you trust would be OK.

Note that I am now out of contact through July 9. I wish you all the best!!

Peace, Gene

From: Phil Jones [mailto:p.jones@uea.ac.uk]
Sent: Wed 6/20/2007 4:06 AM
To: Wahl, Eugene R
Subject: Fwd: Jones et al 1990

Gene,

Thanks for the email of support! I've taken up the idea of asking someone at UEA about legal advice.

I would like to see the original letter if possible. I won't pass this on. Did the NCAR/UCAR legal staff put anything in writing, as this might help me decide if the advice I might get here is reasonable? I'm sure it will be and I know I've nothing to worry about, as I've done nothing wrong and neither has Wei-Chyung.

It is good to share these sorts of things with a few people. I know Ben and Mike have been through this, but wasn't aware you and Caspar had. Thanks for your strength !

Cheers
Phil

Wei-Chyung, Tom,

I won't be replying to either of the emails below, nor to any of the accusations on the Climate Audit website.

I've sent them on to someone here at UEA to see if we should be discussing anything with our legal staff.

The second letter seems an attempt to be nice to me, and somehow split up the original author team.

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X-Mailer: Microsoft Outlook, Build 10.0.2627
X-UEA-Spam-Score: 0.0
X-UEA-Spam-Level: /
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Date: Tue, 19 Jun 2007 20:45:15 +0100
X-Mailer: Microsoft Outlook Express 6.00.2900.3138
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Kind wishes, Doug

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Attachment Converted: "c:\eudora\attach\MM_request_to_UCAR.doc"

Attachment Converted: "c:\eudora\attach\UCAR_response_to_MM V6.doc"

From: rob.allan@metoffice.gov.uk
To: Malcolm.Haylock@partnerre.com
Subject: Re: hello
Date: Mon, 25 Jun 2007 14:20:42 +0100
Cc: Gil Compo <compo@colorado.edu>, Gil Compo <Gilbert.P.Compo@noaa.gov>, Henry Beverley <Beverley.Henry@nrw.qld.gov.au>, Roger Stone <stone@usq.edu.au>, Adrian Simmons <Adrian.Simmons@ecmwf.int>, Br+nnimann Stefan <stefan.bronnimann@env.ethz.ch>, Frank Le Blancq <leblancq.f@jerseymet.gov.je>, Phil Jones <p.jones@uea.ac.uk>, Pamela_Heck@swissre.com, Paul.Della-Marta@meteoswiss.ch, Scott D Woodruff <Scott.D.Woodruff@noaa.gov>, Meinke@metoffice.gov.uk, Holger <holger.meinke@wur.nl>, Juerg Luterbacher <juerg@giub.unibe.ch>, tlourencak@bluewin.ch

On Mon, 2007-06-25 at 14:50 +0200, Malcolm.Haylock@partnerre.com wrote:

>
> Hi Rob,
>
> Great to hear about the new project and the support of the Queensland
> Government. It sounds like a very worthwhile project from both a
> scientific and user's perspective.

>
> I wrote a summary of your email and your good work with historical SLP
> and sent it to my boss, Hervé Castella, who is the head of research at
> PartnerRe. He is well aware of the value of reanalyses as we use ERA40
> extensively for developing our European storm climatology.

>
> We would be very interested to attend such a meeting bringing the data
> developers and users together. We would also be happy to partly
> sponsor such a meeting. However the main concern, as with the case of
> ERA40 data, is that the final data can be very expensive for
> commercial users so sponsorship would probably require an agreement
> about access.

>
> Regarding venues, if you'd like input from the reinsurance industry
> then there is no better location than Zurich. It also has excellent
> access to Nth America because of the financial connections.

>
> Malcolm

>
> rob.allan@metoffice.gov.uk wrote on 19/06/2007 11:15:06:

>
> > On Tue, 2007-06-19 at 10:45 +0200, Malcolm.Haylock@partnerre.com
> wrote:

> > >
> > > Hi Rob,
> > >
> > > How's it going? Paul and I saw Tara yesterday. It's great to have
> > > her
> > > in Zurich. She said things are looking brighter for you at the
> > > MetOffice. Still, why not come and join the growing Aussie empire
> > > in
> > > Switzerland?
> > >

> > > Malcolm
> > > DISCLAIMER: This e-mail contains information solely intended for
> > named recipients and is confidential and proprietary to PartnerRe.
> > If you are not one of the intended recipients of this message, you
> > must not read, use or disseminate the information in it and should
> > notify the sender by replying to this message and deleting it
> > afterwards from your mail system. Please be aware that unauthorized
> > reproduction or distribution of this communication is prohibited.
> > >

> >
> >

> > Malcolm,
> > Good to hear from you.

> >
> > Glad that you guys caught up with Tara, it's great that she
> > has fellow Aussies in the
> > vicinity to catch up with.

> >
> > I just spoke to Paul Della-Marta on the phone about matters
> > to do with my new role
> > here in the Hadley Centre, and I'd like any thoughts you might have
> > on a potential meeting
> > linked to that new role.

> >
> > NEW ROLE

> >
> > Basically, as of next month, I'll be officially the Project
> > Manager of an initiative
> > called ACRE (Atmospheric Circulation Reconstructions over the
> > Earth). Though based in the
> > Hadley Centre, this post is being primarily funded by the Queensland
> > Climate Change Centre of
> > Excellence (QCCCE) in Australia!! It is an 'end-to-end' project
> > covering data and reanalyses
> > at one end and looking to make the reanalyses products flow
> > 'seamlessly' into various climate
> > applications models at the other. I came up with the concept, got
> > the infrastructure together
> > to make it work and sold QCCCE on it without any Met Office or
> > Hadley Centre input initially.

> >
> > Anyway, a major component of my new role is to support and
> > facilitate the global daily
> > to sub-daily surface pressure data requirements for historical
> > surface
> > observations only reanalyses (the 20th Century Reanalysis Project)
> > that
> > a colleague, Dr Gil Compo at NOAA ESRL/CIRES/CDC in the US, is
> > leading -
> > see this link for an overview of the 20th Century Reanalysis Project
> > (<http://www.noaanews.noaa.gov/stories2007/s2771.htm>).

> >
> > We aim to build on the expertise developed by the 20th
> > Century

> > Reanalysis Project to provide the basis for surface observations-
> based
> > reanalyses which have sufficient data coverage to be valid globally
> back
> > to the mid-19th century and specifically over the North Atlantic-
> > European region from the mid-18th century to the present.
> >
> > MEETING AS PART OF MY NEW ROLE
> >
> > The background to this is as follows:
> >
> > Gil Compo and I plus those in the GCOS AOPC/OOPC Surface
> > Pressure Working Group (SPWG) have had the hope for a while now
> > of being able to fund a meeting of the SPWG in its own right, rather
> > than 'piggy backing' on other meetings all the time. The US members
> of
> > the SPWG had been hoping for a meeting in, or closer to, the US.
> With
> > all that in mind I suggested Bermuda as a venue, given that the
> > Biological Institute of Ocean Sciences there have strong links to
> the
> > reinsurance industry and a particular focus on European storminess.
> >
> > The Bermuda idea has waxed and waned a bit, and though there
> is
> > now the possibility of some potential funding via Howard Diamond
> (the US
> > GCOS Rep) to support such a meeting, doing the figures shows that it
> is
> > going to be too expensive to hold it in Bermuda. However, with my
> new
> > role as the Project Manager of the ACRE initiative developing in
> > parallel with the above, I'm now thinking of a somewhat more
> effective
> > and reshaped meeting probably held in Europe.
> >
> > My current thoughts revolve around the idea of holding a
> smallish
> > but manageable meeting. The focus being on bringing together the
> GCOS
> > AOPC/OOPC Working Groups on pressure (SPWG), SST and sea-ice,
> > atmospheric reference observations plus the new one on observational
> > datasets for reanalysis, with climate applications and reinsurance
> > people, to focus on the various reanalysis data needs and on
> potential
> > climate applications and impacts usage of such reanalysis products.
> > This type of meeting fits the very core of what my ACRE Project
> > Manager's role is about. I also think strategically it might provide
> a
> > very useful focus all round which will promote the need for more
> data,
> > clarify the current and potential situation with the various
> reanalysis
> > efforts and their needs, and give the climate applications community

> a
> > better idea of what the data and reanalysis products can be best
> used
> > for.
> >
> > One recent example highlights the sort of problem that exists
> > over this way with reanalyses and the climate applications side. The
> > European Environment Agency (EEA) have been talking to ECMWF about
> using
> > their reanalysis products (for wind and energy planning plus
> storminess
> > trends), but from what I've heard and discussed with Adrian Simmons
> (the
> > AOPC Chair and ECMWF ERA reanalysis person), the EEA really don't
> > understand the strengths and weaknesses of the ERA reanalysis
> product
> > and how best to use it for their needs. As a result, this potential
> > linkage has tended to flounder somewhat.
> >
> > I also understand that a Spanish colleague is looking to set
> up a
> > COST (Co-operation on Science and Technology) Action under the EC
> COST
> > program that would focus on reanalyses and I think applications.
> I'm
> > going to suggest to him that the sort of meeting I'm looking to
> initiate
> > could also be linked to his efforts and be an initial meeting for
> such a
> > COST Action.
> >
> > I've talked to Roger Stone and Holger Meinke on the climate
> > applications side, plus others on the climate and reanalysis side of
> > things (Gil Compo, Adrian Simmons, Stefan Bronnimann) about such a
> > meeting and have had considerable interest. Roger mentioned his
> links
> > with the reinsurance industry in Europe in looking to link them
> (maybe
> > even part fund) into such a meeting, and I'm going to follow up on a
> > similar tack. I'm thinking that it could be a milestone for the
> first
> > year of my contract, and something that could also be duplicated in
> > Australia or elsewhere.
> >
> > Thus, I'd be very keen to hear your thoughts on any of the
> above,
> > and how we might be able to make it happen for the benefit of all.
> Some
> > ideas for venues I've had are Jersey or Guernsey in the Channel
> Islands
> > and Dublin (this might be easiest for US attendees to get to).
> >
> >
> > Cheers, Rob.
> >

> >
> > Dr Rob Allan Climate Scientist
> > Met Office FitzRoy Road Exeter EX1 3PB United Kingdom
> > Tel: +44 (0)1392 886904 Fax: +44 (0)1392 885681
> > E-mail (W): rob.allan@metoffice.gov.uk <http://www.metoffice.gov.uk>
> > E-mail (H): rallan@onetel.com

Malcolm,

Thanks for that, much appreciated.

I'll forward it on to Gil Compo and others linked to ACRE and the AOPC WGs. I think that Roger Stone from Queensland knows some of your people, so there should be some good links all round.

I've also gone back to Howard Diamond, the US GCOS Rep, from whom I'm hoping to get some financial support for such a meeting to gauge his reaction to holding it in Europe.

Cheers, Rob.

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--

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From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>, Tom Melvin <t.m.melvin@uea.ac.uk>
Subject: Re: AD 536
Date: Tue, 3 Jul 2007 17:52:39 +0600
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith and Tom,
thank you to include me in co-authors list of the paper.
I'm not sure that it is right, nevertheless I can't refuse. However,
if you consider to reduce number of co-authors I would not be offended
if you exclude me.

My corrections and suggestions:

1) Table S1: for Yamal - elevation 10-60 m, east - 70°, north - 67°30'

2) may be add reference to presence of frost rings in AD 536 in
Siberian pine in Mongolia (D'Arrigo et al., Climatic change, 49,
239-246, 2001) and frost and light rings in larch from Yamal (our
data)?

3) if possible, add to acknowledgments my thanks for funding to
Russian Foundation for Basic Research (project # 07-05-00989)

4) just to satisfy my curiosity - if dating of ice layers is not too
precise, why not suppose that first peak of sulphate deposits (about
AD 529 in fig. 3b) correspond to AD 536? May be two eruptions are
reason of relative long growth suppression? By the way, in larch from
Yamal frost rings formed in 536, 543 (two times as much as 536), and
545 (previous frost rings year was AD 404, next AD 627).

I'm sorry, I didn't reply to your previous letter concerning
manuscript to Philosophical Transactions of Royal Society. If it is
not too late, please correct my name in co-authors list (Rashit).

Best regards

Rashit Hantemirov

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E-mail: rashit@ipae.uran.ru

2 èþëý 2007 ã., 19:29:57 you wrote:

- > Dear Matti, Kurt, Hakan, Bjorn, Rashit and Mukhtar,
- > Attached is a letter of explanation from Keith (Briffa) and a draft
- > of a paper to be submitted with a request for you all to be co-authors.
- > The list of authors details, the tree-ring data Figure 1, and
- > supporting table all need to be checked.
- > (e.g. Kurt - is there a better name for your sites?)
- > Thanks
- > Tom
- > Dr. Tom Melvin
- > Climatic Research Unit
- > University of East Anglia
- > Norwich, NR4 7TJ, U.K.
- > Phone: +44-1603-593161
- > Fax: +44-1603-507784
- > _____ NOD32 2369 (20070702) Information _____
- > This message was checked by NOD32 antivirus system.
- > <http://www.eset.com>

From: Martin Juckes <m.n.juckes@rl.ac.uk>

To: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: Mitrie

Date: Fri, 6 Jul 2007 16:23:18 +0100

Cc: Jan Esper <esper@wsl.ch>, anders.moberg@natgeo.su.se, Eduardo.Zorita@gkss.de, hegerl@duke.edu, k.briffa@uea.ac.uk, m.allen1@physics.ox.ac.uk, weber@knmi.nl

Content-Type: text/plain;

charset="utf-8"

Content-Disposition: inline

X-MIME-Autoconverted: from 8bit to quoted-printable by oin.rl.ac.uk id l66FNNrC019808

Thanks to Tim and Keith for that correction.

I've inserted that, and also reworded the paragraph in the conclusions which talked about "serious flaws" along the lines suggested by Tim. It now reads:

"The IPCC2001 conclusion that temperatures of the past millennium are unlikely to have been as warm, at any time prior to the 20th century, as the last decades of the 20th century is supported by subsequent research and by the results obtained here.

We have also reviewed and, in some cases, tested with new analysis, papers which claim to refute that IPCC2001 conclusion and found that their claims are not well supported."

This version attached with the revised supplementary material.

I need to go over the `changes' document again, and the response, but I hope to send it in on Monday.

cheers,

Martin

On Wednesday 04 July 2007 16:54, Tim Osborn wrote:

> Hi Martin & Jan (and others)

>

> Keith and I have put together the attached text as an alternative,

> hopefully more accurate, version to the current paragraph about

> differences between tree series. We did this before/while Jan's

> email arrived, so some overlap but hopefully what we say is

> compatible with Jan's comment. Note we haven't discussed the ice

> core data from Fisher, just the tree-ring series.

>

> How does the attached sound?

>

> Cheers

>
> **Tim**
>
> At 22:15 03/07/2007, Jan Esper wrote:
> >Martin
> >This is quite a task, as I do not really remember which version of a
> >dataset was used in which paper.
> >
> >For ECS2002, I detrended all data via two RCS runs applied to the
> >"linear" and "non-linear" sub-groups as identified in that paper.
> >All data except for Boreal and Upper Wrigth (both from Lisa
> >Graumlich) and Mongolia (from Gordon Jacoby) were measured at WSL.
> >
> >I wouldn't necessarily claim that the regional chronologies from the
> >ECS approach are highly useful records, i.e. for a regional analysis
> >I would use data that are detrended region-by-region.
> >
> >(Åthat used by ECS2002 is based on the same tree-ring data as that
> >used by MSH2005, but with a different standardisation method.)
> >Not fully sure what MSH2005 did, but this is very likely correct,
> >i.e. they likely used a "regional" version from Briffa and/or Grudd.
> >
> >(The Fennoscandia data used by JBB1998, MBH1999 also come from the
> >Tornetraesk area, but from a different group of trees.)
> >Hm..., I don't believe that these studies used different trees. Up
> >to the recent update by Hakan Grudd, that is currently in review
> >with Climate Dynamics, there was effectively only one dataset from
> >Tornetrask. Keith or Tim might know this better.
> >
> >(The Polar Urals series used by ECS2005 is also a reanalysis of the
> >data used to create the Northern Urals series used by JBB1998, MBH1999.)
> >I wouldn't necessarily call this a reanalysis. Perhaps better say
> >'differently detrended'. Anyway, I doubt that there is a long
> >dataset from the Northern Ural as there is little wood preserved in
> >that area. This is likely the same data, i.e. both are Polar Ural.
> >
> >(The Taymir data used by HCA2007 is a smoothed version of that used
> >in ECS2002, MSH2005.)
> >This I really don't knowÅ but it would be better to use a regionally
> >detrended version of the data...
> >
> >(The Greenland stack data used by MBH1999 is a composite of data
> >analysed by \cite{fisher_etal1996}, but the precise nature of the
> >composite is not described by \cite{fisher_etal1996}.)"

>>Agreed. Just read the paper again, and it is indeed difficult to say
>>which data was combined.

>>

>>(I've kept the phrase about "serious flaws" in the conclusion,
>>despite Tim's suggestion, supported by Nanne, of a weaker wording,
>>because I think it is important to draw attention to the serious
>>flaws which are there.)

>>I also think that a less aggressive wording would be more effective.

>>

>>-- Jan

>>

>>

>>

>>

>>At 16:41 Uhr +0100 3.7.2007, Martin Juckes wrote:

>>>Hello,

>>>

>>>another version of our paper is attached.

>>>

>>>I've added the following paragraph to the discussion of Table 1, and I'd
be

>>>grateful if Jan and Keith could check that it is accurate:

>>>"Evaluation of past work is further compicated by confusion between
closely

>>>related proxy series. In Tab.~1 there are two series referred to as

>>>Tornetraesk: that used by ECS2002 is based on the same tree-ring data as
that

>>>used by MSH2005, but with a different standardisation method. The

>>>Fennoscandia data used by JBB1998, MBH1999 also come from the Tornetraesk

>>>area, but from a different group of trees. The Polar Urals series used by

>>>ECS2005 is also a reanalysis of the data used to create the Northern Urals

>>>series used by JBB1998, MBH1999. The Taymir data used by HCA2007 is a

>>>smoothed version of that used in ECS2002, MSH2005.

>>>The Greenland stack data used by MBH1999 is a composite of data analysed
by

>>>\citet{fisher_etal1996}, but the precise nature of the composite is not

>>>described by \citet{fisher_etal1996}."

>>>

>>>I've also moved a few things around and tried to follow most of the

>>>suggestions from Anders and Nanne. I've kept the phrase about "serious
flaws"

>>>in the conclusion, despite Tim's suggestion, supported by Nanne, of a
weaker

>>>wording, because I think it is important to draw attention to the serious

>>>flaws which are there. One reviewer has implied that we should not discuss
>>>flawed work at length because in doing so we give it credibility it does
not
>>>deserve. I believe that since this stuff is published and influential in
some
>>>quarters we should discuss it and draw attention to the fact that it is
>>>seriously flawed.
>>>
>>>cheers,
>>>Martin
>>>
>>>Attachment converted: Hennes:cp-2006-0049-rv 3.pdf (PDF /Â«ICÂ») (001588D6)
>>
>>
>>
>>--
>>
>>Jan Esper
>>Head Dendro Sciences Unit
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>>Zuercherstrasse 111, 8903 Birmensdorf, Switzerland
>>Voice: +41-44-739 2510 or +41-44-739 2579
>>Fax: +41-44-739 2515<http://www.wsl.ch/staff/jan.esper>
>

Attachment Converted: "c:\eudora\attach\cp-2006-0049-rv4.pdf"

Attachment Converted: "c:\eudora\attach\cp-2006-0049-sp1.pdf"

From: P.Jones@uea.ac.uk
To: david.parker@metoffice.gov.uk
Subject: RE: UHI corrections
Date: Wed, 18 Jul 2007 13:21:59 +0100 (BST)
Cc: "Jenkins, Geoff" <geoff.jenkins@metoffice.gov.uk>, "Jones, Phil" <p.jones@uea.ac.uk>

Geoff,

David is essentially right. In 1986 we rejected 38 (if my memory from 1986) is correct! I don't recall the number we looked at so I can't give a percentage, as I'm not that much of a trainspotter.

The % would be small though, as we looked the homogeneity of about 2500 then. Also some which might have been affected by urbanization might have been rejected for other reason. I'm half asleep here in my hotel room in Beijing (same hotel as the IPCC meeting David!) as it is just gone 8pm! I have the pdf of the 1986 paper and 38 rejected for urban warming trends (31 in N. America and 7 in Europe - none elsewhere) out of 2666. 239 were rejected for other reasons.

Brohan et al is the best reference. We included urbanization as one of the biases (one sided as urban should lead to warming, so if you look very, very closely at the error range in the paper you'll see it is slightly one-sided.

I've been giving some talks here and have more tomorrow. At CMA I've found they have a homogenized dataset of 745 stations for the country which they are preapred to give me at some point for inclusion. They have adjusted for all site moves but not for urbanization. It seems that it is almost impossible for sites here to be rural (maybe only 1% of the total). Sites move out of the city at regular intervals as the cities expand. So Beijing has 6-7 site moves since 1951! Also China seems to be the only country that doesn't use airport sites. None are located at airports. I'm going to give them my Chinese sites in return so they can do some comparisons. I'll talk with their person (Mr Li) more tomorrow.

Another interesting bit of work here is that they also have an homogenized set of monthly wind speed data from 1951. Not sure how they homogenize this for site moves, but almost all the sites (about 200) show declines in mean wind speeds since 1951. NCEP and ERA-40 also show this for wind speeds at 1000, 925 and 850hPa as well. Odd thing is that they think the decline in wind speeds is due to urbanization! - Li's English isn't great though, so I could be wrong. Another person I've been talking to has been looking at precip trends from 1951 - again they think declines in N. China are due to urbanization! Odd then that there are increases in S. China, which is also urbanized at similar rates.

Air quality here is awful - I saw the sun for the first time since arrival on Sunday, after a long downpour cleared the air this morning! The haze will be back tomorrow. Apparently they will closing the worst factories and getting half the cars off the road next August for the Olympics! Traffic might flow better for the latter, but can't see the former doing that much good. What they need to do is to get a heavy downpour every early morning!

Cheers
Phil

> Geoff
>
> It is correct that Phil Jones removes stations that appear to have urban
> warming, unlike Hansen et al. who correct them. I don't know the
> percentage of stations that Phil removes; details were probably
> originally given in the Jones et al 1985 and 1986 USDoE reports (see
> references given in Jones and Moberg, 2003 (attached); the reports are
> probably only available on paper and are not now in my collection of
> box-files!) and could take some time to collate. But to do this might
> not be useful as Phil could have rejected further stations from the
> additional datasets he accrued since then. Nevertheless I expect the
> rejection rate is small.
>
> Brohan et al is the best reference for a discussion of the urbanization
> uncertainty in land surface air temperatures.
>

> I hope this helps somewhat.

>

> Regards

>

> David

>

>

> On Wed, 2007-07-18 at 11:46 +0100, Jenkins, Geoff wrote:

>> David

>>

>> If I understand Phil right, there are no stations which are CORRECTED
>> for UHI effects, but there are several (roughly what percentage?) which
>> are REMOVED. I would be grateful if you could give me the best ref to
>> this (is it Brohan et al 2006), to pass to an outside sceptical enquirer
>> (one Nigel Lawson, remember him?). He already knows about yr recent
>> windy/calm comparison paper via the "Briefing" booklet I did.

>>

>> Thanks

>>

>> Geoff

>>

>> -----Original Message-----

>> From: P.Jones@uea.ac.uk [mailto:P.Jones@uea.ac.uk]

>> Sent: 16 July 2007 21:59

>> To: Jenkins, Geoff

>> Subject: Re: UHI corrections

>>

>>

>>

>> Geoff,

>> In China this week and away next week. Best Ref is
>> really Ch3 of AR4 (IPCC). We don't make adjustments
>> just remove the stations affected.

>>

>> Best if you contact David Parker. There is also
>> some stuff in Brohan et al. (2006) in JGR. Also
>> David P has a couple of papers on the subject.

>>

>> We incorporate possible residual urban effects into
>> the error estimates of global T.

>>

>> Cheers

>> Phil

>>

>>

>> > Phil

>> >

>> > Sorry to keep bombarding you. What is the best ref to your corrections

>>

>> > of land surface temps (in CRUTEM, presumably) for heat island effects,

>>

>> > please?

>> >

>> > Geoff

>> >

>> > Dr Geoff Jenkins

>> > Manager, Climate Change Scenarios

>> > Hadley Centre

>> > Met Office

>> > FitzRoy Road, EXETER, EX1 3PB, UK

>> > tel: +44 (0) 787 966 1136

>> > geoff.jenkins@metoffice.gov.uk

>> > www.metoffice.gov.uk

>> >

>> >

>>

> --

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> E-mail: david.parker@metoffice.gov.uk

> Tel: +44-1392-886649 Fax: +44-1392-885681

> <http://www.metoffice.gov.uk>

>

>

>

From: Kevin Trenberth <trenbert@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Something not to pass on
Date: Wed, 29 Aug 2007 14:41:06 -0600
Cc: "Michael E. Mann" <mann@meteo.psu.edu>

Phil

Confidential: Dennis Shea just had angiogram: 75% blockage: having open heart surgery tomorrow morning. He does not want this known till the operation results are known.

=====

This is awful stuff and I can't imagine that this could be published. I know of this fellow Peiser though and he is extremely biased (against you likely). So treading with caution is warranted. The email seems to invite a comment but not a review. You should probably only respond with something that you would not mind being published. You can also point out errors of fact. Whether you point out errors of logic or opinion is another matter altogether. If you write just to the editor you can try to evaluate the comment and point out that it lacks substance.

I think my approach would be to try to stick to science.e.g.

I don't know what was done for the 1990 paper but obviously sound practice is

- 1) we attempt to use homogeneous data
- 2) Site moves are one indication of lack of homogeneity but there are standard means of adjusting for such moves especially when there is an overlap in the record.
- 3) All data are scrutinized for possible problems and discontinuities, especially if there is a question about a possible move and the date is known.
- 4) Site movements do not necessarily prejudice the record toward warming or cooling: a move from the inner city to an outlying airport can result in cooling, for instance.
- 5) Revisions are made when new information becomes available.
- 6) It is helpful if researchers can improve the records and provide updated analyses.

Or something to this effect. You could try a patronizing approach of over explaining the difficulties.

At the very least you should be critical of the statement in 4. that he "politely requested an explanation". He quotes you as saying:

"Why should I make the data available to you, when your aim is to try and find something wrong with it?".[1][1]

[2][1] McIntyre S. (19 July 2006), Submission to the Subcommittee on Oversight and Investigations (Committee on Energy and Commerce, U.S. House of Representatives). This is a sworn statement by McIntyre. [It is available at
[3]<http://energycommerce.house.gov/reparchives/108/Hearings/07192006hearing1987/McIntyre.pdf>.]

but you have no reason to be defensive: if there was a problem with the data and all due care was taken, then if there is something wrong with it, it was the responsibility of those who took the data, not those who used it responsibly. You should also point out that the data are just as available to anyone as to you.

In the IPCC report we are careful to say that there are urban effects and they are important and we have a lot about them. But they are small on the global scale. His conclusions are wrong. Also the IPCC evaluates published works and does not do research or deal with raw data.

In the appendix, presumably the quotes are based on the best information at the time. That was then.

The conclusions of the author that fabrication occurred is not valid. Maybe things could have been done better, but that universally applies.

Let me know if you want more concrete suggestions

Kevin

Phil Jones wrote:

Kevin, Mike,

Sending just for your thoughts. The Appendix of this attachment has gone to SUNY Albany and is being dealt with by them. Not sure when, but Wei-Chyung has nothing to worry about.

I've sent to Wei-Chyung and also to Tom Karl. Q is should I respond? If I don't they will misconstrue this to suit their ends. I could come up with a few sentences pointing out the need to look at the Chinese data rather than just the locations of the sites. Looking further at Keenan's web site, he's not looked at the temperature data, nor realised that the sites he's identified are the urban stations from the 1990 paper. He has no idea if the sites for the rural Chinese stations moved, as he doesn't seem to have this detail. Whatever I say though will be used for whatever, so it seems as though I'm damned if I do and damned if I don't.

Does the email suggest to you this is a request for a formal review?

E&E have an awful track record as a peer-review journal.

Footnote 8 is interesting. Grape harvest dates are one of the best documentary proxies.

Cheers

Phil

Subject: review of E&E paper on alleged Wang fraud

Date: Wed, 29 Aug 2007 15:18:04 +0100

X-MS-Has-Attach: yes

X-MS-TNEF-Correlator:

Thread-Topic: review of E&E paper on alleged Wang fraud

thread-index: AcfqPgYII3NKEW8US8uwftlkhnXNhGAB/4xQAAA5K8A=

From: "Peiser, Benny" [4]<B.J.Peiser@ljmu.ac.uk>

To: [5]<p.jones@uea.ac.uk>

X-OriginalArrivalTime: 29 Aug 2007 14:18:06.0729 (UTC) FILETIME=[6B4F5F90:01C7EA47]

X-UEA-Spam-Score: 0.0

X-UEA-Spam-Level: /

X-UEA-Spam-Flag: NO

Dear Dr Jones

I have attached a copy of Doug Keenan's paper on the alleged Wang fraud that was submitted for the forthcoming issue of Energy & Environment

[6]<http://www.ingentaconnect.com/content/mscp/ene>.

I was wondering whether you would be happy to comment on its content and factual accuracy. Your comments and suggestions would be much appreciated. We would need your feedback by Sept 17.

I look forward to hearing from you.

Yours sincerely

Benny Peiser

Guest editor, E&E

Liverpool John Moores University, UK

Prof. Phil Jones

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--

Kevin E. Trenberth e-mail: [8]trenbert@ucar.edu
Climate Analysis Section, [9]www.cgd.ucar.edu/cas/trenbert.html
NCAR
P. O. Box 3000, (303) 497 1318
Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

References

Visible links

1. file:///localhost/tmp/convertmbox5320.html#_ftn1

2. file://localhost/tmp/convertmbox5320.html#_ftnref1
3. <http://energycommerce.house.gov/reparchives/108/Hearings/07192006hearing1987/McIntyre.pdf>
4. <mailto:B.J.Peiser@ljmu.ac.uk>
5. <mailto:p.jones@uea.ac.uk>
6. <http://www.ingentaconnect.com/content/mscp/ene>
7. <mailto:p.jones@uea.ac.uk>
8. <mailto:trenbert@ucar.edu>
9. <http://www.cgd.ucar.edu/cas/trenbert.html>

Hidden links:

10. file://localhost/tmp/convertmbox5320.html#_ftn1

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: RE: review of E&E paper on alleged Wang fraud
Date: Thu, 30 Aug 2007 09:01:41 -0400
Cc: Kevin Trenberth <trenbert@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>

<x-flowed>
thanks Phil,

I did take the liberty of discussing w/ Gavin, who can of course be trusted to maintain the confidentiality of this. We're in agreement that Keenan has wandered his way into dangerous territory here, and that in its current form this is clearly libellous; there is not even a pretense that he is only investigating the evidence. Furthermore, while many of us fall under the category of 'limited public figures' and therefore the threshold for proving libel is quite high, this is **not** the case for Wei-Chyung. He is not a public figure. I believe they have made a major miscalculation here in treating him as if he is. In the UK, where E&E is published, the threshold is even lower than it is in the states for proving libel. We both think he should seek legal advice on this, as soon as possible.

With respect to Peiser's guest editing of E&E and your review, following up on Kevin's suggestions, we think there are two key points. First, if there are factual errors (other than the fraud allegation) it is very important that you point them out now. If not, Keenan could later allege that he made the claims in good faith, as he provided you an opportunity to respond and you did now. Secondly, we think you need to also focus on the legal implications. In particular, you should mention that the publisher of a libel is also liable for damages - that might make Sonja B-C be a little wary. Of course, if it does get published, maybe the resulting settlement would shut down E&E and Benny and Sonja all together! We can only hope, anyway. So maybe in an odd way its actually win-win for us, not them. Lets see how this plays out...

RealClimate is of course always available to you as an outlet, if it seems an appropriate venue. But we should be careful not to jump the gun here.

Kevin: very sorry to hear about Dennis. Please pass along my best wishes for a speedy recovery if and when it seems appropriate to do so...

Mike

Phil Jones wrote:

> Mike, Kevin,
> Thanks for your sets of thoughts. I've been in touch with Wei-Chyung,
> who's in China at the moment. He forwarded the 'paper!' to the people
> dealing
> with Keenan's allegations at SUNY. He got a reply to say that Keenan
> has now violated the confidentiality agreement related to
> the allegation. So, it isn't right to respond whilst this is
> ongoing. I will
> draft something short though, whilst it's all fresh in my mind. Then
> I can
> get onto something else.
> I did send the email below to Peiser clarifying whether he wanted
> a review or just thoughts. I got the amazing reply - sent to three
> reviewers!
> So, letting the SUNY process run its course. Once finished, Real
> Climate
> may be one avenue to lay out all the facts/details.
>
> Away tomorrow. I think you have Monday off, so have a good long
> weekend!
>
> Cheers
> Phil

>> Subject: RE: review of E&E paper on alleged Wang fraud
>> Date: Wed, 29 Aug 2007 17:48:43 +0100
>> X-MS-Has-Attach:
>> X-MS-TNEF-Correlator:
>> Thread-Topic: review of E&E paper on alleged Wang fraud
>> thread-index: AcfqVG3NykjMc9doTBWIfTqkHPH+xwACAfp3
>> From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
>> To: "Phil Jones" <p.jones@uea.ac.uk>
>> X-OriginalArrivalTime: 29 Aug 2007 16:53:26.0748 (UTC)
>> FILETIME=[1E7969C0:01C7EA5D]
>> X-UEA-Spam-Score: 0.0
>> X-UEA-Spam-Level: /
>> X-UEA-Spam-Flag: NO
>>
>> Dear Phil
>>

>> The paper has been sent to three reviewers. Of course I will take
>> your comments and assessment into consideration. Indeed, if the
>> claims are unsubstantiated, I would certainly reject the paper.

>>
>> I hope this clarifies your query.

>>
>> With best regards
>> Benny

>>
>>
>>
>>
>>
>> _____

>>
>> From: Phil Jones [mailto:p.jones@uea.ac.uk]
>> Sent: Wed 8/29/2007 16:51
>> To: Peiser, Benny
>> Subject: Re: review of E&E paper on alleged Wang fraud

>>
>>
>>
>>
>> Benny,
>> Energy and Environment is presumably a peer-review journal. Your
>> email wasn't clear as to whether you want me to review the paper?
>> If you
>> want me to, will you take any notice of what I might say - such as
>> reject the paper? Or has the contribution already been reviewed?

>>
>> Phil

>>
>>
>> At 15:18 29/08/2007, you wrote:

>> >Dear Dr Jones
>> >
>> >I have attached a copy of Doug Keenan's paper on the alleged Wang fraud
>> >that was submitted for the forthcoming issue of Energy & Environment
>> ><http://www.ingentaconnect.com/content/mscp/ene>.

>> >
>> >
>> >I was wondering whether you would be happy to comment on its content
>> and
>> >factual accuracy. Your comments and suggestions would be much

>> >appreciated. We would need your feedback by Sept 17.

>> >

>> >I look forward to hearing from you.

>> >

>> >Yours sincerely

>> >

>> >Benny Peiser

>> >Guest editor, E&E

>> >Liverpool John Moores University, UK

>> >

>> >

>>

>> Prof. Phil Jones

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>> -----

>>

>

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>

--

Michael E. Mann

Associate Professor

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The Pennsylvania State University email: mann@psu.edu

University Park, PA 16802-5013

<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: wang@climate.cestm.albany.edu
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: review of E&E paper on alleged Wang fraud
Date: Thu, 30 Aug 2007 17:20:27 -0400
Cc: Thomas.R.Karl@noaa.gov, 'Wei-Chyung Wang' <wang@climate.cestm.albany.edu>

Phil,

I think you need to respond by providing E&E with a simple answer of "false" to Keenan's write-up, based on the communication with me (but no mention of SUNYA confidentiality issue, it has to come directly from SUNYA). That will force E*E to contact either me directly or SUNYA. If the former, I can refer to SUNYA also, and let the university to handle it.

My reading is that, since the IPCC policy report is coming out soon (in October?), Keenan is in panic and wants to tint the Nature paper as much and as soon as possible, so he can not wait for SUNYA to conduct "inquiry" (not investigation) which he knows he is not getting what he wants. Going to news medium will not do his trick because he can not really explain it. So in a way Keenan traps himself now, betting on that the "station history" was not available and that the stations have moved a lot (he does not know that at all). We are facing a tricky person and group, and the only way to do it is to follow the procedure to drive them crazy. E&E is not going to publish it without giving me the chance to respond, and that is when SUNYA comes in and that is what Keenan does not want to see as well, he wants to create a smocky screen before the truth comes out. We are not going to let Keenan doing things his way. So be easy, and respond directly what you learn from me (and any other scientific issues you can identify) and perhaps even ask E&E to contact me/or SUNYA for verification.

I know you are under tremendous pressure, but Keenan is in panic and what he has done is going back to burn him, badly. We should be thinking, after the whole ordeal is over, to take legal (or other) actions against Keenan. This is time I regret not been a rich person, otherwise I can throw a million dollar lawsuit against him.

Let me know what you want to do. I have also asked SUNYA's opinion about what you should do within the SUNYA framework. But be careful that you do not know much about SUNYA action.

WCW

----- Original Message -----

From: Phil Jones <p.jones@uea.ac.uk>

Date: Thursday, August 30, 2007 10:16 am

Subject: Re: Fwd: review of E&E paper on alleged Wang fraud

>

> Wei-Chyung,

> Been thinking. A couple of thoughts:

>

> 1. Libel is quite easy to prove in the UK as you're not a public
> figure. Perhaps when you're back you ought to consider taking
> some legal

> advice from SUNY. Assuming the paper is published that is.

>

> 2. More important. I think I should send a short email to the editor
> Peiser and inform him that Keenan has broken his agreement with
> SUNY over this issue. If I don't, they could say I had the chance
> and didn't. Can you check with SUNY whether the folks there think
> I should? I just don't want to do anything that later could be
> construed as the wrong thing now. I could also point out some
> factual errors.

>

> Cheers

> Phil

>

>

> At 10:06 30/08/2007, wang@climate.cestm.albany.edu wrote:

> >the confidentiality means that keenan needs to keep the "inquiry"

> >confidential during the process of sunya "inquiry".

> >

> >wcw

> >----- Original Message -----

> >From: Phil Jones <p.jones@uea.ac.uk>

> >Date: Thursday, August 30, 2007 4:03 am

> >Subject: Re: Fwd: review of E&E paper on alleged Wang fraud

> >

> > >

> > > Wei-Chyung and Tom,

> > > Thanks for the quick response. I won't do anything then

until

>>> the SUNY process has run its course. Can you clarify what you
> mean>> by violated confidentiality? I presume you mean that
> Keenan agreed
>>> to do nothing on the issue until the SUNY process has run its
>>> course. I presume this will conclude sometime this autumn. Keep
>>> me informed of when the final decision might be, as after this
>>> we
>>> ought to do
>>> something about the paper in Energy and Environment. I checked
>>> with their guest editor and got this amazing reply! See below.
>>> So, if we didn't already think this was the worst journal in the
>>> world, now we know for certain it is, and have clear information
>>> from them
>>> to prove it.
>>>
>>> When I mean doing something, I don't mean sending anything
> to E&E,
>>> as that will be useless. The Real Climate blog site is a
>>> possibility, but
>>> there are other avenues.
>>> I will make a few notes and send them to you to forward to
> SUNY.>> Only after doing this can I get onto something else!
>>>
>>> I'm away tomorrow - back in on Monday.
>>>
>>> Cheers
>>> Phil
>>>
>>>
>>> From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
>>> To: "Phil Jones" <p.jones@uea.ac.uk>
>>> X-OriginalArrivalTime: 29 Aug 2007 16:53:26.0748 (UTC)
>>> FILETIME=[1E7969C0:01C7EA5D]
>>> X-UEA-Spam-Score: 0.0
>>> X-UEA-Spam-Level: /
>>> X-UEA-Spam-Flag: NO
>>>
>>> Dear Phil
>>>
>>> The paper has been sent to three reviewers. Of course I will take
>>> your comments and assessment into consideration. Indeed, if the
>>> claims are unsubstantiated, I would certainly reject the paper.
>>>

>>> I hope this clarifies your query.

>>>

>>> With best regards

>>> Benny

>>>

>>>

>>>

>>>

>>> _____

>>>

>>> From: Phil Jones [mailto:p.jones@uea.ac.uk]

>>> Sent: Wed 8/29/2007 16:51

>>> To: Peiser, Benny

>>> Subject: Re: review of E&E paper on alleged Wang fraud

>>>

>>>

>>>

>>>

>>> Benny,

>>> Energy and Environment is presumably a peer-review

> journal. Your

>>> email wasn't clear as to whether you want me to review the

>>> paper? If you

>>> want me to, will you take any notice of what I might say -

> such as

>>> reject the paper? Or has the contribution already been

reviewed?

>>>

>>> Phil

>>>

>>>

>>>

>>>

>>> At 23:17 29/08/2007, wang@climate.cestm.albany.edu wrote:

>>>

>>> >hi from beijing. thanks for the information, and i have

>>> forwarded the

>>> >file to the vp research and she wrote back to me that keenan has

>>> >violettet the confidentiality, as i have told her in the very

>>> >beginning. in any case, i am letting the university to

> handle this.

>>> >send me whatever you have and i will forward to sunya.

> keenan does

>>>>not follow on any rules at all, reasoning with him is
> useless, but
>>>>this will come back to badly hurt him.
>>>>
>>>>before i left for beijing, i wrote my official responses (see
>>>>attached). please keep it to yourself. there is no doubt
> that zeng
>>>>had access and examined the station history to pick up the 42-
> pair>>>stations. also remember that, the statements made in
> both papers
>>>>address changes in all the relevant parameters "location,
>>>>instrumentation, observation time, etc." without specifically
>>> focus on
>>>>relocation.
>>>>
>>>>sunya is going through a very careful procedure, as i request
>>> them to
>>>>do because keenan will jump on any slip in procedure.
the "fraud"
>>>>charge, which will not stand any chance, is just his strategy of
>>>>getting attention on the station relocation effect. so
> better to
>>>>start thinking along that line.
>>>>
>>>>i am here attending the meeting of The 3rd Alexander von
Humboldt
>>>>International Conference on "the East Asian monsoon, past,
>>> present and
>>>>future" in Beijing. I am going to take some time off
> travelling in
>>>>southern China after the meeting, when my wife join me this
> weekend.>>>There is a good chance that I might not have e-mail
> access. Have a
>>>>good day.
>>>>
>>>>wcw
>>>>
>>>>
>>>>
>>>>----- Original Message -----
>>>>From: Phil Jones <p.jones@uea.ac.uk>
>>>>Date: Wednesday, August 29, 2007 10:46 am
>>>>Subject: Fwd: review of E&E paper on alleged Wang fraud

>>>>
>>>>> Wei-Chyung and Tom,
>>>>>
>>>>> Just received this. I won't be responding.
>>>>>
>>>>> Knowing this journal there is no point, not even if I said
>>>>> I ought to review the paper. Peiser is a well-known skeptic
>>>>> in the UK. Not sure what to do. I guess you (WCW) should
>>>>> forward this to whoever needs to see it at Albany.
>>>>>
>>>>> If you think I should respond then I can. I will
> forward this
>>>>> to someone here, but mainly for their file.
>>>>>
>>>>> I did say the quote on p3 about 2-3 years ago. I am still
>>>>> not releasing the CRU station data collected over all the
> last>>>> 25 years.
>>>>>
>>>>> Cheers
>>>>> Phil
>>>>>
>>>>>>Subject: review of E&E paper on alleged Wang fraud
>>>>>>Date: Wed, 29 Aug 2007 15:18:04 +0100
>>>>>>X-MS-Has-Attach: yes
>>>>>>X-MS-TNEF-Correlator:
>>>>>>Thread-Topic: review of E&E paper on alleged Wang fraud
>>>>>>thread-index: AcfqPgYII3NKEW8US8uwftlkhnXNhgAB/4xQAAA5K8A=
>>>>>>From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
>>>>>>To: <p.jones@uea.ac.uk>
>>>>>>X-OriginalArrivalTime: 29 Aug 2007 14:18:06.0729 (UTC)
>>>>>>FILETIME=[6B4F5F90:01C7EA47]
>>>>>>X-UEA-Spam-Score: 0.0
>>>>>>X-UEA-Spam-Level: /
>>>>>>X-UEA-Spam-Flag: NO
>>>>>>
>>>>>>Dear Dr Jones
>>>>>>
>>>>>>I have attached a copy of Doug Keenan's paper on the alleged
>>Wang
>>>>>> fraud>that was submitted for the forthcoming issue of
> Energy &
>>>>>> Environment><http://www.ingentaconnect.com/content/mscp/ene>.
>>>>>>

>>>>>

>>>>>I was wondering whether you would be happy to comment on its
>>>>> content and
>>>>>factual accuracy. Your comments and suggestions would be
much

>>>>> appreciated. We would need your feedback by Sept 17.

>>>>>

>>>>>I look forward to hearing from you.

>>>>>

>>>>>Yours sincerely

>>>>>

>>>>>Benny Peiser

>>>>>Guest editor, E&E

>>>>>Liverpool John Moores University, UK

>>>>>

>>>>>

>>>>>

>>>>> Prof. Phil Jones

>>>>> Climatic Research Unit Telephone +44 (0) 1603 592090

>>>>> School of Environmental Sciences Fax +44 (0) 1603 507784

>>>>> University of East Anglia

>>>>> Norwich Email p.jones@uea.ac.uk

>>>>> NR4 7TJ

>>>>> UK

>>>>> -----

> ----

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>>>>>

>>>>

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> ----

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>>>

>>>

>>>
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> University of East Anglia
> Norwich Email p.jones@uea.ac.uk
> NR4 7TJ
> UK
> -----
> -----
>

From: Tom Wigley <wigley@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: review of E&E paper on alleged Wang fraud
Date: Fri, 31 Aug 2007 06:54:58 -0600

<x-flowed>

Phil,

Seems to me that Keenan has a valid point. The statements in the papers that he quotes seem to be incorrect statements, and that someone (WCW at the very least) must have known at the time that they were incorrect.

Whether or not this makes a difference is not the issue here.

Tom.

+++++

Phil Jones wrote:

- > Tom,
- > Just for interest! Keep quiet about both issues.
- >
- > In touch with Wei-Chyung Wang. Just agreed with him
- > that I will send a brief response to Peiser. The allegation by Keenan
- > has
- > gone to SUNY. Keenan's about to be told by SUNY that submitting this has
- > violated a confidentiality agreement he entered into with SUNY when he
- > sent the complaint. WCW has nothing to worry about, but it still
- > unsettling!
- > All related to a paper in Nature from 1990! Keenan ought to look at the
- > temperature data (which he has) rather than going on and on about
- > site moves.
- >
- > See the end of this email and the response about E&E and the 3
- > reviewers.
- > Amazing! We all knew the journal was awful.
- >
- > On something completely different - just agreed to review another
- > crappy
- > paper by Chappell/Agnew on Sahel Rainfall. Chappell is out of a job -
- > and still

> he tries to write papers saying the Sahel drought might not have
> happened!

>
> Both are just time wasters - but necessary to do unfortunately.

>
> Weekend away with the family now - back Monday!

>
> Cheers
> Phil

>
>> Subject: review of E&E paper on alleged Wang fraud
>> Date: Wed, 29 Aug 2007 15:18:04 +0100
>> X-MS-Has-Attach: yes
>> X-MS-TNEF-Correlator:
>> Thread-Topic: review of E&E paper on alleged Wang fraud
>> thread-index: AcfqPgYII3NKEW8US8uwftlkhnXNhgAB/4xQAAA5K8A=
>> From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
>> To: <p.jones@uea.ac.uk>
>> X-OriginalArrivalTime: 29 Aug 2007 14:18:06.0729 (UTC)
>> FILETIME=[6B4F5F90:01C7EA47]
>> X-UEA-Spam-Score: 0.0
>> X-UEA-Spam-Level: /
>> X-UEA-Spam-Flag: NO

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>> Dear Dr Jones

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>> <http://www.ingentaconnect.com/content/mscp/ene>.

>>
>>
>> I was wondering whether you would be happy to comment on its content and
>> factual accuracy. Your comments and suggestions would be much
>> appreciated. We would need your feedback by Sept 17.

>>
>> I look forward to hearing from you.

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>> Yours sincerely

>>
>> Benny Peiser
>> Guest editor, E&E
>> Liverpool John Moores University, UK

>

> Dear Phil

>

> The paper has been sent to three reviewers. Of course I will take your
> comments and assessment into consideration. Indeed, if the claims are
> unsubstantiated, I would certainly reject the paper.

>

> I hope this clarifies your query.

>

> With best regards

> Benny

>

>

>

>

>

>

> From: Phil Jones [mailto:p.jones@uea.ac.uk]

> Sent: Wed 8/29/2007 16:51

> To: Peiser, Benny

> Subject: Re: review of E&E paper on alleged Wang fraud

>

>

>

>

> Benny,

> Energy and Environment is presumably a peer-review journal. Your
> email wasn't clear as to whether you want me to review the paper? If

> you

> want me to, will you take any notice of what I might say - such as
> reject the paper? Or has the contribution already been reviewed?

>

> Phil

>

>

>

>

>

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> University of East Anglia

> Norwich Email p.jones@uea.ac.uk

> NR4 7TJ

> UK

> -----

</x-flowed>

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Fwd: paper on alleged Wang fraud
Date: Tue, 11 Sep 2007 09:02:54 -0400
Reply-to: mann@psu.edu
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

<x-flowed>

Phil,

sorry, first version of my message was a bit garbled. Here is the full message:

thanks for forwarding. It may be difficult for me to sue them over a footnote, and in fact he is very careful only to intimate accusations against me in a response to your comments. Note that he does not do so in the paper. I'm sure they know that I would sue them for that, and that I have a top lawyer already representing me.

Wei Chyung needs to sue them, or at the least threaten a lawsuit. If he doesn't, this will set a dangerous new precedent. I could put him in touch w/ anleading attorney who would do this pro bono. Of course, this has to be done quickly. The threat of a lawsuit alone my prevent them from publishing this paper, so time is of the essence. Please feel free to mention this directly to Wei Chyung, in particular that I think he needs to pursue a legal course her independent of whatever his university is doing. He cannot wait for Stony Brook to complete its internal investigations! If he does so, it will be too late to stop this.

Gavin is in Shanghai, but perhaps may be able to provide some brief thoughts himself on this,

mike

Michael E. Mann wrote:

> Phil,

>

> thanks for forwarding. It may be difficult for me to sue them over a
> footnote, and in fact he is very careful only to intimate accusations
> against me in a response to your comments. Note that he does not do so
> in the paper. I'm sure they know that I would sue them for that, and
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> this has to be done quickly. The threat of a lawsuit alone my prevent
> them from publishing this paper, so time is of the essence. Please
> feel free to mention this directly to Wei Chyung, in particular that I
> think he needs to pursue a legal course here here independent of
> whatever his university is doing. He wait for Stony Brook to complete
> its internal investigations!

>
> Gavin is in Shanghai, but hopefully

>
> Phil Jones wrote:

>> Mike, Gavin,
>> Don't pass on, just for interest. It seems as though E&E will likely
>> publish this paper. I've responded briefly, pointing out that Tao et al
>> (1991) doesn't claim that it explicitly states...

>> The response to my point 7 sums up Keenan. It also seems
>> as though he will run with the footnote 3, but it's only a footnote!
>> The fraud allegation against you Mike is only in passing!

>>
>> Wei-Chyung is in Vienna. Have forwarded this to him to pass onto
>> SUNY.

>> I wish they would conclude their assessment of malpractice.

>>
>> Cheers

>> Phil

>>
>> PS to Gavin - been following (sporadically) the CA stuff about the
>> GISS data and
>> release of the code etc by Jim. May take some of the pressure of you
>> soon, by releasing a list of the stations we use - just a list, no code
>> and no data. Have agreed to under the FOIA here in the UK.

>>
>> Oh Happy days!

>>
>>> Subject: paper on alleged Wang fraud
>>> Date: Mon, 10 Sep 2007 18:39:02 +0100
>>> X-MS-Has-Attach: yes
>>> X-MS-TNEF-Correlator:
>>> Thread-Topic: paper on alleged Wang fraud
>>> thread-index: AcfzsbCIIqEe9LxLSeGz6CASIEIWmgAHs4oa

Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: mann@psu.edu
University Park, PA 16802-5013

<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: "Burgess Jacquelin Prof \(\ENV\)" <Jacquie.Burgess@uea.ac.uk>
To: "Jones Philip Prof \(\ENV\)" <P.Jones@uea.ac.uk>
Subject: RE: Possible problem looming
Date: Tue, 11 Sep 2007 14:40:59 +0100

Thanks Phil,
I will keep your email and hope we don't have to mobilise. This is very close to harassment, isn't it.
Jacquie

-----Original Message-----

From: Phil Jones [mailto:p.jones@uea.ac.uk]
Sent: 11 September 2007 14:06
To: Burgess Jacquelin Prof (ENV)
Cc: Mcgarvie Michael Mr (ACAD)
Subject: Possible problem looming

Jacquie,

I've been in discussion with Michael over the past several months about a number of Freedom of Information (FOI) requests for CRU data. I've responded to one and will be responding to another in the next few days. Michael suggested I bring you up to speed on the issue. To cut a very long story short, I'm attaching 3 things that relate to what's happened since responding to the first request.

1. A paper from 1990 by me and others in Nature. The request was for the station data from the rural station networks in the three regions studied.

This led to a person in London (Douglas Keenan) putting some material on his website claiming fraud against one of the co-authors on the paper (Wei-Chyung Wang of the State University of Albany, SUNY, in NY, USA). He then put an allegation of fraud into SUNY against Wang. SUNY are dealing with this - not quickly, but I have seen Wang's response.

2. Keenan then submitted a paper (attached) to the world's worst journal,

Energy and Environment. According to Wang this is in breach of an agreement

with SUNY not to do anything whilst the allegation is being dealt with.

According to Wang, SUNY have told Keenan this.

I was sent the paper to comment on the factual allegations in the paper. After

discussing this with Wang (who informed SUNY) I sent 9 comments.

3. My comments - with Keenan's responses embedded within (this is the new bit for you Michael).

I have subsequently told the E&E guest editor that Keenan's response to my point

5 is wrong. I sent him Tao et al. (1991) so he can see this. Keenan's response to my point 7

illustrates his arrogance.

I have loads more background to all this, and it has taken some time over the

last few weeks and months in responding.

You are now partly up to speed on the issue. I'm away next week. I don't know when E&E might publish, nor when the SUNY review process (which is being dealt with by their Director of Research) will conclude. Wang and I both know that the allegations are groundless, but it is likely it will not look good when it first comes out. This is just

another of the attempts by climate skeptics to get the public and the media thinking that there is disagreement amongst scientists and that we shouldn't be doing anything about global warming. I will be discussing

this with some IPCC people when I meet them in early October.

Cheers

Phil

Phil,

Thanks for forwarding this. I am shocked about this - if a formal review

is underway at the University of Albany it is surely improper to publish a paper in a journal about the matter!

I suggest that you alert Jacquie Burgess to this, as the new Head of School.

I would like to suggest that we ask Dave Palmer to comment on the events on the FOIA request - I don't think I fully agree with the story presented here. Do you agree?

I also think we should alert the Press Office in due course.

Regards

Michael

Michael McGarvie
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Norwich Email p.jones@uea.ac.uk
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UK

From: "Wahl, Eugene R" <wahle@alfred.edu>
To: "Jonathan Overpeck" <jto@u.arizona.edu>
Subject: RE: Wahl & Ammann AND Ammann & Wahl papers
Date: Thu, 13 Sep 2007 18:34:11 -0400
Cc: "Eystein Jansen" <eystein.jansen@geo.uib.no>, <t.osborn@uea.ac.uk>, "Keith Briffa" <k.briffa@uea.ac.uk>

Hello Peck, Eystein, Tim, Keith:

Please find attached the e-versions of the WA and AW papers re: the "hockey-stick". These are now available as "to-come-in-print" articles from Climatic Change. I believe the WA one was just loaded yesterday. As I understand it, official "print" publication will be this November. These versions HAVE gone through the author proof process, and thus I anticipate no possibility of them being further changed before print publication.

Note brief correspondence yesterday with Phil Jones re: proof-level changes that were made to WA (copied below).

Peace, Gene

Dr. Eugene R. Wahl
Assistant Professor of Environmental Studies
Division of Environmental Studies and Geology
Alfred University

One Saxon Drive
Alfred, NY 14802
607.871.2604

From: Wahl, Eugene R
Sent: Wednesday, September 12, 2007 6:44 PM
To: 'Phil Jones'; Caspar Ammann
Subject: RE: Wahl/Ammann

Hi Phil:

There were inevitably a few things that needed to be changed in the final version of the WA paper, such as the reference to the GRL paper that was not published (replaced by the AW paper here), two or three additional pointers to the AW paper, changed references of a Mann/Rutherford/Wahl/Ammann paper from 2005 to 2007, and a some other very minor grammatical/structural things. I tried to keep all of this

to the barest minimum possible, while still providing a good reference structure. I imagine that MM will make the biggest issue about the very existence of the AW paper, and then the referencing of it in WA; but that was simply something we could not do without, and indeed AW does a good job of contextualizing the whole matter.

Steve Schneider seemed well satisfied with the entire matter, including its intellectual defensibility (sp?) and I think his confidence is warranted. That said, any other thoughts/musings you have are quite welcome.

Peace, Gene

-----Original Message-----

From: Phil Jones [mailto:p.jones@uea.ac.uk]
Sent: Wednesday, September 12, 2007 11:30 AM
To: Wahl, Eugene R; Caspar Ammann
Subject: Wahl/Ammann

Gene/Caspar,

Good to see these two out. Wahl/Ammann doesn't appear to be in CC's online first, but comes up if you search.

You likely know that McIntyre will check this one to make sure it hasn't changed since the IPCC close-off date July 2006!

Hard copies of the WG1 report from CUP have arrived here today.

Ammann/Wahl - try and change the Received date! Don't give those skeptics something to amuse themselves with.

Cheers

Phil

Attachment Converted: "c:\eudora\attach\Ammann_ClimChange2007.pdf"

Attachment Converted: "c:\eudora\attach\Wahl_ClimChange2007.pdf"

From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: recent WSJ article
Date: Fri, 14 Sep 2007 15:26:13 -0400
Reply-to: mann@psu.edu
Cc: Phil Jones <p.jones@uea.ac.uk>, Richard Somerville <richard.somerville@wanadoo.fr>

Kevin,
can you send me the link once its up?
thanks,
Mike
Kevin Trenberth wrote:

Mike
You should have seen the first version. I drafted that yesterday and then today toned it down. I did add a couple of points, including the link you suggested. Will try to send off later today but just to nature.com
Thanks
Kevin
Michael E. Mann wrote:

guys, I've got a few minutes before I have to head out again.
Kevin--thanks for helping return the Nature blog to respectability after a dubious start...I'd like to direct RealClimate readers to your piece as soon as it is up, so please let me know when that happens...
Looks like Phil has hit several of the key points, but here are a few more:
1. The 'discrediting' that Akasofu cites has been discredited. IPCC Chapter 6 rejected the McIntyre and McKittrick's claims in no uncertain terms, referencing the Wahl and Ammann work (reprints attached) who show that (a) the reconstruction is readily reproducible and (b) McIntyre and McKittrick only failed to reproduce the reconstruction because of multiple errors on their part. This is true in addition to the more general point that Kevin has made (that multiple independent studies confirm and in fact now extend the previous conclusions, rather than contradict them).
2. To the extent that the "LIA" and "MWP" can be meaningfully defined, there has been much work (published in Nature, Science, etc.) showing that the main variations (both in terms of hemispheric mean changes and spatial patterns) can indeed be explained in terms of the response of the climate system to natural radiative forcing changes (solar and volcanism). Only someone completely unfamiliar with the advances of the past ten years in climate science would claim that there are no explanations for these.
3. Continuing in this theme, to claim that the modern warming is some sort of 'rebound' reflects a thorough apparent lack of understanding of how the climate system works. The climate doesn't rebound. It responds (with some lag) to changes in radiative forcing. The

main patterns of variation of past centuries have been explained in terms of such responses to natural radiative forcing changes. As shown in countless studies, the late 20th century warming can only be explained in terms of the response to anthropogenic changes in radiative forcing. Kevin has more or less already made this point, in different words, in the current draft.

4. The bogus talking point that co2 lagging the warming in the ice cores has been debunked countless times before, and its an embarassment that it continues to be raised by one who ostensibly considers himself a scientist. This is total nonsense, and a nice refutation has been provided by Eric Steig on RealClimate here:

[1]<http://www.realclimate.org/index.php/archives/2007/04/the-lag-between-temp-and-co2/>
Perhaps worth just linking to that explanation?

Kevin, perhaps you're too gentle in attributing this simply to some 'confusion' about the facts. Either Mr. Akasofu has literally no familiarity whatsoever with the advances in climate science of the past two decades, or he has intentionally sought to deceive. In either case, his piece is embarassment.

Finally, let me withdraw my initial suggestion. For strategic reasons, it might make sense to submit this as letter to editor to WSJ (easy and quick to do online), and then publish it on the Nature blog in short order. I sea that as win-win because you can either call the WSJ for refusing to run your letter (which is very likely what will happen), or use the Nature blog piece to draw attention to your letter, should WSJ actually choose to publish your letter...

please don't hesitate to let me know if I can be of any further help here. Will be back online a bit later today,

mike

Phil Jones wrote:

Kevin,

A few quick thoughts. Article is awful as we all know.

It is important to learn about past climate change, especially over the past 1000 years, but it is even important to use new and improved evidence from proxy sources (i.e. not to cling to outdated concepts of the past such as the MWP and LIA). How can we ever hope to progress if we have conform to incorrect concepts?

On the early mid-20th century warming - look at the figures in Ch 9. The decrease from 1940-75 didn't happen if you look at global records. MBH was published in 1998 and wasn't just a tree-ring study. The Thames doesn't and never did freeze solid. It did so 25 times between 1400 and 1820. Only about 5-6 of these were frost fairs. Most of these have CET data, so what is the use of the freeze dates!

He plucks various figures out of the air!

I think the reductions in Arctic sea ice this summer/September are

alarming. They are 20% below the 2005 record. He comes from Alaska. Has he not seen the effects on the coast there?

Cheers

Phil

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich Email [2]p.jones@uea.ac.uk

NR4 7TJ

UK

--

Michael E. Mann

Associate Professor

Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075

503 Walker Building FAX: (814) 865-3663

The Pennsylvania State University email: [3]mann@psu.edu

University Park, PA 16802-5013

[4]<http://www.met.psu.edu/dept/faculty/mann.htm>

--

Kevin E. Trenberth e-mail: [5]trenbert@ucar.edu

Climate Analysis Section, NCAR [6]www.cgd.ucar.edu/cas/

P. O. Box 3000, (303) 497 1318

Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

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Associate Professor

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The Pennsylvania State University email: [7]mann@psu.edu
University Park, PA 16802-5013

[8]<http://www.met.psu.edu/dept/faculty/mann.htm>

References

1. <http://www.realclimate.org/index.php/archives/2007/04/the-lag-between-temp-and-co2/>
2. <mailto:p.jones@uea.ac.uk>
3. <mailto:mann@psu.edu>
4. <http://www.met.psu.edu/dept/faculty/mann.htm>
5. <mailto:trenbert@ucar.edu>
6. <http://www.cgd.ucar.edu/cas/>
7. <mailto:mann@psu.edu>
8. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Peter Thorne <peterwthorne@btinternet.com>
To: "Smith, Fiona" <fiona.smith@metoffice.gov.uk>
Subject: Re: URGENT: Press office ...
Date: Thu, 4 Oct 2007 22:08:49 +0000 (GMT)
Cc: p.jones@uea.ac.uk

Thanks Fiona, I am cc'ing in Phil who will let relevant people at UEA know. Please can you get press office to advise if I will have to be in during next week or whether solely being on my mobile will suffice. I am flexible on the TOIL next week Tuesday onwards (land Monday at 06.00) but would like to know by the time I leave if poss. Just to remind that my mobile is 07834034418.

Cheers

Peter

----- Original Message -----

From: "Smith, Fiona" <fiona.smith@metoffice.gov.uk>
To: Peter Thorne <peterwthorne@btinternet.com>
Cc: "Gromett, Barry" <barry.gromett@metoffice.gov.uk>
Sent: Friday, 5 October, 2007 1:40:04 AM
Subject: RE: URGENT: Press office ...

Peter,

Sorry for the delay. The head of the press office was off sick for a few days and they have been incredibly busy.

Yes, the Press Office will go ahead with a press release and we will contact UEA to make sure we have a consistent message.

Will let you see any relevant communication.

Fiona

Fiona Smith
Met Office Hadley Centre for Climate Change
FitzRoy Road Exeter EX1 3PB United Kingdom
Tel: +44 (0) 1392 884240
E-mail: fiona.smith@metoffice.gov.uk [1]<http://www.metoffice.gov.uk>

From: Peter Thorne [mailto:peterwthorne@btinternet.com]

Sent: Thursday, October 04, 2007 9:26 AM

To: Smith, Fiona

Subject: URGENT: Press office ...

intentional silence? I need a decision ASAP to plan next week and let Phil Jones and UEA know. Please request resolution on whether we will run something or not so wheels can be set rolling if necessary.

Thanks

Peter

References

1. <http://www.metoffice.gov.uk/>

From: carl mears <mears@remss.com>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Tue, 04 Dec 2007 14:17:24 -0800
Cc: Phil Jones <p.jones@uea.ac.uk>, santer1@llnl.gov, Tom Wigley <wigley@cgd.ucar.edu>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, John Lanzante <John.Lanzante@noaa.gov>, Karl Taylor <taylor13@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Frank Wentz <frank.wentz@remss.com>

<x-flowed>

But you are assuming that there is no noise (instrumental or "weather") in the observations.

-Carl

At 01:57 PM 12/4/2007, Tom Wigley wrote:

>All,

>

>Depends on whether the runs are independent. Are models independent?

>

>A billion runs would indeed reduce the statistical uncertainty to near zero. What is left (if one compared with absolutely correct observed data)

>is the mean model bias.

>

>Tom.

>

>+++++

>

>carl mears wrote:

>

>>Hi Ben, Phil and others

>>

>>To me, the fundamental error is 2.3.1. Expecting the observed values to

>>lie within

>>+/- 2*sigma(SE) (i.e. sigma/(sqrt(N-1)) of the distribution of N model trends) is just

>>wrong.

>>If this were correct, we could just run the models a lot of times, say a

>>billion or so, and have a

>>very, very, very small sigma(SE) (assuming the sigma didn't grow much) and we'd never

>>have "agreement" with anything. Absurd.

>>

>>Does IJC publish comments?

>>

>>-Carl

>>

>>At 02:09 AM 12/4/2007, Phil Jones wrote:

>>
>>> Ben,
>>> It sure does! Have read briefly - the surface arguments are wrong.
>>> I know editors have difficulty finding reviewers, but letting this
one
>>> pass is awful - and IJC was improving.
>>>
>>> Cheers
>>> Phil
>>>
>>>
>>>At 17:53 30/11/2007, Ben Santer wrote:
>>>
>>>>Dear folks,
>>>>
>>>>I'm forwarding this to you in confidence. We all knew that some
>>>>journal, somewhere, would eventually publish this stuff. Turns out
that
>>>>it was the International Journal of Climatology. Strengthens the need
>>>>for some form of update of the Santer et al. (2005) Science paper.
>>>>
>>>>With best regards,
>>>>
>>>>Ben
>>>>-----

>>>>Benjamin D. Santer
>>>>Program for Climate Model Diagnosis and Intercomparison
>>>>Lawrence Livermore National Laboratory
>>>>P.O. Box 808, Mail Stop L-103
>>>>Livermore, CA 94550, U.S.A.
>>>>Tel: (925) 422-2486
>>>>FAX: (925) 422-7675
>>>>email: santer1@llnl.gov
>>>>-----

>>>>
>>>>
>>>>
>>>>X-Account-Key: account1
>>>>Return-Path: <anrevk@nytimes.com>
>>>>Received: from mail-2.llnl.gov ([unix socket])
>>>> by mail-2.llnl.gov (Cyrus v2.2.12) with LMTPA;
>>>> Fri, 30 Nov 2007 08:39:49 -0800
>>>>Received: from smtp.llnl.gov (nspiron-3.llnl.gov [128.115.41.83])
>>>> by mail-2.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.6 \$) with
>>>> ESMTP id 1AUGdl5E004790
>>>> for <santer1@mail.llnl.gov>; Fri, 30 Nov 2007 08:39:48 -0800
>>>>X-Attachments: DCPS-proofs_IJC07.pdf
>>>>X-IronPort-AV: E=McAfee;i="5100,188,5173"; a="21323766"
>>>>X-IronPort-AV: E=Sophos;i="4.23,235,1194249600";
>>>> d="pdf'?scan'208,217";a="21323766"
>>>>Received: from nsziron-1.llnl.gov ([128.115.249.81])
>>>> by smtp.llnl.gov with ESMTP; 30 Nov 2007 08:39:47 -0800

>>>>X-Attachments: DCPS-proofs_IJC07.pdf
>>>>X-IronPort-AV: E=McAfee;i="5100,188,5173"; a="6674079"
>>>>X-IronPort-AV: E=Sophos;i="4.23,235,1194249600";
>>>> d="pdf'?scan'208,217";a="6674079"
>>>>Received: from smtp-nv-vipl.nytimes.com (HELO nytimes.com)
>>>>([199.181.175.116])
>>>> by nsziron-1.llnl.gov with ESMTP; 30 Nov 2007 08:39:43 -0800
>>>>Message-Id: <6.2.5.6.2.20071130111858.03540590@nytimes.com>
>>>>X-Mailer: QUALCOMM Windows Eudora Version 6.2.5.6
>>>>Date: Fri, 30 Nov 2007 11:38:52 -0500
>>>>To: santer1@llnl.gov, broccoli@envsci.rutgers.edu, mears@remss.com
>>>>From: Andrew Revkin <anrevk@nytimes.com>
>>>>Subject: sorry to take your time up, but really do need a scrub of
this
>>>> singer/christy/etc effort
>>>>Mime-Version: 1.0
>>>>Content-Type: multipart/mixed;
>>>> boundary="====_67524015==_"
>>>>X-NYTOriinatingHost: [10.149.144.50]
>>>>
>>>>hi,
>>>>for moment please do not distribute or discuss.
>>>>trying to get a sense of whether singer / christy can get any
traction
>>>>with this at all.
>>>>
>>>>
>>>>
>>>>*_ ANDREW C. REVKIN
>>>><<http://www.nytimes.com/revkin>>*_The New York Times / Environment /
Dot
>>>>Earth <<http://dotearth.blogs.nytimes.com/>>Blog
>>>><<http://dotearth.blogs.nytimes.com/>>620 Eighth Ave., NY, NY 10018-
1405
>>>>phone: 212-556-7326 fax: 509/ /-357-0965 mobile: 914-441-5556
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>>>>School of Environmental Sciences Fax +44 (0) 1603 507784
>>>>University of East Anglia
>>>>Norwich Email p.jones@uea.ac.uk
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From: "Michael E. Mann" <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: [Fwd: Re: Even more on Loehle's 2000 year climate analysis]
Date: Wed, 05 Dec 2007 11:37:40 -0500
Reply-to: mann@psu.edu

well put Phil,

I think you've put your finger right on it. JGR-Atmospheres has been publishing some truly awful papers lately; we responded (Gavin, me, James Annan) to the awful Schwartz sensitivity estimate paper, but there are so many other bad papers that are appearing there (Chylak, etc.) that its just impossible to respond to them all.

I hadn't seen this latest one though. McKitrick and Michaels team up again, wow! maybe McKitrick has figured ou the difference between radians and degrees this time!

talk to you later,

mike

Phil Jones wrote:

Mike,

Also I see him writing things - then people saying you should write this up for a paper, as though it can be knocked up in an afternoon. He realises he can't do this - as it takes much longer. Then we wastes more and more time opening up new threads. He doesn't seem clever enough to realise this.

Gavin and Rasmus have seen the attached piece of garbage! UAH is correct, therefore the land surface must be wrong. Let's adjust it for a dodgy reason - ah, it now agrees with UAH. Let's forget that the land now disagrees with the ocean surface. If only I'd thought of that first, I could have not bothered with the awful analysis. If only I'd just believed RSS in the first place.

Cheers

Phil

At 15:16 05/12/2007, you wrote:

HI Phil,

thanks--thats good.

Re, Loehle, McIntyre. Funny--w/ each awful paper E&E publishes, McIntyre realizes that it compromises the integrity of his own "work" even further. He can't distance himself from E&E much as he'd like to. He also seems to be losing lots of credibility now w/ all but his most loyal followers, which is good to see...

mike

Phil Jones wrote:

Mike,

Yes the 1990 graphic is in an Appendix. The last few are being regularly hassled by Thorsten. The guy from EPRI (Larry) really wants something submitted soon. So working here to get something in by end of Jan. Keith is going to get it fast-tracked through the Holocene - well that's the plan.

The Loehle paper is awful as you know. So is another article on the IPCC process in E&E. I did look at Climate Audit a week or two back - I got the impression that McIntyre is trying to distance himself from some of these E&E articles by saying we have to be equally skeptical about them as well.

Cheers

Phil

At 14:00 04/12/2007, you wrote:

Hey Phil,

thanks--nice coincidence in timing. So the 1990 graphic will be discussed in this review paper, right? Perfect, I'll let Gavin know.

Will look into the AGU fellowship situation ASAP.

I don't read E&E, gives me indigestion--I don't even consider it peer-reviewed science, and in my view we should treat it that way. i.e., don't cite, and if journalists ask us about a paper, simply explain its not peer-reviewed science, and Sonja B-C, the editor, has even admitted to an anti-Kyoto agenda!

I do hope that Wei-Chyung pursues legal action here.

So didn't see this recent paper, nor have I heard about the IJC paper, Christy and Spencer continue to lose more and more scientific credibility with each awful paper they publish.

Gavin is planning to do something on the Loehle paper on RealClimate, I'm staying away from it. I have a revised set of hemispheric reconstructions which I'll send you soon, its basically what I showed at AGU last year. Submitted to PNAS--more soon on that, mike

Phil Jones wrote:

Mike,

Some text came last night from Caspar. Keith/Tim writing their parts still. I have text from Francis, so almost all here now. Still need to find some time - maybe the Christmas/New Year break here - to put it all together. There is so much else going on here at the moment with other papers, it will be hard to find some time. I wish they had all responded much sooner!

As for AGU - just getting one of their Fellowships would be fine.

I take it you've seen the attached in E&E. I've not heard any more from Wei-Chyung in the past couple of months. I'm working on a paper on urbanization. I can show China is hardly affected. Will send for you to look over when I have it in a form that is sendable. Would appreciate

your thoughts on how I will have said things.

Have another awful pdf of a paper accepted in IJC !! It ws rejected by all three reviewers for GRL! It is by Douglass, Christy , Singer et al - thus you'll know what it is on.

Have booked flights for Tahiti in April, just need to do the hotel now.

Cheers

Phil

Cheers

Phil

At 02:07 04/12/2007, you wrote:

Hi Phil,

I hope things are going well these days, and that the recent round of attacks have died down. seems like some time since I've heard from you.

Please see below: Gavin was wondering if there is any update in status on this?

By the way, still looking into nominating you for an AGU award, I've been told that the Ewing medal wouldn't be the right one. Let me know if you have any particular options you'd like me to investigate...

thanks,

mike

----- Original Message -----

Subject: Re: Even more on Loehle's 2000 year climate analysis

Date: 03 Dec 2007 20:59:58 -0500

From: Gavin Schmidt [1]<gschmidt@giss.nasa.gov>

To: Michael E. Mann [2]<mann@psu.edu>

References: [3]<3.0.3.32.20071203130209.0123fd18@mail.skybest.com>

[4]<3.0.3.32.20071202224717.012384a8@mail.skybest.com>

[5]<3.0.3.32.20071201123550.01237954@mail.skybest.com>

[6]<3.0.3.32.20071201123550.01237954@mail.skybest.com>

[7]<3.0.3.32.20071202224717.012384a8@mail.skybest.com>

[8]<3.0.3.32.20071203130209.0123fd18@mail.skybest.com>

[9]<3.0.3.32.20071203141259.0126c33c@mail.skybest.com>

[10]<475457F3.9070102@meteo.psu.edu>

this reminds me. What's the status of Phil Jones and Caspar's investigation of the IPCC90 curve? Phil wanted us to hold off for some reason, but is that done with?

That's a great story that needs to be told.

Gavin

On Mon, 2007-12-03 at 14:24, Michael E. Mann wrote:

> thanks Eric,

>
> That's great. I've again copied in Gavin so that he has this info too.
>
> Will keep you in the loop!
>
> mike
>
> Eric Swanson wrote:
>> Hi Mike,
>>
>> I do hope you all are able to put this all together.
>> There were several comments on CA about RealClimate, suggesting that
>> RC wouldn't say anything, as E&E publication has such a bad rap.
>>
>> Perhaps my biggest complaint was also one mentioned by another
>> poster
>> on CA. I don't like using a simple linear interpolation between
>> data points for these series where there are many years between
>> samples.
>> Here's the other fellow's comments:
>>
>>

[11]
<http://www.climateaudit.org/?p=2380#comment-162478>
>>

[12]
<http://www.climateaudit.org/?p=2380#comment-162654>
>>

[13]

<http://www.climateaudit.org/?p=2380#comment-162665>

>>

>> I would go further than that. These data sets represent samples of

>> time records. The sampling does not produce a value for a single

>> year.

>> Rather, each sample represents some number of years of the variable

>> as averaged in the process of collecting the material to be analyzed.

>>

>> Consider an ocean sediment core, such as Keigwin's data. The subcores

>> are sampled every 1.0 cm. Assume the material is taken with a device

>> that

>> collects mud from a 0.4 cm area along the core. Thus, the sample

>> would

>> contain 4/10 of the material deposited at that 1 cm per sample rate

>> of

>> change in time. If the age/depth model at that point yields a 100

>> year

>> per cm rate, then the sample would represent an average over 40

>> years.

>> Simple linear interpolation assumes a continuously varying change

>> between

>> the points, while the sampling process would give a brief 40 year

>> value

>> with the other 60 years being unknown. What if the entire cm of the

the

>> core

>> were analyzed? One would not know unless one had contacted each

>> research

>> group that did the analysis and requested more information
than
that
>> which
>> might be found in the published reports.
>>
>> NOTE: I looked at Keigwin's data when I wrote a comment on
Loehle's
>> 2004 paper
>>
>> Comments on "Climate change: detection and attribution of
trends
>> from long-term
>> geologic data" by C. Loehle [Ecological Modelling 171 (4)
(2004)
>> 433-450],
>> Ecological Modelling 192 (2006) 314-316
>>
>> You may add my name to the list for what it's worth.
>>
>> Best Regards,
>>
>> Eric Swanson
>> -----
>> At 01:18 PM 12/3/07 -0500, you wrote:
>> >>>>
>> Eric--this is
great, thanks for all of the info. I've taken
>> the liberty of
forwarding to Gavin, as we're thinking of
>> doing an RC
post on this, and this would be very useful. We
>> should
certainly list you as a "co-author" on this, if thats
>> ok w/ you?
>>
>> Looking
forward
to hearing what else you find here!
>>
>> mike
>>
>>

>
>
>
> --
> Michael E. Mann
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> Director, Earth System Science Center (ESSC)
>
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References

Visible links

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2. <mailto:mann@psu.edu>
3. <mailto:3.0.3.32.20071203130209.0123fd18@mail.skybest.com>
4. <mailto:3.0.3.32.20071202224717.012384a8@mail.skybest.com>
5. <mailto:3.0.3.32.20071201123550.01237954@mail.skybest.com>
6. <mailto:3.0.3.32.20071201123550.01237954@mail.skybest.com>
7. <mailto:3.0.3.32.20071202224717.012384a8@mail.skybest.com>
8. <mailto:3.0.3.32.20071203130209.0123fd18@mail.skybest.com>
9. <mailto:3.0.3.32.20071203141259.0126c33c@mail.skybest.com>
10. <mailto:475457F3.9070102@meteo.psu.edu>
11. <http://www.climateaudit.org/?p=2380#comment-162478>
12. <http://www.climateaudit.org/?p=2380#comment-162654>
13. <http://www.climateaudit.org/?p=2380#comment-162665>
14. <mailto:mann@psu.edu>
15. <http://www.met.psu.edu/dept/faculty/mann.htm>
16. <mailto:mann@psu.edu>
17. <http://www.met.psu.edu/dept/faculty/mann.htm>
18. <mailto:p.jones@uea.ac.uk>
19. <mailto:mann@psu.edu>
20. <http://www.met.psu.edu/dept/faculty/mann.htm>
21. <mailto:p.jones@uea.ac.uk>
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Hidden links:

27. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Ben Santer <santer1@llnl.gov>
To: Peter Thorne <peter.thorne@metoffice.gov.uk>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Wed, 05 Dec 2007 13:04:05 -0800
Reply-to: santer1@llnl.gov
Cc: Carl Mears <mears@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>, Steve Sherwood <Steven.Sherwood@yale.edu>, John Lanzante <John.Lanzante@noaa.gov>, Dian Seidel <dian.seidel@noaa.gov>, Melissa Free <melissa.free@noaa.gov>, Frank Wentz <frank.wentz@remss.com>, Steve Klein <klein21@mail.llnl.gov>

<x-flowed>

Dear folks,

Thank you very much for all of your emails, and my apologies for the delay in replying - I've been on travel for much of the past week.

Peter, I think you've done a nice job in capturing some of my concerns about the Douglass et al. paper. Our CCSP Report helped to illustrate that there were large structural uncertainties in both the radiosonde- and MSU-based estimates of tropospheric temperature change. The scientific evidence available at the time we were finalizing the CCSP Report - from Sherwood et al. (2005) and the (then-unpublished) Randel and Wu paper - strongly suggested that a residual cooling bias existed in the sonde-based estimates of tropospheric temperature change. As you may recall, we showed results from both the RATPAC and HadAT2 radiosonde datasets in the CCSP Report and the Santer et al. (2005) Science paper. From the latter (see, e.g., our Figure 3B and Figures 4C,D), it was clear that there were physically-significant differences between the simulated temperature trends in the tropical lower troposphere (over 1979 to 1999) and the trends estimated from RATPAC, HadAT2, and UAH data. In both the Science paper and the CCSP Report, we judged that residual biases in the observations provided the most likely explanation for these model-versus-data trend discrepancies.

Douglass et al. come to a fundamentally different conclusion, and ascribe model-versus-data differences to model error. They are not really basing this conclusion on new model data or on new observational data. The only "new" observational dataset that they use is an early version of Leo Haimberger's radiosonde dataset (RAOBCORE v1.2). Leo's dataset was under development at the time all of us were working on the CCSP Report and the Santer et al. Science paper. It was not available for our assessment in 2005. As Leo has already shared with you, newer versions of RAOBCORE (v1.3 and v1.4) show amplification of surface warming in the tropical troposphere, in reasonable agreement with the model results that we presented in Fig. 3B of our Science paper. Douglass et al. did not use these newer versions of RAOBCORE v1.2. Nor did Douglass et al. use any "inconvenient" observational datasets (such as the NESDIS-based MSU T2 dataset of Zou et al., or the MSU T2 product of Vinnikov and Grody) showing pronounced tropospheric warming over the satellite era. Nor did Douglass et al. discuss the "two timescale issue"

that formed an important part of our Science paper (i.e., how could models and multiple observational datasets show amplification behavior that was consistent in terms of monthly variability but inconsistent in terms of decadal trends?) Nor did Douglass et al. fairly portray results from Peter's 2007 GRL paper. In my personal opinion, Douglass et al. have ignored all scientific evidence that is in disagreement with their view of how the real world should be behaving.

I don't think it's a good strategy to submit a response to the Douglass et al. paper to the International Journal of Climatology (IJC). As Phil pointed out, IJC has a large backlog, so it might take some time to get a response published. Furthermore, Douglass et al. probably would be given the final word.

My suggestion is to submit (to Science) a short "update" of our 2005 paper. This update would only be submitted AFTER publication of the four new radiosonde-based temperature datasets mentioned by Peter. The update would involve:

- 1) Use of all four new radiosonde datasets.
- 2) Use of the latest versions of the UAH and RSS TLT data, and the latest versions of the T2 data from UAH, RSS, UMD (Vinnikov and Grody), and NESDIS (Zou et al.).
- 3) Use of the T2 data in 2) above AND the UAH and RSS T4 data to calculate tropical "TFu" temperatures, with all possible combinations of T4 and T2 datasets (e.g., RSS T4 and UMD T2, UAH T4 and UMD T2, etc.)
- 4) Calculating synthetic MSU temperatures from all model 20c3m runs currently available in the IPCC AR4 database. Calculation of synthetic MSU temperatures would rely on a method suggested by Carl (using weighting functions that depend on both the surface type [land, ocean] and the surface pressure at each grid-point) rather than on the static global-mean weighting function that we used previously. This is probably several months of work - but at least it will keep me off the streets and out of trouble.
- 5) Formal determination of statistical significance of model-versus-observed trend differences.
- 6) Brief examination of timescale-dependence of amplification factors.
- 7) As and both Peter and Melissa suggested, brief examination of sensitivity of estimated trends to the selected analysis period (e.g., use of 1979 to 1999; use of 1979 to 2001 or 2003 [for the small number of model 20c3m runs ending after 1999]; use of data for the post-NOAA9 period).

This will be a fair bit of effort, but I think it's worth it. Douglass et al. will try to make maximum political hay out of their IJC paper - which has already been sent to Andy Revkin at the New York Times. You can bet they've sent it elsewhere, too. I'm pretty sure that our colleague JC will portray Douglass et al. as definitive "proof" that all

climate models are fundamentally flawed, UAH data are in amazing agreement with sonde-based estimates of tropospheric temperature change, global warming is not a serious problem, etc.

One of the most disturbing aspects of Douglass et al. is its abrupt dismissal of the finding (by Sherwood et al. and Randel and Wu) of a residual tropospheric cooling bias in the sonde data. Douglass et al. base this dismissal on the Christy et al. (2007) JGR paper, and on Christy's finding of biases in the night-time sonde data that magically offset the biases in the day-time data. Does that sound familiar? When did we last hear about new biases magically offsetting the effect of recently-discovered biases? As Yogi Berra would say, this is *deja vu* all over again....

I hope that one of the papers on the new sonde-based datasets directly addresses the subject of 'error compensation' in the day-time and night-time sonde data. This would be important to do.

It's unfortunate that Douglass et al. will probably be published well before the appearance of the papers on the new radiosonde datasets, and before an updated comparison of modeled-and observed tropospheric temperature trends.

I'd be grateful if you could let me know whether you are in agreement with the response strategy I've outlined above, and would like to be involved with an update of our 2005 Science paper.

With best regards,

Ben

Peter Thorne wrote:

> All,

>

> There are several additional reasons why we may not expect perfect
> agreement between models and obs that are outlined in the attached
> paper.

>

> It speaks in part to the trend uncertainty that Carl alluded to -
taking

> differences between linear trend estimates is hard when the underlying
> series is noisy and perhaps non-linear. Work that John and Dian have
> done also shows this. Taking the ratio between two such estimates is
> always going to produce noisy results over relatively short trend
> periods when the signal is small relative to the natural variability.

>

> Also, 1979 as a start date may bias those estimates towards a "bias", I
> believe (this is unproven) because of endpoint effects due to natural
> variability that tend to damp the ratio of Trop/Surf trends (ENSO
> phasing and El Chichon) for any trend period with this start date.

Given

> the N-9 uncertainty a reasonable case could be made for an evaluation
of

> the obs that started only after N-9 and this may yield a very different
> picture.

>
> It also shows that the model result really is constrained to perturbed
> physics, at least for HadCM3. Unsurprising as convective adjustment is
> at the heart of most models. Certainly ours anyway. This result was
> cherry-picked and the rest of the paper discarded by Douglass et al.
>
> In addition to this, the state of play on the radiosondes has moved on
> substantially with RAOBCORE 1.4 (accepted I believe, Leo Haimberger
> should be in this - I'm adding him) which shows warming intermediate
> between UAH and RSS and I know of three additional efforts on
> radiosondes all of which strongly imply that the raobs datasets used in
> this paper are substantially under-estimating the warming rate (Steve
> Sherwood x2 and our automated system). So, there's going to be a whole
> suite of papers hopefully coming out within the next year or so that
> imply we at least cannot rule out from the radiosonde data warming
> consistent even with the absurd "mean of the model runs" criteria that
> is used in this paper.
>
> For info, our latest results imply a true raobs trend for 2LT in the
> tropics somewhere $>0.08\text{K/decade}$ (we cannot place a defensible upper
> limit) ruling out most of the datasets used in the Douglass paper and
> ruling in possibility of consistency with models.
>
> Douglass et al also omit the newer MSU studies from the NESDIS group
> which in the absence of a reasonable criteria (a criteria I think we
are
> some way away from still) to weed out bad obs datasets should be
> considered. Placing all obs datasets and the likely new raobs datasets
> would pretty much destroy this paper's main point. There's been a fair
> bit of cherry picking on the obs side which needs correcting here.
>
> Peter
>
> On Tue, 2007-12-04 at 15:40 -0800, carl mears wrote:
>> Karl -- thanks for clarifying what I was trying to say
>>
>> Some further comments.....
>>
>> At 02:53 PM 12/4/2007, Karl Taylor wrote:
>>> Dear all,
>>> 2) unforced variability hasn't dominated the observations.
>> But on this short time scale, we strongly suspect that it has
>> dominated. For example, the
>> 2 sigma error bars from table 3.4, CCSP for satellite TLT are 0.18
(UAH) or
>> 0.19 (RSS), larger
>> than either group's trends (0.05, 0.15) for 1979-2004. These were
>> calculated using a "goodness
>> of linear fit" criterion, corrected for autocorrelation. This is a
>> probably a reasonable
>> estimate of the contribution of unforced variability to trend
uncertainty.
>>
>>

>>
>>> Douglass et al. have *not* shown that every individual model is in fact
>>> inconsistent with the observations. If the spread of individual model
>>> results is large enough and at least 1 model overlaps the observations,
>>> then one cannot claim that all models are wrong, just that the mean is biased.
>>
>> Given the magnitude of the unforced variability, I would say "the mean
>> *may* be biased." You can't prove this
>> with only one universe, as Tom alluded. All we can say is that the
>> observed trend cannot be proven to
>> be inconsistent with the model results, since it is inside their range.
>>
>> It we interesting to see if we can say anything more, when we start culling
>> out the less realistic models,
>> as Ben has suggested.
>>
>> -Carl
>>
>>
>>
>>

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From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Wed, 05 Dec 2007 14:19:17 -0800
Reply-to: santer1@llnl.gov
Cc: carl mears <mears@remss.com>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@ucar.edu>, Tom Wigley <wigley@cgd.ucar.edu>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, John Lanzante <John.Lanzante@noaa.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Frank Wentz <frank.wentz@remss.com>, Steve Klein <klein21@mail.llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, peter gleckler <gleckler1@llnl.gov>

<x-flowed>
Dear Phil,

Just a quick response to the issue of "model weighting" which you and Carl raised in your emails.

We recently published a paper dealing with the identification of an anthropogenic fingerprint in SSM/I-based estimates of total column water vapor changes. This was a true multi-model detection and attribution ("D&A") study, which made use of results from 22 different A/OGCMs for fingerprint and noise estimation. Together with Peter Gleckler and Karl Taylor, I'm now in the process of repeating our water vapor D&A study using a subset of the original 22 models. This subset will comprise 10-12 models which are demonstrably more successful in capturing features of the observed mean state and variability of water vapor and SST - particularly features crucial to the D&A problem (such as the low-frequency variability). We've had fun computing a whole range of metrics that might be used to define such a subset of "better" models. The ultimate goal is to determine the sensitivity of our water vapor D&A results to model quality. I think that this kind of analysis will be unavoidable in the multi-model world in which we now live. Given substantial inter-model differences in simulation quality, "one model, one vote" is probably not the best policy for D&A work!

Once we've used Carl's method to calculate synthetic MSU temperatures from the IPCC AR4 20c3m data (as described in my previous email), it should be relatively easy to do a similar "model culling" exercise with MSU T2, T4, and TLT. In fact, this is what we had already planned to do in collaboration with Carl and Frank.

One key point in any model weighting or selection strategy is to avoid circularity. In the D&A context, it would be impermissible to include information on trend behavior as a criterion used for selecting "better" models. Likewise, if our interest is in assessing the statistical significance of model-versus-observed trend differences, we can't use model performance in simulating "observed" tropospheric or stratospheric trends (whatever those might be!) as a means of identifying more credible models.

A further issue, of course, is that we are relying on results from fully coupled A/OGCMs, and are making trend comparisons over relatively short periods (several decades). On these short timescales, estimates of the "true" trend in response to the applied 20c3m forcings are quite sensitive to natural variability noise (as Peter Thorne's 2007 GRL paper clearly illustrates). Because of such chaotic variability, even a hypothetical model with perfect physics and forcings would yield a distribution of tropospheric temperature trends over 1979 to 1999, some of which would show larger or smaller cooling than observed. This is why it's illogical to stratify model results according to correspondence between modeled and observed surface warming - something which John Christy is very fond of doing.

What we've done (in the new water vapor work described above) is to evaluate the fidelity with which the AR4 models simulate the observed mean state and variability of precipitable water and SST - not the trends in these quantities. We've looked at a model performance in a variety of different regions, and on multiple timescales. The results are fascinating, and show (at least for water vapor and SST) that every model has its own individual strengths and weaknesses. It is difficult to identify a subset of models that CONSISTENTLY does well in many different regions and over a range of different timescales.

My guess is that we would obtain somewhat different results for MSU temperatures - particularly for comparisons involving variability. Clearly, the absence of volcanic forcing in roughly half of the 20c3m experiments will have a large impact on the estimated variability of synthetic T4 temperatures (and perhaps even on T2), and hence on model-versus-data variability comparisons. It's also quite possible that the inclusion or absence of volcanic forcing has an impact not only on the amplitude of the variability of global-mean T4 anomalies, but also on the pattern of T4 variability. So model ranking exercises based on performance in simulating the mean state and variability of T4 and T2 may show some connection to the presence or absence of volcanic/ozone forcing.

The sad thing is we are being distracted from doing this fun stuff by the need to respond to Douglass et al. That's a real shame.

With best regards,

Ben

Phil Jones wrote:

> All,
> IJC do have comments but only very rarely. I see little point in
> doing this
> as there is likely to be a word limit, and if the system works
properly
> Douglass et al would get the final say. There is also a large backlog
in
> papers awaiting to appear, so even if the comment were accepted it
would
> be some time after Douglass et al that it would appear.

> Better would be a submission to another journal (JGR?) which
> would be quicker. This could go in before Douglass et al appeared in
> print - it should be in the IJC early online view fairly soon based on
> recent experiences.
> A paper pointing out the issues of trying to weight models in some
way
> would be very beneficial to the community. AR5 will have to go down
this
> route at some point. How models simulate the
> recent trends at the surface and in the troposphere/stratosphere and
> how they might be ranked is a possibility. This could bring in the
> new work Peter alludes to with the sondes.
> There are also some aspects of recent surface T changes that could
be
> discussed as well. These relate to the growing dominance of buoy SSTs
> (now 70% of the total) vs conventional ships. There is a paper in J.
> Climate
> accepted from Smith/Reynolds et al at NCDC, which show that buoys
> could conceivably be cooler than ship-based SST by about 0.1C -
meaning
> that the last 5-10 years are being gradually underestimated over the
> oceans.
> Overlap is still too short to be confident about this, but it
highlights a
> major systematic change occurring in surface ocean measurements. As
the
> buoys are presumably better for absolute SSTs, this means models
> driven with fixed SSTs should be using fields that are marginally
cooler.
>
> And then there is the continual reference to Kalnay and Cai, when
> Simmons et al (2004) have shown the problems with NCEP. It is possible
> to add in the ERA-Interim analyses and operational analyses to
> being results from ERA-40 up to date.
>
> Cheers
> Phil
>
>
> At 23:40 04/12/2007, carl mears wrote:
>> Karl -- thanks for clarifying what I was trying to say
>>
>> Some further comments.....
>>
>> At 02:53 PM 12/4/2007, Karl Taylor wrote:
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>> as Ben has suggested.
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>> -Carl
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From: Ben Santer <santer1@llnl.gov>
To: Melissa Free <Melissa.Free@noaa.gov>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Thu, 06 Dec 2007 10:52:42 -0800
Reply-to: santer1@llnl.gov
Cc: Phil Jones <p.jones@uea.ac.uk>, carl mears <mears@remss.com>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <>wigley@ucar.edu>, Tom Wigley <>wigley@cgd.ucar.edu>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, John Lanzante <John.Lanzante@noaa.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Frank Wentz <frank.wentz@remss.com>, Steve Klein <klein21@mail.llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, peter gleckler <gleckler1@llnl.gov>

<x-flowed>
Dear Melissa,

No, this would not be dire. What is dire is Douglass et al.'s willful neglect of any observational datasets that do not support their arguments. Recall that our 2005 Science paper presented information from all observational datasets available to us at that time, even from datasets that showed large differences relative to the model data. We did not present results from RSS alone.

With best regards,

Ben

Melissa Free wrote:

> One further question about the Douglass paper: What about the
> implications of a real model-observation difference for upper-air
> trends? Is this really so dire?
> -Melissa
>

--

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</x-flowed>

From: Dian Seidel <dian.seidel@noaa.gov>
To: santer1@llnl.gov
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Thu, 06 Dec 2007 13:04:20 -0500
Cc: Phil Jones <p.jones@uea.ac.uk>, carl mears <mears@remss.com>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <>wigley@ucar.edu>, Tom Wigley <>wigley@cgd.ucar.edu>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, John Lanzante <John.Lanzante@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Frank Wentz <frank.wentz@remss.com>, Steve Klein <klein21@mail.llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, peter gleckler <gleckler1@llnl.gov>

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Hello Ben and Colleagues,

I've been following these exchanges with interest. One particular point in your message below is a little puzzling to me. That's the issue of trying to avoid circularity in the culling of models for any given D&A study.

Two potential problems occur to me. One is that choosing models on the basis of their fidelity to observed regional and short term variability may not be completely orthogonal to choosing based on long-term trend. That's because those smaller scale changes may contribute to the trends and their patterns. Second, choosing a different set of models for one variable (temperature) than for another (humidity) seems highly problematic. If we are interested in projections of other variables, e.g. storm tracks or cloud cover, for which D&A has not been done, which group of models would we then deem to be most credible? I don't have a good alternative to propose, but, in light of these considerations, maybe one-model-one-vote doesn't appear so unreasonable after all.

With regards,
Dian

Ben Santer wrote:

> Dear Phil,

>

> Just a quick response to the issue of "model weighting" which you and
> Carl raised in your emails.

>

> We recently published a paper dealing with the identification of an
> anthropogenic fingerprint in SSM/I-based estimates of total column
> water vapor changes. This was a true multi-model detection and
> attribution ("D&A") study, which made use of results from 22 different
> A/OGCMs for fingerprint and noise estimation. Together with Peter
> Gleckler and Karl Taylor, I'm now in the process of repeating our
> water vapor D&A study using a subset of the original 22 models. This
> subset will comprise 10-12 models which are demonstrably more
> successful in capturing features of the observed mean state and
> variability of water vapor and SST - particularly features crucial to
> the D&A problem (such as the low-frequency variability). We've had fun

> computing a whole range of metrics that might be used to define such a
> subset of "better" models. The ultimate goal is to determine the
> sensitivity of our water vapor D&A results to model quality. I think
> that this kind of analysis will be unavoidable in the multi-model
> world in which we now live. Given substantial inter-model differences
> in simulation quality, "one model, one vote" is probably not the best
> policy for D&A work!

>

> Once we've used Carl's method to calculate synthetic MSU temperatures
> from the IPCC AR4 20c3m data (as described in my previous email), it
> should be relatively easy to do a similar "model culling" exercise
> with MSU T2, T4, and TLT. In fact, this is what we had already planned
> to do in collaboration with Carl and Frank.

>

> One key point in any model weighting or selection strategy is to avoid
> circularity. In the D&A context, it would be impermissible to include
> information on trend behavior as a criterion used for selecting
> "better" models. Likewise, if our interest is in assessing the
> statistical significance of model-versus-observed trend differences,
> we can't use model performance in simulating "observed" tropospheric
> or stratospheric trends (whatever those might be!) as a means of
> identifying more credible models.

>

> A further issue, of course, is that we are relying on results from
> fully coupled A/OGCMs, and are making trend comparisons over
> relatively short periods (several decades). On these short timescales,
> estimates of the "true" trend in response to the applied 20c3m
> forcings are quite sensitive to natural variability noise (as Peter
> Thorne's 2007 GRL paper clearly illustrates). Because of such chaotic
> variability, even a hypothetical model with perfect physics and
> forcings would yield a distribution of tropospheric temperature trends
> over 1979 to 1999, some of which would show larger or smaller cooling
> than observed. This is why it's illogical to stratify model results
> according to correspondence between modeled and observed surface
> warming - something which John Christy is very fond of doing.

>

> What we've done (in the new water vapor work described above) is to
> evaluate the fidelity with which the AR4 models simulate the observed
> mean state and variability of precipitable water and SST - not the
> trends in these quantities. We've looked at a model performance in a
> variety of different regions, and on multiple timescales. The results
> are fascinating, and show (at least for water vapor and SST) that
> every model has its own individual strengths and weaknesses. It is
> difficult to identify a subset of models that CONSISTENTLY does well
> in many different regions and over a range of different timescales.

>

> My guess is that we would obtain somewhat different results for MSU
> temperatures - particularly for comparisons involving variability.
> Clearly, the absence of volcanic forcing in roughly half of the 20c3m
> experiments will have a large impact on the estimated variability of
> synthetic T4 temperatures (and perhaps even on T2), and hence on
> model-versus-data variability comparisons. It's also quite possible
> that the inclusion or absence of volcanic forcing has an impact not
> only on the amplitude of the variability of global-mean T4 anomalies,

> but also on the pattern of T4 variability. So model ranking exercises
> based on performance in simulating the mean state and variability of
> T4 and T2 may show some connection to the presence or absence of
> volcanic/ozone forcing.
>
> The sad thing is we are being distracted from doing this fun stuff by
> the need to respond to Douglass et al. That's a real shame.
>
> With best regards,
>
> Ben
>
> Phil Jones wrote:
>> All,
>> IJC do have comments but only very rarely. I see little point in
>> doing this
>> as there is likely to be a word limit, and if the system works
properly
>> Douglass et al would get the final say. There is also a large
>> backlog in
>> papers awaiting to appear, so even if the comment were accepted it
>> would
>> be some time after Douglass et al that it would appear.
>> Better would be a submission to another journal (JGR?) which
>> would be quicker. This could go in before Douglass et al appeared in
>> print - it should be in the IJC early online view fairly soon based
on
>> recent experiences.
>> A paper pointing out the issues of trying to weight models in some
>> way
>> would be very beneficial to the community. AR5 will have to go down
>> this
>> route at some point. How models simulate the
>> recent trends at the surface and in the troposphere/stratosphere and
>> how they might be ranked is a possibility. This could bring in the
>> new work Peter alludes to with the sondes.
>> There are also some aspects of recent surface T changes that could
be
>> discussed as well. These relate to the growing dominance of buoy SSTs
>> (now 70% of the total) vs conventional ships. There is a paper in J.
>> Climate
>> accepted from Smith/Reynolds et al at NCDC, which show that buoys
>> could conceivably be cooler than ship-based SST by about 0.1C -
meaning
>> that the last 5-10 years are being gradually underestimated over the
>> oceans.
>> Overlap is still too short to be confident about this, but it
>> highlights a
>> major systematic change occurring in surface ocean measurements. As
the
>> buoys are presumably better for absolute SSTs, this means models
>> driven with fixed SSTs should be using fields that are marginally
>> cooler.
>>

>> And then there is the continual reference to Kalnay and Cai, when
>> Simmons et al (2004) have shown the problems with NCEP. It is
possible
>> to add in the ERA-Interim analyses and operational analyses to
>> being results from ERA-40 up to date.
>>
>> Cheers
>> Phil
>>
>>
>> At 23:40 04/12/2007, carl mears wrote:
>>> Karl -- thanks for clarifying what I was trying to say
>>>
>>> Some further comments.....
>>>
>>> At 02:53 PM 12/4/2007, Karl Taylor wrote:
>>>> Dear all,
>>>> 2) unforced variability hasn't dominated the observations.
>>>>
>>>> But on this short time scale, we strongly suspect that it has
>>>> dominated. For example, the
>>>> 2 sigma error bars from table 3.4, CCSP for satellite TLT are 0.18
>>>> (UAH) or 0.19 (RSS), larger
>>>> than either group's trends (0.05, 0.15) for 1979-2004. These were
>>>> calculated using a "goodness
>>>> of linear fit" criterion, corrected for autocorrelation. This is a
>>>> probably a reasonable
>>>> estimate of the contribution of unforced variability to trend
>>>> uncertainty.
>>>>
>>>>
>>>>
>>>>> Douglass et al. have *not* shown that every individual model is in
>>>>> fact inconsistent with the observations. If the spread of
>>>>> individual model results is large enough and at least 1 model
>>>>> overlaps the observations, then one cannot claim that all models
>>>>> are wrong, just that the mean is biased.
>>>>>
>>>>>
>>>>> Given the magnitude of the unforced variability, I would say "the
>>>>> mean *may* be biased." You can't prove this
>>>>> with only one universe, as Tom alluded. All we can say is that the
>>>>> observed trend cannot be proven to
>>>>> be inconsistent with the model results, since it is inside their
range.
>>>>>
>>>>> It we interesting to see if we can say anything more, when we start
>>>>> culling out the less realistic models,
>>>>> as Ben has suggested.
>>>>>
>>>>> -Carl
>>>>>
>>>>>
>>>>>

>>>
>>
>> Prof. Phil Jones
>> Climatic Research Unit Telephone +44 (0) 1603 592090
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>> Norwich Email p.jones@uea.ac.uk
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>>
>
>

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~~~~~  
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Fax: +1-301-713-0119  
<http://www.arl.noaa.gov/ss/climate>

~~~~~

</x-flowed>

From: Tom Wigley <wigley@ucar.edu>
To: santer1@llnl.gov
Subject: Re: [Fwd: [Fwd: FW: Press Release from The Science & Environmental Policy Project]]
Date: Mon, 10 Dec 2007 17:17:14 -0700
Cc: carl mears <mears@remss.com>, Frank Wentz <frank.wentz@remss.com>, Tom Wigley <wigley@cgd.ucar.edu>, Steven Sherwood <Steven.Sherwood@yale.edu>, John Lanzante <John.Lanzante@noaa.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Karl Taylor <taylor13@llnl.gov>, Steve Klein <klein21@mail.llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, "'Philip D. Jones'" <p.jones@uea.ac.uk>

<x-flowed>

Dear all,

I think the scientific fraud committed by Douglass needs to be exposed. His co-authors may be innocent bystanders, but I doubt it.

In normal circumstances, what Douglass has done would cause him to lose his job -- a parallel is the South Korean cloning fraud case.

I have suggested that someone like Chris Mooney should be told about this.

Tom.

+++++

Ben Santer wrote:

> Dear folks,
>
> I knew this would happen. In my opinion, we should respond to this
> continued misrepresentation of the science sooner rather than later.

> With best regards,

> Ben

> -----
> -----

>
> Benjamin D. Santer
> Program for Climate Model Diagnosis and Intercomparison
> Lawrence Livermore National Laboratory
> P.O. Box 808, Mail Stop L-103
> Livermore, CA 94550, U.S.A.
> Tel: (925) 422-2486
> FAX: (925) 422-7675
> email: santer1@llnl.gov

> -----
>
>
> -----
>
> Subject:
> [Fwd: FW: Press Release from The Science & Environmental Policy
Project]
> From:
> "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
> Date:
> Mon, 10 Dec 2007 17:23:12 -0500
> To:
> _NESDIS NCDC CCSP Temp Trends Lead Authors
> <CCSPTempTrendAuthors.NCDC@noaa.gov>
>
> To:
> _NESDIS NCDC CCSP Temp Trends Lead Authors
> <CCSPTempTrendAuthors.NCDC@noaa.gov>
>
>
> FYI --- related to trop-sfc temps
>
> -----
>
> *From:* George Marshall Institute [mailto:info@marshall.org]
> *Sent:* Monday, December 10, 2007 4:24 PM
> *To:* info@marshall.org
> *Subject:* Press Release from The Science & Environmental Policy
Project
>
> */Press Release from The Science & Environmental Policy Project/**/ /*
>
> **Where & When**
>
> *The National Press Club*
>
> *529 14th Street, NW, 13th Floor*
>
> *Lisagor Room*
>
> *Washington, DC 20045*
>
> **
>
> **December 14, 2007 **
>
> **8am-11am **
>
> **
>
> *Breakfast refreshments will be served.*

>
> **
>
> **/To RSVP, please email info@sepp.org <<mailto:info@sepp.org>>. /**
>
> //
>
>
>
> You are invited to a timely breakfast briefing
>
> on December 14, 2007 at 8:30 a.m. at the National Press Club,
> organized by
>
> The Science & Environmental Policy Project (SEPP).
>
> As Al Gore collects his Nobel Prize and 15,000 (more or less) in Bali
> struggle to find a successor regime for the ineffective and unlamented
> Kyoto Protocol, an 'inconvenient truth' has emerged:
>
> NATURE RULES THE CLIMATE: HUMAN-PRODUCED GREENHOUSE GASES ARE NOT
> RESPONSIBLE FOR GLOBAL WARMING. Therefore, schemes to control CO2
> emissions are ineffective and pointless, though very costly.
>
> Come and listen to the authors of a peer-reviewed scientific study,
> just published in the International Journal of Climatology (of the
> Royal Meteorological Society), present their startling findings.
>
> Presenters:
>
> *Prof. David Douglass*, University of Rochester: GH Models clash with
> best observations
>
> *Prof. John Christy*, University of Alabama: How GH models
> overestimate GH warming
>
> *Prof. S. Fred Singer*, University of Virginia: Changes in solar
> activity control the climate.
>
> I am sure you will appreciate the importance of their new result. Once
> one accepts the documented evidence that CO2 is insignificant in
> warming the climate, all kinds of consequences follow logically:
>
> * ¶* Unburdened by climate fears, the US can pursue a more
>
> rational energy policy, leading to less dependence on oil/gas
>
> imports.
>
> * ¶* The current legislative efforts to cap CO2, or to control its
>
> emission in other ways, are utterly useless.
>
> * ¶* Ambitious programs claiming to reduce CO2 emissions (like

>
> ethanol, wind power, carbon sequestration, etc.) are a
>
> complete waste.
>
> *¶* The EPA can now deny California's request for a waiver on
>
> CAFE.
>
> *¶* The EPA can now respond properly to the Supreme Court
>
> ruling on CO2.
>
> *¶* International negotiations can assume a different dimension.
>
> SEPP has reserved the Lisagor Room at the National Press Club for
> Friday December 14 from 8-11 am. Breakfast will be served.
>
> **_Please e-mail your acceptance to info@sepp.org._**
>
>
>
> *Forward email
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> Washington | DC | 20006
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>
> *Dr. Thomas R. Karl, L.H.D.*
>
> */Director/*//
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</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: Douglass paper
Date: Wed, 12 Dec 2007 19:51:32 -0800
Reply-to: santer1@llnl.gov
Cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Tom Wigley <>wigley@cgd.ucar.edu>

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Dear Tim,

Thanks for the "heads up". As Phil mentioned, I was already aware of this. The Douglass et al. paper was rejected twice before it was finally accepted by IJC. I think this paper is a real embarrassment for the IJC. It has serious scientific flaws. I'm already working on a response.

Phil can tell you about some of the other sordid details of Douglass et al. These guys ignored information from radiosonde datasets that did not support their "models are wrong" argument (even though they had these datasets in their possession). Pretty deplorable behaviour...

Douglass is the guy who famously concluded (after examining the temperature response to Pinatubo) that the climate system has negative sensitivity. Amazingly, he managed to publish that crap in GRL. Christy sure does manage to pick some brilliant scientific collaborators...

With best regards,

Ben

Tim Osborn wrote:

> Hi Ben,
>
> I guess it's likely that you're aware of the Douglass paper that's just
> come out in IJC, but in case you aren't then a reprint is attached.
> They are somewhat critical of your 2005 paper, though I recall that some
> (most?) of Douglass' previous papers -- and papers that he's tried to
> get through the review process -- appear to have serious problems.
>
> cc Phil & Keith for your interest too!
>
> Cheers
>
> Tim
> Dr Timothy J Osborn, Academic Fellow
> Climatic Research Unit

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> web: <http://www.cru.uea.ac.uk/~timo/>
> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
>

--

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</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: carl mears <mears@remss.com>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Thu, 13 Dec 2007 18:58:12 -0800
Reply-to: santer1@llnl.gov
Cc: SHERWOOD Steven <steven.sherwood@yale.edu>, Tom Wigley <wigley@cgd.ucar.edu>, Frank Wentz <frank.wentz@remss.com>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Karl Taylor <taylor13@llnl.gov>, Steve Klein <klein21@mail.llnl.gov>, John Lanzante <John.Lanzante@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, "David C. Bader" <bader2@llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>

<x-flowed>
Dear folks,

I've been doing some calculations to address one of the statistical issues raised by the Douglass et al. paper in the International Journal of Climatology. Here are some of my results.

Recall that Douglass et al. calculated synthetic T2LT and T2 temperatures from the CMIP-3 archive of 20th century simulations ("20c3m" runs). They used a total of 67 20c3m realizations, performed with 22 different models. In calculating the statistical uncertainty of the model trends, they introduced $\sigma\{SE\}$, an "estimate of the uncertainty of the mean of the predictions of the trends". They defined $\sigma\{SE\}$ as follows:

$\sigma\{SE\} = \sigma / \sqrt{N - 1}$, where

"N = 22 is the number of independent models".

As we've discussed in our previous correspondence, this definition has serious problems (see comments from Carl and Steve below), and allows Douglass et al. to reach the erroneous conclusion that modeled T2LT and T2 trends are significantly different from the observed T2LT and T2 trends in both the RSS and UAH datasets. This comparison of simulated and observed T2LT and T2 trends is given in Table III of Douglass et al. [As an amusing aside, I note that the RSS datasets are referred to as "RSS" in this table, while UAH results are designated as "MSU". I guess there's only one true "MSU" dataset...]

I decided to take a quick look at the issue of the statistical significance of differences between simulated and observed tropospheric temperature trends. My first cut at this "quick look" involves only UAH and RSS observational data - I have not yet done any tests with radiosonde data, UMD T2 data, or satellite results from Zou et al.

I operated on the same 49 realizations of the 20c3m experiment that we

used in Chapter 5 of CCSP 1.1. As in our previous work, all model results are synthetic T2LT and T2 temperatures that I calculated using a static weighting function approach. I have not yet implemented Carl's more sophisticated method of estimating synthetic MSU temperatures from model data (which accounts for effects of topography and land/ocean differences). However, for the current application, the simple static weighting function approach is more than adequate, since we are focusing on T2LT and T2 changes over tropical oceans only - so topographic and land-ocean differences are unimportant. Note that I still need to calculate synthetic MSU temperatures from about 18-20 20c3m realizations which were not in the CMIP-3 database at the time we were working on the CCSP report. For the full response to Douglass et al., we should use the same 67 20c3m realizations that they employed.

For each of the 49 realizations that I processed, I first masked out all tropical land areas, and then calculated the spatial averages of monthly-mean, gridded T2LT and T2 data over tropical oceans (20N-20S). All model and observational results are for the common 252-month period from January 1979 to December 1999 - the longest period of overlap between the RSS and UAH MSU data and the bulk of the 20c3m runs. The simulated trends given by Douglass et al. are calculated over the same 1979 to 1999 period; however, they use a longer period (1979 to 2004) for calculating observational trends - so there is an inconsistency between their model and observational analysis periods, which they do not explain. This difference in analysis periods is a little puzzling given that we are dealing with relatively short observational record lengths, resulting in some sensitivity to end-point effects.

I then calculated anomalies of the spatially-averaged T2LT and T2 data (w.r.t. climatological monthly-means over 1979-1999), and fit least-squares linear trends to model and observational time series. The standard errors of the trends were adjusted for temporal autocorrelation of the regression residuals, as described in Santer et al. (2000) ["Statistical significance of trends and trend differences in layer-average atmospheric temperature time series"; JGR 105, 7337-7356.]

Consider first panel A of the attached plot. This shows the simulated and observed T2LT trends over 1979 to 1999 (again, over 20N-20S, oceans only) with their adjusted 1-sigma confidence intervals). For the UAH and RSS data, it was possible to check against the adjusted confidence intervals independently calculated by Dian during the course of work on the CCSP report. Our adjusted confidence intervals are in good agreement. The grey shaded envelope in panel A denotes the 1-sigma standard error for the RSS T2LT trend.

There are 49 pairs of UAH-minus-model trend differences and 49 pairs of RSS-minus-model trend differences. We can therefore test - for each model and each 20c3m realization - whether there is a statistically significant difference between the observed and simulated trends.

Let b_x and b_y represent any single pair of modeled and observed trends, with adjusted standard errors $s\{b_x\}$ and $s\{b_y\}$. As in our previous work (and as in related work by John Lanzante), we define the normalized trend difference d as:

$$d = (bx - by) / \text{sqrt}[(s\{bx\})^{**2} + (s\{by\})^{**2}]$$

Under the assumption that d is normally distributed, values of $d > +1.96$ or $d < -1.96$ indicate observed-minus-model trend differences that are significant at the 5% level. We are performing a two-tailed test here, since we have no information a priori about the "direction" of the model trend (i.e., whether we expect the simulated trend to be significantly larger or smaller than observed).

Panel c shows values of the normalized trend difference for T2LT trends. The grey shaded area spans the range $+1.96$ to -1.96 , and identifies the region where we fail to reject the null hypothesis (H_0) of no significant difference between observed and simulated trends.

Consider the solid symbols first, which give results for tests involving RSS data. We would reject H_0 in only one out of 49 cases (for the CCCma-CGCM3.1(T47) model). The open symbols indicate results for tests involving UAH data. Somewhat surprisingly, we get the same qualitative outcome that we obtained for tests involving RSS data: only one of the UAH-model trend pairs yields a difference that is statistically significant at the 5% level.

Panels b and d provide results for T2 trends. Results are very similar to those achieved with T2LT trends. Irrespective of whether RSS or UAH T2 data are used, significant trend differences occur in only one of 49 cases.

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Attachment Converted: "c:\eudora\attach\douglass_reply1.pdf"

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To: carl mears <mears@remss.com>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Thu, 13 Dec 2007 18:58:12 -0800
Reply-to: santer1@llnl.gov
Cc: SHERWOOD Steven <steven.sherwood@yale.edu>, Tom Wigley <wigley@cgd.ucar.edu>, Frank Wentz <frank.wentz@remss.com>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Karl Taylor <taylor13@llnl.gov>, Steve Klein <klein21@mail.llnl.gov>, John Lanzante <John.Lanzante@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, "David C. Bader" <bader2@llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>

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For each of the 49 realizations that I processed, I first masked out all tropical land areas, and then calculated the spatial averages of monthly-mean, gridded T2LT and T2 data over tropical oceans (20N-20S). All model and observational results are for the common 252-month period from January 1979 to December 1999 - the longest period of overlap between the RSS and UAH MSU data and the bulk of the 20c3m runs. The simulated trends given by Douglass et al. are calculated over the same 1979 to 1999 period; however, they use a longer period (1979 to 2004) for calculating observational trends - so there is an inconsistency between their model and observational analysis periods, which they do not explain. This difference in analysis periods is a little puzzling given that we are dealing with relatively short observational record lengths, resulting in some sensitivity to end-point effects.

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Consider first panel A of the attached plot. This shows the simulated and observed T2LT trends over 1979 to 1999 (again, over 20N-20S, oceans only) with their adjusted 1-sigma confidence intervals). For the UAH and RSS data, it was possible to check against the adjusted confidence intervals independently calculated by Dian during the course of work on the CCSP report. Our adjusted confidence intervals are in good agreement. The grey shaded envelope in panel A denotes the 1-sigma standard error for the RSS T2LT trend.

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Attachment Converted: "c:\documents and settings\tim osborn\my
documents\eudora\attach\douglass_reply1.pdf"

From: Ben Santer <santer1@llnl.gov>
To: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Fri, 14 Dec 2007 14:31:15 -0800
Reply-to: santer1@llnl.gov
Cc: carl mears <mears@remss.com>, SHERWOOD Steven <steven.sherwood@yale.edu>, Tom Wigley <wigley@cgd.ucar.edu>, Frank Wentz <frank.wentz@remss.com>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Karl Taylor <taylor13@llnl.gov>, Steve Klein <klein21@mail.llnl.gov>, John Lanzante <John.Lanzante@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Tim Osborn <t.osborn@uea.ac.uk>, "David C. Bader" <bader2@llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>

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Dear Tom,

As promised, I've now repeated all of the significance testing involving model-versus-observed trend differences, but this time using spatially-averaged T2 and T2LT changes that are not "masked out" over tropical land areas. As I mentioned this morning, the use of non-masked data facilitates a direct comparison with Douglass et al.

The results for combined changes over tropical land and ocean are very similar to those I sent out yesterday, which were for T2 and T2LT changes over tropical oceans only:

COMBINED LAND/OCEAN RESULTS (WITH STANDARD ERRORS ADJUSTED FOR TEMPORAL AUTOCORRELATION EFFECTS; SPATIAL AVERAGES OVER 20N-20S; ANALYSIS PERIOD 1979 TO 1999)

T2LT tests, RSS observational data: 0 out of 49 model-versus-observed trend differences are significant at the 5% level.

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So our conclusion - that model tropical T2 and T2LT trends are, in virtually all realizations and models, not significantly different from either RSS or UAH trends - is not sensitive to whether we do the significance testing with "ocean only" or combined "land+ocean" temperature changes.

With best regards, and happy holidays to all!

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Thomas.R.Karl wrote:

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>

> This is very informative. One question I raise is whether the results
> would have been at all different if you had not masked the land. I
> doubt it, but it would be nice to know.

>

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> Ben Santer said the following on 12/13/2007 9:58 PM:

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>>>> climate model because it did not consistently predict a particular
>>>> storm on a particular day two years from now.
>>>
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</x-flowed>

From: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
To: santer1@llnl.gov
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Sat, 15 Dec 2007 12:21:48 -0500
Cc: carl mears <mears@remss.com>, SHERWOOD Steven <steven.sherwood@yale.edu>, Tom Wigley <wigley@cgd.ucar.edu>, Frank Wentz <frank.wentz@remss.com>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Karl Taylor <taylor13@llnl.gov>, Steve Klein <klein21@mail.llnl.gov>, John Lanzante <John.Lanzante@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Tim Osborn <t.osborn@uea.ac.uk>, "David C. Bader" <bader2@llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>

Thanks Ben,

You have the makings of a nice article.

I note that we would expect to 10 cases that are significantly different by chance (based

on the 196 tests at the .05 sig level). You found 3. With appropriately corrected Leopold

I suspect you will find there is indeed stat sig. similar trends incl. amplification.

Setting up the statistical testing should be interesting with this many combinations.

Regards, Tom

Ben Santer said the following on 12/14/2007 5:31 PM:

Dear Tom,

As promised, I've now repeated all of the significance testing involving

model-versus-observed trend differences, but this time using spatially-averaged T2 and

T2LT changes that are not "masked out" over tropical land areas. As I mentioned this

morning, the use of non-masked data facilitates a direct comparison with Douglass et al.

The results for combined changes over tropical land and ocean are very similar to those

I sent out yesterday, which were for T2 and T2LT changes over tropical oceans only:

COMBINED LAND/OCEAN RESULTS (WITH STANDARD ERRORS ADJUSTED FOR TEMPORAL AUTOCORRELATION

EFFECTS; SPATIAL AVERAGES OVER 20N-20S; ANALYSIS PERIOD 1979 TO 1999)

T2LT tests, RSS observational data: 0 out of 49 model-versus-observed trend differences

are significant at the 5% level.

T2LT tests, UAH observational data: 1 out of 49 model-versus-observed trend differences

are significant at the 5% level.

T2 tests, RSS observational data: 1 out of 49 model-versus-observed trend differences

are significant at the 5% level.

T2 tests, UAH observational data: 1 out of 49 model-versus-observed trend differences

are significant at the 5% level.

So our conclusion - that model tropical T2 and T2LT trends are, in virtually all

realizations and models, not significantly different from either RSS or UAH trends - is

not sensitive to whether we do the significance testing with "ocean only" or combined

"land+ocean" temperature changes.

With best regards, and happy holidays to all!

Ben

Thomas.R.Karl wrote:

Ben,

This is very informative. One question I raise is whether the results would have been

at all different if you had not masked the land. I doubt it, but it would be nice to

know.

Tom

Ben Santer said the following on 12/13/2007 9:58 PM:

Dear folks,

I've been doing some calculations to address one of the statistical issues raised by the

Douglass et al. paper in the International Journal of Climatology.

Here are some of my

results.

Recall that Douglass et al. calculated synthetic T2LT and T2 temperatures from the

CMIP-3 archive of 20th century simulations ("20c3m" runs). They used a total of 67 20c3m

realizations, performed with 22 different models. In calculating the statistical

uncertainty of the model trends, they introduced $\sigma\{SE\}$, an "estimate of the

uncertainty of the mean of the predictions of the trends". They defined

$\sigma\{SE\}$ as follows:

$\sigma\{SE\} = \sigma / \sqrt{N - 1}$, where

"N = 22 is the number of independent models".

As we've discussed in our previous correspondence, this definition has serious problems

(see comments from Carl and Steve below), and allows Douglass et al. to reach the

erroneous conclusion that modeled T2LT and T2 trends are significantly different from

the observed T2LT and T2 trends in both the RSS and UAH datasets.

This comparison of

simulated and observed T2LT and T2 trends is given in Table III of Douglass et al.

[As an amusing aside, I note that the RSS datasets are referred to as "RSS" in this table, while UAH results are designated as "MSU". I guess there's only one true "MSU" dataset...]

I decided to take a quick look at the issue of the statistical significance of differences between simulated and observed tropospheric temperature trends. My first cut at this "quick look" involves only UAH and RSS observational data - I have not yet done any tests with radiosonde data, UMD T2 data, or satellite results from Zou et al.

I operated on the same 49 realizations of the 20c3m experiment that we used in Chapter 5

of CCSP 1.1. As in our previous work, all model results are synthetic T2LT and T2 temperatures that I calculated using a static weighting function approach. I have not yet implemented Carl's more sophisticated method of estimating synthetic MSU

temperatures from model data (which accounts for effects of topography and land/ocean differences). However, for the current application, the simple static weighting function approach is more than adequate, since we are focusing on T2LT and T2 changes over

tropical oceans only - so topographic and land-ocean differences are unimportant. Note

that I still need to calculate synthetic MSU temperatures from about 18-20 20c3m

realizations which were not in the CMIP-3 database at the time we were working on the

CCSP report. For the full response to Douglass et al., we should use the same 67 20c3m

realizations that they employed.

For each of the 49 realizations that I processed, I first masked out all tropical land

areas, and then calculated the spatial averages of monthly-mean, gridded T2LT and T2

data over tropical oceans (20N-20S). All model and observational results are for the

common 252-month period from January 1979 to December 1999 - the longest period of

overlap between the RSS and UAH MSU data and the bulk of the 20c3m runs. The simulated

trends given by Douglass et al. are calculated over the same 1979 to 1999 period;

however, they use a longer period (1979 to 2004) for calculating observational trends -

so there is an inconsistency between their model and observational analysis periods,

which they do not explain. This difference in analysis periods is a little puzzling

given that we are dealing with relatively short observational record lengths, resulting

in some sensitivity to end-point effects.

I then calculated anomalies of the spatially-averaged T2LT and T2 data (w.r.t.

climatological monthly-means over 1979-1999), and fit least-squares linear trends to

model and observational time series. The standard errors of the trends were adjusted for

temporal autocorrelation of the regression residuals, as described in Santer et al.

(2000) ["Statistical significance of trends and trend differences in layer-average

atmospheric temperature time series"; JGR 105, 7337-7356.]

Consider first panel A of the attached plot. This shows the simulated and observed T2LT

trends over 1979 to 1999 (again, over 20N-20S, oceans only) with their adjusted 1-sigma

confidence intervals). For the UAH and RSS data, it was possible to check against the

adjusted confidence intervals independently calculated by Dian during the course of work

on the CCSP report. Our adjusted confidence intervals are in good agreement. The grey

shaded envelope in panel A denotes the 1-sigma standard error for the RSS T2LT trend.

There are 49 pairs of UAH-minus-model trend differences and 49 pairs of RSS-minus-model

trend differences. We can therefore test - for each model and each 20c3m realization -

whether there is a statistically significant difference between the observed and

simulated trends.

Let b_x and b_y represent any single pair of modeled and observed trends, with adjusted

standard errors $s\{b_x\}$ and $s\{b_y\}$. As in our previous work (and as in related work by John

Lanzante), we define the normalized trend difference d as:

$$d = (b_x - b_y) / \sqrt{(s\{b_x\})^2 + (s\{b_y\})^2}$$

Under the assumption that d is normally distributed, values of $d > +1.96$ or < -1.96

indicate observed-minus-model trend differences that are significant at the 5% level. We

are performing a two-tailed test here, since we have no information a priori about the

"direction" of the model trend (i.e., whether we expect the simulated trend to be

significantly larger or smaller than observed).

Panel c shows values of the normalized trend difference for T2LT trends.

the grey shaded area spans the range +1.96 to -1.96, and identifies the region where we

fail to reject the null hypothesis (H0) of no significant difference between observed and simulated trends.

Consider the solid symbols first, which give results for tests involving RSS data. We

would reject H0 in only one out of 49 cases (for the CCCma-CGCM3.1(T47) model). The open

symbols indicate results for tests involving UAH data. Somewhat surprisingly, we get the

same qualitative outcome that we obtained for tests involving RSS data: only one of the

UAH-model trend pairs yields a difference that is statistically significant at the 5%

level.

Panels b and d provide results for T2 trends. Results are very similar to those achieved

with T2LT trends. Irrespective of whether RSS or UAH T2 data are used, significant trend

differences occur in only one of 49 cases.

Bottom line: Douglass et al. claim that "In all cases UAH and RSS satellite trends are

inconsistent with model trends." (page 6, lines 61-62). This claim is categorically

wrong. In fact, based on our results, one could justifiably claim that THERE IS ONLY ONE

CASE in which model T2LT and T2 trends are inconsistent with UAH and RSS results! These

guys screwed up big time.

SENSITIVITY TESTS

QUESTION 1: Some of the model-data trend comparisons made by Douglass et al. used

temperatures averaged over 30N-30S rather than 20N-20S. What happens if we repeat our

simple trend significance analysis using T2LT and T2 data averaged over ocean areas

between 30N-30S?

ANSWER 1: Very little. The results described above for oceans areas between 20N-20S are

virtually unchanged.

QUESTION 2: Even though it's clearly inappropriate to estimate the standard errors of

the linear trends WITHOUT accounting for temporal autocorrelation effects (the 252 time

sample are clearly not independent; effective sample sizes typically range from 6 to

56), someone is bound to ask what the outcome is when one repeats the paired trend tests

with non-adjusted standard errors. So here are the results:

T2LT tests, RSS observational data: 19 out of 49 trend differences are significant at

the 5% level.

T2LT tests, UAH observational data: 34 out of 49 trend differences are significant at

the 5% level.

T2 tests, RSS observational data: 16 out of 49 trend differences are significant at the 5% level.

T2 tests, UAH observational data: 35 out of 49 trend differences are significant at the 5% level.

So even under the naive (and incorrect) assumption that each model and observational

time series contains 252 independent time samples, we STILL find no support for Douglass

et al.'s assertion that: "In all cases UAH and RSS satellite trends are inconsistent with model trends."

Q.E.D.

If Leo is agreeable, I'm hopeful that we'll be able to perform a similar trend

comparison using synthetic MSU T2LT and T2 temperatures calculated from the RAOBCORE

radiosonde data - all versions, not just v1.2!

As you can see from the email list, I've expanded our "focus group" a little bit, since

a number of you have written to me about this issue.

I am leaving for Miami on Monday, Dec. 17th. My Mom is having cataract surgery, and I'd

like to be around to provide her with moral and practical support. I'm not exactly sure

when I'll be returning to PCMDI - although I hope I won't be gone longer than a week. As

soon as I get back, I'll try to make some more progress with this stuff. Any suggestions

or comments on what I've done so far would be greatly appreciated. And for the time

being, I think we should not alert Douglass et al. to our results.

With best regards, and happy holidays! May all your "Singers" be carol singers, and not

of the S. Fred variety...

Ben

(P.S.: I noticed one unfortunate typo in Table II of Douglass et al. The MIROC3.2

(medres) model is referred to as "MIROC3.2_Merdes"....)

carl mears wrote:

Hi Steve

I'd say it's the equivalent of rolling a 6-sided die a hundred times, and

finding a mean value of ~3.5 and a standard deviation of ~1.7, and calculating the standard error of the mean to be ~0.17 (so far so good). An then rolling the die one more time, getting a 2, and claiming that the die is no longer 6 sided because the new measurement

is more than 2 standard errors from the mean.

In my view, this problem trumps the other problems in the paper.

I can't believe Douglas is a fellow of the American Physical Society.

-Carl

At 02:07 AM 12/6/2007, you wrote:

If I understand correctly, what Douglass et al. did makes the stronger assumption that unforced variability is *insignificant*. Their statistical test is logically equivalent to falsifying a climate model because it did not consistently predict a particular storm on a particular day two years from now.

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References

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4. <mailto:Thomas.R.Karl@noaa.gov>

From: Leopold Haimberger <leopold.haimberger@univie.ac.at>
To: John.Lanzante@noaa.gov
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Sun, 23 Dec 2007 15:50:17 +0100
Cc: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>, carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Karl Taylor <taylor13@llnl.gov>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, santer1@llnl.gov, Sherwood Steven <steven.sherwood@yale.edu>, Steve Klein <klein21@llnl.gov>, 'Susan Solomon' <susan.solomon@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>

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Dear all,

I have attached a plot which summarizes the recent developments concerning tropical radiosonde temperature datasets and which could be a candidate to be included in a reply to Douglass et al. It contains trend profiles from unadjusted radiosondes, HadAT2-adjusted radiosondes, RAOBCORE (versions 1.2-1.4) adjusted radiosondes and from radiosondes adjusted with a neighbor composite method (RICH) that uses the break dates detected with RAOBCORE (v1.4) as metadata. RAOBCORE v1.2,v1.3 are documented in Haimberger (2007), RAOBCORE v1.4 and RICH are discussed in the manuscript I mentioned in my previous email.

Latitude range is 20S-20N, only time series with less than 24 months of missing data are included. Spatial sampling of all curves is the same except HadAT which contains less stations that meet the 24month criterion. Sampling uncertainty of the trend curves is ca. $\pm 0.1\text{K/decade}$ (95% percentiles estimated with bootstrap method).

RAOBCORE v1.3,1.4 and RICH are results from ongoing research and warming trends from radiosondes may still be underestimated. The upper tropospheric warming maxima from RICH are even larger (up to 0.35K/decade , not shown), if only radiosondes within the tropics (20N-20S) are allowed as reference for adjustment of tropical radiosonde temperatures. The pink/blue curves in the attached plot should therefore not be regarded as upper bound of what may be achieved with plausible choices of reference series for homogenization.

Please let me know your comments.

I wish you a merry Christmas.

With best regards

Leo

John Lanzante wrote:

> Ben,
>
> Perhaps a resampling test would be appropriate. The tests you have performed
> consist of pairing an observed time series (UAH or RSS MSU) with each one
> of 49 GCM times series from your "ensemble of opportunity".
Significance
> of the difference between each pair of obs/GCM trends yields a certain
> number of "hits".
>
> To determine a baseline for judging how likely it would be to obtain the
> given number of hits one could perform a set of resampling trials by
> treating one of the ensemble members as a surrogate observation. For each
> trial, select at random one of the 49 GCM members to be the "observation".
> From the remaining 48 members draw a bootstrap sample of 49, and perform
> 49 tests, yielding a certain number of "hits". Repeat this many times to
> generate a distribution of "hits".
>
> The actual number of hits, based on the real observations could then be
> referenced to the Monte Carlo distribution to yield a probability that this
> could have occurred by chance. The basic idea is to see if the observed
> trend is inconsistent with the GCM ensemble of trends.
>
> There are a couple of additional tweaks that could be applied to your method.
> You are currently computing trends for each of the two time series in the
> pair and assessing the significance of their differences. Why not first
> create a difference time series and assess the significance of it's trend?
> The advantage of this is that you would reduce somewhat the autocorrelation
> in the time series and hence the effect of the "degrees of freedom"
> adjustment. Since the GCM runs are based on coupled model runs this
> differencing would help remove the common externally forced variability,
> but not internally forced variability, so the adjustment would still be
> needed.
>
> Another tweak would be to alter the significance level used to assess
> differences in trends. Currently you are using the 5% level, which yields
> only a small number of hits. If you made this less stringent you would get
> potentially more weaker hits. But it would all come out in the wash so to
to

> speak since the number of hits in the Monte Carlo simulations would increase
> as well. I suspect that increasing the number of expected hits would make the
> whole procedure more powerful/efficient in a statistical sense since you
> would no longer be dealing with a "rare event". In the current scheme, using
> a 5% level with 49 pairings you have an expected hit rate of $0.05 \times 49 = 2.45$.
> For example, if instead you used a 20% significance level you would have an
> expected hit rate of $0.20 \times 49 = 9.8$.
>
> I hope this helps.
>
> On an unrelated matter, I'm wondering a bit about the different versions of
> Leo's new radiosonde dataset (RAOBCORE). I was surprised to see that the
> latest version has considerably more tropospheric warming than I recalled
> from an earlier version that was written up in JCLI in 2007. I have a
> couple of questions that I'd like to ask Leo. One concern is that if we use
> the latest version of RAOBCORE is there a paper that we can reference -
> -
> if this is not in a peer-reviewed journal is there a paper in submission?
> The other question is: could you briefly comment on the differences in
> methodology used to generate the latest version of RAOBCORE as compared to
> the version used in JCLI 2007, and what/when/where did changes occur to
> yield a stronger warming trend?
>
> Best regards,
>
> _____ John
>
>
>
> On Saturday 15 December 2007 12:21 pm, Thomas.R.Karl wrote:
>
>> Thanks Ben,
>>
>> You have the makings of a nice article.
>>
>> I note that we would expect to 10 cases that are significantly different
>> by chance (based on the 196 tests at the .05 sig level). You found 3.
>> With appropriately corrected Leopold I suspect you will find there is
>> indeed stat sig. similar trends incl. amplification. Setting up the
>> statistical testing should be interesting with this many combinations.
>>

>> Regards, Tom

>>

>

>

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Attachment Converted: "c:\documents and settings\tim osborn\my documents\eutora\attach\t00_trendbeltbg_Tropics_1979-2004_1.4.eps"

From: Ben Santer <santer1@llnl.gov>
To: John Lanzante <John.Lanzante@noaa.gov>, Thomas R Karl
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Wigley <>wigley@cgd.ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: More significance testing
Date: Thu, 27 Dec 2007 16:26:19 -0800
Reply-to: santer1@llnl.gov

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Dear folks,

This email briefly summarizes the trend significance test results. As I mentioned in yesterday's email, I've added a new case (referred to as "TYPE3" below). I've also added results for tests with a stipulated 10% significance level. Here is the explanation of the four different types of trend test:

1. "OBS-vs-MODEL": Observed MSU trends in RSS and UAH are tested against trends in synthetic MSU data in 49 realizations of the 20c3m experiment. Results from RSS and UAH are pooled, yielding a total of 98 tests for T2 trends and 98 tests for T2LT trends.

2. "MODEL-vs-MODEL (TYPE1)": Involves model data only. Trend in synthetic MSU data in each of 49 20c3m realizations is tested against each trend in the remaining 48 realizations (i.e., no trend tests involving identical data). Yields a total of $49 \times 48 = 2352$ tests. The significance of trend differences is a function of BOTH inter-model differences (in climate sensitivity, applied 20c3m forcings, and the amplitude of variability) AND "within-model" effects (i.e., is related to the different manifestations of natural internal variability superimposed on the underlying forced response).

3. "MODEL-vs-MODEL (TYPE2)": Involves model data only. Limited to the M models with multiple realizations of the 20c3m experiment. For each of these M models, the number of unique combinations C of N 20c3m realizations into R trend pairs is determined. For example, in the case of $N = 5$, $C = N! / [R!(N-R)!] = 10$. The significance of trend differences is solely a function of "within-model" effects (i.e., is related to the different manifestations of natural internal variability superimposed on the underlying forced response). There are a total of 62 tests (not 124, as I erroneously reported yesterday!)

4. "MODEL-vs-MODEL (TYPE3)": Involves model data only. For each of the 19 models, only the first 20c3m realization is used. The trend in each model's first 20c3m realization is tested against each trend in the

first 20c3m realization of the remaining 18 models. Yields a total of 19 x 18 = 342 tests. The significance of trend differences is solely a function of inter-model differences (in climate sensitivity, applied 20c3m forcings, and the amplitude of variability).

REJECTION RATES FOR STIPULATED 5% SIGNIFICANCE LEVEL

| Test type | No. of tests | T2 "Hits" | T2LT "Hits" |
|---------------------------|----------------|------------|-------------|
| 1. OBS-vs-MODEL | 49 x 2 (98) | 2 (2.04%) | 1 (1.02%) |
| 2. MODEL-vs-MODEL (TYPE1) | 49 x 48 (2352) | 58 (2.47%) | 32 (1.36%) |
| 3. MODEL-vs-MODEL (TYPE2) | --- (62) | 0 (0.00%) | 0 (0.00%) |
| 4. MODEL-vs-MODEL (TYPE3) | 19 x 18 (342) | 22 (6.43%) | 14 (4.09%) |

REJECTION RATES FOR STIPULATED 10% SIGNIFICANCE LEVEL

| Test type | No. of tests | T2 "Hits" | T2LT "Hits" |
|---------------------------|----------------|------------|-------------|
| 1. OBS-vs-MODEL | 49 x 2 (98) | 4 (4.08%) | 2 (2.04%) |
| 2. MODEL-vs-MODEL (TYPE1) | 49 x 48 (2352) | 80 (3.40%) | 46 (1.96%) |
| 3. MODEL-vs-MODEL (TYPE2) | --- (62) | 1 (1.61%) | 0 (0.00%) |
| 4. MODEL-vs-MODEL (TYPE3) | 19 x 18 (342) | 28 (8.19%) | 20 (5.85%) |

REJECTION RATES FOR STIPULATED 20% SIGNIFICANCE LEVEL

| Test type | No. of tests | T2 "Hits" | T2LT "Hits" |
|---------------------------|----------------|-------------|-------------|
| 1. OBS-vs-MODEL | 49 x 2 (98) | 7 (7.14%) | 5 (5.10%) |
| 2. MODEL-vs-MODEL (TYPE1) | 49 x 48 (2352) | 176 (7.48%) | 100 (4.25%) |
| 3. MODEL-vs-MODEL (TYPE2) | --- (62) | 4 (6.45%) | 3 (4.84%) |
| 4. MODEL-vs-MODEL (TYPE3) | 19 x 18 (342) | 42 (12.28%) | 28 (8.19%) |

Features of interest:

A) As you might expect, for each of the three significance levels, TYPE3 tests yield the highest rejection rates of the null hypothesis of "No significant difference in trend". TYPE2 tests yield the lowest rejection rates. This is simply telling us that the inter-model differences in trends tend to be larger than the "between-realization" differences in trends in any individual model.

B) Rejection rates for the model-versus-observed trend tests are consistently LOWER than for the model-versus-model (TYPE3) tests. On average, therefore, the tropospheric trend differences between the observational datasets used here (RSS and UAH) and the synthetic MSU temperatures calculated from 19 CMIP-3 models are actually LESS SIGNIFICANT than the inter-model trend differences arising from differences in sensitivity, 20c3m forcings, and levels of variability.

I also thought that it would be fun to use the model data to explore the implications of Douglass et al.'s flawed statistical procedure. Recall that Douglass et al. compare (in their Table III) the observed T2 and T2LT trends in RSS and UAH with the overall means of the multi-model distributions of T2 and T2LT trends. Their standard error, $\sigma\{SE\}$, is meant to represent an "estimate of the uncertainty of the mean" (i.e., the mean trend). $\sigma\{SE\}$ is given as:

$$\sigma\{SE\} = \sigma / \sqrt{N - 1}$$

where σ is the standard deviation of the model trends, and N is "the

number of independent models" (22 in their case). Douglass et al. apparently estimate sigma using ensemble-mean trends for each model (if 20c3m ensembles are available).

So what happens if we apply this procedure using model data only? This is rather easy to do. As above (in the TYPE1, TYPE2, and TYPE3 tests), I simply used the synthetic MSU trends from the 19 CMIP-3 models employed in our CCSP Report and in Santer et al. 2005 (so N = 19). For each model, I calculated the ensemble-mean 20c3m trend over 1979 to 1999 (where multiple 20c3m realizations were available). Let's call these mean trends $b\{j\}$, where j (the index over models) = 1, 2, .. 19. Further, let's regard $b\{1\}$ as the surrogate observations, and then use Douglass et al.'s approach to test whether $b\{1\}$ is significantly different from the overall mean of the remaining 18 members of $b\{j\}$. Then repeat with $b\{2\}$ as surrogate observations, etc. For each layer-averaged temperature series, this yields 19 tests of the significance of differences in mean trends.

To give you a feel for this stuff, I've reproduced below the results for tests involving T2LT trends. The "OBS" column is the ensemble-mean T2LT trend in the surrogate observations. "MODAVE" is the overall mean trend in the 18 remaining members of the distribution, and "SIGMA" is the 1-sigma standard deviation of these trends. "SIGMA{SE}" is $1 \times \text{SIGMA}\{\text{SE}\}$ (note that Douglass et al. give $2 \times \text{SIGMA}\{\text{SE}\}$ in their Table III; multiplying our $\text{SIGMA}\{\text{SE}\}$ results by two gives values similar to theirs). "NORMD" is simply the normalized difference $(\text{OBS} - \text{MODAVE}) / \text{SIGMA}\{\text{SE}\}$, and "P-VALUE" is the p-value for the normalized difference, assuming that this difference is approximately normally distributed.

| MODEL | "OBS" | MODAVE | SIGMA | SIGMA{SE} | NORMD | P-VALUE |
|---------------|--------|--------|--------|-----------|---------|---------|
| CCSM3.0 | 0.1580 | 0.2179 | 0.0910 | 0.0215 | 2.7918 | 0.0052 |
| GFDL2.0 | 0.2576 | 0.2124 | 0.0915 | 0.0216 | 2.0977 | 0.0359 |
| GFDL2.1 | 0.3567 | 0.2069 | 0.0854 | 0.0201 | 7.4404 | 0.0000 |
| GISS_EH | 0.1477 | 0.2185 | 0.0906 | 0.0214 | 3.3153 | 0.0009 |
| GISS_ER | 0.1938 | 0.2159 | 0.0919 | 0.0217 | 1.0205 | 0.3075 |
| MIROC3.2_T42 | 0.1285 | 0.2196 | 0.0897 | 0.0211 | 4.3094 | 0.0000 |
| MIROC3.2_T106 | 0.2298 | 0.2139 | 0.0920 | 0.0217 | 0.7305 | 0.4651 |
| MRI2.3.2a | 0.2800 | 0.2111 | 0.0907 | 0.0214 | 3.2196 | 0.0013 |
| PCM | 0.1496 | 0.2184 | 0.0907 | 0.0214 | 3.2170 | 0.0013 |
| HADCM3 | 0.1936 | 0.2159 | 0.0919 | 0.0217 | 1.0327 | 0.3018 |
| HADGEM1 | 0.3099 | 0.2095 | 0.0891 | 0.0210 | 4.7784 | 0.0000 |
| CCCMA3.1 | 0.4236 | 0.2032 | 0.0769 | 0.0181 | 12.1591 | 0.0000 |
| CNRM3.0 | 0.2409 | 0.2133 | 0.0918 | 0.0216 | 1.2762 | 0.2019 |

| | | | | | | |
|---------------|--------|--------|--------|--------|---------|--------|
| CSIRO3.0 | 0.2780 | 0.2113 | 0.0908 | 0.0214 | 3.1195 | 0.0018 |
| ECHAM5 | 0.1252 | 0.2197 | 0.0895 | 0.0211 | 4.4815 | 0.0000 |
| IAP_FGOALS1.0 | 0.1834 | 0.2165 | 0.0917 | 0.0216 | 1.5314 | 0.1257 |
| GISS_AOM | 0.1788 | 0.2168 | 0.0916 | 0.0216 | 1.7579 | 0.0788 |
| INMCM3.0 | 0.0197 | 0.2256 | 0.0790 | 0.0186 | 11.0541 | 0.0000 |
| IPSL_CM4 | 0.2258 | 0.2142 | 0.0920 | 0.0217 | 0.5359 | 0.5920 |

T2LT: No. of p-values .le. 0.05: 12. Rejection rate: 63.16%
T2LT: No. of p-values .le. 0.10: 13. Rejection rate: 68.42%
T2LT: No. of p-values .le. 0.20: 14. Rejection rate: 73.68%

The corresponding rejection rates for the tests involving T2 data are:

T2: No. of p-values .le. 0.05: 12. Rejection rate: 63.16%
T2: No. of p-values .le. 0.10: 13. Rejection rate: 68.42%
T2: No. of p-values .le. 0.20: 15. Rejection rate: 78.95%

Bottom line: If we applied Douglass et al.'s ridiculous test of difference in mean trends to model data only - in fact, to virtually the same model data they used in their paper - one would conclude that nearly two-thirds of the individual models had trends that were significantly different from the multi-model mean trend! To follow Douglass et al.'s flawed logic, this would mean that two-thirds of the models really aren't models after all...

Happy New Year to all of you!

With best regards,

Ben

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email: santer1@llnl.gov

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From: Leopold Haimberger <leopold.haimberger@univie.ac.at>
To: santer1@llnl.gov
Subject: Re: [Fwd: sorry to take your time up, but really do need a scrub of this singer/christy/etc effort]
Date: Sat, 29 Dec 2007 22:10:30 +0100
Cc: John.Lanzante@noaa.gov, "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>, carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Karl Taylor <taylor13@llnl.gov>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Sherwood Steven <steven.sherwood@yale.edu>, Steve Klein <klein21@llnl.gov>, 'Susan Solomon' <susan.solomon@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>

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Ben,

I have attached the tropical mean trend profiles, now for the period 1979-1999.

RAOBCORE versions show much more upper tropospheric heating for this period, RICH shows slightly more heating. Note also stronger cooling of unadjusted radiosondes in stratospheric layers compared to 1999-2004.

Just for information I have included also zonal mean trend plots for the unadjusted radiosondes (tm), RAOBCORE v1.4 (tmcrr) and RICH (rgmra) I do not suggest that these plots should be included but some of you maybe want to know about the spatial coherence of the zonal mean trends. It is interesting to see the lower tropospheric warming minimum in the tropics in all three plots, which I cannot explain. I believe it is spurious but it is remarkably robust against my adjustment efforts.

Meridional resolution is 10 degrees.

As you can imagine, the tropical upper tropospheric heating maximum at 5S and the cooling in the unadjusted radiosondes at 5N are based on very few long records in these belts. 2-3 in 5S, about 5 in 5N.

Best regards and I wish you all a happy new year.

Leo

Ben Santer wrote:

> Dear Leo,

>

> The Figure that you sent is extremely informative, and would be great
> to include in a response to Douglass et al. The Figure clearly
> illustrates that the "structural uncertainties" inherent in
> radiosonde-based estimates of tropospheric temperature change are much
> larger than Douglass et al. have claimed. This is an important point

> to make.
>
> Would it be possible to produce a version of this Figure showing
> results for the period 1979 to 1999 (the period that I've used for
> testing the significance of model-versus-observed trend differences)
> instead of 1979 to 2004?
>
> With best regards, and frohes Neues Jahr!
>
> Ben
> Leopold Haimberger wrote:
>> Dear all,
>>
>> I have attached a plot which summarizes the recent developments
>> concerning tropical radiosonde temperature datasets and which could
>> be a candidate to be included in a reply to Douglass et al.
>> It contains trend profiles from unadjusted radiosondes,
>> HadAT2-adjusted radiosondes, RAOBCORE (versions 1.2-1.4) adjusted
>> radiosondes
>> and from radiosondes adjusted with a neighbor composite method (RICH)
>> that uses the break dates detected with RAOBCORE (v1.4) as metadata.
>> RAOBCORE v1.2,v1.3 are documented in Haimberger (2007), RAOBCORE v1.4
>> and RICH are discussed in the manuscript I mentioned in my previous
>> email.
>> Latitude range is 20S-20N, only time series with less than 24 months
>> of missing data are included. Spatial sampling of all curves is the
>> same except HadAT which contains less stations that meet the 24month
>> criterion. Sampling uncertainty of the trend curves is ca.
>> +/-0.1K/decade (95% percentiles estimated with bootstrap method).
>>
>> RAOBCORE v1.3,1.4 and RICH are results from ongoing research and
>> warming trends from radiosondes may still be underestimated.
>> The upper tropospheric warming maxima from RICH are even larger (up
>> to 0.35K/decade, not shown), if only radiosondes within the tropics
>> (20N-20S) are allowed as reference for adjustment of tropical
>> radiosonde temperatures. The pink/blue curves in the attached plot
>> should therefore not be regarded as upper bound of what may be
>> achieved with plausible choices of reference series for
homogenization.
>> Please let me know your comments.
>>
>> I wish you a merry Christmas.
>>
>> With best regards
>>
>> Leo
>>
>> John Lanzante wrote:
>>> Ben,
>>>
>>> Perhaps a resampling test would be appropriate. The tests you have
>>> performed
>>> consist of pairing an observed time series (UAH or RSS MSU) with
>>> each one

```
>>> of 49 GCM times series from your "ensemble of opportunity".
>>> Significance
>>> of the difference between each pair of obs/GCM trends yields a
certain
>>> number of "hits".
>>>
>>> To determine a baseline for judging how likely it would be to obtain
>>> the
>>> given number of hits one could perform a set of resampling trials by
>>> treating one of the ensemble members as a surrogate observation. For
>>> each
>>> trial, select at random one of the 49 GCM members to be the
>>> "observation".
>>> From the remaining 48 members draw a bootstrap sample of 49, and
>>> perform
>>> 49 tests, yielding a certain number of "hits". Repeat this many
>>> times to
>>> generate a distribution of "hits".
>>>
>>> The actual number of hits, based on the real observations could then
be
>>> referenced to the Monte Carlo distribution to yield a probability
>>> that this
>>> could have occurred by chance. The basic idea is to see if the
observed
>>> trend is inconsistent with the GCM ensemble of trends.
>>>
>>> There are a couple of additional tweaks that could be applied to
>>> your method.
>>> You are currently computing trends for each of the two time series
>>> in the
>>> pair and assessing the significance of their differences. Why not
first
>>> create a difference time series and assess the significance of it's
>>> trend?
>>> The advantage of this is that you would reduce somewhat the
>>> autocorrelation
>>> in the time series and hence the effect of the "degrees of freedom"
>>> adjustment. Since the GCM runs are based on coupled model runs this
>>> differencing would help remove the common externally forced
>>> variability,
>>> but not internally forced variability, so the adjustment would still
be
>>> needed.
>>>
>>> Another tweak would be to alter the significance level used to assess
>>> differences in trends. Currently you are using the 5% level, which
>>> yields
>>> only a small number of hits. If you made this less stringent you
>>> would get
>>> potentially more weaker hits. But it would all come out in the wash
>>> so to
>>> speak since the number of hits in the Monte Carlo simulations would
>>> increase
```

>>> as well. I suspect that increasing the number of expected hits would
>>> make the
>>> whole procedure more powerful/efficient in a statistical sense since
>>> you
>>> would no longer be dealing with a "rare event". In the current
>>> scheme, using
>>> a 5% level with 49 pairings you have an expected hit rate of $0.05 \times$
>>> $49 = 2.45$.
>>> For example, if instead you used a 20% significance level you would
>>> have an
>>> expected hit rate of $0.20 \times 49 = 9.8$.
>>>
>>> I hope this helps.
>>>
>>> On an unrelated matter, I'm wondering a bit about the different
>>> versions of
>>> Leo's new radiosonde dataset (RAOBCORE). I was surprised to see that
>>> the
>>> latest version has considerably more tropospheric warming than I
>>> recalled
>>> from an earlier version that was written up in JCLI in 2007. I have a
>>> couple of questions that I'd like to ask Leo. One concern is that if
>>> we use
>>> the latest version of RAOBCORE is there a paper that we can
>>> reference --
>>> if this is not in a peer-reviewed journal is there a paper in
>>> submission?
>>> The other question is: could you briefly comment on the differences
>>> in methodology used to generate the latest version of RAOBCORE as
>>> compared to the version used in JCLI 2007, and what/when/where did
>>> changes occur to
>>> yield a stronger warming trend?
>>>
>>> Best regards,
>>>
>>> _____ John
>>>
>>>
>>>
>>> On Saturday 15 December 2007 12:21 pm, Thomas.R.Karl wrote:
>>>
>>>> Thanks Ben,
>>>>
>>>> You have the makings of a nice article.
>>>>
>>>> I note that we would expect to 10 cases that are significantly
>>>> different by chance (based on the 196 tests at the .05 sig level).
>>>> You found 3. With appropriately corrected Leopold I suspect you
>>>> will find there is indeed stat sig. similar trends incl.
>>>> amplification. Setting up the statistical testing should be
>>>> interesting with this many combinations.
>>>>
>>>> Regards, Tom
>>>>

>>>
>>>
>>
>
>

--

Ao. Univ. Prof. Dr. Leopold Haimberger
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Attachment Converted: "c:\documents and settings\tim osborn\my documents\eutora\attach\t00_trendzonalGlobe_tm_1979-1999.ps"

From: Susan Solomon <Susan.Solomon@noaa.gov>
To: Tom Wigley <wigley@ucar.edu>, "Thomas.R.Karl"
<Thomas.R.Karl@noaa.gov>
Subject: Re: Douglass et al. paper
Date: Sun, 30 Dec 2007 10:18:04 -0700
Cc: John.Lanzante@noaa.gov, carl mears <mears@remss.com>, "David C.
Bader" <bader2@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>,
"'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz
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"Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn
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Dear All,

Thanks very much for the helpful discussion on these issues.

I write to make a point that may not be well recognized regarding the character of the temperature trends in the lowermost stratosphere/upper troposphere. I have already discussed this with Ben but want to share with others since I believe it is relevant to this controversy at least at some altitudes. The question I want to raise is not related to the very important dialogue on how to handle the errors and the statistics, but rather how to think about the models.

The attached paper by Forster et al. appeared recently in GRL. It taught me something I didn't realize, namely that ozone losses and accompanying temperature trends at higher altitudes can strongly affect lower altitudes, through the influence of downwelling longwave. There is now much evidence that ozone has decreased significantly in the tropics near 70 mbar. What we show in the attached paper by Forster et al is that ozone depletion near 70 mbar affects temperatures not only at that level, but also down to lower altitudes. I think this is bound to be important to the tropical temperature trends at least in the 100-50 mbar height range, possibly lower down as well, depending upon the degree to which there is a 'substratosphere' that is more radiatively influenced than the rest of the troposphere. Whether it can have an influence as low as 200 mbar - I don't know. But note that having an

influence could mean reducing the warming there, not necessarily flipping it over to a net cooling. This 'long-distance' physics, whereby ozone depletion and associated cooling up high can affect the thermal structure lower down, is not a point I had understood despite many years of studying the problem so I thought it worthwhile to point it out to you here. It has often been said (I probably said it myself five years ago) that ozone losses and associated cooling can't happen or aren't important in this region - but that is wrong.

Further, the fundamental point made in the paper of Thompson and Solomon a few years back remains worth noting, and is, I believe, now resolved in the more recent Forster et al paper: that the broad structure of the temperature trends, with quite large cooling in the lowermost stratosphere in the tropics, comparable to that seen at higher latitudes, is a feature NOT explained by e.g. CO2 cooling, but now can be explained by the observed ozone losses. Exactly how big the tropical cooling is, and exactly how low down it goes, remains open to quantitative question and improvement of radiosonde datasets. But I believe the fundamental point we made in 2005 remains true: the temperature trends in the lower stratosphere in the tropics are, even with corrections, quite comparable to that seen at other latitudes. We can now say it is surely linked to the now-well-observed trends in ozone there. The new paper further shows that you don't have to have ozone trends at 100 mbar to have a cooling there, due to down-welling longwave, possibly lower down still. Whether enhanced upwelling is a factor is a central question.

No global general circulation model can possibly be expected to simulate this correctly unless it has interactive ozone, or prescribes an observed tropical ozone trend. The AR4 models did not include this, and any 'discrepancies' are not relevant at all to the issue of the fidelity of those models for global warming. So in closing let me just say that just how low down this effect goes needs more study, but that it does happen and is relevant to the key problem of tropical temperature trends is one that I hope this email has clarified.

Happy new year,
Susan

At 6:13 PM -0700 12/29/07, Tom Wigley wrote:

>Tom,

>

>Yes -- I had this in an earlier version, but I did not want to
>overwhelm people with the myriad errors in the D et al. paper.

>

>I liked the attached item -- also in an earlier version.

>

>Tom.

>

>+++++

>

>Thomas.R.Karl wrote:

>

>>Tom,

>>

>>This is a very nice set of slides clearly
>>showing the problem with the Douglass et al
>>paper. One other aspect of this issue that
>>John L has mentioned and we discussed when we
>>were doing SAP 1.1 relates to difference
>>series. I am not sure whether Ben was
>>calculating the significance of the difference
>>series between sets of observations and model
>>simulations (annually). This would help offset
>>the effects of El-Nino and Volcanoes on the
>>trends.

>>

>>Tom K.

>>

>>Tom Wigley said the following on 12/29/2007 1:05 PM:

>>

>>>Dear all,

>>>

>>>I was recently at a meeting in Rome where Fred Singer was a
participant.

>>>He was not on the speaker list, but, in

>>>advance of the meeting, I had thought

>>>he might raise the issue of the Douglass et

>>>al. paper. I therefore prepared the

>>>attached power point -- modified slightly since returning from Rome.

As it

>>>happened, Singer did not raise the Douglass et al. issue, so I did not
use

>>>the ppt. Still, it may be useful for members

>>>of this group so I am sending it

>>>to you all.

>>>

>>>Please keep this in confidence. I do not want

>>>it to get back to Singer or any

>>>of the Douglass et al. co-authors -- at least

>>>not at this stage while Ben is still

>>>working on a paper to rebut the Douglass et al. claims.
>>>
>>>On slide 6 I have attributed the die tossing
>>>argument to Carl Mears -- but, in
>>>looking back at my emails I can't find the
>>>original. If I've got this attribution
>>>wrong, please let me know.
>>>
>>>Other comments are welcome. Mike MacCracken and Ben helped in putting
>>>this together -- thanks to both.
>>>
>>>Tom.
>>>
>>>+++++
>>
>>
>>--
>>
>>*Dr. Thomas R. Karl, L.H.D.*
>>
>>*/Director/*//
>>
>>NOAA's National Climatic Data Center
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>>Veach-Baley Federal Building
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>>151 Patton Avenue
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>>
>>Thomas.R.Karl@noaa.gov <mailto:Thomas.R.Karl@noaa.gov>
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Attachment Converted: "c:\eudora\attach\ThompsonSolomon2005.pdf"

From: Peter Thorne <peter.thorne@metoffice.gov.uk>
To: Susan Solomon <Susan.Solomon@noaa.gov>
Subject: Re: Douglass et al. paper
Date: Wed, 02 Jan 2008 10:08:31 +0000
Cc: Tom Wigley <wigley@ucar.edu>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, Dian Seidel <dian.seidel@noaa.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Karl Taylor <taylor13@llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <melissa.free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Ben Santer <santer1@llnl.gov>, Steve Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@llnl.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <wigley@cgd.ucar.edu>, Myles Allen <m.allen1@physics.ox.ac.uk>, Bill Fulkerson <wfulk@utk.edu>

Susan et al.,

I had also seen the Forster et al paper and was glad to see he had followed up on work and ideas we had discussed some years ago when he was at Reading and from the Exeter workshop. At the time I had done some simple research on whether the stratosphere could affect the tropical troposphere - possibly through convection modification or radiative cooling. I'd done a simple timeseries regression of $T2LT = a * T_{surf} + b * T4 + c$ and got some regression coefficients out that suggested an influence. Now, this was with old and now discredited data and the Fu et al. technique has since superseded it to some extent (or at least cast considerable doubt upon its efficacy) ... it would certainly be hard to prove in a regression what was cause and effect with such broad weighting functions even using T2LT which still isn't *really* independent from T4.

But one thing I did do to try to "prove" the regression result was real is take the composite differences between QBO phases on 45 years of detrended (can't remember exactly how but I think I took differences from decadal filtered data) data from radiosondes (HadAT1 at the time). This showed a really very interesting result and suggested that this communication if it was real went quite far down in to the troposphere and was statistically significant, particularly in those seasons when the ITCZ and QBO were geographically coincident. I attach the slide for interest. I think this is the only scientifically valid part of the analysis that I would stand by today given the rather massive developments since. I doubt that raobs inhomogeneities could explain the plot result as they project much more onto the trend than they would onto this type of analysis.

The cooling stratosphere may really have an influence even quite low down if this QBO composite technique is a good analogue for a cooling stratosphere's impact, and timeseries regression analysis supports it in some obs (it would be interesting to repeat such an analysis with the newer obs but I don't have time). A counter, however, is that surely the models do radiation so those with ozone loss should do a good job of this effect. This could be checked in Ben's ensemble in a poor man's sense at least because some have ozone depletion and some don't.

The only way this could be a real factor not picked by the models, I concluded at the time, is if models are far too keen to trigger convection and that any real-world increased radiative cooling efficiency effect is masked in the models because they convect far too often and regain CAPE closure as a condition.

On another matter, we seem to be concentrating entirely on layer-average temperatures. This is fine, but we know from CCSP these show little in the way of differences. The key, and much harder test is to capture the differences in behaviour between layers / levels - the "amplification" behaviour. This was the focus of Santer et al. and I still believe is the key scientific question given that each model realisation is inherently so different but that we believe the physics determining the temperature profile to be the key test that has to be answered. Maybe we need to step back and rephrase the question in terms of the physics rather than aiming solely to rebutt Douglass et al? In this case the key physical questions in my view would be:

1. Why is there such strong evidence from sondes for a minima at c. 500? Is this because it is near the triple point of water in the tropics? Or at the top of the shallow convection? Or simply an artefact? [I don't have any good ideas how we would answer the first two of these questions]

2. Is there really a stratospheric radiative influence? If so, how low does it go? What is the cause? Are the numbers consistent with the underlying governing physics or simply an artefact of residual obs errors?

3. Can any models show trend behaviour that deviates from a SALR on multi-decadal timescales? If so, what is it about the model that causes this effect? Physics? Forcings? Phasing of natural variability? Is it also true on shorter timescales in this model?

It seems to me that trying to do an analysis based upon such physical understanding / questions will clarify things far better than simply doing another set of statistical analysis. I'm still particularly interested if #2 is really true in the raobs (its not possible to do with satellites I suspect, but if it is true it means we need to massively rethink Fu et al. type analysis at least in the tropics) and would be interested in helping someone follow up on that ... I think in the future the Forster et al paper may be seen as the more scientifically significant result when Douglass et al is no longer cared about ...

Happy new year to you all.

Peter

--

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Attachment Converted: "c:\eudora\attach\qbo_slide.ppt"

From: Susan Solomon <Susan.Solomon@noaa.gov>
To: P.Jones@uea.ac.uk, Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: urban stuff
Date: Wed, 02 Jan 2008 14:59:03 -0700
Cc: Phil Jones <p.jones@uea.ac.uk>

<x-flowed>

Phil

Thanks for the Benestad reference, which I hadn't seen and will read with interest.

Please keep me in the loop on your reprints.

I'm aware of the work with Dave Thompson, which is very interesting.

Happy new year to you too.

We can all look back on 2007 as a year in which we, the scientists, did a fantastic job.

best

Susan

At 8:59 PM +0000 1/2/08, P.Jones@uea.ac.uk wrote:

- > Kevin, Susan,
- > Working on several things at the moment, so won't
- > have much time for a few weeks. Rasmus Benestad of
- > the Norwegian Met Service wrote a paper on a very similar
- > earlier verion of this McKittrick/Michaels paper (both
- > were in Climate Research). There is nothing new in this
- > paper in JGR.
- > The only thing new in both this JGR paper and the
- > Douglass et al one in IJC is the awful reviewing!!!!
- > Rebuttals help, but often the damage is done once the
- > paper comes out. The MM paper is bad, but the reviewing
- > is even worse. Why did MM refer to an erratum on their
- > paper which is essentially the same? Any reviewer worth
- > any salt should have spotted that and then they would have
- > seen the Benestad comment, which MM surprisingly don't refer to.
- >
- > I'm hoping to submit a paper on urbanization soon -

> based on work with Chinese series - this relates to the
> fraud allegation against Wei-Chyung Wang that Kevin knows
> about.

>
> Also should be a press release tomorrow or Friday about
> the forecast for 2008 temperatures. La Nina looks like making
> it coolish - cooler just than all years since 2001 (including
> 2001) and 1998. Pointing out that 2001-2007 is 0.21 warmer
> than 1991-2000 which is exactly as it should be with ghg-related
> warming of 0.2 per decade.

> [Also working on something with Dave Thompson (Dave's laeding)
> that will have an ENSO-factored out (and COWL) global T series.]

>
> We're (with the Met Office) extending the press release
> due to the silly coverage in mid-December about global warming
> ending, as all years since 1998 are cooler than it. Mostly this
> was by people just parroting the same message from the same
> people. It is a case of people who should know better (and check
> their sources) just copying from people who don't know any
> better.

> Oh - forgot - Happy New Year!

> Any pictures on the IPCC web site of Oslo on Dec 10 !

> Patchy is on the front cover of the last issue of the 2007 in Nature.

> Cheers

> Phil

> Susan

>> Not me. Phil has been involved in various stuff related to this but I
>> am not up to speed. I'll cc him.

>> I recall some exchanges a while ago now.

>> Kevin

>> Susan Solomon wrote:

>>> Kevin

>>> Happy new year to you. All's well here. Have you or other

>>> colleagues organized a rebuttal to the McKittrick and Michaels JGR 2007

>>> material on urbanization? It's getting exposure, along with the
>>> Douglass et al. paper. On the latter, you probably know Ben Santer is
>>> preparing one.
>>> best
>>> Susan

>>

>> --

>> *****

>> Kevin E. Trenberth e-mail: trenbert@ucar.edu
>> Climate Analysis Section, www.cgd.ucar.edu/cas/trenbert.html
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>> P. O. Box 3000, (303) 497 1318
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>>

>> Street address: 1850 Table Mesa Drive, Boulder, CO 80305

>>

>>

>>

</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: More significance testing stuff
Date: Wed, 02 Jan 2008 20:52:31 -0800
Reply-to: santer1@llnl.gov
Cc: John.Lanzante@noaa.gov, carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Dian J. Seidel'" <dian.seidel@noaa.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Karl Taylor <taylor13@llnl.gov>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Sherwood Steven <steven.sherwood@yale.edu>, Steve Klein <klein21@llnl.gov>, 'Susan Solomon' <Susan.Solomon@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Tom Wigley <>wigley@cgd.ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>

<x-flowed>
Dear Tom,

In the end, I decided to test the significance of trends in the $O(t)$ minus $M(t)$ difference time series, as you and John Lanzante have suggested. I still think that this "difference series test" is more appropriate when one is operating on a pair of time series with correlated variability (for example, if you wished to test whether an observed tropical T2LT trend was significantly different from the T2LT trend simulated in an AMIP experiment). But you and John convinced me that our response to Douglass et al. would be strengthened by using several different approaches to address the statistical significance of differences between modeled and observed temperature trends.

The Tables given below show the results from two different types of test. You've already seen the "TYPE1" or "PAIRED TREND" results. These involve $b\{0\}$ and $b\{M\}$, which represent any single pair of Observed and Modeled trends, with standard errors $s\{b0\}$ and $s\{bM\}$ (which are adjusted for temporal autocorrelation effects). As in our previous work (and as in related work by John Lanzante), we define the normalized trend difference d as:

$$d1 = (b\{0\} - b\{M\}) / \text{sqrt}[(s\{b0\})^2 + (s\{bM\})^2]$$

Under the assumption that $d1$ is normally distributed, values of $d1 > +1.96$ or < -1.96 indicate observed-minus-model trend differences that are significant at the 5% level, and one can easily calculate a p-value for each value of d . These p-values for the 98 pairs of trend tests (49 involving UAH data and 49 involving RSS data) are what we use for determining the total number of "hits", or rejections of the null hypothesis of no significant difference between modeled and observed trends. I note that each test is two-tailed, since we have no information a priori about the "direction" of the model trend (i.e., whether we expect the simulated trend to be significantly larger or smaller than observed).

The "TYPE2" results are the "DIFFERENCE SERIES" tests. These involve

$O(t)$ and $M(t)$, which represent any single pair of modeled and observed layer-averaged temperature time series. One first defines the difference time series $D(t) = O(t) - M(t)$, and then calculates the trend $b\{D\}$ in $D(t)$ and its adjusted standard error, $s\{bD\}$. The test statistic is then simply $d2 = b\{D\} / s\{bD\}$. As in the case of the "PAIRED TREND" tests, we assume that $d2$ is normally distributed, and then calculate p-values for the 98 pairs of difference series tests.

As I mentioned in a previous email, the interpretation of the "DIFFERENCE SERIES" tests is a little complicated. Over half (35) of the 49 model simulations examined in the CCSP report include some form of volcanic forcing. In these 35 cases, differencing the $O(t)$ and $M(t)$ time series reduces the amplitude of this externally-forced component in $D(t)$. This will tend to reduce the overall temporal variability of $D(t)$, and hence reduce $s\{bD\}$, the standard error of the trend in $D(t)$. Such noise reduction should make it easier to identify true differences in the anthropogenically-forced components of $b\{O\}$ and $b\{D\}$. But since the internally-generated variability in $O(t)$ and $M(t)$ is uncorrelated, differencing $O(t)$ and $M(t)$ has the opposite effect of amplifying the noise, thus inflating $s\{bD\}$ and making it more difficult to identify model-versus-observed trend differences.

The results given below show that the "PAIRED TREND" and "DIFFERENCE SERIES" tests yield very similar rejection rates of the null hypothesis. The bottom line is that, regardless of which test we use, which significance level we stipulate, which observational dataset we use, or which atmospheric layer we focus on, there is no evidence to support Douglass et al.'s assertion that all "UAH and RSS satellite trends are inconsistent with model results".

REJECTION RATES FOR STIPULATED 5% SIGNIFICANCE LEVEL

| Test type | No. of tests | T2 "Hits" | T2LT "Hits" |
|-------------------------|--------------|-----------|-------------|
| 1. OBS-vs-MODEL (TYPE1) | 49 x 2 (98) | 2 (2.04%) | 1 (1.02%) |
| 2. OBS-vs-MODEL (TYPE2) | 49 x 2 (98) | 2 (2.04%) | 2 (2.04%) |

REJECTION RATES FOR STIPULATED 10% SIGNIFICANCE LEVEL

| Test type | No. of tests | T2 "Hits" | T2LT "Hits" |
|-------------------------|--------------|-----------|-------------|
| 1. OBS-vs-MODEL (TYPE1) | 49 x 2 (98) | 4 (4.08%) | 2 (2.04%) |
| 2. OBS-vs-MODEL (TYPE2) | 49 x 2 (98) | 3 (3.06%) | 3 (3.06%) |

REJECTION RATES FOR STIPULATED 20% SIGNIFICANCE LEVEL

| Test type | No. of tests | T2 "Hits" | T2LT "Hits" |
|-------------------------|--------------|-------------|-------------|
| 1. OBS-vs-MODEL (TYPE1) | 49 x 2 (98) | 7 (7.14%) | 5 (5.10%) |
| 2. OBS-vs-MODEL (TYPE2) | 49 x 2 (98) | 10 (10.20%) | 7 (7.14%) |

As I've mentioned in previous emails, I think it's a little tricky to figure out the null distribution of rejection rates - i.e., the distribution that might be expected by chance alone. My gut feeling is that this is easiest to do by generating distributions of the $d1$ and $d2$ statistics using model control run data only. Use of Monte Carlo procedures gets into issues of whether one should use "block resampling", and attempt to preserve the characteristic decorrelation times of the model and observational data being tested, etc., etc.

Thanks very much to all of you for your advice and comments. I still believe that there is considerable merit in a brief response to Douglass et al. I think this could be done relatively quickly. From my perspective, this response should highlight four issues:

- 1) It should identify the flaws in the statistical approach used by Douglass et al. to compare modeled and observed trends.
- 2) It should do the significance testing properly, and report on the results of "PAIRED TREND" and "DIFFERENCE SERIES" tests.
- 3) It should show something similar to the figure that Leo recently distributed (i.e., zonal-mean trend profiles in various versions of the RAOBCORE data), and highlight the fact that the structural uncertainty in sonde-based estimates of tropospheric temperature change is much larger than was claimed in Douglass et al.
- 4) It should note and discuss the considerable body of "complementary evidence" supporting the finding that the tropical lower troposphere has warmed over the satellite era.

With best regards,

Ben

Thomas.R.Karl wrote:

> Thanks Ben,
>
> You have been busy! I sent Tom an email before reading the last
> paragraph of this note. Recognizing the "random" placement of ENSO in
> the models and volcanic effects (in a few) and the known impact of the
> occurrence of these events on the trends, I think it is appropriate
> that
> the noise and related uncertainty about the trend differences be
> increased. Amplifying the noise could be argued as an appropriate
> conservative approach, since we know that these events are confounding
> our efforts to see differences between models and obs w/r to greenhouse
> forcing.
>
> I know it is more work, but I think it does make sense to calculate
> $O(1)-M(1)$, $O(2)-M(2)$... $O(n)-M(n)$ for all combinations of observed
> data sets and model simulations. You could test for significance by
> using a Monte Carlo bootstrap approach by randomizing the years for
> both
> models and data.
>
> Regards, Tom
>
>
> Ben Santer said the following on 12/26/2007 9:50 PM:
>> Dear John,

>>
>> Thanks for your email. As usual, your comments were constructive and
>> thought-provoking. I've tried to do some of the additional tests that
>> you suggested, and will report on the results below.
>>
>> But first, let's have a brief recap. As discussed in my previous
>> emails, I've tested the significance of differences between trends in
>> observed MSU time series and the trends in synthetic MSU temperatures
>> in a multi-model "ensemble of opportunity". The "ensemble of
>> opportunity" comprises results from 49 realizations of the CMIP-3
>> "20c3m" experiment, performed with 19 different A/OGCMs. This is the
>> same ensemble that was analyzed in Chapter 5 of the CCSP Synthesis and
>> Assessment Product 1.1.
>> I've used observational results from two different groups (RSS and
>> UAH). From each group, we have results for both T2 and T2LT. This
>> yields a total of 196 different tests of the significance of
>> observed-versus-model trend differences (2 observational datasets x 2
>> layer-averaged temperatures x 49 realizations of the 20c3m
>> experiment). Thus far, I've tested the significance of trend
>> differences using T2 and T2LT data spatially averaged over oceans only
>> (both 20N-20S and 30N-30S), as well as over land and ocean (20N-20S).
>> All results described below focus on the land and ocean results, which
>> facilitates a direct comparison with Douglass et al.
>>
>> Here was the information that I sent you on Dec. 14th:
>>
>> COMBINED LAND/OCEAN RESULTS (WITH STANDARD ERRORS ADJUSTED FOR
>> TEMPORAL AUTOCORRELATION EFFECTS; SPATIAL AVERAGES OVER 20N-20S;
>> ANALYSIS PERIOD 1979 TO 1999)
>>
>> T2LT tests, RSS observational data: 0 out of 49 model-versus-observed
>> trend differences are significant at the 5% level.
>> T2LT tests, UAH observational data: 1 out of 49 model-versus-observed
>> trend differences are significant at the 5% level.
>>
>> T2 tests, RSS observational data: 1 out of 49 model-versus-observed
>> trend differences are significant at the 5% level.
>> T2 tests, UAH observational data: 1 out of 49 model-versus-observed
>> trend differences are significant at the 5% level.
>>
>> In other words, at a stipulated significance level of 5% (for a
>> two-tailed test), we rejected the null hypothesis of "No significant
>> difference between observed and simulated tropospheric temperature
>> trends" in only 1 out of 98 cases (1.02%) for T2LT and 2 out of 98
>> cases (2.04%) for T2.
>>
>> You asked, John, how we might determine a baseline for judging the
>> likelihood of obtaining the 'observed' rejection rate by chance alone.
>> You suggested use of a bootstrap procedure involving the model data
>> only. In this procedure, one of the 49 20c3m realizations would be
>> selected at random, and would constitute the "surrogate observations".
>> The remaining 48 members would be randomly sampled (with replacement)
>> 49 times. The significance of the difference between the surrogate
>> "observed" trend and the 49 simulated trends would then be assessed.

>> This procedure would be repeated many times, yielding a distribution
>> of rejection rates of the null hypothesis.
>>
>> As you stated in your email, "The actual number of hits, based on the
>> real observations could then be referenced to the Monte Carlo
>> distribution to yield a probability that this could have occurred by
>> chance."
>>
>> One slight problem with your suggested bootstrap approach is that it
>> convolves the trend differences due to internally-generated
>> variability with trend differences arising from inter-model
>> differences in both climate sensitivity and in the forcings applied in
>> the 20c3m experiment. So the distribution of "hits" (as you call it;
>> or "rejection rates" in my terminology) is not the distribution that
>> one might expect due to chance alone.
>>
>> Nevertheless, I thought it would be interesting to generate a
>> distribution of "rejection rates" based on model data only. Rather
>> than implementing the resampling approach that you suggested, I
>> considered all possible combinations of trend pairs involving model
>> data, and performed the paired difference test between the trend in
>> each 20c3m realization and in each of the other 48 realizations. This
>> yields a total of 2352 (49 x 48) non-identical pairs of trend tests
>> (for each layer-averaged temperature time series).
>>
>> Here are the results:
>>
>> T2: At a stipulated 5% significance level, 58 out of 2352 tests
>> involving model data only (2.47%) yielded rejection of the null
>> hypothesis of no significant difference in trend.
>>
>> T2LT: At a stipulated 5% significance level, 32 out of 2352 tests
>> involving model data only (1.36%) yielded rejection of the null
>> hypothesis of no significant difference in trend.
>>
>> For both layer-averaged temperatures, these numbers are slightly
>> larger than the "observed" rejection rates (2.04% for T2 and 1.02% for
>> T2LT). I would conclude from this that the statistical significance of
>> the differences between the observed and simulated MSU tropospheric
>> temperature trends is comparable to the significance of the
>> differences between the simulated 20c3m trends from any two CMIP-3
>> models (with the proviso that the simulated trend differences arise
>> not only from internal variability, but also from inter-model
>> differences in sensitivity and 20th century forcings).
>>
>> Since I was curious, I thought it would be fun to do something a
>> little closer to what you were advocating, John - i.e., to use model
>> data to look at the statistical significance of trend differences that
>> are NOT related to inter-model differences in the 20c3m forcings or in
>> climate sensitivity. I did this in the following way. For each model
>> with multiple 20c3m realizations, I tested each realization against
>> all other (non-identical) realizations of that model - e.g., for a
>> model with an 20c3m ensemble size of 5, there are 20 paired trend
>> tests involving non-identical data. I repeated this procedure for the

>> next model with multiple 20c3m realizations, etc., and accumulated
>> results. In our CCSP report, we had access to 11 models with multiple
>> 20c3m realizations. This yields a total of 124 paired trend tests for
>> each layer-averaged temperature time series of interest.
>>
>> For both T2 and T2LT, NONE of the 124 paired trend tests yielded
>> rejection of the null hypothesis of no significant difference in trend
>> (at a stipulated 5% significance level).
>>
>> You wanted to know, John, whether these rejection rates are sensitive
>> to the stipulated significance level. As per your suggestion, I also
>> calculated rejection rates for a 20% significance level. Below, I've
>> tabulated a comparison of the rejection rates for tests with 5% and
>> 20% significance levels. The two "rows" of "MODEL-vs-MODEL" results
>> correspond to the two cases I've considered above - i.e., tests
>> involving 2352 trend pairs (Row 2) and 124 trend pairs (Row 3). Note
>> that the "OBSERVED-vs-MODEL" row (Row 1) is the combined number of
>> "hits" for 49 tests involving RSS data and 49 tests involving UAH
data:

>>
>> REJECTION RATES FOR STIPULATED 5% SIGNIFICANCE LEVEL:
>> Test type No. of tests T2 "Hits" T2LT "Hits"
>>
>> Row 1. OBSERVED-vs-MODEL 49 x 2 2 (2.04%) 1 (1.02%)
>> Row 2. MODEL-vs-MODEL 2352 58 (2.47%) 32 (1.36%)
>> Row 3. MODEL-vs-MODEL 124 0 (0.00%) 0 (0.00%)
>>

>> REJECTION RATES FOR STIPULATED 20% SIGNIFICANCE LEVEL:
>> Test type No. of tests T2 "Hits" T2LT "Hits"
>>
>> Row 1. OBSERVED-vs-MODEL 49 x 2 7 (7.14%) 5 (5.10%)
>> Row 2. MODEL-vs-MODEL 2352 176 (7.48%) 100 (4.25%)
>> Row 3. MODEL-vs-MODEL 124 8 (6.45%) 6 (4.84%)
>>

>> So what can we conclude from this?
>>
>> 1) Irrespective of the stipulated significance level (5% or 20%), the
>> differences between the observed and simulated MSU trends are, on
>> average, substantially smaller than we might expect if we were
>> conducting these tests with trends selected from a purely random
>> distribution (i.e., for the "Row 1" results, 2.04 and 1.02% << 5%, and
>> 7.14% and 5.10% << 20%).
>>

>> 2) Why are the rejection rates for the "Row 3" results substantially
>> lower than 5% and 20%? Shouldn't we expect - if we are only testing
>> trend differences between multiple realizations of the same model,
>> rather than trend differences between models - to obtain rejection
>> rates of roughly 5% for the 5% significance tests and 20% for the 20%
>> tests? The answer is clearly "no". The "Row 3" results do not involve
>> tests between samples drawn from a population of randomly-distributed
>> trends! If we were conducting this paired test using randomly-sampled
>> trends from a long control simulation, we would expect (given a
>> sufficiently large sample size) to eventually obtain rejection rates
>> of 5% and 20%. But our "Row 3" results are based on paired samples

>> from individual members of a given model's 20c3m experiment, and thus
>> represent both signal (response to the imposed forcing changes) and
>> noise - not noise alone. The common signal component makes it more
>> difficult to reject the null hypothesis of no significant difference
>> in trend.
>>
>> 3) Your point about sensitivity to the choice of stipulated
>> significance level was well-taken. This is obvious by comparing "Row
>> 3" results in the 5% and 20% test cases.
>>
>> 4) In both the 5% and 20% cases, the rejection rate for paired tests
>> involving model-versus-observed trend differences ("Row 1") is
>> comparable to the rejection rate for tests involving inter-model trend
>> differences ("Row 2") arising from the combined effects of differences
>> in internal variability, sensitivity, and applied forcings. On
>> average, therefore, model versus observed trend differences are not
>> noticeably more significant than the trends between any given pair of
>> CMIP-3 models. [N.B.: This inference is not entirely justified, since,
>> "Row 2" convolves the effects of both inter-model differences and
>> "within model" differences arising from the different manifestations
>> of natural variability superimposed on the signal. We would need a
>> "Row 4", which involves 19 x 18 paired tests of model results, using
>> only one 20c3m realization from each model. I'll generate "Row 4"
>> tomorrow.]
>>
>> John, you also suggested that we might want to look at the statistical
>> significance of trends in time series of differences - e.g., in $O(t)$
>> minus $M(t)$, or in $M_1(t)$ minus $M_2(t)$, where "O" denotes observations,
>> and "M" denotes model, and t is an index of time in months. While I've
>> done this in previous work (for example in the Santer et al. 2000 JGR
>> paper, where we were looking at the statistical significance of trend
>> differences between multiple observational upper air temperature
>> datasets), I don't think it's advisable in this particular case. As
>> your email notes, we are dealing here with A/OGCM results in which the
>> phasing of El Ninos and La Ninas (and the effects of ENSO variability
>> on T2 and T2LT) differs from the phasing in the real world. So
>> differencing $M(t)$ from $O(t)$, or $M_2(t)$ from $M_1(t)$, probably actually
>> amplifies rather than damps noise, particularly in the tropics, where
>> the externally-forced component of $M(t)$ or $O(t)$ over 1979 to 1999 is
>> only a relatively small fraction of the overall variance of the time
>> series. I think this amplification of noise is a disadvantage in
>> assessing whether trends in $O(t)$ and $M(t)$ are significantly different.
>>
>> Anyway, thanks again for your comments and suggestions, John. They
>> gave me a great opportunity to ignore the hundreds of emails that
>> accumulated in my absence, and instead do some science!
>>
>> With best regards,
>>
>> Ben
>>
>> John Lanzante wrote:
>>> Ben,
>>>

>>> Perhaps a resampling test would be appropriate. The tests you have
>>> performed
>>> consist of pairing an observed time series (UAH or RSS MSU) with each
>>> one
>>> of 49 GCM times series from your "ensemble of opportunity".
Significance
>>> of the difference between each pair of obs/GCM trends yields a
certain
>>> number of "hits".
>>>
>>> To determine a baseline for judging how likely it would be to obtain
the
>>> given number of hits one could perform a set of resampling trials by
>>> treating one of the ensemble members as a surrogate observation. For
>>> each
>>> trial, select at random one of the 49 GCM members to be the
>>> "observation".
>>> >From the remaining 48 members draw a bootstrap sample of 49, and
>>> perform
>>> 49 tests, yielding a certain number of "hits". Repeat this many times
to
>>> generate a distribution of "hits".
>>>
>>> The actual number of hits, based on the real observations could then
be
>>> referenced to the Monte Carlo distribution to yield a probability
>>> that this
>>> could have occurred by chance. The basic idea is to see if the
observed
>>> trend is inconsistent with the GCM ensemble of trends.
>>>
>>> There are a couple of additional tweaks that could be applied to your
>>> method.
>>> You are currently computing trends for each of the two time series in
>>> the
>>> pair and assessing the significance of their differences. Why not
first
>>> create a difference time series and assess the significance of it's
>>> trend?
>>> The advantage of this is that you would reduce somewhat the
>>> autocorrelation
>>> in the time series and hence the effect of the "degrees of freedom"
>>> adjustment. Since the GCM runs are based on coupled model runs this
>>> differencing would help remove the common externally forced
variability,
>>> but not internally forced variability, so the adjustment would still
be
>>> needed.
>>>
>>> Another tweak would be to alter the significance level used to assess
>>> differences in trends. Currently you are using the 5% level, which
>>> yields
>>> only a small number of hits. If you made this less stringent you
>>> would get

>>> potentially more weaker hits. But it would all come out in the wash
>>> so to
>>> speak since the number of hits in the Monte Carlo simulations would
>>> increase
>>> as well. I suspect that increasing the number of expected hits would
>>> make the
>>> whole procedure more powerful/efficient in a statistical sense since
>>> you
>>> would no longer be dealing with a "rare event". In the current
>>> scheme, using
>>> a 5% level with 49 pairings you have an expected hit rate of $0.05 \times$
>>> $49 = 2.45$.
>>> For example, if instead you used a 20% significance level you would
>>> have an
>>> expected hit rate of $0.20 \times 49 = 9.8$.
>>>
>>> I hope this helps.
>>>
>>> On an unrelated matter, I'm wondering a bit about the different
>>> versions of
>>> Leo's new radiosonde dataset (RAOBCORE). I was surprised to see that
>>> the
>>> latest version has considerably more tropospheric warming than I
>>> recalled
>>> from an earlier version that was written up in JCLI in 2007. I have a
>>> couple of questions that I'd like to ask Leo. One concern is that if
>>> we use
>>> the latest version of RAOBCORE is there a paper that we can reference
>>> --
>>> if this is not in a peer-reviewed journal is there a paper in
>>> submission?
>>> The other question is: could you briefly comment on the differences
>>> in methodology used to generate the latest version of RAOBCORE as
>>> compared to the version used in JCLI 2007, and what/when/where did
>>> changes occur to
>>> yield a stronger warming trend?
>>>
>>> Best regards,
>>>
>>> _____ John
>>>
>>>
>>>
>>> On Saturday 15 December 2007 12:21 pm, Thomas.R.Karl wrote:
>>>> Thanks Ben,
>>>>
>>>> You have the makings of a nice article.
>>>>
>>>> I note that we would expect to 10 cases that are significantly
>>>> different by chance (based on the 196 tests at the .05 sig level).
>>>> You found 3. With appropriately corrected Leopold I suspect you
>>>> will find there is indeed stat sig. similar trends incl.
>>>> amplification. Setting up the statistical testing should be
>>>> interesting with this many combinations.

>>>>
>>>> Regards, Tom
>>>
>>
>>
>
> --
>
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>
> */Director/*//
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</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Thanks for the photos of Nick !
Date: Fri, 04 Jan 2008 09:57:21 -0800
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

I was very sorry to hear of Hannah's health problems. I hope she makes a speedy recovery. Please give her my best wishes, and tell her that there is life and love after divorce!

My Mom's cataract surgery did not go very well, and it looks like she won't be able to drive any longer. Nick and I are best placed to take care of her, so I'm trying to persuade her to move to California. So there could be some big changes in our lives in 2008.

Nick has turned into a fine young man. It's going to be tough to see him leave for college in three and a half years.

I share your frustration about having to devote valuable time to the rebuttal of crappy papers. Douglass et al. is truly awful. It should never have been published. Any residual respect I might have had for John Christy has now vanished. I can't believe that he's a coauthor on this garbage.

Best wishes to all of you from rainy Livermore,

Ben

Phil Jones wrote:

>

>> Ben,

> Thanks for the card and photos of Nick and your caving exploits
> with Tom and Karl !

> Had a quiet Christmas and New Year. We did get to see Poppy
> at Hannah's house in Deal in Kent. Matthew and Miranda came as well
> along with Ruth's mum - so she saw her great granddaughter.

> We were there as Hannah had to have another cyst removed from around
> her ovary - all is well and she's recovering. Ruth has been with her since
> mid-December. Hannah had an earlier cyst when she was 12, but this time
> they managed to save the ovary. She still needs to see a gynaecologist to

> see if the ovary is still working OK.
> 2007 hasn't been a great year for Hannah, as she has started divorce
> proceedings from her husband (Gordon). They only married in 2005. He
> seemed fine initially, but has had at least 2 affairs.
>
> Keep up the good work on the Douglass et al comment. I'm trying to
> finish
> a few things in the next couple of months. I will comment on drafts if
> you want.
> Susan Solomon is trying to encourage me to respond to this piece of
> rubbish. I'll try and encourage Rasmus Benestad of DNMI to respond. He did
> so last time to a very similar paper in Climate Research. MM don't
> refer to
> that and MM don't use RSS data! Their analysis is flawed anyway, but it
> would
> all go away if they had used RSS instead of UAH!
>
> What gets me is who are the reviewers of these two awful papers. I know
> editors have a hard time finding reviewers, but they must have known that
> both papers were likely awful. It seems that editors (even of these
> two used-to-be OK
> journals) just want more papers.
>
> Sad day - coming in to hear of Bert Bolin's death.
>
> Cheers
> Phil
>
>
>
>
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From: Phil Jones <p.jones@uea.ac.uk>
To: "Humphrey, Kathryn (CEOSA)" <kathryn.humphrey@DEFRA.GSI.GOV.UK>, "Stephens, A (Ag)" <A.Stephens@rl.ac.uk>
Subject: RE: Questions on the weather generator
Date: Fri Jan 4 12:07:45 2008
Cc: "David Sexton" <david.sexton@metoffice.gov.uk>, <C.G.Kilsby@newcastle.ac.uk>, "Jenkins, Geoff" <geoff.jenkins@metoffice.gov.uk>

Kathryn,

I did talk to the Metro yesterday - no idea what they used. Maybe a few will have read it - before copies are tossed around on the tube!

Added Geoff on this email.

Ag has answered the second question. I may come back to that after trying to answer the first part.

There are two aspects to the WG work we're doing. The first, which I've mentioned on a number of occasions, is to prove that the perturbation process used with the WG works. Colin Harpham sent around a load of plots to Chris/Ag/David/Geoff just before Christmas. I have a rough draft of a paper on this which I sent to Chris yesterday. This involves the UKCIP08 WG, but is totally independent of the change factors David is developing for UKCIP08. This uses some earlier HadRM3 model runs. The WG is fit to 10 grid box series across the UK and then perturbed according to the differences between the future model integrations and the control runs. We then generate future weather and show that its characteristics are similar to what HadRM3 got directly. This has used the same change factors (same variables) but from a different set of RCM runs.

The whole purpose of this exercise is to show that the perturbation process works. The only way we can test this is to use RCM model runs - because they have future runs with a big climate change. We can't use past weather data as it doesn't have enough of a climate change. This is validation of the perturbation process.

We can additionally validate the WG using observational data - which we've done earlier.

Return to Q2. Ag has said how the model variants get chosen. The model variants used have a variety of ways of being chosen. Let's say we start with the 50th percentile for rainfall. We select all model variants between 45 and 55%. Then we want temperature at the 90th percentile. We then do a second selection of the variants already selected that have temperature changes between 85 and 95%. As we had initially 10,000 variants, the first selection reduced this to a 1000 (as we chose 10% of them). The second selection reduced this to 100 (as we've again chosen only 10% of them).

Now with these 100 variants, most users will average the change factors (from David) across these 100. These average change factors (which will approximately be at the 50% and 90% value for precipitation and temperature respectively) get passed to the WG. The WG then simulates 100 runs of 30 years - for the already pre-selected location (small area) and future period.

There are obviously loads of permutations as we will be allowing users to select all percentile levels (singly for temperature or precipitation) or jointly for both from 5 to 95 % in steps of 5.

The percentile levels can be chosen based on seasons (4) and years (1). If you select summer say, users will also get the rest of the year - using the change factors that

go along with those for the selected model variants.

Another possibility is to select one model variant within the chosen percentile bands and pass these change factors to the WG.

There are other possibilities, but I think we've limited the choices to these two. The other possibility was a variant (can't think of a better word here - but not related to the model variants) to the first. As you have 100 chosen model variants in this example, you could choose one at random or allow each of the 100 WG integrations to be based on a different one of the model variants. These generated sequences will likely have greater variability than that based on the average of the 100 or that based on the single model variant.

I think this may open up a can of worms with Ag when he reads it !

Whichever of these are chosen, the use should still run the WG for 100 30-year sequences.

I think I've made the last bit on model variant selection complicated and haven't gone back to look at what Ag has written in the User Guidance. It ought to tell you how the change factors that the WG needs will get selected.

Cheers
Phil

At 10:07 04/01/2008, Humphrey, Kathryn (CEOSA) wrote:

Hi Ag,

Yes that makes perfect sense in terms of selecting one/several model variant/s, thanks. I'm still a bit confused about the utility of random sampling though as this won't give you results for a particular probability level (will it?). I think Phil was going to get back to me on this as well as the change factors question.

Phil, I liked your quote in the Metro this morning!

Kathryn

From: Stephens, A (Ag) [[1]mailto:A.Stephens@rl.ac.uk]
Sent: 04 January 2008 08:56
To: Humphrey, Kathryn (CEOSA)
Cc: Phil Jones; David Sexton; C.G.Kilsby@newcastle.ac.uk
Subject: RE: Questions on the weather generator
Hi Kathryn,

I can comment on your second question. Here is my understanding:

Firstly, users must run a minimum of 100 WG runs regardless of which ones they run. This is to enforce the use of a "probabilistic" approach.

Selection by model variant will only make sense once a user has produced some runs. After any run they will have access to the model variant IDs that were used. The use case that gave rise to us including "selection by model variant ID" was as follows:

1. Person X does some WG runs (sampling by whatever method she chooses).
2. She uses/analyses a set of runs to produce some interesting results.
3. She is keen to do more/different analyses using the model variants that represented that part of parameter space.
4. She has the list of model variant IDs so she can publish these so that others can use them or she can re-use them herself in other experiments.
5. Person Y can read about what Person X did and re-produce exactly her results, or use the same set of interesting model variants for some other experiments.

Does that make sense?

Cheers,

Ag

From: Humphrey, Kathryn (CEOSA) [[2]mailto:kathryn.humphrey@DEFRA.GSI.GOV.UK]

Sent: 03 January 2008 16:58
To: Stephens, A (Ag)
Subject: FW: Questions on the weather generator

From: Humphrey, Kathryn (CEOSA)
Sent: 03 January 2008 16:55
To: 'Phil Jones'; 'Chris Kilsby'; 'Stephens, Ag'
Subject: Questions on the weather generator
Phil/Chris/Ag,

I'm putting together a "quick and easy" presentation on the UKCIP08 methodology for Defra officials to give them some idea of how it's all done so they can better appreciate what's its potential uses may, and may not, be. However I'm getting stuck still on some of the WG methodology! Can you help? (I'm not planning on telling them this level of detail about the WG but am just bothered by the issues below).

I'm firstly confused about the RCM change factors; are you using these to validate the WG runs (which I do understand) or to generate them (which I don't as I thought they were being generated using the data in final PDFs themselves)?

And I'm still confused about the reasons for allowing users to select runs by model variant. I think by model variant you mean each perturbed version of HadCM3 or other single model run or emulator result that creates a point in parameter space. Is this right? If so then I understand why you can't run your WG on all model variants (too many) so selecting a random sample is a representation of parameter space. But my initial understand of how the WG works is that you pick a point on the PDF (say 50th percentile) with a given probability and run the WG for that point. But this doesn't make sense if you are allowing users to select random/ single model variants seasons etc. because these won't reflect a particular percentile. Maybe it's the case that you don't need a particular percentile for whatever use the WG data is for, but if you don't know, how do you know how likely your WG output is and therefore what to do with the result in terms of planning?

Apologies for my ignorance and assistance would be gratefully received!

Kind Regards,

Kathryn

Kathryn Humphrey

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Department for Environment, Food and Rural Affairs (Defra)

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References

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To: Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "Dian J. Seidel" <dian.seidel@noaa.gov>, "Francis W. Zwiers" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "Philip D. Jones" <p.jones@uea.ac.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>
Subject: Update on response to Douglass et al.
Date: Wed, 09 Jan 2008 19:52:15 -0800
Reply-to: santer1@llnl.gov

<x-flowed>

Dear folks,

I just wanted to update you on my progress in formulating a response to the Douglass et al. paper in the International Journal of Climatology (IJC). There have been several developments.

First, I contacted Science to gauge their level of interest in publishing a response to Douglass et al. I thought it was worthwhile to "test the water" before devoting a lot of time to the preparation of a manuscript for submission to Science. I spoke with Jesse Smith, who handles most of the climate-related papers at Science magazine.

The bottom line is that, while Science is interested in this issue (particularly since Douglass et al. are casting doubt on the findings of the 2005 Santer et al. Science paper), Jesse Smith thought it was highly unlikely that Science would carry a rebuttal of work published in a different journal (IJC). Regretfully, I agree. Our response to Douglass et al. does not contain any fundamentally new science - although it does contain some new and interesting work (see below).

It's an unfortunate situation. Singer is promoting the Douglass et al. paper as a startling "new scientific evidence", which undercuts the key conclusions of the IPCC and CCSP Reports. Christy is using the Douglass et al. paper to argue that his UAH group is uniquely positioned to perform "hard-nosed" and objective evaluation of model performance, and that it's dangerous to leave model evaluation in the hands of biased modelers. Much as I would like to see a high-profile rebuttal of Douglass et al. in a journal like Science or Nature, it's unlikely that either journal will publish such a rebuttal.

So what are our options? Personally, I'd vote for GRL. I think that it is important to publish an expeditious response to the statistical flaws in Douglass et al. In theory, GRL should be able to give us the desired fast turnaround time. Would GRL accept our contribution, given that the Douglass et al. paper was published in IJC? I think they would - we've

done a substantial amount of new work (see below), and can argue, with some justification, that our contribution is more than just a rebuttal of Douglass et al.

Why not go for publication of a response in IJC? According to Phil, this option would probably take too long. I'd be interested to hear any other thoughts you might have on publication options.

Now to the science (with a lower-case "s"). I'm appending three candidate Figures for a GRL paper. The first Figure was motivated by discussions I've had with Karl Taylor and Tom Wigley. It's an attempt to convey the differences between our method of comparing observed and simulated trends (panel A) and the approach used by Douglass et al. (panel B).

In our method, we account for both statistical uncertainties in fitting least-squares linear trends to noisy, temporally-autocorrelated data and for the effects of internally-generated variability. As I've described in previous emails, we compare each of the 49 simulated T2 and T2LT trends (i.e., the same multi-model ensemble used in our 2005 Science paper and in the 2006 CCSP Report) with observed T2 and T2LT trends obtained from the RSS and UAH groups. Our 2-sigma confidence intervals on the model and observed trends are estimated as in Santer et al. (2000). [Santer, B.D., T.M.L. Wigley, J.S. Boyle, D.J. Gaffen, J.J. Hnilo, D. Nychka, D.E. Parker, and K.E. Taylor, 2000: Statistical significance of trends and trend differences in layer-average atmospheric temperature time series, *J. Geophys. Res.*, 105, 7337-7356]

The method that Santer et al. (2000) used to compute "adjusted" trend confidence intervals accounts for the fact that, after fitting a trend to T2 or T2LT data, the regression residuals are typically highly autocorrelated. If this autocorrelation is not accounted for, one could easily reach incorrect decisions on whether the trend in an individual time series is significantly different from zero, or whether two time series have significantly different trends. Santer et al. (2000) accounted for temporal autocorrelation effects by estimating $r\{1\}$, the lag-1 autocorrelation of the regression residuals, using $r\{1\}$ to calculate an effective sample size $n\{e\}$, and then using $n\{e\}$ to determine an adjusted standard error of the least-squares linear trend. Panel A of Figure 1 shows the 2-sigma "adjusted" standard errors for each individual trend. Models with excessively large tropical variability (like FGOALS-g1.0 and GFDL-CM2.1) have large adjusted standard errors. Models with coarse-resolution OGCMs and low-amplitude ENSO variability (like the GISS-AOM) have smaller than observed adjusted standard errors. Neglect of volcanic forcing (i.e., absence of El Chichon and Pinatubo-induced temperature variability) can also contribute to smaller than observed standard errors, as in CCCma-CGCM3.1(T47).

The dark and light grey bars in Panel A show (respectively) the 1- and 2-sigma standard errors for the RSS T2LT trend. As is visually obvious, 36 of the 49 model trends are within 1 standard error of the RSS trend, and 47 of the 49 model trends are within 2 standard errors of the RSS trend.

I've already explained our "paired trend test" procedure for calculating the statistical significance of the model-versus-observed trend differences. This involves the normalized trend difference d_1 :

$$d_1 = (b\{O\} - b\{M\}) / \sqrt{[(s\{bO\})^{**2} + (s\{bM\})^{**2}]}$$

where $b\{O\}$ and $b\{M\}$ represent any single pair of Observed and Modeled trends, with adjusted standard errors $s\{bO\}$ and $s\{bM\}$.

Under the assumption that d_1 is normally distributed, values of $d_1 > +1.96$ or < -1.96 indicate observed-minus-model trend differences that are significant at some stipulated significance level, and one can easily calculate a p-value for each value of d_1 . These p-values for the 98 pairs of trend tests (49 involving UAH data and 49 involving RSS data) are what we use for determining the total number of "hits", or rejections of the null hypothesis of no significant difference between modeled and observed trends. I note that each test is two-tailed, since we have no information a priori about the "direction" of the model trend (i.e., whether we expect the simulated trend to be significantly larger or smaller than observed).

REJECTION RATES FOR "PAIRED TREND TESTS, OBS-vs-MODEL

| Stipulated sign. level | No. of tests | T2 "Hits" | T2LT "Hits" |
|------------------------|--------------|-----------|-------------|
| 5% | 49 x 2 (98) | 2 (2.04%) | 1 (1.02%) |
| 10% | 49 x 2 (98) | 4 (4.08%) | 2 (2.04%) |
| 15% | 49 x 2 (98) | 7 (7.14%) | 5 (5.10%) |

Now consider Panel B of Figure 1. It helps to clarify the differences between the Douglass et al. comparison of model and observed trends and our own comparison. The black horizontal line ("Multi-model mean trend") is the T2LT trend in the 19-model ensemble, calculated from model ensemble mean trends (the colored symbols). Douglass et al.'s "consistency criterion", $\sigma\{SE\}$, is given by:

$$\sigma\{SE\} = \sigma / \sqrt{N - 1}$$

where σ is the standard deviation of the 19 ensemble-mean trends, and N is 19. The orange and yellow envelopes denote the 1- and 2- $\sigma\{SE\}$ regions.

Douglass et al. use $\sigma\{SE\}$ to decide whether the multi-model mean trend is consistent with either of the observed trends. They conclude that the RSS and UAH trends lie outside of the yellow envelope (the 2- $\sigma\{SE\}$ region), and interpret this as evidence of a fundamental inconsistency between modeled and observed trends. As noted previously,

Douglass et al. obtain this result because they fail to account for statistical uncertainty in the estimation of the RSS and UAH trends. They ignore the statistical error bars on the RSS and UAH trends (which are shown in Panel A). As is clear from Panel A, the statistical error bars on the RSS and UAH trends overlap with the Douglass et al. 2- $\sigma\{SE\}$ region. Had Douglass et al. accounted for statistical uncertainty in estimation of the observed trends, they would have been unable to conclude that all "UAH and RSS satellite trends are

inconsistent with model trends".

The second Figure plots values of our test statistic (d_1) for the "paired trend test". The grey histogram is based on the values of d_1 for the 49 tests involving the RSS T2LT trend and the simulated T2LT trends from 20c3m runs. The green histogram is for the 49 paired trend tests involving model 20c3m data and the UAH T2LT trend. Note that the d_1 distribution obtained with the UAH data is negatively skewed. This is because the numerator of the d_1 test statistic is $b\{0\} - b\{M\}$, and the UAH tropical T2LT trend over 1979-1999 is smaller than most of the model trends (see Figure 1, panel A).

The colored dots are values of the d_1 test statistic for what I referred to previously as "TYPE2" tests. These tests are limited to the M models with multiple realizations of the 20c3m experiment. Here, $M = 11$. For each of these M models, I performed paired trend tests for all C unique combinations of trends pairs. For example, for a model with 5 realizations of the 20c3m experiment, like GISS-EH, $C = 10$. The significance of trend differences is solely a function of "within-model" effects (i.e., is related to the different manifestations of natural internal variability superimposed on the underlying forced response). There are a total of 62 paired trend tests. Note that the separation of the colored symbols on the y-axis is for visual display purposes only, and facilitates the identification of results for individual models.

The clear message from Figure 2 is that the values of d_1 arising from internal variability alone are typically as large as the d_1 values obtained by testing model trends against observational data. The two negative "outlier" values of d_1 for the model-versus-observed trend tests involve the large positive trend in CCCma-CGCM3.1(T47). If you have keen eagle eyes, you'll note that the distribution of colored symbols is slightly skewed to the negative side. If you look at Panel A of Figure 1, you'll see that this skewness arises from the relatively small ensemble sizes. Consider results for the 5-member ensemble of 20c3m trends from the MRI-CGCM2.3.2. The trend in realization 1 is close to zero; trends in realizations 2, 3, 4, and 5 are large, positive, and vary between 0.27 to 0.37 degrees C/decade. So d_1 is markedly negative for tests involving realization 1 versus realizations 2, 3, 4, and 5. If we showed non-unique combinations of trend pairs (e.g., realization 2 versus realization 1, as well as 1 versus 2), the distribution of colored symbols would be symmetric. But I was concerned that we might be accused of "double counting" if we did this....

The third Figure is the most interesting one. You have not seen this yet. I decided to examine how the Douglass et al. "consistency test" behaves with synthetic data. I did this as a function of sample size N , for N values ranging from 19 (the number of models we used in the CCSP report) to 100. Consider the $N = 19$ case first. I generated 19 synthetic time series using an AR-1 model of the form:

$$xt(i) = a_1 * (xt(i-1) - am) + zt(i) + am$$

where a_1 is the coefficient of the AR-1 model, $zt(i)$ is a randomly-generated noise term, and am is a mean (set to zero here).

Here, I set a_1 to 0.86, close to the lag-1 autocorrelation of the UAH T2LT anomaly data. The other free parameter is a scaling term which controls the amplitude of $z_t(i)$. I chose this scaling term to yield a temporal standard deviation of $x_t(i)$ that was close to the temporal standard deviation of the monthly-mean UAH T2LT anomaly data. The synthetic time series had the same length as the observational and model data (252 months), and monthly-mean anomalies were calculated in the same way as we did for observations and models.

For each of these 19 synthetic time series, I first calculated least-squares linear trends and adjusted standard errors, and then performed the "paired trends". The test involves all 171 unique pairs of trends: $b\{1\}$ versus $b\{2\}$, $b\{1\}$ versus $b\{3\}$, ... $b\{1\}$ versus $b\{19\}$, $b\{2\}$ versus $b\{3\}$, etc. I then calculate the rejection rates of the null hypothesis of "no significant difference in trend", for stipulated significance levels of 5%, 10%, and 20%. This procedure is repeated 1000 times, with 1000 different realizations of 19 synthetic time series. We can therefore build up a distribution of rejection rates for $N = 19$, and then do the same for $N = 20$, etc.

The "paired trend" results are plotted as the blue lines in Figure 3. Encouragingly, the percentage rejections of the null hypothesis are close to the theoretical expectations. The 5% significance tests yield a rejection rate of a little over 6%; 10% tests have a rejection rate of over 11%, and 20% tests have a rejection rate of 21%. I'm not quite sure why this slight positive bias arises. This bias does show some small sensitivity (1-2%) to choice of the a_1 parameter and the scaling term. Different choices of these parameters can give rejection rates that are closer to the theoretical expectation. But my parameter choices for the AR-1 model were guided by the goal of generating synthetic data with roughly the same autocorrelation and variance properties as the UAH data, and not by a desire to get as close as I possibly could to the theoretical rejection rates.

So why is there a small positive bias in the empirically-determined rejection rates? Perhaps Francis can provide us with some guidance here. Karl believes that the answer may be partly linked to the skewness of the empirically-determined rejection rate distributions. For example, for the $N = 19$ case, and for 5% tests, values of rejection rates in the 1000-member distribution range from a minimum of 0 to a maximum of 24%, with a mean value of 6.7% and a median of 6.4%. Clearly, the minimum value is bounded by zero, but the maximum is not bounded, and in rare cases, rejection rates can be quite large, and influences the mean. This inherent skewness must make some contribution to the small positive bias in rejection rates in the "paired trends" test.

What happens if we naively perform the paired trends test WITHOUT adjusting the standard errors of the trends for temporal autocorrelation effects? Results are shown by the black lines in Figure 3. If we ignore temporal autocorrelation, we get the wrong answer. Rejection rates for 5% tests are 60%!

We did not publish results from any of these synthetic data experiments in our 2000 JGR paper. In retrospect, this is a bit of a shame, since

Figure 3 nicely shows that the adjustment for temporal autocorrelation effects works reasonably well, while failure to adjust yields completely erroneous results.

Now consider the red lines in Figure 3. These are the results of applying the Douglass et al. "consistency test" to synthetic data. Again, let's consider the $N = 19$ case first. I calculate the trends in all 19 synthetic time series. Let's consider the first of these 19 time series as the surrogate observations. The trend in this time series, $b\{1\}$, is compared with the mean trend, $b\{\text{Synth}\}$, computed from the remaining 18 synthetic time series. The Douglass $\sigma\{SE\}$ is also computed from these 18 remaining trends. We then form a test statistic $d2 = (b\{1\} - b\{\text{Synth}\}) / \sigma\{SE\}$, and calculate rejection rates for the null hypothesis of no significant difference between the mean trend and the trend in the surrogate observations. This procedure is then repeated with the trend in time series 2 as the surrogate observations, and $b\{\text{Synth}\}$ and $\sigma\{SE\}$ calculated from time series 1, 3, 4, ..19. This yields 19 different tests of the null hypothesis. Repeat 1,000 times, and build up a distribution of rejection rates, as in the "paired trends" test.

The results are truly alarming. Application of the Douglass et al. "consistency test" to synthetic data - data generated with the same underlying AR-1 model! - leads to rejection of the above-stated null hypothesis at least 65% of the time (for $N = 19$, 5% significance tests).

As expected, rejection rates for the Douglass consistency test rise as N increases. For $N = 100$, rejection rates for 5% tests are nearly 85%. As my colleague Jim Boyle succinctly put it when he looked at these results, "This is a pretty hard test to pass".

I think this nicely illustrates the problems with the statistical approach used by Douglass et al. If you want to demonstrate that modeled and observed temperature trends are fundamentally inconsistent, you devise a fundamentally flawed test is very difficult to pass.

I hope to have a first draft of this stuff written up by the end of next week. If Leo is agreeable, Figure 4 of this GRL paper would show the vertical profiles of tropical temperature trends in the various versions of the RAOBCORE data, plus model results.

Sorry to bore you with all the gory details. But as we've seen from Douglass et al., details matter.

With best regards,

Ben

Benjamin D. Santer
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Attachment Converted: "c:\eudora\attach\santer_fig01.pdf"

Attachment Converted: "c:\eudora\attach\santer_fig02.pdf"

Attachment Converted: "c:\eudora\attach\santer_fig03.pdf"

From: dian.seidel@noaa.gov
To: santer1@llnl.gov
Subject: Re: Update on response to Douglass et al.
Date: Thu, 10 Jan 2008 08:40:28 -0500
Cc: Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, carl mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <Melissa.Free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Steven Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

Dear Ben,

Thank you for this detailed update of your work. A few thoughts for your consideration ...

Where to submit this: Although I understand your and Phil's reluctance to try IJC, it seems to me that, despite the new work presented, this is really a comment on Douglass et al. and so rightly belongs in IJC. If you suspect the review and publication process there is unacceptably long, perhaps this should be confirmed by inquiring with the editor, as a professional courtesy. Decide in advance what you'd consider a reasonable turn-around time, and if the editor says it will take longer, going with another journal makes sense.

Figures: They look great. As usual, you've done a super job telling the story in pictures. One suggestion would be to indicate in Fig. 3 which test, or trio of tests, is the most appropriate. Now it is shown as the blue curves, but I'd suggest making these black (and the black ones blue) and thicker than the rest. That way those readers who just skim the paper and look at the figures will get the message quickly.

Observations: Have you considered including results from HadAT and RATPAC as well as RAOBCOR? For even greater completeness, a version of RATPAC pared down based on the results of Randel and Wu could be added, as could Steve Sherwood's adjusted radiosonde data. I'd suggest adding results from these datasets to your Fig. 1, not the planned Fig 4, which I gather is meant to show the differences in versions of RAOBCOR and the impact of Douglass et al.'s choice to use and early version.

With best wishes,
Dian

----- Original Message -----

From: Ben Santer <santer1@llnl.gov>
Date: Wednesday, January 9, 2008 10:52 pm

Subject: Update on response to Douglass et al.

> Dear folks,

>

> I just wanted to update you on my progress in formulating a

> response to

> the Douglass et al. paper in the International Journal of

> Climatology

> (IJC). There have been several developments.

>

> First, I contacted Science to gauge their level of interest in

> publishing a response to Douglass et al. I thought it was

> worthwhile to

> "test the water" before devoting a lot of time to the preparation

> of a

> manuscript for submission to Science. I spoke with Jesse Smith,

> who

> handles most of the climate-related papers at Science magazine.

>

> The bottom line is that, while Science is interested in this issue

> (particularly since Douglass et al. are casting doubt on the

> findings of

> the 2005 Santer et al. Science paper), Jesse Smith thought it was

> highly

> unlikely that Science would carry a rebuttal of work published in

> a

> different journal (IJC). Regretfully, I agree. Our response to

> Douglass

> et al. does not contain any fundamentally new science - although

> it does

> contain some new and interesting work (see below).

>

> It's an unfortunate situation. Singer is promoting the Douglass et

> al.

> paper as a startling "new scientific evidence", which undercuts

> the key

> conclusions of the IPCC and CCSP Reports. Christy is using the

> Douglass

> et al. paper to argue that his UAH group is uniquely positioned to

> perform "hard-nosed" and objective evaluation of model

> performance, and

> that it's dangerous to leave model evaluation in the hands of

> biased

> modelers. Much as I would like to see a high-profile rebuttal of

> Douglass et al. in a journal like Science or Nature, it's unlikely

> that

> either journal will publish such a rebuttal.

>

> So what are our options? Personally, I'd vote for GRL. I think

> that it

> is important to publish an expeditious response to the statistical

> flaws

> in Douglass et al. In theory, GRL should be able to give us the

> desired

> fast turnaround time. Would GRL accept our contribution, given
> that the
> Douglass et al. paper was published in IJC? I think they would -
> we've
> done a substantial amount of new work (see below), and can argue,
> with
> some justification, that our contribution is more than just a
> rebuttal
> of Douglass et al.
>
> Why not go for publication of a response in IJC? According to
> Phil, this
> option would probably take too long. I'd be interested to hear any
> other
> thoughts you might have on publication options.
>
> Now to the science (with a lower-case "s"). I'm appending three
> candidate Figures for a GRL paper. The first Figure was motivated
> by
> discussions I've had with Karl Taylor and Tom Wigley. It's an
> attempt to
> convey the differences between our method of comparing observed
> and
> simulated trends (panel A) and the approach used by Douglass et
> al.
> (panel B).
>
> In our method, we account for both statistical uncertainties in
> fitting
> least-squares linear trends to noisy, temporally-autocorrelated
> data and
> for the effects of internally-generated variability. As I've
> described
> in previous emails, we compare each of the 49 simulated T2 and
> T2LT
> trends (i.e., the same multi-model ensemble used in our 2005
> Science
> paper and in the 2006 CCSP Report) with observed T2 and T2LT
> trends
> obtained from the RSS and UAH groups. Our 2-sigma confidence
> intervals
> on the model and observed trends are estimated as in Santer et al.
> (2000). [Santer, B.D., T.M.L. Wigley, J.S. Boyle, D.J. Gaffen,
> J.J.
> Hnilo, D. Nychka, D.E. Parker, and K.E. Taylor, 2000: Statistical
> significance of trends and trend differences in layer-average
> atmospheric temperature time series, J. Geophys. Res., 105, 7337-
> 7356]
>
> The method that Santer et al. (2000) used to compute "adjusted"
> trend
> confidence intervals accounts for the fact that, after fitting a
> trend
> to T2 or T2LT data, the regression residuals are typically highly

> autocorrelated. If this autocorrelation is not accounted for, one
 > could
 > easily reach incorrect decisions on whether the trend in an
 > individual
 > time series is significantly different from zero, or whether two
 > time
 > series have significantly different trends. Santer et al. (2000)
 > accounted for temporal autocorrelation effects by estimating $r\{1\}$,
 > the
 > lag-1 autocorrelation of the regression residuals, using $r\{1\}$ to
 > calculate an effective sample size $n\{e\}$, and then using $n\{e\}$ to
 > determine an adjusted standard error of the least-squares linear
 > trend.
 > Panel A of Figure 1 shows the 2-sigma "adjusted" standard errors
 > for
 > each individual trend. Models with excessively large tropical
 > variability (like FGOALS-g1.0 and GFDL-CM2.1) have large adjusted
 > standard errors. Models with coarse-resolution OGCMs and low-
 > amplitude
 > ENSO variability (like the GISS-AOM) have smaller than observed
 > adjusted
 > standard errors. Neglect of volcanic forcing (i.e., absence of El
 > Chichon and Pinatubo-induced temperature variability) can also
 > contribute to smaller than observed standard errors, as in
 > CCCma-CGCM3.1(T47).
 >
 > The dark and light grey bars in Panel A show (respectively) the 1-
 > and
 > 2-sigma standard errors for the RSS T2LT trend. As is visually
 > obvious,
 > 36 of the 49 model trends are within 1 standard error of the RSS
 > trend,
 > and 47 of the 49 model trends are within 2 standard errors of the
 > RSS
 > trend.
 >
 > I've already explained our "paired trend test" procedure for
 > calculating
 > the statistical significance of the model-versus-observed trend
 > differences. This involves the normalized trend difference $d1$:
 >
 > $d1 = (b\{O\} - b\{M\}) / \sqrt{ (s\{bO\})^2 + (s\{bM\})^2 }$
 >
 > where $b\{O\}$ and $b\{M\}$ represent any single pair of Observed and
 > Modeled
 > trends, with adjusted standard errors $s\{bO\}$ and $s\{bM\}$.
 >
 > Under the assumption that $d1$ is normally distributed, values of $d1$
 > >
 > +1.96 or < -1.96 indicate observed-minus-model trend differences
 > that
 > are significant at some stipulated significance level, and one can
 > easily calculate a p-value for each value of $d1$. These p-values
 > for the

> 98 pairs of trend tests (49 involving UAH data and 49 involving
 > RSS
 > data) are what we use for determining the total number of "hits",
 > or
 > rejections of the null hypothesis of no significant difference
 > between
 > modeled and observed trends. I note that each test is two-tailed,
 > since
 > we have no information a priori about the "direction" of the model
 > trend
 > (i.e., whether we expect the simulated trend to be significantly
 > larger
 > or smaller than observed).

> REJECTION RATES FOR "PAIRED TREND TESTS, OBS-vs-MODEL

| Stipulated sign. level | No. of tests | T2 "Hits" | T2LT |
|------------------------|--------------|-----------|-----------|
| "Hits" 5% | 49 x 2 | (98) | 2 (2.04%) |
| 1 (1.02%) | | | |
| 10% | 49 x 2 | (98) | 4 (4.08%) |
| (2.04%)15% | 49 x 2 | (98) | 7 (7.14%) |
| 5 (5.10%) | | | |

> Now consider Panel B of Figure 1. It helps to clarify the
 > differences
 > between the Douglass et al. comparison of model and observed
 > trends and
 > our own comparison. The black horizontal line ("Multi-model mean
 > trend")
 > is the T2LT trend in the 19-model ensemble, calculated from model
 > ensemble mean trends (the colored symbols). Douglass et al.'s
 > "consistency criterion", $\sigma\{SE\}$, is given by:

>
$$\sigma\{SE\} = \sigma / \sqrt{N - 1}$$

> where σ is the standard deviation of the 19 ensemble-mean
 > trends,
 > and N is 19. The orange and yellow envelopes denote the 1- and
 > 2- $\sigma\{SE\}$ regions.

> Douglass et al. use $\sigma\{SE\}$ to decide whether the multi-model
 > mean
 > trend is consistent with either of the observed trends. They
 > conclude
 > that the RSS and UAH trends lie outside of the yellow envelope
 > (the
 > 2- $\sigma\{SE\}$ region), and interpret this as evidence of a
 > fundamental
 > inconsistency between modeled and observed trends. As noted
 > previously,
 > Douglass et al. obtain this result because they fail to account
 > for
 > statistical uncertainty in the estimation of the RSS and UAH
 > trends.
 > They ignore the statistical error bars on the RSS and UAH trends

> (which
> are shown in Panel A). As is clear from Panel A, the statistical
> error
> bars on the RSS and UAH trends overlap with the Douglass et al.
> $2\text{-}\sigma\{SE\}$ region. Had Douglass et al. accounted for statistical
> uncertainty in estimation of the observed trends, they would have
> been
> unable to conclude that all "UAH and RSS satellite trends are
> inconsistent with model trends".
>
> The second Figure plots values of our test statistic (d_1) for the
> "paired trend test". The grey histogram is based on the values of
> d_1 for
> the 49 tests involving the RSS T2LT trend and the simulated T2LT
> trends
> from 20c3m runs. The green histogram is for the 49 paired trend
> tests
> involving model 20c3m data and the UAH T2LT trend. Note that the
> d_1
> distribution obtained with the UAH data is negatively skewed. This
> is
> because the numerator of the d_1 test statistic is $b\{0\} - b\{M\}$, and
> the
> UAH tropical T2LT trend over 1979-1999 is smaller than most of the
> model
> trends (see Figure 1, panel A).
>
> The colored dots are values of the d_1 test statistic for what I
> referred
> to previously as "TYPE2" tests. These tests are limited to the M
> models
> with multiple realizations of the 20c3m experiment. Here, $M = 11$.
> For
> each of these M models, I performed paired trend tests for all C
> unique
> combinations of trends pairs. For example, for a model with 5
> realizations of the 20c3m experiment, like GISS-EH, $C = 10$. The
> significance of trend differences is solely a function of "within-
> model"
> effects (i.e., is related to the different manifestations of
> natural
> internal variability superimposed on the underlying forced
> response).
> There are a total of 62 paired trend tests. Note that the
> separation of
> the colored symbols on the y-axis is for visual display purposes
> only,
> and facilitates the identification of results for individual models.
>
> The clear message from Figure 2 is that the values of d_1 arising
> from
> internal variability alone are typically as large as the d_1 values
> obtained by testing model trends against observational data. The
> two

> negative "outlier" values of d1 for the model-versus-observed
 > trend
 > tests involve the large positive trend in CCCma-CGCM3.1(T47). If
 > you
 > have keen eagle eyes, you'll note that the distribution of colored
 > symbols is slightly skewed to the negative side. If you look at
 > Panel A
 > of Figure 1, you'll see that this skewness arises from the
 > relatively
 > small ensemble sizes. Consider results for the 5-member ensemble
 > of
 > 20c3m trends from the MRI-CGCM2.3.2. The trend in realization 1 is
 > close
 > to zero; trends in realizations 2, 3, 4, and 5 are large,
 > positive, and
 > vary between 0.27 to 0.37 degrees C/decade. So d1 is markedly
 > negative
 > for tests involving realization 1 versus realizations 2, 3, 4, and
 > 5. If
 > we showed non-unique combinations of trend pairs (e.g.,
 > realization 2
 > versus realization 1, as well as 1 versus 2), the distribution of
 > colored symbols would be symmetric. But I was concerned that we
 > might be
 > accused of "double counting" if we did this....
 >
 > The third Figure is the most interesting one. You have not seen
 > this
 > yet. I decided to examine how the Douglass et al. "consistency
 > test"
 > behaves with synthetic data. I did this as a function of sample
 > size N,
 > for N values ranging from 19 (the number of models we used in the
 > CCSP
 > report) to 100. Consider the N = 19 case first. I generated 19
 > synthetic
 > time series using an AR-1 model of the form:
 >
 >
$$xt(i) = a1 * (xt(i-1) - am) + zt(i) + am$$

 >
 > where a1 is the coefficient of the AR-1 model, zt(i) is a
 > randomly-generated noise term, and am is a mean (set to zero
 > here).
 > Here, I set a1 to 0.86, close to the lag-1 autocorrelation of the
 > UAH
 > T2LT anomaly data. The other free parameter is a scaling term
 > which
 > controls the amplitude of zt(i). I chose this scaling term to
 > yield a
 > temporal standard deviation of xt(i) that was close to the
 > temporal
 > standard deviation of the monthly-mean UAH T2LT anomaly data. The
 > synthetic time series had the same length as the observational and
 > model

> data (252 months), and monthly-mean anomalies were calculated in
> the
> same way as we did for observations and models.
>
> For each of these 19 synthetic time series, I first calculated
> least-squares linear trends and adjusted standard errors, and then
> performed the "paired trends". The test involves all 171 unique
> pairs of
> trends: $b\{1\}$ versus $b\{2\}$, $b\{1\}$ versus $b\{3\}$, ... $b\{1\}$ versus $b\{19\}$,
> $b\{2\}$
> versus $b\{3\}$, etc. I then calculate the rejection rates of the null
> hypothesis of "no significant difference in trend", for stipulated
> significance levels of 5%, 10%, and 20%. This procedure is
> repeated 1000
> times, with 1000 different realizations of 19 synthetic time
> series. We
> can therefore build up a distribution of rejection rates for $N =$
> 19, and
> then do the same for $N = 20$, etc.
>
> The "paired trend" results are plotted as the blue lines in Figure
> 3.
> Encouragingly, the percentage rejections of the null hypothesis
> are
> close to the theoretical expectations. The 5% significance tests
> yield a
> rejection rate of a little over 6%; 10% tests have a rejection
> rate of
> over 11%, and 20% tests have a rejection rate of 21%. I'm not
> quite sure
> why this slight positive bias arises. This bias does show some
> small
> sensitivity (1-2%) to choice of the a_1 parameter and the scaling
> term.
> Different choices of these parameters can give rejection rates
> that are
> closer to the theoretical expectation. But my parameter choices
> for the
> AR-1 model were guided by the goal of generating synthetic data
> with
> roughly the same autocorrelation and variance properties as the
> UAH
> data, and not by a desire to get as close as I possibly could to
> the
> theoretical rejection rates.
>
> So why is there a small positive bias in the empirically-
> determined
> rejection rates? Perhaps Francis can provide us with some guidance
> here.
> Karl believes that the answer may be partly linked to the skewness
> of
> the empirically-determined rejection rate distributions. For
> example,

> for the N = 19 case, and for 5% tests, values of rejection rates
> in the
> 1000-member distribution range from a minimum of 0 to a maximum of
> 24%,
> with a mean value of 6.7% and a median of 6.4%. Clearly, the
> minimum
> value is bounded by zero, but the maximum is not bounded, and in
> rare
> cases, rejection rates can be quite large, and influences the
> mean. This
> inherent skewness must make some contribution to the small
> positive bias
> in rejection rates in the "paired trends" test.
>
> What happens if we naively perform the paired trends test WITHOUT
> adjusting the standard errors of the trends for temporal
> autocorrelation
> effects? Results are shown by the black lines in Figure 3. If we
> ignore
> temporal autocorrelation, we get the wrong answer. Rejection rates
> for
> 5% tests are 60%!
>
> We did not publish results from any of these synthetic data
> experiments
> in our 2000 JGR paper. In retrospect, this is a bit of a shame,
> since
> Figure 3 nicely shows that the adjustment for temporal
> autocorrelation
> effects works reasonably well, while failure to adjust yields
> completely
> erroneous results.
>
> Now consider the red lines in Figure 3. These are the results of
> applying the Douglass et al. "consistency test" to synthetic data.
> Again, let's consider the N = 19 case first. I calculate the
> trends in
> all 19 synthetic time series. Let's consider the first of these 19
> time
> series as the surrogate observations. The trend in this time
> series,
> $b\{1\}$, is compared with the mean trend, $b\{\text{Synth}\}$, computed from the
> remaining 18 synthetic time series. The Douglass $\sigma\{\text{SE}\}$ is also
> computed from these 18 remaining trends. We then form a test
> statistic
> $d2 = (b\{1\} - b\{\text{Synth}\}) / \sigma\{\text{SE}\}$, and calculate rejection rates
> for
> the null hypothesis of no significant difference between the mean
> trend
> and the trend in the surrogate observations. This procedure is
> then
> repeated with the trend in time series 2 as the surrogate
> observations,
> and $b\{\text{Synth}\}$ and $\sigma\{\text{SE}\}$ calculated from time series 1, 3,

> 4,..19.
> This yields 19 different tests of the null hypothesis. Repeat
> 1,000
> times, and build up a distribution of rejection rates, as in the
> "paired
> trends" test.
>
> The results are truly alarming. Application of the Douglass et al.
> "consistency test" to synthetic data - data generated with the
> same
> underlying AR-1 model! - leads to rejection of the above-stated
> null
> hypothesis at least 65% of the time (for N = 19, 5% significance
> tests).
> As expected, rejection rates for the Douglass consistency test
> rise as
> N increases. For N = 100, rejection rates for 5% tests are nearly
> 85%.
> As my colleague Jim Boyle succinctly put it when he looked at
> these
> results, "This is a pretty hard test to pass".
>
> I think this nicely illustrates the problems with the statistical
> approach used by Douglass et al. If you want to demonstrate that
> modeled
> and observed temperature trends are fundamentally inconsistent,
> you
> devise a fundamentally flawed test is very difficult to pass.
>
> I hope to have a first draft of this stuff written up by the end
> of next
> week. If Leo is agreeable, Figure 4 of this GRL paper would show
> the
> vertical profiles of tropical temperature trends in the various
> versions
> of the RAOBCORE data, plus model results.
>
> Sorry to bore you with all the gory details. But as we've seen
> from
> Douglass et al., details matter.
>
> With best regards,
>
> Ben
> -----
> -----
> Benjamin D. Santer
> Program for Climate Model Diagnosis and Intercomparison
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> -----
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>
>

From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: [Fwd: Re: John Christy's latest ideas]
Date: Thu, 10 Jan 2008 12:06:45 -0800
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

If you get a chance, could you call me up at work (+1 925 423-3364) to talk about the "IJC publication" option? I'd really like to discuss that with you.

With best regards,

Ben

Phil Jones wrote:

>
> Ben,
> Almost said something about this in the main email about the diagrams!
> Other emails and a couple of phone calls distracting me - have to make
> sure
> I'm sending the right email to the right list/person!
> He's clearly biased, but he gets an audience unfortunately. There are
> enough people out there who think we're wrong to cause me to worry at
> times.
> I'd like the world to warm up quicker, but if it did, I know that
> the sensitivity
> is much higher and humanity would be in a real mess!
>
> I'm getting people misinterpreting my comment that went along with
> Chris Folland's press release about the 2008 forecast. It says we're
> warming at 0.2 degC/decade and that is exactly what we should be.
> The individual years don't matter.
>
> CA are now to send out FOIA requests for the Review Editor comments
> on the AR4 Chapters. For some reason they think they exist!
>
> Cheers
> Phil
>
>

> At 16:52 09/01/2008, you wrote:

>> Dear Phil,

>>

>> I can't believe John is now arguing that he's the only guy who can
>> provide unbiased assessments of model performance. After all the
>> mistakes he's made with MSU, and after the Douglass et al. fiasco, he
>> should have acquired a little humility. But I guess "humility" isn't
>> in his dictionary...

>>

>> With best regards,

>>

>> Ben

>> Phil Jones wrote:

>>> Ben,

>>> I'll give up on trying to catch him on the road to Damascus -
>>> he's beyond redemption.

>>> Glad to see that someone's rejected something he's written.

>>> Jim Hack's good, so I'm confident he won't be fooled.

>>> Cheers

>>> Phil

>>>

>>> At 17:28 07/01/2008, you wrote:

>>>> Dear Phil,

>>>>

>>>> More Christy stuff... The guy is just incredible...

>>>>

>>>> With best regards,

>>>>

>>>> Ben

>>>> -----

>>>>

>>>> Benjamin D. Santer

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>>>>

>>>> X-Account-Key: account1
>>>> Return-Path: <santer1@llnl.gov>
>>>> Received: from mail-2.llnl.gov ([unix socket])
>>>> by mail-2.llnl.gov (Cyrus v2.2.12) with LMTPA;
>>>> Mon, 07 Jan 2008 09:00:41 -0800
>>>> Received: from nspiron-2.llnl.gov (nspiron-2.llnl.gov [128.115.41.82])
>>>> by mail-2.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.6 \$) with
>>>> ESMTP id m07H0edp031523;
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>>>> by nspiron-2.llnl.gov with ESMTP; 07 Jan 2008 09:00:40 -0800
>>>> Message-ID: <47825AB8.5000608@llnl.gov>
>>>> Date: Mon, 07 Jan 2008 09:00:40 -0800
>>>> From: Ben Santer <santer1@llnl.gov>
>>>> Reply-To: santer1@llnl.gov
>>>> Organization: LLNL
>>>> User-Agent: Thunderbird 1.5.0.12 (X11/20070529)
>>>> MIME-Version: 1.0
>>>> To: "Hack, James J." <jhack@ornl.gov>
>>>> Subject: Re: John Christy's latest ideas
>>>> References:
>>>> <537C6C0940C6C143AA46A88946B854170B9FAF74@ORNLEXCHANGE.ornl.gov>
>>>> In-Reply-To:
>>>> <537C6C0940C6C143AA46A88946B854170B9FAF74@ORNLEXCHANGE.ornl.gov>
>>>> Content-Type: text/plain; charset=ISO-8859-1; format=flowed
>>>> Content-Transfer-Encoding: 7bit
>>>>
>>>> Dear Jim,
>>>>
>>>> I'm well aware of this paper, and am currently preparing a reply
>>>> (together with many others who were involved in the first CCSP
>>>> report). To put it bluntly, the Douglass paper is a piece of
>>>> worthless garbage. It has serious statistical flaws. Christy should
>>>> be ashamed that he's a co-author on this. His letter to Dr. Strayer
>>>> is deplorable and offensive. For over a decade, Christy has
>>>> portrayed himself as the only guy who is smart enough to develop
>>>> climate-quality data records from MSU. Recently, he's also portrayed
>>>> himself as the only guy who's smart enough to develop
>>>> climate-quality data records from radiosonde data. And now he's the

>>>> only scientist who is capable of performing "hard-nosed",
>>>> independent assessments of climate model performance.

>>>>
>>>> John Christy has made a scientific career out of being wrong. He's
>>>> not even a third-rate scientist. I'd be happy to discuss Christy's
>>>> "unique ways of validating climate models" with you.

>>>>
>>>> With best regards,

>>>>
>>>> Ben

>>>> Hack, James J. wrote:

>>>>> Dear Ben,

>>>>>
>>>>> Happy New Year. Hope all is well. I was wondering if you're
>>>>> familiar with the attached paper? I thought that you had recently
>>>>> published something that concludes something quite different. Is
>>>>> that right? If yes, could you forward me a copy? And, any
>>>>> comments are also welcome.
>>>>> He's coming to ORNL next week to under the premise that he has some
>>>>> unique ways to validate climate models (this time with regard to
>>>>> the lower thermodynamic structure). I'd be happy to chat with you
>>>>> about this as well if you would like. I'm appending what I know to
>>>>> the bottom of this note.

>>>>>
>>>>> Best regards ...

>>>>>
>>>>> Jim

>>>>>
>>>>> James J. Hack Director, National Center for Computational Sciences
>>>>> Oak Ridge National Laboratory
>>>>> One Bethel Valley Road
>>>>> P.O. Box 2008, MS-6008
>>>>> Oak Ridge, TN 37831-6008

>>>>>
>>>>> email: jhack@ornl.gov <<mailto:jhack@ornl.gov>>
>>>>> voice: 865-574-6334
>>>>> fax: 865-241-9578
>>>>> cell: 865-206-9001

>>>>>
>>>>>
>>>>>> >> -----Original Message-----
>>>>>> >> From: John Christy [[_mailto:john.christy@nsstc.uah.edu_](mailto:john.christy@nsstc.uah.edu)]
>>>>>> >> Sent: Tuesday, October 23, 2007 9:16 AM

>>>>>> >> To: Strayer, Michael
>>>>>> >> Cc: Salmon, Jeffrey
>>>>>> >> Subject: Climate Model Evaluation
>>>>>> >>
>>>>>> >> Dr. Strayer:
>>>>>> >>
>>>>>> >> Jeff Salmon is aware of a project we at UAHuntsville believe is
>>>>>> >> vital and that you may provide a way to see it accomplished.
>>>>>> >> As you
>>>>>> >> know, our nation's energy and climate change policies are being
>>>>>> >> driven by output from global climate models. However, there has
>>>>>> >> never been a true "red team" assessment of these model
>>>>>> >> projections
>>>>>> >> in the way other government programs are subjected to hard-nosed,
>>>>>> >> independent evaluations. To date, most of the "evaluation" of
>>>>>> >> these
>>>>>> >> models has been left in the hands of the climate modelers
>>>>>> >> themselves. This has the potential of biasing the entire process.
>>>>>> >>
>>>>>> >> It is often a climate modeler's claim (and promoted in IPCC
>>>>>> >> documents - see attached) that the models must be correct because
>>>>>> >> the global surface
>>>>>> >> temperature variations since 1850 are reproduced (somewhat) by
>>>>>> >> the
>>>>>> >> models when run in hindcast mode. However, this is not a
>>>>>> >> scientific
>>>>>> >> experiment for the simple reason that every climate modeler
>>>>>> >> saw the
>>>>>> >> answer ahead of time. It is terribly easy to get the right answer
>>>>>> >> for the wrong reason, especially if you already know the answer.
>>>>>> >>
>>>>>> >> A legitimate experiment is to test the models' output against
>>>>>> >> variables to which modelers did not have access ... a true blind
>>>>>> >> test of the models.
>>>>>> >>
>>>>>> >> I have proposed and have had rejected a model evaluation
>>>>>> >> project to
>>>>>> >> DOE based on the utilization of global datasets we build here at
>>>>>> >> UAH. We have published many of these datasets (most are
>>>>>> >> satellite-based) which document the complexity of the climate
>>>>>> >> system and which we think models should replicate in some way,
>>>>>> >> and
>>>>>> >> to aid in model development where shortcomings are found.

>>>>>> These are
>>>>>> >> datasets of quantities that modelers in general were not aware of
>>>>>> >> when doing model testing. We have performed
>>>>>> >> a few of these tests and have found models reveal serious
>>>>>> >> shortcomings in some of the most fundamental aspects of energy
>>>>>> >> distribution. We believe a rigorous test of climate models is in
>>>>>> >> order as the congress starts considering energy reduction
>>>>>> >> strategies which can have significant consequences on our
>>>>>> economy.
>>>>>> >> Below is an abstract of a retooled proposal I am working on.
>>>>>> >>
>>>>>> >> If you see a possible avenue for research along these lines,
>>>>>> please
>>>>>> >> let me know. Too, we have been considering some type of
>>>>>> partnership
>>>>>> >> with Oakridge since the facility is nearby, and this may be a way
>>>>>> >> to do that.
>>>>>> >>
>>>>>> >> John C.
>>>>>> >>
>>>>>> >>
>>>>>> >>
>>>>>> >> Understanding the vertical energy distribution of the Earth's
>>>>>> atmosphere
>>>>>> >> and its expression in global climate model simulations
>>>>>> >>
>>>>>> >> John R. Christy, P.I., University of Alabama in Huntsville
>>>>>> >>
>>>>>> >> Abstract
>>>>>> >>
>>>>>> >> Sets of independent observations indicate, unexpectedly, that the
>>>>>> >> warming of the tropical atmosphere since 1978 is proceeding at a
>>>>>> >> rate much less than that anticipated from climate model
>>>>>> simulations.
>>>>>> >> Specifically, while the surface has warmed, the lower troposphere
>>>>>> >> has experienced less warming. In contrast, all climate models we
>>>>>> >> and others have examined indicate the lower tropical atmosphere
>>>>>> >> should be warming at a rate 1.2 to 1.5 times greater than the
>>>>>> >> surface when forced with increasing greenhouse gases within the
>>>>>> >> context of other observed forcings (the so-called "negative lapse
>>>>>> >> rate feedback".) We propose to diagnose this curious phenomenon
>>>>>> >> with several satellite-based datasets to document its relation to
>>>>>> >> other climate variables. We shall do the same for climate model

>>>>>> >> output of the same simulated variables. This will
>>>>>> >> enable us to propose an integrated conceptual framework of the
>>>>>> >> phenomenon for further testing. Tied in with this research are
>>>>> potential
>>>>>> >> answers to fundamental questions such as the following: (1) In
>>>>>> >> response to increasing surface temperatures, is the lower
>>>>>> >> atmosphere reconfiguring the way heat energy is transported which
>>>>>> >> allows for an increasing amount of heat to more freely escape to
>>>>>> >> space? (2) Could there be a natural thermostatic effect in the
>>>>>> >> climate system which acts in a different way than parameterized
>>>>>> >> convective-adjustment schemes dependent upon current
>>>>>> assumptions of
>>>>>> >> heat deposition and retention? (3)
>>>>>> >> If observed atmospheric heat retention is considerably less than
>>>>>> >> model projections, what impact will lower retention rates have on
>>>>>> >> anticipated increases in surface temperatures in the 21st
>>>>>> century?
>>>>>> >>

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>>>> Lawrence Livermore National Laboratory
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>>>> FAX: (925) 422-7675
>>>> email: santer1@llnl.gov

>>>> -----
>>>>

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>>>> School of Environmental Sciences Fax +44 (0) 1603 507784
>>>> University of East Anglia
>>>> Norwich Email p.jones@uea.ac.uk
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>>>> UK

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From: Ben Santer <santer1@llnl.gov>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: Update on response to Douglass et al.
Date: Thu, 10 Jan 2008 13:00:28 -0800
Reply-to: santer1@llnl.gov
Cc: "Philip D. Jones" <p.jones@uea.ac.uk>

<x-flowed>

Dear Tim,

Thanks very much for your email. I greatly appreciate the additional information that you've given me. I am a bit conflicted about what we should do.

IJC published a paper with egregious statistical errors. Douglass et al. was essentially a commentary on work by myself and colleagues - work that had been previously published in Science in 2005 and in Chapter 5 of the first U.S. CCSP Report in 2006. To my knowledge, none of the authors or co-authors of the Santer et al. Science paper or of CCSP 1.1 Chapter 5 were used as reviewers of Douglass et al. I am assuming that, when he submitted his paper to IJC, Douglass specifically requested that certain scientists should be excluded from the review process. Such an approach is not defensible for a paper which is largely a comment on previously-published work.

It would be fair and reasonable to give IJC the opportunity to "set the record straight", and correct the harm they have done by publication of Douglass et al. I use the word "harm" advisedly. The author and coauthors of the Douglass et al. IJC paper are using this paper to argue that "Nature, not CO₂, rules the climate", and that the findings of Douglass et al. invalidate the "discernible human influence" conclusions of previous national and international scientific assessments.

Quick publication of a response to Douglass et al. in IJC would go some way towards setting the record straight. I am troubled, however, by the very real possibility that Douglass et al. will have the last word on this subject. In my opinion (based on many years of interaction with these guys), neither Douglass, Christy or Singer are capable of admitting that their paper contained serious scientific errors. Their "last word" will be an attempt to obfuscate rather than illuminate. They are not interested in improving our scientific understanding of the nature and causes of recent changes in atmospheric temperature. They are

solely interested in advancing their own agendas. It is telling and troubling that Douglass et al. ignored radiosonde data showing substantial warming of the tropical troposphere - data that were in accord with model results - even though such data were in their possession. Such behaviour constitutes intellectual dishonesty. I strongly believe that leaving these guys the last word is inherently unfair.

If IJC are interested in publishing our contribution, I believe it's fair to ask for the following:

1) Our paper should be regarded as an independent contribution, not as a comment on Douglass et al. This seems reasonable given i) The substantial amount of new work that we have done; and ii) The fact that the Douglass et al. paper was not regarded as a comment on Santer et al. (2005), or on Chapter 5 of the 2006 CCSP Report - even though Douglass et al. clearly WAS a comment on these two publications.

2) If IJC agrees to 1), then Douglass et al. should have the opportunity to respond to our contribution, and we should be given the chance to reply. Any response and reply should be published side-by-side, in the same issue of IJC.

I'd be grateful if you and Phil could provide me with some guidance on 1) and 2), and on whether you think we should submit to IJC. Feel free to forward my email to Glenn McGregor.

With best regards,

Ben

Tim Osborn wrote:

> At 03:52 10/01/2008, Ben Santer wrote:

>> ...Much as I would like to see a high-profile rebuttal of Douglass et al. in a journal like Science or Nature, it's unlikely that either journal will publish such a rebuttal.

>>

>> So what are our options? Personally, I'd vote for GRL. I think that it is important to publish an expeditious response to the statistical flaws in Douglass et al. In theory, GRL should be able to give us the desired fast turnaround time...

>>

>> Why not go for publication of a response in IJC? According to Phil, this option would probably take too long. I'd be interested to hear any other thoughts you might have on publication options.

>
> Hi Ben and Phil,
>
> as you may know (Phil certainly knows), I'm on the editorial board of
> IJC. Phil is right that it can be rather slow (though faster than
> certain other climate journals!). Nevertheless, IJC really is the
> preferred place to publish (though a downside is that Douglass et al.
> may have the opportunity to have a response considered to accompany any
> comment).
>
> I just contacted the editor, Glenn McGregor, to see what he can do. He
> promises to do everything he can to achieve a quick turn-around time (he
> didn't quantify this) and he will also "ask (the publishers) for
> priority in terms of getting the paper online asap after the authors
> have received proofs". He genuinely seems keen to correct the
> scientific record as quickly as possible.
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> emailed to you and Phil only) that he may be able to hold back the
> hardcopy (i.e. the print/paper version) appearance of Douglass et al.,
> possibly so that any accepted Santer et al. comment could appear
> alongside it. Presumably depends on speed of the review process.
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> If this does persuade you to go with IJC, Glenn suggested that I could
> help (because he is in Kathmandu at present) with achieving the quick
> turn-around time by identifying in advance reviewers who are both
> suitable and available. Obviously one reviewer could be someone who is
> already familiar with this discussion, because that would enable a fast
> review - i.e., someone on the email list you've been using - though I
> don't know which of these people you will be asking to be co-authors and
> hence which won't be available as possible reviewers. For objectivity
> the other reviewer would need to be independent, but you could still
> suggest suitable names.
>
> Well, that's my thoughts... let me know what you decide.
>
> Cheers
>
> Tim
>
>
> Dr Timothy J Osborn, Academic Fellow
> Climatic Research Unit

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> University of East Anglia
> Norwich NR4 7TJ, UK
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>

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</x-flowed>

From: Peter Thorne <peter.thorne@metoffice.gov.uk>
To: Dian Seidel <dian.seidel@noaa.gov>
Subject: Dian, something like this?
Date: Thu, 10 Jan 2008 14:43:30 +0000
Cc: Ben Santer <santer1@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Melissa Free <melissa.free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Steve Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

All,

as it happens I am preparing a figure precisely as Dian suggested. This has only been possible due to substantial efforts by Leo in particular, but all the other dataset providers also. I wanted to give a feel for where we are at although I want to tidy this substantially if we were to use it. To do this I've taken every single scrap of info I have in my possession that has a status of at least submitted to a journal. I have considered the common period of 1979-2004. So, assuming you are all sitting comfortably:

Grey shading is a little cheat from Santer et al using a trusty ruler. See Figure 3.B in this paper, take the absolute range of model scaling factors at each of the heights on the y-axis and apply this scaling to HadCRUT3 tropical mean trend denoted by the star at the surface. So, if we assume HadCRUT3 is correct then we are aiming for the grey shading or not depending upon one's pre-conceived notion as to whether the models are correct.

Red is HadAT2 dataset.

black dashed is the raw data used in Titchner et al. submitted (all tropical stations with a 81-2000 climatology)

Black whiskers are median, inter-quartile range and max / min from Titchner et al. submission. We know, from complex error-world assessments, that the median under-cooks the required adjustment here and that the truth may conceivably lie (well) outside the upper limit.

Bright green is RATPAC

Then, and the averaging and trend calculation has been done by Leo here and not me so any final version I'd want to get the raw gridded data and do it exactly the same way. But for the raw raobs data that Leo provided as a sanity check it seems to make a miniscule (<0.05K/decade even at height) difference:

Lime green: RICH (RAOBCORE 1.4 breaks, neighbour based adjustment

estimates)

Solid purple: RAOBCORE 1.2
Dotted purple: RAOBCORE 1.3
Dashed purple: RAOBCORE 1.4

I am also in possession of Steve's submitted IUK dataset and will be adding this trend line shortly.

I'll be adding a legend in the large white space bottom left.

My take home is that all datasets are heading the right way and that this reduces the probability of a discrepancy. Compare this with Santer et al. Figure 3.B.

I'll be using this in an internal report anyway but am quite happy for it to be used in this context too if that is the general feeling. Or for Leo's to be used. Whatever people prefer.

Peter

--

Peter Thorne Climate Research Scientist
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www.metoffice.gov.uk/hadobs

Attachment Converted: "c:\eudora\attach\trend_profiles_dogs_dinner.png"

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: An issue/problem with Tim's idea !!!!!!!
Date: Thu Jan 10 16:14:28 2008

Ben,

Tim's idea is a possibility. I've not always got on that well great with Glenn McGregor, but Tim seems to have a reasonable rapport with him. Dian has suggested that this would be the best route - it is the logical one. I also think that Glenn would get quick reviews, as Tim thinks he realises he's made a mistake.

Tim has let me into part of secret. Glenn said the paper had two reviews - one positive, the other said it wasn't great, but would leave it up to the editor's discretion. This is why Glenn knows he made the wrong choice.

The problem !! The person who said they would leave it to the editor's discretion is on your email list! I don't know who it is - Tim does - maybe they have told you? I don't want to put pressure on Tim. He doesn't know I'm sending this. It isn't me by the way - nor Tim ! Tim said it was someone who hasn't contributed to the discussion - which does narrow the possibilities down!

Tim/Glenn discussed getting quick reviews. Whoever this person is they could be the familiar reviewer - and we could then come up with another reasonable name (Kevin - he does everything at the speed of light) as the two reviewers.

Colour in IJC costs a bit, but I'm sure we can lean on Glenn. Also we can just have colour in the pdf.

I'll now send a few thoughts on the figures!

Cheers

Phil

Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>,
Thomas R Karl <Thomas.R.Karl@noaa.gov>,
John Lanzante <John.Lanzante@noaa.gov>, carl mears <mears@remss.com>,
"David C. Bader" <bader2@llnl.gov>,
"Francis W. Zwiers" <francis.zwiers@ec.gc.ca>,
Frank Wentz <frank.wentz@remss.com>,
Leopold Haimberger <leopold.haimberger@univie.ac.at>,
Melissa Free <Melissa.Free@noaa.gov>,
"Michael C. MacCracken" <maccrac@comcast.net>,
"Philip D. Jones" <p.jones@uea.ac.uk>,
Steven Sherwood <Steven.Sherwood@yale.edu>,
Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>,

"Thorne, Peter" <peter.thorne@metoffice.gov.uk>,
Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>,
"Hack, James J." <jhack@ornl.gov>

X-Mailer: QUALCOMM Windows Eudora Version 7.1.0.9

Date: Thu, 10 Jan 2008 13:00:39 +0000

To: santer1@llnl.gov,"Philip D. Jones" <p.jones@uea.ac.uk>

From: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: Update on response to Douglass et al.

At 03:52 10/01/2008, Ben Santer wrote:

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He also said (and please treat this in confidence, which is why I emailed to you and Phil only) that he may be able to hold back the hardcopy (i.e. the print/paper version) appearance of Douglass et al., possibly so that any accepted Santer et al. comment could appear alongside it. Presumably depends on speed of the review process.

If this does persuade you to go with IJC, Glenn suggested that I could help (because he is in Kathmandu at present) with achieving the quick turn-around time by identifying in advance reviewers who are both suitable and available. Obviously one reviewer could be someone who is already familiar with this discussion, because that would enable a fast review - i.e., someone on the email list you've been using - though I don't know which of these people you will be asking to be co-authors and hence which won't be available as possible reviewers. For objectivity the other reviewer would need to be independent, but you could still suggest suitable names.

Well, that's my thoughts... let me know what you decide.

Cheers

Tim

Dr Timothy J Osborn, Academic Fellow

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School of Environmental Sciences

University of East Anglia

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sunclock: [2]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

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References

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Ben et al,
As Dian has said Ben's diagrams are as usual great! I also like the one that Peter has just sent around as that illustrates the issue with the various RAOBCORE versions. Although I still think they should have used HadCRUT3v for the surface, I know HadCRUT2v shows much the same. What this figure shows is the differences between the various sonde datasets. Dian/Peter also make the point that there are other new datasets to be added - so the sondes are very much still work in progress. I know you will point out all the analytical/statistical issues see the series brings home the issues better. I know you could add the values to your Fig1, a plot like this is much better.
In the email Ben, you seem to have written much of the response! Whichever route you go down (GRL/IJC) the text can't be too long. I would favour copious captions, and even an Appendix, to get the main points across quickly.
Cheers
Phil

At 14:43 10/01/2008, Peter Thorne wrote:

All,
as it happens I am preparing a figure precisely as Dian suggested. This has only been possible due to substantial efforts by Leo in particular, but all the other dataset providers also. I wanted to give a feel for where we are at although I want to tidy this substantially if we were to use it. To do this I've taken every single scrap of info I have in my

possession that has a status of at least submitted to a journal. I have considered the common period of 1979-2004. So, assuming you are all sitting comfortably:

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NR4 7TJ
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References

1. <http://www.metoffice.gov.uk/hadobs>

From: Ben Santer <santer1@llnl.gov>
To: Leopold Haimberger <leopold.haimberger@univie.ac.at>
Subject: Re: Update on response to Douglass et al., Dian, something like this?
Date: Thu, 10 Jan 2008 19:07:03 -0800
Reply-to: santer1@llnl.gov
Cc: Peter Thorne <peter.thorne@metoffice.gov.uk>, Dian Seidel <dian.seidel@noaa.gov>, Tom Wigley <>wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Melissa Free <melissa.free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Steve Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

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Dear Leo,

Thanks very much for your email. I can easily make the observations a bit more prominent in Figure 1. As you can see from today's (voluminous!) email traffic, I've received lots of helpful suggestions regarding improvements to the Figures. I'll try to produce revised versions of the Figures tomorrow.

On the autocorrelation issue: The models have a much larger range of lag-1 autocorrelation coefficients (0.66 to 0.95 for T2LT, and 0.69 to 0.95 for T2) than the UAH or RSS data (which range from 0.87 to 0.89). I was concerned that if we used the model lag-1 autocorrelations to guide the choice of AR-1 parameter in the synthetic data analysis, Douglass and colleagues would have an easy opening for criticising us ("Aha! Santer et al. are using model results to guide them in their selection of the coefficients for their AR-1 model!") I felt that it was much more difficult for Douglass et al. to criticize what we've done if we used UAH data to dictate our choice of the AR-1 parameter and the "scaling factor" for the amplitude of the temporal variability.

As you know, my personal preference would be to include in our response to Douglass et al. something like the Figure 4 that Peter has produced. While inclusion of a Figure 4 is not essential for the purpose of illuminating the statistical flaws in the Douglass et al. "consistency test", such a Figure would clearly show the (currently large) structural uncertainties in radiosonde-based estimates of the vertical profile of atmospheric temperature changes. I think this is an important point, particularly in view of the fact that Douglass et al. failed to discuss versions 1.3 and 1.4 of your RAOBCORE data - even though they had information from those datasets in their possession.

However, I fully agree with Tom's comment that we don't want to do anything to "steal the thunder" from ongoing efforts to improve sonde-based estimates of atmospheric temperature change, and to better quantify structural uncertainties in those estimates. Your group,

together with the groups at the Hadley Centre, Yale, NOAA ARL and NOAA GFDL, deserve great credit for making significant progress on a difficult, time-consuming, yet important problem.

I guess the best solution is to leave this decision up to all of you (the radiosonde dataset developers). I'm perfectly happy to include a version of Figure 4 in our response to Douglass et al. If we do go with inclusion of a Figure 4, you, Peter, Dian, Melissa, Steve Sherwood and John should decide whether you feel comfortable providing radiosonde data for such a Figure. I will gladly abide by your decisions. As you note in your email, our use of a Figure 4 would not preclude a more detailed and thorough comparison of simulated and observed amplification in some later publication.

Once again, thanks for all your help with this project, Leo.

With best regards,

Ben

Leopold Haimberger wrote:

> All,
>
> These three figures are really very clear and leave no doubts that the
> Douglass et al analysis is flawed. This is true especially for Fig. 1.
> In Fig. 1 one has to look carefully to find the RSS and UAH "observed"
> trends to the right of all the model trends. Maybe one can make their
> symbols more prominent.
>
> Concerning Fig. 3 I wonder whether the UAH autocorrelation is the
lowest
> of all available data. .86 is quite substantial autocorrelation. Maybe
> it is a good idea to be on the safe side and use the lowest
> autocorrelation of all datasets (models, RSS, UAH) for this analysis.
>
> Concerning Fig. 4, I like Peter's and Dian's idea to include RAOBCORE,
> HadAT2, RATPAC and Steve's data and compare it in one plot with model
> output. While I agree that the first three figures and the
corresponding
> text are already sufficient for the reply, they target mainly to the
> right panel of Fig. 1 in Douglass et al's paper. The trend profile plot
> of Fig. 4 is complementary as a counterpart to the left panel of their
> plot. To see the trend amplification in in some of the vertical
profiles
> is much more suggestive than seeing the LT trends being larger than
> surface trends, at least for me. Showing all available profiles adds
> value beyond the RAOBCORE v1.2 vs RAOBCORE v1.4 issue. Yes, it is work
> in progress and such a plot as drafted by Peter makes that very clear.
> In this paper it is sufficient to show that the uncertainty of
> radiosonde trends is much larger than suggested by Douglass et al. and
> we do not need to have the final answer yet. I have nothing against
> Peter doing the drawing of the figure, since he has most of the
> necessary data. The plot would be needed for 1979-1999, however. Peter,
> I will send you the trend profiles for this period a bit later.
>

> Publishing the reply in either IJC or GRL including Fig. 4 is fine for me.

> When we first discussed a follow up of the Santer et al paper in October, we had in mind to publish post-FAR climate model data up to present (not just 1999) and also new radiosonde data up to present in a highest ranking journal. I am confident that this is still possible even

> if some of the new material planned for such a paper is submitted already now. What do you think?

>

> With best Regards,

>

> Leo

>

> Peter Thorne wrote:

>> All,

>>

>> as it happens I am preparing a figure precisely as Dian suggested. This

>> has only been possible due to substantial efforts by Leo in particular,

>> but all the other dataset providers also. I wanted to give a feel for where we are at although I want to tidy this substantially if we were to

>> use it. To do this I've taken every single scrap of info I have in my possession that has a status of at least submitted to a journal. I have

>> considered the common period of 1979-2004. So, assuming you are all sitting comfortably:

>>

>> Grey shading is a little cheat from Santer et al using a trusty ruler. See Figure 3.B in this paper, take the absolute range of model scaling factors at each of the heights on the y-axis and apply this scaling to HadCRUT3 tropical mean trend denoted by the star at the surface. So, if

>> we assume HadCRUT3 is correct then we are aiming for the grey shading or

>> not depending upon one's pre-conceived notion as to whether the models are correct.

>>

>> Red is HadAT2 dataset.

>>

>> black dashed is the raw data used in Titchner et al. submitted (all tropical stations with a 81-2000 climatology)

>>

>> Black whiskers are median, inter-quartile range and max / min from Titchner et al. submission. We know, from complex error-world assessments, that the median under-cooks the required adjustment here and that the truth may conceivably lie (well) outside the upper limit.

>>

>> Bright green is RATPAC

>>

>> Then, and the averaging and trend calculation has been done by Leo here

>> and not me so any final version I'd want to get the raw gridded data
and
>> do it exactly the same way. But for the raw raobs data that Leo
provided
>> as a sanity check it seems to make a miniscule (<0.05K/decade even at
>> height) difference:
>>
>> Lime green: RICH (RAOBCORE 1.4 breaks, neighbour based adjustment
>> estimates)
>>
>> Solid purple: RAOBCORE 1.2
>> Dotted purple: RAOBCORE 1.3
>> Dashed purple: RAOBCORE 1.4
>>
>> I am also in possession of Steve's submitted IUK dataset and will be
>> adding this trend line shortly.
>>
>> I'll be adding a legend in the large white space bottom left.
>>
>> My take home is that all datasets are heading the right way and that
>> this reduces the probability of a discrepancy. Compare this with
Santer
>> et al. Figure 3.B.
>>
>> I'll be using this in an internal report anyway but am quite happy for
>> it to be used in this context too if that is the general feeling. Or
for
>> Leo's to be used. Whatever people prefer.
>>
>> Peter
>>
>>
>> -----
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>>
>

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From: Ben Santer <santer1@llnl.gov>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Potential reviewers
Date: Fri, 11 Jan 2008 08:43:23 -0800
Reply-to: santer1@llnl.gov
Cc: "Philip D. Jones" <p.jones@uea.ac.uk>

<x-flowed>

Dear Tim,

Here are some suggestions for potential reviewers of a Santer et al. IJoC submission on issues related to the consistency between modeled and observed atmospheric temperature trends. None of the suggested reviewers have been involved in the recent "focus group" that has discussed problems with the Douglass et al. IJoC paper.

1. Mike Wallace, University of Washington. U.S. National Academy member. Expert on atmospheric dynamics. Chair of National Academy of Sciences committee on "Reconciling observations of global temperature change" (2000). Email: wallace@atmos.washington.edu

2. Qiang Fu, University of Washington. Expert on atmospheric radiation, dynamics, radiosonde and satellite data. Published 2004 Nature paper and 2005 GRL paper dealing with issues related to global and tropical temperature trends. Email: qfu@atmos.washington.edu

3. Gabi Hegerl, University of Edinburgh. Expert on detection and attribution of externally-forced climate change. Co-Convening Lead Author of "Understanding and Attributing Climate Change" chapter of IPCC Fourth Assessment Report. Email: Gabi.Hegerl@ed.ac.uk

4. Jim Hurrell, National Center for Atmospheric Research (NCAR). Former Director of Climate and Global Dynamics division at NCAR. Expert on climate modeling, observational data. Published a number of papers on MSU-related issues. Email: jhurrell@cgd.ucar.edu

5. Myles Allen, Oxford University. Expert in Climate Dynamics, detection and attribution, application of statistical methods in climatology. Email: allen@atm.ox.ac.uk

6. Peter Stott, Hadley Centre for Climate Prediction and Research. Expert in climate modeling, detection and attribution. Email:

peter.stott@metoffice.gov.uk

With best regards,

Ben

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: Update on response to Douglass et al.
Date: Fri, 11 Jan 2008 13:41:18 +0000
Cc: "Philip D. Jones" <p.jones@uea.ac.uk>

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Hi Ben (cc Phil),

just heard back from Glenn. He's prepared to treat it as a new submission rather than a comment on Douglass et al. and he also reiterates that "Needless to say my offer of a quick turn around time etc still stands".

So basically this makes the IJC option more attractive than if it were treated as a comment. But whether IJC is still a less attractive option than GRL is up to you to decide :-) (or feel free to canvas your potential co-authors [the only thing I didn't want to make more generally known was the suggestion that print publication of Douglass et al. might be delayed... all other aspects of this discussion are unrestricted]).

Cheers

Tim

At 21:00 10/01/2008, Ben Santer wrote:

>Dear Tim,

>

>Thanks very much for your email. I greatly appreciate the additional
>information that you've given me. I am a bit conflicted about what
>we should do.

>

>IJC published a paper with egregious statistical errors. Douglass et
>al. was essentially a commentary on work by myself and colleagues -
>work that had been previously published in Science in 2005 and in
>Chapter 5 of the first U.S. CCSP Report in 2006. To my knowledge,
>none of the authors or co-authors of the Santer et al. Science paper
>or of CCSP 1.1 Chapter 5 were used as reviewers of Douglass et al. I
>am assuming that, when he submitted his paper to IJC, Douglass
>specifically requested that certain scientists should be excluded
>from the review process. Such an approach is not defensible for a

>paper which is largely a comment on previously-published work.

>

>It would be fair and reasonable to give IJC the opportunity to "set
>the record straight", and correct the harm they have done by
>publication of Douglass et al. I use the word "harm" advisedly. The
>author and coauthors of the Douglass et al. IJC paper are using this
>paper to argue that "Nature, not CO2, rules the climate", and that
>the findings of Douglass et al. invalidate the "discernible human
>influence" conclusions of previous national and international
>scientific assessments.

>

>Quick publication of a response to Douglass et al. in IJC would go
>some way towards setting the record straight. I am troubled,
>however, by the very real possibility that Douglass et al. will have
>the last word on this subject. In my opinion (based on many years of
>interaction with these guys), neither Douglass, Christy or Singer
>are capable of admitting that their paper contained serious
>scientific errors. Their "last word" will be an attempt to obfuscate
>rather than illuminate. They are not interested in improving our
>scientific understanding of the nature and causes of recent changes
>in atmospheric temperature. They are solely interested in advancing
>their own agendas. It is telling and troubling that Douglass et al.
>ignored radiosonde data showing substantial warming of the tropical
>troposphere - data that were in accord with model results - even
>though such data were in their possession. Such behaviour
>constitutes intellectual dishonesty. I strongly believe that leaving
>these guys the last word is inherently unfair.

>

>If IJC are interested in publishing our contribution, I believe it's
>fair to ask for the following:

>

>1) Our paper should be regarded as an independent contribution, not
>as a comment on Douglass et al. This seems reasonable given i) The
>substantial amount of new work that we have done; and ii) The fact
>that the Douglass et al. paper was not regarded as a comment on
>Santer et al. (2005), or on Chapter 5 of the 2006 CCSP Report - even
>though Douglass et al. clearly WAS a comment on these two publications.

>

>2) If IJC agrees to 1), then Douglass et al. should have the
>opportunity to respond to our contribution, and we should be given
>the chance to reply. Any response and reply should be published
>side-by-side, in the same issue of IJC.

>

>I'd be grateful if you and Phil could provide me with some guidance
>on 1) and 2), and on whether you think we should submit to IJC. Feel
>free to forward my email to Glenn McGregor.

>

>With best regards,

>

>Ben

>Tim Osborn wrote:

>>At 03:52 10/01/2008, Ben Santer wrote:

>>>...Much as I would like to see a high-profile rebuttal of Douglass
>>>et al. in a journal like Science or Nature, it's unlikely that
>>>either journal will publish such a rebuttal.

>>>

>>>So what are our options? Personally, I'd vote for GRL. I think
>>>that it is important to publish an expeditious response to the
>>>statistical flaws in Douglass et al. In theory, GRL should be able
>>>to give us the desired fast turnaround time...

>>>

>>>Why not go for publication of a response in IJC? According to
>>>Phil, this option would probably take too long. I'd be interested
>>>to hear any other thoughts you might have on publication options.

>>Hi Ben and Phil,

>>as you may know (Phil certainly knows), I'm on the editorial board
>>of IJC. Phil is right that it can be rather slow (though faster
>>than certain other climate journals!). Nevertheless, IJC really is
>>the preferred place to publish (though a downside is that Douglass
>>et al. may have the opportunity to have a response considered to
>>accompany any comment).

>>I just contacted the editor, Glenn McGregor, to see what he can
>>do. He promises to do everything he can to achieve a quick
>>turn-around time (he didn't quantify this) and he will also "ask
>>(the publishers) for priority in terms of getting the paper online
>>asap after the authors have received proofs". He genuinely seems
>>keen to correct the scientific record as quickly as possible.

>>He also said (and please treat this in confidence, which is why I
>>emailed to you and Phil only) that he may be able to hold back the
>>hardcopy (i.e. the print/paper version) appearance of Douglass et
>>al., possibly so that any accepted Santer et al. comment could
>>appear alongside it. Presumably depends on speed of the review process.

>>If this does persuade you to go with IJC, Glenn suggested that I
>>could help (because he is in Kathmandu at present) with achieving
>>the quick turn-around time by identifying in advance reviewers who
>>are both suitable and available. Obviously one reviewer could be

>>someone who is already familiar with this discussion, because that
>>would enable a fast review - i.e., someone on the email list you've
>>been using - though I don't know which of these people you will be
>>asking to be co-authors and hence which won't be available as
>>possible reviewers. For objectivity the other reviewer would need
>>to be independent, but you could still suggest suitable names.
>>Well, that's my thoughts... let me know what you decide.

>>Cheers

>>Tim

>>

>>Dr Timothy J Osborn, Academic Fellow

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: Potential reviewers
Date: Fri Jan 11 17:22:46 2008

I didn't know about the link between John and Kevin. Sounds like Qiang or Myles, plus Francis, would be best combination of expertise and speediness.

By the way, for online submission you'll just need to convert the Latex to a PDF file and submit that.

Have a good weekend,

Tim

At 17:07 11/01/2008, you wrote:

Dear Phil and Tim,

I did leave Kevin's name off because of concerns that he might be extremely upset by Christy's involvement in Douglass et al. I guess you know that John was a Ph.D. student of Kevin's. It must be tough to have a student who's the antithesis of everything you stand for and care about - careful, thorough science.

Qiang Fu would be great, since he's so knowledgeable about MSU-related issues. I think he would be fast, too. Myles reviewed one of the GRL versions of Douglass et al., so he's very familiar with this territory.

With best regards,

Ben

Phil Jones wrote:

Ben,

I briefly discussed this with Tim a few minutes ago.

With IDAG coming up, it is probably best not to use Gabi and Myles.

I also suggested that Mike Wallace might be slow - as Myles would have been. Peter S might not be right for the IDAG reason and he does work for the HC - where Peter T does.

If Jim is back working he would be good. So would Fu. If Tim can just persuade them to do it - and quickly.

I did suggest Kevin - he would do it quickly - but it may be a read rag to a bull with John Christy on the other paper.

Glad to see you've gone down his route!

Have a good weekend!

Ruth says hello!

Cheers

Phil

At 16:43 11/01/2008, Ben Santer wrote:

Dear Tim,

Here are some suggestions for potential reviewers of a Santer et al. IJoC submission on issues related to the consistency between modeled and observed atmospheric temperature trends. None of the suggested reviewers have been involved in the recent "focus group" that has discussed problems with the Douglass et al. IJoC paper.

1. Mike Wallace, University of Washington. U.S. National Academy member. Expert on atmospheric dynamics. Chair of National Academy of Sciences committee on "Reconciling observations of global temperature change" (2000). Email: wallace@atmos.washington.edu

2. Qiang Fu, University of Washington. Expert on atmospheric radiation, dynamics, radiosonde and satellite data. Published 2004 Nature paper and 2005 GRL paper dealing with issues related to global and tropical temperature trends. Email: qfu@atmos.washington.edu

3. Gabi Hegerl, University of Edinburgh. Expert on detection and attribution of externally-forced climate change. Co-Convening Lead Author of "Understanding and Attributing Climate Change" chapter of IPCC Fourth Assessment Report. Email: Gabi.Hegerl@ed.ac.uk

4. Jim Hurrell, National Center for Atmospheric Research (NCAR). Former Director of Climate and Global Dynamics division at NCAR. Expert on climate modeling, observational data. Published a number of papers on MSU-related issues. Email: jhurrell@cgd.ucar.edu

5. Myles Allen, Oxford University. Expert in Climate Dynamics, detection and attribution, application of statistical methods in climatology. Email: allen@atm.ox.ac.uk

6. Peter Stott, Hadley Centre for Climate Prediction and Research. Expert in climate modeling, detection and attribution. Email: peter.stott@metoffice.gov.uk

With best regards,

Ben

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From: Leopold Haimberger <leopold.haimberger@univie.ac.at>
To: santer1@llnl.gov
Subject: Re: IJoC and Figure 4
Date: Fri, 11 Jan 2008 23:33:28 +0100
Cc: Peter Thorne <peter.thorne@metoffice.gov.uk>, Dian Seidel <dian.seidel@noaa.gov>, Tom Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, John Lanzante <John.Lanzante@noaa.gov>, Carl Mears <mears@remss.com>, "David C. Bader" <bader2@llnl.gov>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Melissa Free <melissa.free@noaa.gov>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Phil Jones <p.jones@uea.ac.uk>, Steve Sherwood <Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, 'Susan Solomon' <ssolomon@al.noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

<x-flowed>
Dear folks,

I believe Ben's suggestion is very good compromise and we should prepare a Fig. 4 with three RAOBCORE versions, RICH, HadAT and RATPAC. As I have understood Ben in his first description of Fig. 4, also the range of model trend profiles should be included.

Who will actually draw the figure? I can do this but I do not have the model data and I do not have the RATPAC profiles so far. It would be easiest to remove the Titchner et al. profiles and Steves profiles from Peter's plot. Or should we send our profile data to you, Ben? What do you think?

Concerning the possible reaction of Douglass et al.: RAOBCORE v1.2 and v1.3 are both published in the Haimberger(2007) RAOBCORE paper (where they were labeled differently). Thus they have at least omitted v1.3. RAOBCORE v1.4 time series have published in the May 2007 BAMS climate state of 2006 supplement.

Peter, myself, Dian and probably a few others will meet in Japan by the End of January and a few weeks later in Germany, where we can discuss the latest developments and plan the publishing strategy.

Thanks a lot Ben for moderating this Fig. 4 issue.

Regards,

Leo

Ben Santer wrote:

> Dear folks,

>

> Just a quick update. With the assistance of Tim Osborn, Phil Jones, and
> Dian, I've now come to a decision about the disposition of our response
> to Douglass et al. I've decided to submit to IJoC. I think this is a
> fair and reasonable course of action. The IJoC editor (and various IJoC
> editorial board members and Royal Meteorological Society members) now

> recognize that the Douglass et al. paper contains serious statistical
> flaws, and that its publication in IJoC reflects poorly on the IJoC and
> Royal Meteorological Society. From my perspective, IJoC should be given
> the opportunity to set the record straight.

>

> The editor of IJoC, Glenn McGregor, has agreed to treat our paper as an
> independent submission rather than as a comment on Douglass et al. This
> avoids the situation that I was afraid of - that our paper would be
> viewed as a comment, and Douglass et al. would have the "last word" in
> this exchange. In my opinion (based on many years of interaction with
> these guys), neither Douglass, Christy or Singer are capable of
> admitting that their paper contained serious scientific errors. Their
> "last word" would have been an attempt to obfuscate rather than
> illuminate. That would have been very unfortunate.

>

> If our contribution is published in IJoC, Douglass et al. will have the
> opportunity to comment on it, and we will have the right to reply.
> Ideally, any comment and reply should be published side-by-side in the
> same issue of IJoC.

>

> The other good news is that IJoC is prepared to handle our submission
> expeditiously. My target, therefore, is to finalize our submission by
> the end of next week. I hope to have a first draft to send you by no
> later than next Tuesday.

>

> Now on to the "Figure 4" issue. Thanks to many of you for very helpful
> discussions and advice. Here are some comments:

>

> 1) I think it is important to have a Figure 4. We need to provide
> information on structural uncertainties in radiosonde-based estimates
> of

> profiles of atmospheric temperature change. Douglass et al. did not
> accurately portray the full range of structural uncertainties.

>

> 2) I do not want our submission to detract from other publications
> dealing with recent progress in the development of sonde-based
> atmospheric temperature datasets. I am aware of at least four such
> publications which are "in the pipeline".

>

> 3) So here is my suggestion for a compromise.

>

> o If Leo is agreeable, I would like to show results from his three
> RAOBCORE versions (v1.2, v1.3, and v1.4) in Figure 4. I'd also like to
> include results from the RATPAC and HadAT datasets used by Douglass et
> al. This allows us to illustrate that Douglass et al. were highly
> selective in their choice of radiosonde data. They had access to
> results

> from all three versions of RAOBCORE, but chose to show results from
> v1.2

> only - the version that provided the best support for their "models are
> inconsistent with observations" argument.

>

> o I suggest that we do NOT show the most recent radiosonde results
> from the Hadley Centre (described in the Titchner et al. paper) or from

> Steve Sherwood's group. This leaves more scope for a subsequent paper
> along the lines suggested by Leo, which would synthesize the results
> from the very latest sonde- and satellite-based temperature datasets,
> and compare these results with model-based estimates of atmospheric
> temperature change. I think that someone from the sonde community
should
> take the lead on such a paper.
>
> 4) As Melissa has pointed out, Douglass et al. may argue that v1.2 was
> published at the time they wrote their paper, while v1.3 and v1.4 were
> unpublished (but submitted). I'm sure this is how Douglass et al. will
> actually respond. Nevertheless, I strongly believe that Douglass et al.
> should have at least mentioned the existence of the v1.3 and v1.4
results.
>
> Do these suggested courses of action (submission to IJoC and inclusion
> of a Figure 4 with RAOBCOREv1.2,v1.3,v1.4/RATPAC/HadAT data) sound
> reasonable to you?
>
> With best regards,
>
> Ben

> -----

>
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>

--
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</x-flowed>

From: John Lanzante <John.Lanzante@noaa.gov>
To: santer1@llnl.gov, John Lanzante <John.Lanzante@noaa.gov>
Subject: Re: Updated Figures
Date: Sat, 12 Jan 2008 13:20:26 -0500
Reply-to: John.Lanzante@noaa.gov
Cc: Melissa Free <Melissa.Free@noaa.gov>, Peter Thorne
<peter.thorne@metoffice.gov.uk>, Dian Seidel <dian.seidel@noaa.gov>, Tom
Wigley <wigley@cgd.ucar.edu>, Karl Taylor <taylor13@llnl.gov>, Thomas R
Karl <Thomas.R.Karl@noaa.gov>, Carl Mears <mears@remss.com>, "David C.
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<francis.zwiers@ec.gc.ca>, Frank Wentz <frank.wentz@remss.com>, Leopold
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<Steven.Sherwood@yale.edu>, Steve Klein <klein21@mail.llnl.gov>, Susan
Solomon <Susan.Solomon@noaa.gov>, Tim Osborn <t.osborn@uea.ac.uk>, Gavin
Schmidt <gschmidt@giss.nasa.gov>, "Hack, James J." <jhack@ornl.gov>

Dear Ben and All,

After returning to the office earlier in the week after a couple of weeks off during the holidays, I had the best of intentions of responding to some of the earlier emails. Unfortunately it has taken the better part of the week for me to shovel out my avalanche of email. [This has a lot to do with the remarkable progress that has been made -- kudos to Ben and others who have made this possible]. At this point I'd like to add my 2 cents worth (although with the declining dollar I'm not sure it's worth that much any more) on several issues, some from earlier email and some from the last day or two.

I had given some thought as to where this article might be submitted. Although that issue has been settled (IJC) I'd like to add a few related thoughts regarding the focus of the paper. I think Ben has brokered the best possible deal, an expedited paper in IJC, that is not treated as a comment. But I'm a little confused as to whether our paper will be titled "Comments on ... by Douglass et al." or whether we have a bit more latitude.

While I'm not suggesting anything beyond a short paper, it might be possible to "spin" this in more general terms as a brief update, while at the same time addressing Douglass et al. as part of this. We could begin in the introduction by saying that this general topic has been much studied and debated in the recent past [e.g. NRC (2000), the Science (2005) papers, and CCSP (2006)] but that new developments since these works warrant revisiting the issue. We could consider Douglass et al. as one of several new developments. We could perhaps title the paper something like "Revisiting temperature trends in the atmosphere". The main conclusion will be that, in

stark contrast to Douglass et al., the new evidence from the last couple of years has strengthened the conclusion of CCSP (2006) that there is no meaningful discrepancy between models and observations.

In an earlier email Ben suggested an outline for the paper:

- 1) Point out flaws in the statistical approach used by Douglass et al.
- 2) Show results from significance testing done properly.
- 3) Show a figure with different estimates of radiosonde temperature trends illustrating the structural uncertainty.
- 4) Discuss complementary evidence supporting the finding that the tropical lower troposphere has warmed over the satellite era.

I think this is fine but I'd like to suggest a couple of other items.

First,

some mention could be made regarding the structural uncertainty in satellite

datasets. We could have 3a) for sondes and 3b) for satellite data. The satellite issue could be handled in as briefly as a paragraph, or with a bit more work and discussion a figure or table (with some trends). The main

point to get across is that it's not just UAH vs. RSS (with an implied edge

to UAH because its trends agree better with sondes) it's actually UAH vs all others (RSS, UMD and Zou et al.). There are complications in adding UMD

and Zou et al. to the discussion, but these can be handled either qualitatively or quantitatively. The complication with UMD is that it only

exists for T2, which has stratospheric influences (and UMD does not have a

corresponding measure for T4 which could be used to remove the stratospheric

effects). The complication with Zou et al. is that the data begin in 1987,

rather than 1979 (as for the other satellite products).

It would be possible to use the Fu method to remove the stratospheric influences from UMD using T4 measures from either or both UAH and RSS. It would be possible to directly compare trends from Zou et al. with UAH, RSS

& UMD for a time period starting in 1987. So, in theory we could include some trend estimates from all 4 satellite datasets in apples vs. apples comparisons. But perhaps this is more work than is warranted for this project.

Then at very least we can mention that in apples vs. apples comparisons made

in CCSP (2006) UMD showed more tropospheric warming than both UAH and RSS, and in comparisons made by Zou et al. their dataset showed more warming than both UAH and RSS. Taken together this evidence leaves UAH as the "outlier" compared to the other 3 datasets. Furthermore, better trend agreement between UAH and some sonde data is not necessarily "good" since the sonde data in question are likely to be afflicted with considerable spurious cooling biases.

The second item that I'd suggest be added to Ben's earlier outline (perhaps as item 5) is a discussion of the issues that Susan raised in earlier emails. The main point is that there is now some evidence that inadequacies in the AR4 model formulations pertaining to the treatment of stratospheric ozone may contribute to spurious cooling trends in the troposphere.

Regarding Ben's Fig. 1 -- this is a very nice graphical presentation of the differences in methodology between the current work and Douglass et al. However, I would suggest a cautionary statement to the effect that while error bars are useful for illustrative purposes, the use of overlapping error bars is not advocated for testing statistical significance between two variables following Lanzante (2005).

Lanzante, J. R., 2005: A cautionary note on the use of error bars. *Journal of Climate*, 18(17), 3699-3703.

This is also motivation for application of the two-sample test that Ben has implemented.

Ben wrote:

> So why is there a small positive bias in the empirically-determined
> rejection rates? Karl believes that the answer may be partly linked to
> the skewness of the empirically-determined rejection rate
distributions.

[NB: this is in regard to Ben's Fig. 3 which shows that the rejection rate in simulations using synthetic data appears to be slightly positively biased compared to the nominal (expected) rate].

I would note that the distribution of rejection rates is like the distribution of precipitation in that it is bounded by zero. A quick-and-dirty way to explore this possibility using a "trick" used with precipitation data is to

apply a square root transformation to the rejection rates, average these, then reverse transform the average. The square root transformation should yield data that is more nearly Gaussian than the untransformed data.

Ben wrote:

> Figure 3: As Mike suggested, I've removed the legend from the interior of the Figure (it's now below the Figure), and have added arrows to indicate the theoretically-expected rejection rates for 5%, 10%, and 20% tests. As Dian suggested, I've changed the colors and thicknesses of the lines indicating results for the "paired trends". Visually, attention is now drawn to the results we think are most reasonable - the results for the paired trend tests with standard errors adjusted for temporal autocorrelation effects.

I actually liked the earlier version of Fig. 3 better in some regards. The labeling is now rather busy. How about going back to dotted, thin and thick curves to designate 5%, 10%, and 20%, and also placing labels (5%/10%/20%) on or near each curve? Then using just three colors to differentiate between Douglass, paired/no_SE_adj, and paired/with_SE_adj it will only be necessary to have 3 legends: one for each of the three colors.

This would eliminate most of the legends.

Another topic of recent discussion is what radiosonde datasets to include in the trend figure. My own personal preference would be to have all available datasets shown in the figure. However, I would defer to the individual dataset creators if they feel uncomfortable about including sets that are not yet published.

Peter also raised the point about trends being derived differently for different datasets. To the extent possible it would be desirable to have things done the same for all datasets. This is especially true for using the same time period and the same method to perform the regression. Another issue is the conversion of station data to area-averaged data. It's usually easier to insure consistency if one person computes the trends from the raw data using the same procedures rather than having several people provide the trend estimates.

Karl Taylor wrote:

> The lower panel <of Figure 2> ...
> ... By chance the mean of the results is displaced negatively ...
> ... I contend that the likelihood of getting a difference of x is equal to the likelihood of getting a difference of $-x$...
> ... I would like to see each difference plotted twice, once with a positive sign and again with a negative sign ...
> ... One of the unfortunate problems with the asymmetry of the current figure is that to a casual reader it might suggest a consistency between the

> intra-ensemble distributions and the model-obs distributions that is not real
> Ben and I have already discussed this point, and I think we're both
> still a bit unsure on what's the best thing to do here. Perhaps others
> can provide convincing arguments for keeping the figure as is or making
> it symmetric as I suggest.

I agree with Karl in regard to both his concern for misinterpretation as well as his suggested solution. In the limit as N goes to infinity we expect the distribution to be symmetric since we're comparing the model data

with itself. The problem we are encountering is due to finite sample effects.

For simplicity Ben used a limited number of unique combinations -- using full bootstrapping the problem should go away. Karl's suggestion seems like a simple and effective way around the problem.

Karl Taylor wrote:

> It would appear that if we believe FGOALS or MIROC, then the
> differences between many of the model runs and obs are not likely to be
> due to chance alone, but indicate a real discrepancy ... This would seem
> to indicate that our conclusion depends on which model ensembles we have
> most confidence in.

Given the tiny sample sizes, I'm not sure one can make any meaningful statements regarding differences between models, particularly with regard to

some measure of variability such as is implied by the width of a distribution.

This raises another issue regarding Fig. 2 -- why show the results separately

for each model? This does not seem to be relevant to this project. Our objective is to show that the models as a collection are not inconsistent with the observations -- not that any particular model is more or less consistent with the observations. Furthermore showing results for different

models tempts the reader to make such comparisons. Why not just aggregate the

results over all models and produce a histogram? This would also simplify the figure.

Best regards,

____John

From: Kevin Trenberth <trenbert@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Draft paper on Chinese temperature trends
Date: Mon, 14 Jan 2008 09:03:31 -0700
Cc: david.parker@metoffice.gov.uk, Thomas.C.Peterson@noaa.gov, Reinhard Boehm <Reinhard.Boehm@zamg.ac.at>, Susan Solomon <Susan.Solomon@noaa.gov>, Adrian Simmons <adrian.simmons@ecmwf.int>

Hi Phil

I'll read it more thoroughly later. My quick impression, more from the abstract than the main text, is that you are defensive and it almost seems that there is a denial of the UHI in part. Yet later in the abstract and nicely in the first two sentences of the conclusions, you recognize that the UHI is real and the climate is different in cities. The point is that the homogenization takes care of this wrt the larger scale record and that UHI is essentially constant at many sites so that it does not alter trends. So I urge you to redo the abstract and be especially careful of the wording.

You might even start with:

The Urban Heat Island (UHI) is a real phenomenon in urban settings that generally makes cities warmer than surrounding rural areas. However, UHIs are evident at both London and Vienna, but do not contribute to the warming trends over the 20th century because the city influences have not changed much over that time. Similarly, ...

Regards

Kevin

Phil Jones wrote:

Dear All,

I have mentioned to you all that I've been working on a paper on Chinese temperature trends. This partly started because of allegations about Jones et al. (1990). This shows, as expected, that these claims were groundless.

Anyway - I'd appreciate if you could have a look at this draft. I have spelt things out in some detail at times, but I'm expecting if it is published that it will get widely read and all the words dissected. I know you're all very busy and I could have been doing something more useful, but it hasn't taken too long.

The European examples are just a simple way to illustrate the difference between UHIs and urban-related warming trends, and an excuse to reference Luke Howard.

Cheers

Phil

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

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Norwich

Email [1]p.jones@uea.ac.uk

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Kevin E. Trenberth e-mail: [2]trenbert@ucar.edu
Climate Analysis Section, [3]www.cgd.ucar.edu/cas/trenbert.html
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P. O. Box 3000, (303) 497 1318
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Street address: 1850 Table Mesa Drive, Boulder, CO 80305

References

1. <mailto:p.jones@uea.ac.uk>
2. <mailto:trenbert@ucar.edu>
3. <http://www.cgd.ucar.edu/cas/trenbert.html>

From: Phil Jones <p.jones@uea.ac.uk>
To: James Hansen <jhansen@giss.nasa.gov>
Subject: Differences in our series (GISS/HadCRUT3)
Date: Tue Jan 15 13:17:19 2008
Cc: gschmidt@giss.nasa.gov

Jim, Gavin,

Thanks for the summary about 2007. We're saying much the same things about recent temps, and probably when it comes to those idiots saying global warming is stopping - in some recent RC and CA threads. Gavin has gone to town on this with 6,7, 8 year trends etc.

What I wanted to touch base on is the issue in this figure I got yesterday. This is more of the same. You both attribute the differences to your extrapolation over the Arctic (as does Stefan). I've gone along with this, but have you produced an NH series excluding the Arctic ? Do these agree better?

I reviewed a paper from NCDC (Tom Smith et al) about issues with recent SSTs and the greater number of buoy type data since the late-90s (now about 70%) cf ships. The paper shows ships are very slightly warmer cf buoys (~0.1-0.2 for all SST). I don't think they have implemented an adjustment for this yet, but if done it would raise global T by about 0.1 for the recent few years. The paper should be out in J. Climate soon.

The HC folks are not including SST data appearing in the Arctic for regions where their climatology (61-90) includes years which had some sea ice. I take it you and NCDC are not including Arctic SST data where the climatology isn't correct? You get big positive anomalies if you do.

Some day we will have to solve both these issues. Both are difficult, especially the latter!

Cheers

Phil

At 21:39 14/01/2008, you wrote:

To be removed from Jim Hansen's e-mail list respond with REMOVE as subject
Discussion of 2007 GISS global temperature analysis is posted at Solar and Southern
Oscillations

[1]http://www.columbia.edu/~jeh1/mailings/20080114_GISTEMP.pdf

Jim

Prof. Phil Jones

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Norwich

Email p.jones@uea.ac.uk

NR4 7TJ

UK

References

1. http://www.columbia.edu/~jeh1/mailings/20080114_GISTEMP.pdf

From: Phil Jones <p.jones@uea.ac.uk>
To: trenbert@ucar.edu
Subject: Re: Draft paper on Chinese temperature trends
Date: Tue Jan 15 14:28:18 2008
Cc: david.parker@metoffice.gov.uk, thomas.c.peterson@noaa.gov, "Reinhard Boehm" <reinhard.boehm@zamg.ac.at>, "Susan Solomon" <susan.solomon@noaa.gov>, "Adrian Simmons" <adrian.simmons@ecmwf.int>

Kevin,

Homogeneity only done on mean T. Lots of sites just measure this. A lot will measure max and min, but I haven't got the data. I also didn't want to get into max/min as what is relevant to urban-related warming in the global land series (or China) is the effects on mean T. I can't then look at max or min against a rural series.

I would expect max to have changed less than min, but I can't really look at that.

Also I don't want to confuse readers by saying there is an urban-related temp influence, but it is to a lower DTR. I guess I could refer to Vose et al (our Fig 3.11) which does show a decrease in DTR for 79-04 over China (mostly blues).

I'll work on the text.

Cheers

Phil

At 04:50 15/01/2008, Kevin Trenberth wrote:

Phil

I looked at the paper in more detail. It obviously needs a bit of polishing throughout.

I have a couple of fairly major comments. The first is that you only deal with the mean temperature and nothing on the max and min temperatures. Are those available? It would be much more powerful if those could be included. The second is the special situation in China associated with urbanization and that is air pollution. You do not mention aerosols and their effects. We have some on that in AR4 that may be of value: refer to our chapter.

In China, there has been so much increase in coal fired power and pollution (11 out of the top worst ten polluted cities in the world are in China, or something like that). So you do not see the sun for long periods of time. Presumably that greatly cuts down on the max temp but may also increase the min through a sort of greenhouse effect? Effects of urban runoff tend to warm and space heating also warms but should mainly affect the min. Pollution may not be in the inner city but concentrated more near the sites of industry and power stations; but also may not be that local owing to winds? Pollution may also change fog or smog conditions, and may also change drizzle and precip. Looking at other variables could help with whether the changes are local or linked to atmospheric circulation.

The unique aspect of urbanization related to air pollution should make China different, but may not be easily untangled without max and min temps (and DTR).

Anyway, given these aspects, you may want to at least assemble the expectations somewhere altogether and discuss max (day) vs night (min) effects?

Hope this helps

Kevin

>

>> Dear All,

> I have mentioned to you all that I've been working on a paper on
> Chinese temperature trends. This partly started because of allegations
> about Jones et al. (1990). This shows, as expected, that these claims
> were groundless.
> Anyway - I'd appreciate if you could have a look at this draft. I
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>
> Cheers
> Phil
>
>
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References

1. <http://www.cgd.ucar.edu/cas/trenbert.html>

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@psu.edu
Subject: Re: Edouard Bard
Date: Tue Jan 15 14:49:24 2008
Cc: gschmidt@giss.nasa.gov

Mike,

Good triumphs over bad - eventually!

It does take a long time though!

Maybe Ray P. wants to do something. He is more up to speed
on all this - and reads French!

Cheers

Phil

At 14:33 15/01/2008, Michael Mann wrote:

Phil,

thanks for sending on, I've sent to Ray P. The Passoti piece is remarkably bad for a Science "news" piece, it would be worth discussing this w/ the editor, Donald Kennedy who is quite reasonable, and probably a bit embarrassed by this.

My french isn't great, but I could see there was something also about the Moberg reconstructions, Courtillot obviously trying to use that to arge that the recent warming isn't anomalous (even though the Moberg recon actually supports that it is).

I'll need to read over all of this and try to digest when I have a chance later today.

Keep up the good fight, the attacks are getting more and more desparate as the contrarians are increasingly losing the battle (both scientifically, and in the public sphere). one thing I've learned is that the best way to deal w/ these attacks is just to go on doing good science, something I learned from Ben...

talk to you later,

mike

Well, the Phil Jones wrote:

Gavin, Mike,

Some emails within this and an attachment. Send on to Ray Pierrehumbert.

Maybe you're aware but things in France are getting bad.

One thing might be a letter to Science re the diagram in an editorial in Science.

I did talk to the idiot who wrote this, but couldn't persuade him it was rubbish. This isn't the worst - see this email below from Jean Jouzel and Edouard Bard. My French is poor

at the best of times, but this all seems unfair pressure on Edouard.

See also this in French about me - lucky I can't follow it that well !

I know all this is a storm in a teacup - and I hope I'd show your resilience Mike if this was directed at me. I'm just happy I'm in the UK, and our Royal Society knows

who and why it appoints its fellows!

In the Science piece, the two Courtillot papers are rejected. I have the journal rejection emails - the other reviewer wasn't quite as strong as mine, but they were awful.

Cheers

Phil

From: Jean Jouzel [1]<jean.jouzel@lsce.ipsl.fr>

Subject: Re: Fwd: Re: Fwd: FYI: Daggers Are Drawn

X-Greylist: Sender IP whitelisted, not delayed by milter-greylist-3.0 (shiva.jussieu.fr [134.157.0.166]); Tue, 15 Jan 2008 00:07:14 +0100 (CET)

X-Virus-Scanned: ClamAV 0.92/5483/Mon Jan 14 15:45:01 2008 on shiva.jussieu.fr

X-Virus-Status: Clean

X-Miltered: at shiva.jussieu.fr with ID 478BEB15.002 by Joe's j-chkmail ([2]<http://j-chkmail.ensmp.fr>)!

X-UEA-Spam-Score: 0.3

X-UEA-Spam-Level: /

X-UEA-Spam-Flag: NO

Dear Phil,

Yes the situation is very bad in and I was indeed going to write you to ask somewhat for your help in getting some support to Edouard, which is really needed. Certainly one thing you could do would be to write to the editor of Science at least pointing to the fact that the figure is misleading using again the seasonal above 20°N Briffa et al. data set as global.

May be also at some point write something supporting the answer of Edouard and Gilles Delage, to EPSL (or in answering the letter Courtillot has recently written see attached in which he is very critical with respect to your work). I don't know

Yes I will be in Vienna , this will be a pleasure to meet you With my best Jean

At 15:29 +0000 14/01/08, Phil Jones wrote:

Jean,

Will you be going to the EGU in Vienna this April?

This disagreement with Courtillot seems to be getting out of hand.

Edouard isn't having a great time at the moment.

The data Courtillot used is not on the CRU web site. We did produce it, but for a paper Keith worked on in 2002. Courtillot's global is CRU data, but not the globe - it is land north of 20N and April to September only!

The French Academy is looking a bit of a laughing stock! I did meet Courtillot in March last year - he was courteous, but he should read the literature!

Cheers

Phil

X-Virus-Scanned: amavisd-new at arbois.cerege.fr

Date: Mon, 14 Jan 2008 12:20:00 +0100
To: Phil Jones [3]<p.jones@uea.ac.uk>
From: Edouard BARD [4]<bard@cerege.fr>
Subject: Re: Fwd: FYI: Daggers Are Drawn
X-UEA-Spam-Score: 1.4
X-UEA-Spam-Level: +
X-UEA-Spam-Flag: NO

>Courtilot says he will soon publish two studies arguing that methods used to measure global T need to be revised. Wonder if these are the two that I rejected!

Maybe one day he'll realise that there is oceanic data!

Cheers

Phil

Hello Phil,

These are indeed the papers submitted to EPSL. Courtilot has control on other journals and I'm sure he will manage to publish them somewhere else ...

As you can read below, Courtilot accused me publicly of scientific misconduct in a written message sent in copy to the president of the Academy of Science, to the president of the CNRS and to the Director of the Cabinet of the Ministry of Higher Education and Research. According to Courtilot, my misconduct is that I have acted as a hacker, introducing a "note added in proof" in my EPSL paper without the editor and the publisher even knowing it !

Courtilot even requested the organization this week of a secret meeting at the Academy in order to expose the case (yes, you've read it correctly, this is officially called "un comité secret"). I am not a member of the Academy and nobody is there to defend my case. Hence, I was obliged to write this long email to explain my position to some academicians.

I'm not really planning for sending soon something to Science as my next week will be hectic with this "inquisition" committee against me and the impact of the "droit de réponse" in newspaper(s). I am sure that Courtilot will even use Pasotti's poor paper against me during the audit of the case at the Academy. As I am the main author for the Comment, sending a rebuttal to Science may even be counterproductive. Do you plan to send something to Science about the fact that the Figure misrepresent Tglobe ?

I'm quite depressed because this is taking a lot of my time and energy. Everybody at home is mad at me, children and wife, because I spend hours and days in the lab writing and checking emails and answering phone calls.

Best wishes, Edouard

Date: Sat, 12 Jan 2008 22:13:14 +0100
To: [5]bard@cerege.fr
From: Edouard BARD [6]<bard@cerege.fr>
Subject: Accusations de M. Courtilot

Destinataires:

Madame la Présidente du CNRS, Monsieur le Président de l'Académie des Sciences, Monsieur le Directeur de Cabinet de Madame la Ministre de l'Enseignement Supérieur et de la Recherche,

Mesdames et Messieurs, Membres de l'Académie (incluant M. Courtillot).

Chers Collègues,

Je reviens à l'instant d'une tournée de conférences en Angleterre (Royal Geographical Society de Londres et Université de Cambridge). J'apprends avec stupeur que Monsieur Courtillot m'attaque personnellement et publiquement d'avoir eu un comportement contraire à l'éthique scientifique ("contrairement aux règles déontologiques, la note de M. Bard a été envoyée APRES acceptation de son commentaire critique", cf. plus bas le message envoyé hier le 11 janvier et dont vous êtes destinataires). Cette accusation surprenante est totalement infondée.

Je rappelle que dans mon Commentaire qui vient d'être publié par la revue Earth & Planetary Science Letters (EPSL, pdf attaché), je n'ai proféré aucune accusation à l'égard de M. Courtillot. J'évite justement d'avoir un ton polémique en me cantonnant à des discussions dans l'arène scientifique, par exemple mes interventions lors du colloque organisé par M. Courtillot à l'Académie des Sciences (conférence et débat disponibles sur le site internet de l'Académie:

[7]http://www.academie-sciences.fr/conferences/seances_publicques/html/debat_13_03_07.htm

[8]<http://www.canalacademie.com/Modelisation-du-climat-et-role-du.html>

ou mes publications, notamment ce 'Commentaire' Bard & Delaygue (2008 EPSL). Sur cette affaire, je n'ai accepté de faire aucun commentaire dans la presse et j'ai refusé toutes les demandes d'interview par les media audiovisuels. Même si je ne le voulais pas, je suis maintenant forcé de sortir de ma réserve et de me défendre publiquement contre les accusations de M. Courtillot.

La "note added in proof" dont vous parle M. Courtillot a été soumise normalement pour approbation au rédacteur d'EPSL (editor en anglais), M. Rob van de Hilst du MIT, comme le demande classiquement l'éditeur Elsevier (publisher) lorsqu'il envoie les épreuves d'un article à son auteur. Vous trouverez ci-dessous la copie de mon dernier échange à ce sujet avec M. van der Hilst qui explique clairement que je n'ai absolument rien à me reprocher. M. van der Hilst écrit lui-même "INDEED, YOU DID THE RIGHT THING IN ASKING MY APPROVAL.". Le fait que ma "note added in proof" ait été incluse dans la version sous presse de notre Commentaire est simplement dû à une erreur technique de l'éditeur Elsevier. Il est évidemment IMPOSSIBLE pour un auteur de modifier lui-même quoi que ce soit sur le site web d'Elsevier !

La meilleure preuve que cette fameuse note a été CRUCIALE pour clarifier l'origine des données utilisées par M. Courtillot est que justement le rédacteur, M. van der Hilst, a finalement décidé de la publier in extenso pour expliquer aux lecteurs son importance (sa note éditoriale complète est copiée plus bas). M. van der Hilst écrit ainsi "Bard and Delaygue noticed inconsistencies in the citation of data sources in Courtillot et al. (2008). and Courtillot et al. (2007)..." "instead of global, annual means they are seasonal estimates from land regions north of 20°N. With access to the correct data

files readers can form their own opinion on the analysis of and conclusions by Courtillot et al. (2007)."

Il aura donc fallu une année (voire plus, depuis Le Mouël et al. 2005 EPSL) et de nombreux courriers et publications, pour que l'on sache enfin quelles sont les températures représentées par Courtillot et al. (2007) et Le Mouël et al. (2005). La réalité est que la courbe de température utilisée par Courtillot & Le Mouël provient d'un calcul de moyenne régionale et saisonnière (Briffa et al. JGR 2001) fondé sur les séries de températures de Jones et al. (1999 Rev. Geophys.). Le fichier cité par Courtillot et al. (2007) n'est donc pas un de ceux distribués par M. Philip D. Jones (University of East Anglia & Hadley Center), ni même tiré directement de l'article de Jones et al. (1999). La citation correcte aurait dû être l'article de Briffa et al. (2001) dont Jones est coauteur. Ceci étant dit, le problème CRUCIAL est qu'il ne s'agit pas de moyenne annuelle mondiale (Tglobe) comme l'ont écrit Courtillot et al. (2007) et Le Mouël et al. (2005), mais en fait de données régionales ET saisonnières (latitudes >20°N ET seulement sur les continents ET seulement pendant la saison chaude d'Avril à Septembre). Les courbes de la température moyenne annuelle mondiale (de MM. Phil Jones d'UEA ou de Jim Hansen de la NASA) ne présentent pas de corrélation marquée avec l'éclairement solaire et les indices géomagnétiques, en particulier au niveau des années 70 (voir la Figure 1 de notre Commentaire publié par EPSL qui représente la VERITABLE courbe de température globale distribuée par M. Phil Jones).

Pour ce qui concerne les données d'irradiance solaire totale, les lecteurs d'EPSL sont maintenant pleinement informés du fait que les données utilisées par Courtillot et al. (2007) et Le Mouël et al. (2005) NE sont PAS des données d'irradiance totale (Solanki 2002), mais seulement de la petite composante ultraviolette (Tobiska 2001).

Dans leur Réponse publiée par EPSL, M. Courtillot et ses collègues accompagnent la nouvelle citation d'une note très surprenante (page 2, colonne 1): "(Tobiska, 2001; note that in Le Mouël et al., (2005), this data set was erroneously attributed to Solanki, 2002, although resulting changes are negligible)". Dans leurs deux articles Le Mouël et al. (2005) et Courtillot et al. (2007) auraient donc fait la même erreur de citation (Solanki 2002 au lieu de Tobiska 2001). Le problème est que justement les changements qui en résultent NE sont PAS du tout négligeables. Si Courtillot et al. (2007) et Le Mouël et al. (2005) avaient effectivement utilisé Solanki (2002), ils auraient inévitablement représenté la courbe d'irradiance S(t) sur tout le 20e siècle car leur figure est focalisée sur tout ce siècle et que l'analyse de Solanki (2002) porte précisément sur TOUT le 20e siècle. Pour que l'utilisation de la courbe de Solanki n'entraîne que des changements négligeables, comme ils l'écrivent, il faudrait que ces auteurs tronquent délibérément la courbe de Solanki pour n'en montrer que la moitié (les derniers 50 ans). C'est une accusation grave que je ne fais bien évidemment pas. Par conséquent, l'utilisation de la courbe de Solanki (2002) devant être faite pour tout le 20e siècle, ceci entraîne des changements importants comme les lecteurs de notre Commentaire peuvent le constater (voir la Figure 1 qui représente la VERITABLE courbe d'irradiance solaire totale distribuée par M. S. Solanki pour TOUT le 20e siècle). En particulier, il apparaît clairement que les deux courbes géomagnétiques ESK et SIT proposées par Courtillot et al. (2007) et Le Mouël et al. (2005) sont en DESACCORD

FLAGRANT vers les années 70 avec les VERITABLES courbes de la température moyenne annuelle mondiale et de l'irradiance solaire totale (dans notre Commentaire, nous soulignons au passage que l'index géomagnétique AA est en bien meilleur accord, fait connu et publié depuis dix ans, e.g. Cliver et al. 1998 GRL).

Monsieur Courtillot n'apporte aucune réponse à ces nombreux problèmes. Par ailleurs, il est navrant de constater que dans ses conférences publiques récentes (voir celle donnée lors du 125e anniversaire de l'ESPCI avec fichier powerpoint disponible sur le site web [9]<http://www.espci.fr/actu/espci125/pgm0011.htm>), M. Courtillot continue encore de montrer le même diagramme erroné, avec une courbe de température "Tglobe" qui n'est pas une courbe de moyenne annuelle mondiale de la température et une courbe "S(t)" qui n'est pas une courbe d'irradiance solaire totale. De plus, M. Courtillot montre encore cette courbe sur 50 ans seulement, alors même qu'il a pleinement connaissance de travaux qui donnent l'irradiance sur le tout le 20e siècle (Solanki 2002, article qu'il a lui-même cité depuis 2005).

Je vous prie de croire, Chers Collègues, à l'assurance de mes sentiments respectueux et dévoués.

Edouard Bard

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De : Vincent Courtillot [[12]courtil@ipgp.jussieu.fr]

Envoyé : vendredi 11 janvier 2008 12:13

À : MALAUSSENA Béatrice

Cc : [13]lemouel@ipgp.jussieu.fr; [14]fluteau@ipgp.jussieu.fr;

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[20]J.Hoffmann@ibmc.u-strasbg.fr; [21]jules.hoffmann@academie-sciences.fr; COURTOIS

Gérard; [22]laurentjoffrin@yahoo.fr; [23]smarchand@lefigaro.fr;

[24]pgillet@geologie.ens-lyon.fr; [25]philippe.gillet@recherche.gouv.fr

Objet : Droit de réponse

Importance : Haute

Madame,

étant donné le contenu du message que j'ai reçu de vous hier, je préfère continuer notre échange en en gardant des traces écrites et en le communiquant à des tiers en tant que de

besoin, en cas de suites juridiques.

Un résumé sans doute un peu simplifié mais me semble t'il non faux de notre dernière conversation et de votre message sur ma boîte vocale est le suivant.

"Votre journaliste ne m'a pas calomnié; il a simplement retranscrit des accusations formulées dans une publication scientifique. En revanche, ma réponse vous poserait des problèmes juridiques et vous auriez des amendements à m'y faire apporter."

Je vous demande tout simplement de me renvoyer mon texte avec les modifications qui vous paraissent nécessaires en rouge, naturellement maintenues à leur minimum, puisqu'il s'agit d'une lettre que j'écris et dont j'endosse la responsabilité, avec toute la force que me donne l'évident droit de réponse (pas un lecteur que j'ai rencontré ne l'a nié, quelque soir par ailleurs la réalité du fond) que vous me devez et que Gérard Courtois a reconnu a plusieurs reprises depuis le 21 décembre dernier (tout cela traîne de façon étonnante...).

En ce qui concerne vos échanges avec M. Foucart tels que vous me les rapportez dans le message vocal (que j'ai conservé), juste deux commentaires:

1) vous me dites que le fond du débat entre nous deux est scientifique. M. Foucart est un journaliste pas un scientifique (ce sont deux métiers différents, a priori également estimables). Seuls les propos qu'il peut valablement citer avec leurs sources provenant de scientifiques qui en portent alors la responsabilité sont des débats scientifiques.

2) Vous me dites que M. Foucart a tout fait pour me joindre. Il est exact (je l'ai vérifié) qu'il a téléphoné à M. Dyon à l'IPGP, une demi-heure après que je sois rentré chez moi victime d'une grippe qui m'a tenu au lit trois jours avec 38°5. Une fois guéri, je suis passé à la garde de mes enfants et petits enfants qui étaient balayés par le virus. Rien de grave et nombreux sont ceux qui y sont passés à Noël. Donc j'étais réellement souffrant (et pas entre guillemets) ce soir là. Mais je n'ai fait barrage à aucune demande qui m'aurait été faite: mon numéro de téléphone est public, dans les pages planches du bottin, pas sur liste rouge, il était facile de m'appeler chez moi. Je n'ai reçu aucun coup de téléphone de M. Foucart, ni d'ailleurs d'aucun des autres journalistes. Et faire son travail en l'occurrence, alors que rien ne justifiait l'urgence de la publication de ce sujet, c'était attendre un ou deux jours et faire l'effort de me téléphoner et d'avoir mon témoignage. L'effort fait s'est limité à ce coup de fil à l'IPGP...

J'ai eu la possibilité de démontrer hier pendant deux heures devant un auditoire scientifique de plus de 150 personnes que non seulement les accusations portées contre nous, notamment par votre journaliste, étaient purement et simplement sans fondement, mais que de plus les critiques scientifiques formulées par Edouard Bard et Gilles Delaygue étaient pour l'essentiel fausses ou fondées sur des témoignages faux que leur avaient fourni certains de leurs collègues. Nos conclusions scientifiques restent donc dans leur totalité. mais ceci est la partie scientifique et ce qui m'intéresse en ce qui vous concerne c'est de faire savoir à vos lecteurs que les allégations de M. Foucart dans l'article incriminé étaient fausses, et j'ai le droit de le faire et rapidement et sans censure!

Dans ce séminaire hier, étaient présentes de très nombreuses personnes qui pourront porter témoignage. C'est le cas de l'ancienne directrice de la recherche, Mme Giacobino,

de la présidente du CNRS Madame Catherine Bréchnignac qui m'a publiquement assuré de son soutien et m'a assuré qu'elle était totalement convaincue par ma démonstration (sa compétence scientifique ne devrait pas être trop inférieure à celle de M. Foucart). Etaient également présents un journaliste de l'AFP et un journaliste de Science et Avenir. Cette conférence, comme toutes celles de l'IPGP, sera bientôt disponible sur cd et intranet.

Mardi prochain, je suis invité par le président de l'académie des sciences à exposer pendant 20mn l'ensemble de nos arguments. Je pense que le résultat sera le même qu'hier à l'IPGP. A cette occasion, puisqu'aucune n'est encore sortie, je remettrai à l'ensemble des académiciens une copie des trois lettres de droit de réponse envoyées aux journaux, dont le vôtre. J'aurais préféré qu'elles soient publiées avant cette date (je note que ni Libération ni le Figaro ne m'ont à ce jour donné la moindre indication sur la publication de mes droits de réponse; le Monde aura pour l'instant été le plus réactif. Je mets en copie les trois personnes à qui j'ai originellement envoyé mon droit de réponse dans ces trois journaux).

J'attends donc la version amendée que vous souhaitez me voir accepter.

Sincèrement, VC

PS J'ai eu copie, comme M. Foucart l'évoquait, d'une réponse qu'il a fournie à un de mes amis qui lui avait écrit; je la reproduis ci dessous. Je ne souhaite y relever qu'une seule phrase: "Le blog RealClimate cité dans l'article n'est que la façade d'une polémique qui se joue dans une revue savante, EPSL en l'occurrence. " Cette présentation est inexacte. L'échange dans EPSL est un échange scientifique, sans polémique et surtout sans diffamation. Il est cependant désormais établi que, contrairement aux règles déontologiques, la note de M. Bard a été envoyée APRES acceptation de son commentaire critique. Le rédacteur de la revue vient de nous envoyer copie de l'éditorial qu'il va faire publier en en-tête de nos deux articles dès la publication papier (qui fait autorité et doit avoir lieu très prochainement). Je vous joins également copie de cet éditorial. La ligne jaune est celle qui sépare le débat scientifique de la diffamation. La diffamation n'apparaît pas dans EPSL mais dans le blog Real Climate et elle est reprise activement, sous leur signature et non pas seulement en citation entre guillemets) par les journalistes. Là est la faute juridiquement répréhensible qu'ils ont commise. Là est la base de mon droit de réponse. La journaliste neutre et semblable au lecteur de base du Monde que vous m'avez dit être doit facilement s'en convaincre si elle est impartiale.

Date: Thu, 08 Nov 2007 09:44:17 -0500

From: Rob van der Hilst [26]<hilst@MIT.EDU>

Organization: MIT-EAPS

To: Edouard BARD [27]<bard@cerege.fr>

Subject: Re: ESPL comment & reply

Dear Edouard, a very quick response - I cannot do anything until I hear from Elsevier's production office that changes can or cannot be made. But I want to avoid

misunderstandings between us. I do not ACCUSE you of adding material and hide it from me - indeed, you did the right thing in asking my approval. However, if you now go on line and check your "comment" you will see that it does have the 'note added in proof'. So by returning the proofs with the addition it did make it to the public domain REGARDLESS of me approving it or not. The EPSL production staff should have picked up on this. So I am not pointing fingers here - I just have to deal with an unfortunate situation in which a significant addition to an already accepted text may make it into the literature even if the other party has no chance to repond or clarify the issue.
OK? Cheers, Rob

Date: Thu, 8 Nov 2007 15:27:37 +0100
To: Rob van der Hilst [28]<hilst@MIT.EDU>
From: Edouard BARD [29]<bard@cerege.fr>
Subject: ESPL comment & reply
Dear Rob,

>In principle, after approval of a 'comment' the other party is given the opportunity to respond, and approval of the 'reply' closes the process. To avoid going-back-and-forth, in my view the material should not appear on line until after approval of the corrected proofs.

I agree.

>In this case you added material to the 'comment' after seeing the 'reply', and without my consent.

I disagree with your accusation. I did NOT try to add anything and hide it from you. Indeed, I immediately sent an email to you in order to propose our 'note added in proof'. I did this because I knew very well that such a note could not be published without your consent (during 4 years I also served EPSL). Indeed, the Elsevier message accompanying the uncorrected proofs is very clear on this issue "Significant changes to the article as accepted for publication will only be considered at this stage with permission from the Editor." This was exactly the purpose of my email to you.

>In my view the sole purpose of your addition is - or, at least, should be -
> to help clarify an important issue for the readers.

This is precisely my goal.

>(NB I am sure you realize that your 'note added in proof' could be perceived by readers as an accusation that Courtillot et al are not honest about the source of the data, in particulare related to the Tglobe file, and that would be quite a serious matter.)

I am open for revision of the note if you think it could be misinterpreted. Whatever the reason for the discrepancy, it is important that the reader can identify exactly the source of these important data (published paper or valid URL). This is clearly a problem with many source of data cited by Courtillot et al (and Le Mouel et al.). For example the (flawed) TSI SOLAR2000 curve now cited in the Reply by Courtillot et al. should be accompanied by its URL and/or its reference (Tobiska 2001). It is even worse for the Tglobe curve which source is still completely unclear: it does not correspond to the cited reference and the code file cited in the Reply is not available. As previously stated, it is even possible to see that it does not correspond to the cited Tglobe curve just by looking at their shapes (see Fig. 1 of our Comment that provides the two very similar Tglobe curves developed at UEA by the group of Phil Jones and at NASA by the group of Jim Hansen). If you compare these two Tglobe curves with the one represented on Fig. 3 of Courtillot et al. (2007), you will immediately see that there are important differences in the shape of the maximum in the 40s, the pause (or minimum) in the 60-70s and the phase lag in the 30s. Comparison with the Tglobe curve is obviously central to all climate-related discussions in these papers.

Best regards,
Edouard

Editorial Note

The paper entitled "Are there connections between the Earth's magnetic field and climate?" published in Earth and Planetary Science Letters (Courtillot et al., 2007) triggered a "comment" (Bard and Delaygue, 2008) and a "reply" (Courtillot et al, 2008). These publications, and EPSL's handling of the "comment" and "reply" (hereinafter C08), have received significant attention in electronic and printed news media.

In a "comment-reply" exchange, standard editorial policy gives the responder the last word and requires that the "comment" is not changed once accepted by the Editor and replied to by the authors whose work is being criticized. In this case, Bard and Delaygue noticed inconsistencies in the citation of data sources in C08 and Courtillot et al. (2007) after the (accepted) "comment" and "reply" had appeared online (but before they received galley proofs). They pointed this out in a "Note added in Proof" to their "comment". Being against EPSL's policy this modification was disapproved (and removed). However, properly reporting data is an essential aspect of scientific communication in that it enables independent evaluations of the analysis presented by authors. Therefore, Courtillot et al. were asked to clarify (in C08) the source of the data used.

For full disclosure, the note by Bard and Delaygue is reproduced here:

"In their Response to our Comment, Courtillot et al. state that for the total irradiance curve $S(t)$ they had used the SOLAR2000 model product by Tobiska (2001) instead of the century-long record by Solanki (2002) cited in their original paper (Courtillot et al. 2007). However, the SOLAR2000 model is restricted to the UV component and their total solar irradiance is severely flawed as pointed out by Lean (2002). For the global

temperature Tglobe curve cited from Jones et al. (1999) in Courtillot et al. (2007), these authors now state in their response that they had used the following data file: monthly_land_and_ocean_90S_90N_df_1901-2001mean_dat.txt. We were unable to find this file even by contacting its putative author who specifically stated to us that it is not one of his files (Dr. Philip D. Jones, written communication dated Oct. 23, 2007)." In response, Courtillot et al. (2007) provided two modifications (in italics) in C08: "The solar irradiance daily time series we used is that from the SOLAR2000 research grade model upgraded to v1.23A (file Five_cycle_v1_23a.txt dated 23 April 2003) which covers the time period from 14 February 1947 to 31 May 2002 (Tobiska, 2001; note that in Le Mouél et al, 2005, this data set was erroneously attributed to Solanki, 2002, although resulting changes are negligible)." and "The temperature series we actually used is obtained from Briffa et al. (2001) - specifically, column 7 of [30]ftp://ftp.ncdc.noaa.gov/pub/data/paleo/treering/reconstructions/n_hem_temp/briffa2001jgr3.txt, that is, years 1871 to 1997 - which is, originally, from Jones et al (1999) as quoted. All we did was to average it over an 11yr sliding window."

The ftp link shows that the temperatures used are indeed from Jones and co-workers, but instead of global, annual means they are seasonal estimates from land regions north of 20°N. With access to the correct data files readers can form their own opinion on the analysis of and conclusions by Courtillot et al. (2007).

Robert D. van der Hilst

Editor for Earth and Planetary Science Letters

Bard, E., and Delaygue, G., 'Comment on "Are there connections between the Earth's magnetic field and climate?" by V. Courtillot, Y. Gallet, J.-L. Le Mouél, F. Fluteau, A. Genevey', Earth Planet. Sci. Lett. 265, 302-307, 2008

Courtillot, V., Gallet, Y., Le Mouél, J.-L., Fluteau, F., and Genevey, A., Are there connections between the Earth's magnetic field and climate?, Earth Planet. Sci. Lett., 253, 328-339, 2007

Courtillot, V., Gallet, Y., Le Mouél, J.-L., Fluteau, F., and Genevey, A., 'Response to comment on "Are there connections between Earth's magnetic field and climate" by Bard, E., and Delaygue, G., Earth Planet. Sci. Lett., 265, 302-307, 2008

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30. ftp://ftp.ncdc.noaa.gov/pub/data/paleo/treering/reconstructions/n_hem_temp/briffa2001jgr3.txt
31. <mailto:bard@cerege.fr>
32. http://www.college-de-france.fr/default/EN/all/evo_cli/
33. <mailto:bard@cerege.fr>
34. http://www.college-de-france.fr/default/EN/all/evo_cli/
35. <mailto:p.jones@uea.ac.uk>
36. <mailto:jean.jouzel@lsce.ipsl.fr>
37. <mailto:jzipsl@ipsl.jussieu.fr>
38. <mailto:jean.jouzel@lsce.ipsl.fr>
39. <mailto:p.jones@uea.ac.uk>
40. <mailto:mann@psu.edu>
41. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Phil Jones <p.jones@uea.ac.uk>
To: Raymond P. <rtp1@geosci.uchicago.edu>
Subject: [Fwd: Re: [Fwd: Edouard Bard]]
Date: Wed Jan 16 09:23:52 2008
Cc: Michael Mann <mann@meteo.psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>

Ray,

Glad to see you're onto this. Obviously anything shouldn't make it even worse for Edouard, but you're in contact with him.

I'd be happy to sign onto any letter from Science, but this isn't essential. I know the series Courtillot has used (and Pasotti re-uses) came from here, but it isn't what he and the authors says it was. I also know it doesn't make much difference if the correct one was used - given the smoothing. It is just sloppy and a principle thing. The correct data are sitting on our web site and have been since Brohan et al (2006) appeared in JGR. Even the earlier version (HadCRUT2v) would have been OK, but not a specially produced series for a tree-ring reconstruction paper back in 2001/2 and not on our web site.

Then there are all the science issues you and Edouard have raised in RC and the EPSL comment.

I have had a couple of exchanges with Courtillot. This is the last of them from March 26, 2007. I sent him a number of papers to read. He seems incapable of grasping the concept of spatial degrees of freedom, and how this number can change according to timescale. I also told him where he can get station data at NCDC and GISS (as I took a decision ages ago not to release our station data, mainly because of McIntyre). I told him all this as well when we met at a meeting of the French Academy in early March.

What he understands below is my refusal to write a paper for the proceedings of the French Academy for the meeting in early March. He only mentioned this requirement afterwards and I said I didn't have the time to rewrite was already in the literature. It took me several more months of emails to get my expenses for going to Paris!

Cheers

Phil

From Courtillot 26 March 2007

Dear Phil,

Sure I understand. Now research wise I would like us to remain in contact. Unfortunately, I have too little time to devote to what is in principle not in my main stream of research and has no special funding. But still I intend to try and persist. I find these temperature and pressure series fascinating. I have two queries:

1) how easy is it for me (not a very agile person computer wise) to obtain the files of data you use in the various global or non global averages of T (I mean the actual montly data in each 5° box prior to any processing, including computation of the "temperature anomaly")? How do I do it? What I would like to be able to extract is for instance all of

the data within a given 5° by 5° box with their dates (so: lat, lon, time, value). I understand these are monthly means, though we find that there may be some quite important information in the daily values which is likely lost on monthly averaging, but this is another question...

2) I know you answered my question but still I have trouble grasping the answer. Could you explain how the global T average for periods say before 1900 can have a total uncertainty under 0.2°C back to 1850. This can only be true, given the data distribution in the Rayner et al paper, if T is an incredibly smooth function of location. Did you really answer me that by extracting from the recent (post 1950) database data with the same geographical and temporal distributions as the 1850-1900 data you get almost the same result as with the full modern data (with an uncertainty just above 0.1°C). This seems truly amazing, and would never work with the global magnetic field data I am accustomed to work on. Yet it does not seem to me that climate varies as slowly and with as long spatial scales as the magnetic field...

I will very much appreciate your comments and help on those.

Thank you again for having come to our meeting.

Yours very sincerely,

Vincent

--

Vincent Courtillot

Professor of Geophysics University Paris 7,

Director Institut de Physique du Globe de Paris,

Member Institut Universitaire de France,

Member Academia Europaea and French Academy of Sciences

President, Geomagnetism and Paleomagnetism, American Geophysical Union

President, Scientific Council, City of Paris

Date: Tue, 15 Jan 2008 12:20:57 -0500

From: Michael Mann <mann@meteo.psu.edu>

Reply-To: mann@psu.edu

Organization: Penn State University

User-Agent: Thunderbird 2.0.0.9 (Windows/20071031)

To: Phil Jones <p.jones@uea.ac.uk>, Gavin Schmidt <gschmidt@giss.nasa.gov>

Subject: [Fwd: Re: [Fwd: Edouard Bard]]

X-UEA-Spam-Score: 0.3

X-UEA-Spam-Level: /

X-UEA-Spam-Flag: NO

update from Ray P...

mike

----- Original Message -----

Subject: Re: [Fwd: Edouard Bard]

Date: Tue, 15 Jan 2008 10:20:59 -0600

From: Raymond P. [1]<rtp1@geosci.uchicago.edu>

To: Group RealClimate [2]<group@realclimate.org>
References: [3]<478CC27D.1040900@meteo.psu.edu>

--

Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [4]mann@psu.edu
University Park, PA 16802-5013

[5]<http://www.met.psu.edu/dept/faculty/mann.htm>

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name="[6]file:///C:/DOCUME~1/MICHAE~1/LOCALS~1/TEMP/nsmail.1"

Content-Disposition: inline;

filename="[7]file:///C:/DOCUME~1/MICHAE~1/LOCALS~1/TEMP/nsmail.1"

X-MIME-Autoconverted: from 8bit to quoted-printable by f05n05.cac.psu.edu id
m0FHKxKM050156

Yes indeed. I am writing a letter to Science today regarding Pasotti's ridiculous article. If anybody things the rest of RC should sign on to that as well, just let me know.

I will also have to write a Part III, covering all the junk mentioned by Edouard and by Phil Jones. Courtillot's response (published via a legal device activated where there is the possibility of threatening a libel suit) appeared in Le Monde today. I may give it a week or so for new developments to settle down before writing.

For example, Foucart may get a chance to write a response in Le Monde.

While I'll wait a bit before doing the RC piece, I plan to send off the letter to Science this week.

--Ray

On Jan 15, 2008, at 8:26 AM, Michael Mann wrote:

fyi,
mike

----- Original Message -----

Subject:

Edouard Bard

Date:

Tue, 15 Jan 2008 12:59:44 +0000

From:

Phil Jones <p.jones@uea.ac.uk>

To:
gschmidt@giss.nasa.gov

CC:
Michael E. Mann <mann@meteo.psu.edu>

References:

<46E534DD.30206@met.no> <4756A519.4090906@met.no> <4757EFB1.1000608@met.no>
<477CB5FA.609@met.no> <Pine.LNX.4.61.0801030902200.1581@isotope.giss.nasa.gov>

Some emails within this and an attachment. Send on to Ray Pierrehumbert.

Maybe you're aware but things in France are getting bad.

One thing might be a letter to Science re the diagram in an editorial in Science. I did talk to the idiot who wrote this, but couldn't persuade him it was rubbish. This isn't the worst - see this email below from Jean Jouzel and Edouard Bard. My French is poor

at the best of times, but this all seems unfair pressure on Edouard.

See also this in French about me - lucky I can't follow it that well !

I know all this is a storm in a teacup - and I hope I'd show your resilience Mike if this was directed at me. I'm just happy I'm in the UK, and our Royal Society knows who and why it appoints its fellows!

In the Science piece, the two Courtillot papers are rejected. I have the journal rejection emails - the other reviewer wasn't quite as strong as mine, but they were awful.

Cheers

Phil

From: Jean Jouzel <jean.jouzel@lsce.ipsl.fr>

Subject: Re: Fwd: Re: Fwd: FYI: Daggers Are Drawn

X-Greylist: Sender IP whitelisted, not delayed by milter-greylis-3.0 (shiva.jussieu.fr [134.157.0.166]); Tue, 15 Jan 2008 00:07:14 +0100 (CET)

X-Virus-Scanned: ClamAV 0.92/5483/Mon Jan 14 15:45:01 2008 on shiva.jussieu.fr

X-Virus-Status: Clean

X-Miltered: at shiva.jussieu.fr with ID 478BEB15.002 by Joe's j-chkmail ([8]http://j-chkmail.ensmp.fr)!

X-UEA-Spam-Score: 0.3

X-UEA-Spam-Level: /

X-UEA-Spam-Flag: NO

Dear Phil,

Yes the situation is very bad in and I was indeed going to write you to ask somewhat for your help in getting some support to Edouard, which is really needed. Certainly one thing you could do would be to write to the editor of Science at least pointing to the fact that the figure is misleading using again the seasonal above 20°N Briffa et al. data set as global.

May be also at some point write something supporting the answer of Edouard and Gilles Delaygue, to EPSL (or in answering the letter Courtillot has recently written see attached in which he is very critical with respect to your work). I don't know

Yes I will be in Vienna , this will be a pleasure to meet you With my best Jean

</blockquote></x-html>

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. <mailto:rtp1@geosci.uchicago.edu>
2. <mailto:group@realclimate.org>
3. <mailto:478CC27D.1040900@meteo.psu.edu>
4. <mailto:mann@psu.edu>
5. <http://www.met.psu.edu/dept/faculty/mann.htm>
6. <file:///C:/DOCUME~1/MICHAEL~1/LOCALS~1/TEMP/nsmail.1/>
7. <file:///C:/DOCUME~1/MICHAEL~1/LOCALS~1/TEMP/nsmail.1/>
8. <http://j-chkmail.ensmp.fr/>

From: "James Hansen" <jhansen@giss.nasa.gov>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Re: [Fwd: RE: Dueling climates]
Date: Fri, 18 Jan 2008 05:17:06 -0500
Cc: "Kevin Trenberth" <trenbert@ucar.edu>, "Karl, Tom" <Thomas.R.Karl@noaa.gov>, "Reto Ruedy" <rriedy@giss.nasa.gov>

Thanks, Phil. Here is a way that Reto likes to list the rankings that come out of our version of land-ocean index.

| rank | LOTI |
|--------|-------|
| 1 2005 | 0.62C |
| 2 1998 | 0.57C |
| 2007 | 0.57C |
| 2002 | 0.56C |
| 2003 | 0.55C |
| 2006 | 0.54C |
| 7 2004 | 0.49C |

i.e., the second through sixth are in a statistical tie for second in our analysis. This seems useful, and most reporters are sort of willing to accept it. Given differences in treating the Arctic etc., there will be substantial differences in rankings. I would be a bit surprised is #7 (2004) jumped ahead to be #2 in someone else's analysis, but perhaps even that is possible, given the magnitude of these differences.

Jim

On Jan 18, 2008 5:03 AM, Phil Jones <[1]p.jones@uea.ac.uk> wrote:

Kevin,

When asked I always say the differences are due to the cross-Arctic extrapolation.

Also

as you say there is an issue of SST/MAT coming in from ships/buoys in the Arctic. HadCRUT3 (really HadSST2) doesn't use these where there isn't a 61-90 climatology - a lot of areas with sea ice in most/some years in the base period. Using fixed SST values of -1.8C is possible for months with sea ice, but is likely to be wrong. MAT would be impossible to develop 61-90 climatologies for when sea ice was there. This is an issue that will have to be addressed at some point as the sea ice disappears. Maybe we could develop possible approaches using some AMIP type Arctic RCM simulations?

Agreeing on the ranks is the hardest of all measures. Uncertainties in global averages are of the order of +/- 0.05 for one sigma, so any difference between years of less than 0.1

isn't significant. We (MOHC/CRU) put annual values in press releases, but we also put errors. UK newspapers quote these, and the journalists realise about uncertainties, but prefer to use the word accuracy.

We only make the press releases to get the numbers out at one time, and focus all the calls. We do this through WMO, who want the release in mid-Dec.

There is absolutely no sense of duelling in this. We would be criticised if there were just

one analysis. The science is pushing for multiple analyses of the same measure - partly to make sure people remember RSS and not just believe UAH. As we all know, NOAA/NASA and HadCRUT3 are all much closer than RSS and UAH!

I know we all know all the above. I try to address this when talking to journalists, but they generally ignore this level of detail.

I'll be in Boulder the week after next at the IDAG meeting (Jan 28-30) and another

meeting Jan30/Feb 1. Tom will be also.

Cheers

Phil

At 02:12 18/01/2008, Kevin Trenberth wrote:

FYI
See the discussion below.Â Looks like clarification is called for when these statements are made that consider the other announcements.

Kevin

----- Original Message -----

Subject: RE: Dueling climates

Date: Thu, 17 Jan 2008 18:51:13 -0500

From: Ryan, Bob (NBC Universal) [2]<Bob.Ryan@nbcuni.com>

To: Kevin Trenberth [3]<trenbert@ucar.edu>, [4]<anthes@ucar.edu>

CC: [5]<kseitter@ametsoc.org>

References: [6]<7C368A942599A944A0C43774DE6412EE044C9964@DCNMLVEM01.e2k.ad.ge.com>

[7]<478F89E4.10405@ucar.edu> [8]<478FBF64.1020500@ucar.edu>

Rick, Kevin,

Â

Attached is the NOAA release.Â I believe I had read that the discrepancy with the NASA ("Second hottest year") data/release was also related to how NOAA adjusts for heat island effects and resiteing of climate stations.Â In any event I don't think dueling climate data serves the broad goals of informing/educating the public and decision makers about climate change.Â I can hear some saying, "If NOAA and NASA can't even agree what the temperature was last year, how can we believe what they are saying about the future climate".

Â

Bob

Â

Â

From: Kevin Trenberth [[9]mailto:trenbert@ucar.edu]

Sent: Thursday, January 17, 2008 3:50 PM

To: [10]anthes@ucar.edu

Cc: Ryan, Bob (NBC Universal); [11]kseitter@ametsoc.org

Subject: Re: Dueling climates

Hi Rick

My understanding is that the biggest source of this discrepancy is the way the Arctic is analyzed.Â We know that the sea ice was at record low values, 22% lower than the previous low in 2005.Â Some sea temperatures and air temperatures were as much as 7C above normal.Â But most places there is no conventional data.Â In NASA they extrapolate and build in the high temperatures in the Arctic.Â In the other records they do not.Â They use only the data available and the rest is missing.Â In most cases the values from recent years are about statistically tied and the ranking is one that separates values by hundredths of a degree.Â There is no correct way to do this (especially the treatment of missing data), and different groups do it differently. You typically get different answers if you compute the hemispheric means and average them vs computing the global mean, because more data are missing in the southern hemisphere.Â Although this can be addressed using remote

sensing in recent times, the climatologies differ. Ideally one should have a global analysis with no missing data, and this occurs in the global analyses, but they have other problems.

Hope this helps

Kevin

Rick Anthes wrote:

Bob-

I saw the NASA one (GISS) but not the NOAA release. Could you point me toward it? I see your point. These preliminary analyses may change with time and the press releases have not been peer-reviewed. I am surprised the two estimates disagree this much, but the difference is probably well within the uncertainty of the estimate of annual global temperatures. I'd be interested in Kevin's take on this.

Rick

Ryan, Bob (NBC Universal) wrote:

Rick, Keith,

^

Don't know if this will come up in the Council or if there is time to even discuss but I'm sure you've seen the NOAA/NASA press releases and the news stories about the 2007 global temperatures. NASA says tied for "2nd hottest". . . NOAA says 5th warmest global and only 10th in US. Who does this serve but create confusion and add to the skeptics/denialists argument. . ."They can't even agree on last year's temperatures. . .why should we believe them?"

^

Science by press release doesn't serve anyone and certainly not a curious public.

^

Role for the AMS?

^

^

See you soon.

^

Bob

Subject:

NASA SCIENTISTS RELEASE 2007 TEMPERATURE DATA

From:

"Maria Frostic" [12]<mfrostic@pop100.gsfc.nasa.gov>

Date:

Tue, 15 Jan 2008 18:26:13 -0500

To:

"Maria Frostic" [13]<mfrostic@pop100.gsfc.nasa.gov>

To:

"Maria Frostic" [14]<mfrostic@pop100.gsfc.nasa.gov>

Maria Frostic ^ ^ ^ ^

1/15/08

(301) 286-9017

2007 Among Hottest Years on Record:

NASA Scientists Release Global Temperature Analysis

An analysis of 2007 global temperature data undertaken by scientists at Goddard Institute for Space Studies (GISS), New York, reveals that 2007 is

tied with 1998 as the second hottest year on record. The unusual warmth of

2007 is noteworthy because it occurs at a time when solar irradiance is at a minimum and the equatorial Pacific Ocean has entered the cool phase of its

El Niño-La Niña cycle.

The greatest warming in 2007 occurred in the Arctic. Global warming has a

larger affect in polar areas, as the loss of snow and ice leads to more open

water, which absorbs more sunlight and warmth. The large Arctic warm

anomaly of 2007 is consistent with observations of record low Arctic sea ice in September 2007.

The eight warmest years in the GISS record have all occurred since 1998,

with 2005 ranking as the hottest. Barring a large volcanic eruption, NASA

scientists predict that a record global temperature exceeding that of 2005 can be expected within the next two to three years.

A NASA TV Video File on this topic will run January 16th at 9 A.M., 12, 4,

8, and 10 P.M. EDT on the NASA TV media channel (#103).

Video Highlights:

- * Colorful Visualizations of Global Temperature Data from 1880-2007
- * Animations of Unique Perspectives on Ice Albedo
- * Animated Earth Displaying Seasonal Landcover and Arctic Sea Ice
- * Select Interview Clips with NASA Scientist Dr. James Hansen

For high definition video downloads, print resolution still images, and a short web video on taking Earth's temperature, visit:

[15]http://www.nasa.gov/topics/earth/features/earth_temp.html

NASA Television is carried on an MPEG-2 digital signal accessed via satellite AMC-6, at 72 degrees west longitude, transponder 17C, 4040 MHz, vertical polarization. A Digital Video Broadcast (DVB) - compliant Integrated Receiver Decoder (IRD) with modulation of QPSK/DBV, data rate of 36.86 and FEC <= is needed for reception. NASA TV Multichannel Broadcast includes Public Services Channel (#101), the Education Channel (#102) and the Media Services Channel (#103).

For NASA TV information and schedules on the Web, visit: [16]www.nasa.gov/ntv

Subject:

NOAA: 2007 Was Tenth Warmest for U.S., Fifth Warmest Worldwide

From:

"NOAA News Releases" [17]<Press.Releases@noaa.gov>

Date:

Tue, 15 Jan 2008 15:00:00 -0500

To:

"Ryan, Bob (NBC Universal)" [18]<bob.ryan@nbc.com>

To:

"Ryan, Bob (NBC Universal)" [19]<bob.ryan@nbc.com>

TO: Ryan, Bob; WRC-TV

FOR IMMEDIATE RELEASE January 15, 2008

*** NEWS FROM NOAA ***

NATIONAL OCEANIC & ATMOSPHERIC ADMINISTRATION

U. S. DEPARTMENT OF COMMERCE

WASHINGTON, DC

Contact: John Leslie, 301-713-2087, ext. 174

NOAA: 2007 Was Tenth Warmest for U.S., Fifth Warmest Worldwide

Â Â Â Â Â The average temperature for the contiguous

U.S. in 2007 is officially the tenth warmest on record, according to data from scientists at

NOAA's National Climatic Data Center in

Asheville, N.C. The agency also determined the global surface temperature last year was the fifth warmest on record.

U.S. Temperature Highlights

- * The average U.S. temperature for 2007 was 54.2 degrees F; 1.4 degrees F warmer than the 20th century mean of 52.8 degrees F. NCDC originally estimated in mid-December that 2007 would end as the eighth warmest on record, but below-average temperatures in areas of the country last month lowered the annual ranking. For Alaska, 2007 was the 15th warmest year since statewide records began in 1918.
- * Six of the 10 warmest years on record for the contiguous U.S. have occurred since 1998, part of a three decade period in which mean temperatures for the contiguous U.S. have risen at a rate near 0.6 degrees F per decade.
- * For the contiguous U.S., the December 2007 mean temperature was 33.6 degrees F, near the 20th century average of 33.4 degrees F. The Southeast was much warmer than average, while 11 states, from the Upper Midwest to the West Coast, were cooler than average.
- * Warmer-than-average temperatures for December 2007 in large parts of the more heavily populated eastern U.S. resulted in temperature related energy demand about 1.9 percent below average for

the nation as a whole, based on NOAA's

Residential Energy Demand Temperature Index. For the year, the REDTI estimates that national residential energy consumption was about 2.5 percent below average.

U.S. Precipitation Highlights December 2007

- * December 2007 was wetter than normal for the contiguous U.S., the 18th wettest December since

national records began in 1895. Thirty-seven states were wetter, or much wetter, than average. Only Texas, Louisiana, Mississippi, and North Dakota were drier than average.

* Precipitation was much above average in Washington state, due to a powerful storm that struck the Pacific Northwest in early December. Heavy rain and wind gusts greater than 100 mph caused widespread damage and the worst flooding in more than a decade in parts of western Oregon and Washington. Many locations received more than 10 inches of rainfall during the first three days of the month.

* While above-average precipitation in late November and December led to improving drought conditions in parts of the Southwest, Southeast, and New England, more than three-fourths of the Southeast and half of the West remained in some stage of drought.

Global Highlights

* For December 2007, the combined global land and ocean surface temperature was the 13th warmest on record (0.72 degrees F or 0.40 degrees C above the 20th century mean). Separately, the global December land-surface temperature was the eighth warmest on record. The most anomalously warm temperatures occurred from Scandinavia to central Asia.

* La Niña continued to strengthen as ocean

surface temperatures in large areas of the central and eastern equatorial Pacific were more than 3 degrees F (1.7 degrees C) below average. The continuation of cooler-than-average temperatures dampened the global ocean average, which was the 18th warmest on record for December.

* For 2007, the global land and ocean surface temperature was the fifth warmest on record. Separately, the global land surface temperature was warmest on record while the global ocean temperature was 9th warmest since records began in 1880. Seven of the eight warmest years on record have occurred since 2001, part of a rise in temperatures of more than 1 degree F (0.6 degrees C) since 1900. Within the past three decades, the rate of warming in global temperatures has been approximately three times greater than the century scale trend.

Note to Editors: Additional information on U.S. climate conditions in December and for 2007 is available online at:

[20]<http://www.ncdc.noaa.gov/oa/climate/research/2007/dec/dec07.html>

and [21]<http://www.ncdc.noaa.gov/oa/climate/research/2007/ann/ann07.html>.

- 30 -

Dr.Richard A. Anthes
Phone: 303-497-1652

President
University Corporation for Atmospheric Research
P.O. Box 3000
Boulder, CO 80307-3000

For delivery via express mail, please use:

1850 Table Mesa Drive
Boulder, CO 80305

--

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Street address: 1850 Table Mesa Drive, Boulder, CO 80305

X-MimeOLE: Produced By Microsoft Exchange V6.5

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Received: from [32]int-ch1gw-3.online-age.net ([33]3.159.232.67) by [34]useclpexw213.nbcuni.ge.com (SonicWALL 6.0.1.9157) with ESMTP; Tue, 15 Jan 2008 14:59:24 -0500

Received: from [35]ext-ch1gw-9.online-age.net (int-ch1gw-3 [[36]3.159.232.67]) by [37]int-ch1gw-3.online-age.net (8.13.6/8.13.6/20050510-SVVS) with ESMTP id m0FJxNgI021683 for <[38]bob.ryan@nbc.com>; Tue, 15 Jan 2008 14:59:23 -0500 (EST)

Received: from [39]mmp2.nems.noaa.gov ([40]mmp2.nems.noaa.gov [[41]140.90.121.157]) by [42]ext-ch1gw-9.online-age.net (8.13.6/8.13.6/20051111-SVVS-TLS-DNSBL) with ESMTP id m0FJxKss007414 for <[43]bob.ryan@nbc.com>; Tue, 15 Jan 2008 14:59:23 -0500

Received: from [44]HCHB-WIRNS.noaa.gov ([45]170.110.255.148) by [46]mmp2.nems.noaa.gov (Sun Java System Messaging Server 6.2-6.01 (built Apr 3 2006)) with ESMTPSA id <[47]0JUP00MVJBIAQ7B0@mmp2.nems.noaa.gov> for [48]bob.ryan@nbc.com; Tue, 15 Jan 2008 14:59:16 -0500 (EST)

Content-class: urn:content-classes:message

Subject: NOAA: 2007 Was Tenth Warmest for U.S., Fifth Warmest Worldwide

Date: Tue, 15 Jan 2008 15:00:00 -0500

Message-ID: <[49]0JUP00MZVBISQ7B0@mmp2.nems.noaa.gov>

X-MS-Has-Attach:

X-MS-TNEF-Correlator:

Thread-Topic: NOAA: 2007 Was Tenth Warmest for U.S., Fifth Warmest Worldwide

Thread-Index: AchXsSO/aYafvboCRgCNpqPHISPHPg==

From: "NOAA News Releases" <[50]Press.Releases@noaa.gov>

To: "Ryan, Bob (NBC Universal)" <[51]Bob.Ryan@nbcuni.com>

TO: Ryan, Bob; WRC-TV

FOR IMMEDIATE RELEASE January 15, 2008

*** NEWS FROM NOAA ***

NATIONAL OCEANIC & ATMOSPHERIC ADMINISTRATION

U. S. DEPARTMENT OF COMMERCE

WASHINGTON, DC

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NOAA: 2007 Was Tenth Warmest for U.S., Fifth Warmest Worldwide

The average temperature for the contiguous U.S. in 2007 is officially the tenth warmest on record, according to data from scientists at NOAA's National Climatic Data Center in

Asheville, N.C. The agency also determined the global surface temperature last year was the fifth warmest on record.

U.S. Temperature Highlights

* The average U.S. temperature for 2007 was 54.2 degrees F; 1.4 degrees F warmer than the 20th century mean of 52.8 degrees F. NCDC originally estimated in mid-December that 2007 would end as the eighth warmest on record, but below-average temperatures in areas of the country last month lowered the annual ranking. For Alaska, 2007 was the 15th warmest year since statewide records began in 1918.

* Six of the 10 warmest years on record for the contiguous U.S. have occurred since 1998, part of a three decade period in which mean temperatures for the contiguous U.S. have risen at a rate near 0.6 degrees F per decade.

* For the contiguous U.S., the December 2007 mean temperature was 33.6 degrees F, near the 20th century average of 33.4 degrees F. The Southeast was much warmer than average, while 11 states, from the Upper Midwest to the West Coast, were cooler than average.

* Warmer-than-average temperatures for December 2007 in large parts of the more heavily populated eastern U.S. resulted in temperature related energy demand about 1.9 percent below average for the nation as a whole, based on NOAA's Residential Energy Demand Temperature Index. For the year, the REDTI estimates that national residential energy consumption was about 2.5 percent below average.

U.S. Precipitation Highlights December 2007

* December 2007 was wetter than normal for the contiguous U.S., the 18th wettest December since national records began in 1895. Thirty-seven states were wetter, or much wetter, than average. Only Texas, Louisiana, Mississippi, and North Dakota were drier than average.

* Precipitation was much above average in Washington state, due to a powerful storm that struck the Pacific Northwest in early December. Heavy rain and wind gusts greater than 100 mph caused widespread damage and the worst flooding in more than a decade in parts of western Oregon and Washington. Many locations received more than 10 inches of rainfall during the first three days of the month.

* While above-average precipitation in late November and December led to improving drought conditions in parts of the Southwest, Southeast, and New England, more than three-fourths of the Southeast and half of the West remained in some stage of drought.

Global Highlights

* For December 2007, the combined global land and ocean surface temperature was the 13th warmest on record (0.72 degrees F or 0.40 degrees C above the 20th century mean). Separately, the global December land-surface temperature was the eighth

warmest on record. The most anomalously warm temperatures occurred from Scandinavia to central Asia.

* La Niña continued to strengthen as ocean surface temperatures in large areas of the central and eastern equatorial Pacific were more than 3 degrees F (1.7 degrees C) below average. The continuation of cooler-than-average temperatures dampened the global ocean average, which was the 18th warmest on record for December.

* For 2007, the global land and ocean surface temperature was the fifth warmest on record. Separately, the global land surface temperature was warmest on record while the global ocean temperature was 9th warmest since records began in 1880. Seven of the eight warmest years on record have occurred since 2001, part of a rise in temperatures of more than 1 degree F (0.6 degrees C) since 1900. Within the past three decades, the rate of warming in global temperatures has been approximately three times greater than the century scale trend.

Note to Editors: Additional information on U.S. climate conditions in December and for 2007 is available online at:

[52]<http://www.ncdc.noaa.gov/oa/climate/research/2007/dec/dec07.html>
and [53]<http://www.ncdc.noaa.gov/oa/climate/research/2007/ann/ann07.html> .

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16. <http://www.nasa.gov/ntv>
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From: Michael Mann <mann@meteo.psu.edu>
To: Jean Jouzel <jean.jouzel@lsce.ipsl.fr>
Subject: Re: [Fwd: EGU 2008]
Date: Mon, 28 Jan 2008 18:12:16 -0500
Reply-to: mann@psu.edu
Cc: Phil Jones <p.jones@uea.ac.uk>

Hi Jean,
no problem, I think Phil and I have it all sorted out. Sorry I won't be there to see you
this time,
mike
Jean Jouzel wrote:

Dear Phil, Dear Mike,

I feel that I come too late in the discussion, but it's really fine for me.

Thanks a lot Jean

At 14:24 +0000 18/01/08, Phil Jones wrote:

Mike,

I didn't read it properly! I see the Jan 25 deadline. I was looking
at a Feb date which is for room and scheduling options.

So I will let you enter the session on Monday. I'll send
something over the weekend or first thing Monday, once I've
been through them. There a number of issues which relate to
last year and who got orals/posters then.

The other thing is for a room for 250+ people. If we have a medallist
we want more. We had 500 last year (due to Ray) but we did keep
most for the next few talks. We still had about ~200 for the session after
Ray's.

Cheers
Phil

At 14:01 18/01/2008, Michael Mann wrote:

Hi Phil,

thanks--sounds fine, I'll let you enter the session then.

I thought they wanted it sooner though (before Jan 25). I'm forwarding that email, maybe
I misunderstood it,
mike

Phil Jones wrote:

Mike,

Have printed out the abstracts. Looks like many reasonable ones. Pity we only have the limited numbers. I can put the session in once we're agreed. It seems as though we can't do that till mod-Feb.

I've contacted Gerrit and Gerard to see if we have to accommodate a medalist talk for the Hans Oeschger prize.

Cheers

Phil

At 13:15 18/01/2008, Michael Mann wrote:

Hi Phil,

thanks, that sounds fine to me. I'll await further word from you after you look this over again, and I'll await feedback from Jean. No rush, I'm hoping to finalize the session on Monday.

The Vinther et al stuff sounds very interesting--I'm looking forward to hearing more, sorry I won't actually be at EGU.

talk to you later,

mike

Phil Jones wrote:

Mike, Jean

Thanks. I'll probably go with Vinther et al for the third invited. Not just as I'm on the author list, but because he'll show (will submit soon) that the Greenland borehole records (Dorthe Dahl Jensen) are winter proxies. Has implications for the Norse Vikings - as the summer isotopes (which unfortunately respond much to Icelandic than SW Greenland temps) don't show any Medieval warming.

Jean probably knew all this. The bottom line is that annual isotopes are essentially winter isotopes as they vary 2-3 times as much as summer ones. If the squeezing of the layers doesn't distort anything this implies longer series are very winter half year dominant.

I mostly agree with the other orals, but I have to look at a few. There is one on the Millennium project (EU funded) which Jean knows about. Might have to give this an oral slot.

Jean - any thoughts? I assume you're happy to chair a session.

I also need to check whether we will have to talk a medallist talk? No idea who?

Cheers

Phil

At 17:05 17/01/2008, Michael Mann wrote:

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X-MIME-Autoconverted: from 8bit to quoted-printable by f05n05.cac.psu.edu id m0HH5gQ6025372

Dear Phil and Jean,

We got an impressive turnout this year for our session, 37 total submitted abstracts. Please see attached word document. Based on the rules described by EGU below, I suggest we have 2 oral sessions (consisting of morning and afternoon), with a total of 10 oral presentations w/ 7 of those being regular 15 minutes slots and 3 of those invited 25 minute slots. The other 27 abstracts will be posters, conforming w/ the fairly harsh limits imposed by EGU on oral presentations.

My suggestions would be as follow:

Invited Presentations (25 minutes):

1 Ammann et al

2 Hughes et al

3 either Emile Geay et al OR Vinther et al OR Crespin et al (preferences?)

Other Oral (15 minutes):

4. 3 other of either Emile Geay et al OR Vinther et al OR Crespin et al

5. 3 other of either Emile Geay et al OR Vinther et al OR Crespin et al

6. Riedwyl et al

7. Graham et al

8. Smerdon et al

9. Kleinen et al

10. Jungklaus et al

Posters:

All others

Please let me know what you think. If these sound good to you, I'll go ahead and arrange the session online,

Mike

----- Original Message -----

Subject: EGU 2008

Date: Thu, 17 Jan 2008 10:03:43 +0100

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Dear convener and co-convener,

Thanks a lot for your effort for successful sessions at the EGU 2008.

>From our experience of the last years, there will be an oral-to-poster ratio of about 1:2 (e.g. ~33% of the contributions can get a talk). This means that for a complete session, you need 18 contributions. $18:3 * 15\text{min} = 1.5\text{h} = 1 \text{ block}$

For those of you who are under the number of 18, there are several options:

- 1) a pure poster session
- 2) merging with a related session
- 3) the contributions will go to the open session (CL0)
- 4) if you are just below 18, you may manage to get late contributions within the next days (please no dummy posters)

Please tell me which option do you like most (email to [116]andrea.bleyer@awi.de).

In case 2), please contact the respective conveners in advance.

The session could be also from other divisions (BG, OS, AS, IS, ..).

In case of merging, you may speak with the persons whether it would be appropriate to modify the title of the new session or to have a combined name with both titles.

I think the general rule is that the convener of the merged session is the person with the bigger session.

Kind regards

Gerrit

Prof. Dr. Gerrit Lohmann

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130. <mailto:jzipsl@ipsl.jussieu.fr>
131. <mailto:jean.jouzel@lsce.ipsl.fr>
132. <mailto:mann@psu.edu>
133. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Caspar Ammann <ammann@ucar.edu>
To: P.Jones@uea.ac.uk
Subject: Re: pdf
Date: Wed, 30 Jan 2008 15:18:51 -0700

Phil,

will do. And regarding TSI, it looks like that 1361 or 1362 (+/-) are going to be the new consensus. All I hear is that this seems to be quite robust. Fodder for the critics: all these modelers, they always put in too much energy - no wonder it was warming - and now they want to reduce the natural component? The SORCE meeting is going to be on that satellite stuff but also about climate connections : Sun-Earth. Tom Crowley is going to be there, Gavin Schmidt, David Rind, and a few others; of course Judith.

Thanks for Bo Vinther's manuscript!

Caspar

On Jan 30, 2008, at 3:12 PM, [1]P.Jones@uea.ac.uk wrote:

Caspar,

OK. Keep me informed. Also I'd like to know more the conclusions of the meeting you're going to on the solar constant.

Just that it can change from 1366.5 to 1361!!

Cheers

Phil

Phil,

we should hook together on this 1257 event (I call it 1257 because of the timings but its just a bit better than an informed guess). We now have these simulations of contemporary high-lat eruptions and can

compare them with low-lat ones.

Just a couple thoughts

pro high-lat:

- climate signal looks better in short and longer term
- potential for in-ice-core migration of some sulfur species ... some

new work that has been done ...

con:

- deposition duration
- old fingerprints
- no high-lat calderas/flows of appropriate size : compare it to

Eldgja or Laki, this thing is bigger!

- no large ash layers

What we need is fingerprinting. I'm participating in a project

Icelandic volcanism and climate in the last 2000 years. There we have

money to do some chemical fingerprinting. I'm pursuing to get

somebody to run these samples. That will be the deciding thing.

Remember, instrumentation has dramatically increased in sensitivity,

so I think it should be possible. its not that one would have to go

dig around too much in the ice cores as the depth/location of that

monster sulfate spikes are well known.

Should be interesting.

Caspar

On Jan 30, 2008, at 2:57 PM, [2]P.Jones@uea.ac.uk wrote:

Caspar,

The meeting I'm at is less interesting than IDAG.

I'll send the Greenland isotope data when I get back.

536 is a good story. 1258/9 needs to be good story too...

I think it isn't at the moment.

Cheers

Phil

Thanks Phil,

will have a look. I certainly like it, and I only was a bit picky on

the "largest eruption" versus "largest volcanic signal in trees". I

like the isotope work very much and will now look if I can pick on

something more substantial ;-)

Caspar

On Jan 30, 2008, at 1:24 PM, [3]P.Jones@uea.ac.uk wrote:

<2007GL032450.pdf>

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References

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From: J Shukla <shukla@cola.iges.org>
To: IPCC-Sec <IPCC-Sec@wmo.int>
Subject: Future of the IPCC:
Date: Wed, 13 Feb 2008 16:46:33 -0500
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Dear All,

I would like to respond to some of the items in the attached text on issues etc. in particular to the statement in the section 3.1.1 (sections 3: Drivers of required change in the future).

"There is now greater demand for a higher level of policy relevance in the work of IPCC, which could provide policymakers a robust scientific basis for action".

1. While it is true that a vast majority of the public and the policymakers have accepted the reality of human influence on climate change (in fact many of us were arguing for stronger language with a higher level of confidence at the last meetings of the LAs), how confident are we about the projected regional climate changes?

I would like to submit that the current climate models have such large errors in simulating the statistics of regional (climate) that we are not ready to provide policymakers a robust scientific basis for "action" at regional scale. I am not referring to mitigation, I am strictly referring to science based adaptation.

For example, we can not advise the policymakers about re-building the city of New Orleans - or more generally about the habitability of the Gulf-Coast - using climate models which have serious deficiencies in simulating the strength, frequency and tracks of hurricanes.

We will serve society better by enhancing our efforts on improving our models so that they can simulate the statistics of regional climate fluctuations; for example: tropical (monsoon depressions, easterly waves, hurricanes, typhoons, Madden-Julian oscillations) and extratropical (storms, blocking) systems in the atmosphere; tropical instability waves, energetic eddies, upwelling zones in the oceans; floods and droughts on the land; and various manifestations (ENSO, monsoons, decadal variations, etc.) of the coupled ocean-land-atmosphere processes.

It is inconceivable that policymakers will be willing to make billion-and trillion-dollar decisions for adaptation to the projected regional climate change based on models that do not even describe and simulate the processes that are the building blocks of climate variability. Of course, even a hypothetical, perfect model does not guarantee accurate prediction of the future regional climate, but at the very least, our suggestion for action will be based on the best possible

science.

It is urgently required that the climate modeling community arrive at a consensus on the required accuracy of the climate models to meet the "greater demand for a higher level of policy relevance".

2. Is "model democracy" a valid scientific method? The "I" in the IPCC desires that all models submitted by all governments be considered equally probable. This should be thoroughly discussed, because it may have serious implications for regional adaptation strategies. AR4 has shown that model fidelity and model sensitivity are related. The models used for IPCC assessments should be evaluated using a consensus metric.

3. Does dynamical downscaling for regional climate change provide a robust scientific basis for action?

Is there a consensus in the climate modeling community on the validity of regional climate prediction by dynamical downscaling? A large number of dynamical downscaling efforts are underway worldwide. This is not necessarily because it is meaningful to do it, but simply because it is possible to do it. It is not without precedent that quite deficient climate models are used by large communities simply because it is convenient to use them. It is self-evident that if a coarse resolution IPCC model does not correctly capture the large-scale mean and transient response, a high-resolution regional model, forced by the lateral boundary conditions from the coarse model, can not improve the response. Considering the important role of multi-scale interactions and feedbacks in the climate system, it is essential that the IPCC-class global models themselves be run at sufficiently high resolution.

Regards,
Shukla

IPCC-Sec wrote:
> Dear LAs & CLAs,
>
> Please find attached a letter and issues related to the future of the
> IPCC.
>
> With kind regards,
>
> Annie
>
> IPCC Secretariat
> WMO
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> Fax: +41 22 730 8025/8013
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From: David Thompson <davet@atmos.colostate.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Your ENSO series
Date: Thu, 21 Feb 2008 14:07:14 +0000

Phil,

If it works, let's plan on me visiting for the day April 30 (I'll come out April 29; leave May 1). I'll put the date on my calendar and assume it works unless I hear otherwise. If there is a better day that week, please let me know.

Thanks,

Dave

Dave,

Will send on your details to the seminar organizer here. The week of April 28 - May 2 is OK for me. I hope this is what you meant by last week.

A few thoughts on the plots.

1. There isn't a drop off in land data around 1945 - nor during WW2. So this is different from the ocean data. Most series are complete or have been slightly infilled during the period in Europe. Berlin for example only missed one day's T obs in April 45.
2. Fuego could be underestimated.
3. It could also be that sulphate emissions were very high at this time - late 60s, early 70s.

I'll await the text !

Cheers

Phil

At 16:18 19/02/2008, you wrote:

Hi Phil,

I'd enjoy visiting.... how does the first or last week of April look to you?

As for some new results:

I've attached two figures. Both focus on the land data.

The first figure includes 4 time series. From top to bottom: the global-mean land data (CRUTEM 3); the ENSO fit; the COWL fit; the residual global-mean time series. There is nothing here you haven't seen before - the residual land time series is identical to the one in the Nature paper.

As we've discussed, the residual land time series highlights the signature of the volcanos. And as far as low frequency variability goes: the residual land time series supports the IPCC contention that the global warmed from ~1900-1940; did not warm from ~1940-1980; and warmed substantially from 1980 to present.

OK.... so now I'm going to play with removing the volcanic signal. There are a lot of ways to do this, and I haven't settled on the best method. For now, I am driving the simple climate model I've been using for ENSO with the Ammann et al. volcanic forcing time series. I get identical results using Crowley's estimate and Sato's estimate. The figure on page 2 shows the effect of removing the volcanic signal. From top to bottom: the the global-mean residual land time series (repeated from the previous figure); the volcanic fit; the 'ENSO/COWL/Volcano' residual land time series.

Some key points:

1. the volcanic fit isn't perfect, but captures most of the volcanic signal.
2. the residual time series (bottom of Fig 2) is interesting. If you look closely, it suggests the globe has warmed continuously since 1900 with two exceptions: a 'bite' in the 1970s, and a downwards 'step' in 1945. The step in 1945 is not as dramatic as the step in the ocean data. But it's there. (I'm guessing the corresponding change in variance is due to a sudden increase in data coverage).
3. the volcanic fit highlights the fact that the lack of warming in the middle part of the century comes from only two features: the step in 45 and Agung. When Agung is removed, land temperatures march upwards from 1945-1970 (Fig 2 bottom).
4. the bite in the 1970s could be due to an underestimate of the impact of Fuego (the bite is also evident in the SST data).

What do you think? The step in 1945 is not as dramatic as the step in the SST data. But it's certainly there. It's evident in the COWL/ENSO residual time series (top of Fig 2): removing Agung simply clarifies that without the step temperatures marched steadily upwards from 1900-1970.

-Dave

i;¼

On Feb 19, 2008, at 1:28 PM, Phil Jones wrote:

Dave,

Thanks.

Before seeing what you send, I think I'll find it harder to believe something is wrong with the land data. I can be convinced though....

So you're in Reading now. Do you still want to come up to

distant Norwich

at some point and also give a talk?

Cheers

Phil

At 16:55 18/02/2008, you wrote:

Phil,

I'm really sorry for the delay; my family and I have been in transit from the US to the UK this past week, and it's taken a bit for us to get settled.

I've attached the ENSO index I've been using. The first month is Jan 1850; the last is Dec 2006. The time series has a silly number of sig figures - that's just how Matlab wanted to save it.

The data are in K and are scaled as per the fit to the global-mean (as in the paper).

I've got some new results regarding the land data... I'll think you'll find them interesting. I'll pass them along in the next day or so... the main point is that I suspect the land data might also have some spurious cooling in the middle part of the century. More to come....

-Dave

Ã- Â¿Â¼

On Feb 14, 2008, at 12:35 PM, Phil Jones wrote:

David,

For a presentation I'm due to make in a few months, can you send me the ENSO and the COWL series that are in Figure 1 in the paper.

I'm not sure what I will do with COWL, but I want to compare your ENSO

with some of the ENSO-type indices I have.

These seem monthly from about the 1860s or maybe earlier.

Cheers

Phil

At 16:49 07/02/2008, you wrote:

So it made it past the first hurdle, which is good. My hunch is that the paper will fare OK in review, but you never know with Nature. And it's possible a reviewer will insist on our providing a correction... anyway, we'll see...

-Dave

Begin forwarded message:

From: [1]j.thorpe@nature.com
Date: February 7, 2008 3:44:07 AM PST
To: [2]davet@atmos.colostate.edu
Subject: Nature 2008-01-00939 out to review

Dear Professor Thompson,

Thank you for submitting your manuscript entitled "A discontinuity in the time series of global-mean surface temperature" to Nature. I am pleased to tell you that we are sending your paper out for review.

We will be in touch again as soon as we have received comments from our reviewers.

Yours sincerely

Nichola O'Brien

Staff

Nature

For Dr. Joanna Thorpe

Associate Editor, Nature

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This email has been sent through the NPG Manuscript Tracking System NY-610A-NPG&MTS

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-Dave

Begin forwarded message:

From: [12]j.thorpe@nature.com

Date: February 7, 2008 3:44:07 AM PST

To: [13]davet@atmos.colostate.edu

Subject: Nature 2008-01-00939 out to review

Dear Professor Thompson,

Thank you for submitting your manuscript entitled "A discontinuity in the time series of global-mean surface temperature" to Nature. I am pleased to tell you that we are sending your paper out for review.

We will be in touch again as soon as we have received comments from our reviewers.

Yours sincerely

Nichola O'Brien

Staff

Nature

For Dr. Joanna Thorpe

Associate Editor, Nature

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Hi Phil,

I'd enjoy visiting.... how does the first or last week of April look to you?

As for some new results:

I've attached two figures. Both focus on the land data.

The first figure includes 4 time series. From top to bottom: the global-mean land data (CRUTEM 3); the ENSO fit; the COWL fit; the residual global-mean time series. There is nothing here you haven't seen before - the residual land time series is identical to the one in the Nature paper.

As we've discussed, the residual land time series highlights the signature of the volcanos. And as far as low frequency variability goes: the residual land time series supports the IPCC contention that the global warmed from ~1900-1940; did not warm from ~1940-1980; and warmed substantially from 1980 to present.

OK.... so now I'm going to play with removing the volcanic signal. There are a lot of ways to do this, and I haven't settled on the best method. For now, I am driving the simple climate model I've been using for ENSO with the Ammann et al. volcanic forcing time series. I get identical results using Crowley's estimate and Sato's estimate.

The figure on page 2 shows the effect of removing the volcanic signal. From top to bottom: the the global-mean residual land time series (repeated from the previous figure); the volcanic fit; the 'ENSO/COWL/Volcano' residual land time series.

Some key points:

1. the volcanic fit isn't perfect, but captures most of the volcanic signal.
2. the residual time series (bottom of Fig 2) is interesting. If you look closely, it suggests the globe has warmed continuously since 1900 with two exceptions: a 'bite' in the 1970s, and a downwards 'step' in 1945. The step in 1945 is not as dramatic as the step in the ocean data. But it's there. (I'm guessing the corresponding change in variance is due to a sudden increase in data coverage).
3. the volcanic fit highlights the fact that the lack of warming in the middle part of the century comes from only two features: the step in 45 and Agung. When Agung is removed, land temperatures march upwards from 1945-1970 (Fig 2 bottom).
4. the bite in the 1970s could be due to an underestimate of the impact of Fuego (the bite is also evident in the SST data).

What do you think? The step in 1945 is not as dramatic as the step in the SST data. But it's certainly there. It's evident in the COWL/ENSO residual time series (top of Fig 2): removing Agung simply clarifies that without the step temperatures marched steadily upwards from 1900-1970.

-Dave

On Feb 19, 2008, at 1:28 PM, Phil Jones wrote:

Dave,

Thanks.

Before seeing what you send, I think I'll find it harder to believe something is wrong with the land data. I can be convinced though....

So you're in Reading now. Do you still want to come up to distant Norwich at some point and also give a talk?

Cheers

Phil

At 16:55 18/02/2008, you wrote:

Phil,

I'm really sorry for the delay; my family and I have been in transit from the US to the UK this past week, and it's taken a bit for us to get settled.

I've attached the ENSO index I've been using. The first month is Jan 1850; the last is Dec 2006. The time series has a silly number of sig figures - that's just how Matlab wanted to save it.

The data are in K and are scaled as per the fit to the global-mean (as in the paper).

I've got some new results regarding the land data... I'll think you'll find them interesting. I'll pass them along in the next day or so... the main point is that I suspect the land data might also have some spurious cooling in the middle part of the century. More to come....

-Dave

ï¼¼

On Feb 14, 2008, at 12:35 PM, Phil Jones wrote:

David,

For a presentation I'm due to make in a few months, can you send me the ENSO and the COWL series that are in Figure 1 in the paper.

I'm not sure what I will do with COWL, but I want to compare your ENSO

with some of the ENSO-type indices I have.

These seem monthly from about the 1860s or maybe earlier.

Cheers

Phil

At 16:49 07/02/2008, you wrote:

So it made it past the first hurdle, which is good. My hunch is that the paper will fare OK in review, but you never know with Nature. And it's possible a reviewer will insist on our providing a correction... anyway, we'll see...

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References

1. <mailto:j.thorpe@nature.com>
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3. <http://www.nature.com/nature>
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5. <mailto:nature@naturedc.com>
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From: Ben Santer <santer1@llnl.gov>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Coverage
Date: Thu, 21 Feb 2008 17:12:22 -0800
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

A quick question: Do you happen to have a "percentage land coverage mask" for the HadCRUT3v data? And if so, does this exist as a netCDF file?

With best regards,

Ben
Phil Jones wrote:

>
> Ben,
> Email to Dick reminded me ! Had another phone call and I'd forgotten.
> First file is the coverage.
>
> Second is a program that reads this file - Channel 1.
>
> File is 36 by 72. 5 by 5 degs.
>
> It will start at 85-90N for the 36 subscript.
>
> for 72 it is either dateline or Greenwich.
>
> Cheers
> Phil

> At 16:53 15/02/2008, you wrote:

>> Dear Dick,

>>
>> I'm forwarding an email that I sent out several days ago. For the last
>> month, I've been working hard to respond to a recent paper by David
>> Douglass, John Christy, Benjamin Pearson, and Fred Singer. The paper
>> claims that the conclusions of our CCSP Report were incorrect, and
>> that there is a fundamental discrepancy between simulated and observed
>> temperature changes in the tropical troposphere. Douglass et al. also

>> assert that models cannot represent the "observed" differential
>> warming of the surface and troposphere. To address these claims, I've
>> been updating some of the comparisons of models and observations that
>> we did for the CCSP Report, now using newer observational datasets
>> (among them NOAA ERSST-v2 and v3). As you can see from the forwarded
>> email, the warming rates of tropical SSTs are somewhat different for
>> ERSST-v2 and v3 - ERSST-v3 warms by less than v2. Do you understand
>> why this is?

>>
>> With best regards, and hope you are well!

>>
>> Ben

>> -----

>>
>> Benjamin D. Santer
>> Program for Climate Model Diagnosis and Intercomparison
>> Lawrence Livermore National Laboratory
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>> -----

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>>
>> X-Account-Key: account1
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>> by mail-2.llnl.gov (Cyrus v2.2.12) with LMTPA;
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>> by mail-2.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.6 \$) with
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>> by smtp.llnl.gov with ESMTP; 13 Feb 2008 18:34:51 -0800
>> Message-ID: <47B3A8CB.90605@llnl.gov>
>> Date: Wed, 13 Feb 2008 18:34:51 -0800

>> From: Ben Santer <santer1@llnl.gov>
>> Reply-To: santer1@llnl.gov
>> Organization: LLNL
>> User-Agent: Thunderbird 1.5.0.12 (X11/20070529)
>> MIME-Version: 1.0
>> To: santer1@llnl.gov, Peter Thorne <peter.thorne@metoffice.gov.uk>,
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>> Gavin Schmidt <gschmidt@giss.nasa.gov>,
>> "Hack, James J." <jhack@ornl.gov>, peter gleckler
>> <gleckler1@llnl.gov>
>> Subject: Additional calculations
>> References: <200801121320.26705.John.Lanzante@noaa.gov>
>> <478C528C.8010606@llnl.gov> <p06230904c3b2e6b2c92f@[172.17.135.52]>
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>> <1200995209.23799.95.camel@eld443.desktop.frd.metoffice.com>
>> <47962FD1.1020303@llnl.gov>
>> In-Reply-To: <47962FD1.1020303@llnl.gov>
>> Content-Type: multipart/mixed;
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>>
>> Dear folks,
>>
>> Sorry about the delay in sending you the next version of our
>> manuscript. I decided that I needed to perform some additional
>> calculations. I was concerned that we had not addressed the issue of

>> "differential warming" of the surface and troposphere - an issue which
>> Douglass et al. HAD considered.

>>

>> Our work thus far shows that there are no fundamental inconsistencies
>> between simulated and observed temperature trends in individual
>> tropospheric layers (T2 and T2LT). But we had not performed our
>> "paired trends" test for trends in the surface-minus-T2LT difference
>> time series. This is a much tougher test to pass: differencing
>> strongly damps the correlated variability in each "pair" of surface
>> and T2LT time series. Because of this noise reduction, the standard
>> error of the linear trend in the difference series is typically
>> substantially smaller than the size of the standard error in an
>> individual surface or T2LT time series. This makes it easier to reject
>> the null hypothesis of "no significant difference between simulated
>> and observed trends".

>>

>> In the CCSP Report, the behavior of the trends in the
>> surface-minus-T2LT difference series led us to note that:

>>

>> "Comparing trend differences between the surface and the troposphere
>> exposes potential discrepancies between models and observations in the
>> tropics".

>>

>> So it seemed wise to re-examine this "differential warming" issue. I
>> felt that if we ignored it, Douglass et al. would have grounds for
>> criticizing our response.

>>

>> I've now done the "paired trends" test with the trends in the
>> surface-minus-T2LT difference series. The results are quite
>> interesting. They are at variance with the above-quoted finding of the
>> CCSP Report. The new results I will describe show that the "potential
>> discrepancies" in the tropics have largely been resolved.

>>

>> Here's what I did. I used three different observational estimates of
>> tropical SST changes. These were from NOAA-ERSST-v2, NOAA-ERSST-v3,
>> and HadISST1. It's my understanding that NOAA-ERSST-v3 and HadISST1
>> are the most recent SST products of NCDC and the Hadley Centre. I'm
>> also using T2LT data from RSS v3.0 and UAH v5.2. Here are the tropical
>> (20N-20S) trends in these five datasets over the 252-month period from
>> January 1979 to December 1999, together with their 1-sigma adjusted
>> standard errors (in brackets):

>>

>> UAH v5.2 0.060 (+/-0.137)

>> RSS v3.0 0.166 (+/-0.130)

>> HADISST1 0.108 (+/-0.133)

>> NOAA-ERSST-v2 0.100 (+/-0.131)

>> NOAA-ERSST-v3 0.077 (+/-0.121)

>>

>> (all trends in degrees C/decade).

>>

>> The trends in the three SST datasets are (by definition) calculated from anomaly data that have been spatially-averaged over tropical oceans. The trends in T2LT are calculated from anomaly data that have been spatially averaged over land and ocean. It is physically reasonable to do the differencing over different domains, since the temperature field throughout the tropical troposphere is more or less on the moist adiabatic lapse rate set by convection over the warmest waters.

>>

>> These observational trend estimates are somewhat different from those available to us at the time of the CCSP Report. This holds for both T2LT and SST. For T2LT, the RSS trend used in the CCSP Report and in the Santer et al. (2005) Science paper was roughly 0.13 degrees C/decade. As you can see from the Table given above, it is now ca. 0.17 degrees C/decade. Carl tells me that this change is largely due to a change in how he and Frank adjust for inter-satellite biases. This adjustment now has a latitudinal dependence, which it did not have previously.

>>

>> The tropical SST trends used in the CCSP Report were estimated from earlier versions of the Hadley Centre and NOAA SST data, and were of order 0.12 degrees C/decade. The values estimated from more recent datasets are lower - and markedly lower in the case of NOAA-ERSST-v3 (0.077 degrees C/decade). The reasons for this downward shift in the estimated warming of tropical SSTs are unclear. As Carl pointed out in an email that he sent me earlier today:

>>

>> "One important difference is that post 1985, NOAA-ERSST-v3 directly ingests "bias adjusted" SST data from AVHRR, a big change from v2, which didn't use any satellite data (directly). AVHRR is strongly affected in the tropics by the Pinatubo eruption in 1991. If the "bias adjustment" doesn't completely account for this, the trends could be changed".

>>

>> Another possibility is treatment of biases in the buoy data. It would be nice if Dick Reynolds could advise us as to the most likely

>> explanation for the different warming rates inferred from

>> NOAA-ERSST-v2 and v3.

>>

>> Bottom line: The most recent estimates of tropical SST changes over
>> 1979 to 1999 are smaller than we reported in the CCSP Report, while
>> the T2LT trend (at least in RSS) is larger. The trend in the observed
>> difference series, NOAA-ERSST-v3 Ts minus RSS T2LT, is now -0.089
>> degrees C/decade, which is very good agreement with the multi-model
>> ensemble trend in the Ts minus T2LT difference series (-0.085 degrees
>> C/decade). Ironically, if Douglass et al. had applied their flawed
>> "consistency test" to the multi-model ensemble mean trend and the
>> trend in the NOAA-ERSST-v3 Ts minus RSS T2LT difference series, they
>> would not have been able to conclude that models and observations are
>> inconsistent!

>>

>> Here are the observed trends in the tropical Ts minus T2LT difference
>> series in the six different pairs of Ts and T2LT datasets, together
>> with the number of "Hits" (rejections of the null hypothesis of no
>> significant difference in trends) and the percentage rejection rate
>> (based on 49 tests in each case)

>>

| >> "Pair" | Trend | 1-sigma C.I. | Hits | Rej.Rate |
|------------------------------------|---------|--------------|------|----------|
| >> HadISST1 Ts minus RSS T2LT | -0.0577 | (+/-0.0347) | 1 | (2.04%) |
| >> NOAA-ERSST-v2 Ts minus RSS T2LT | -0.0660 | (+/-0.0382) | 1 | (2.04%) |
| >> NOAA-ERSST-v3 Ts minus RSS T2LT | -0.0890 | (+/-0.0350) | 0 | (0.00%) |
| >> HadISST1 Ts minus UAH T2LT | +0.0488 | (+/-0.0371) | 28 | (57.14%) |
| >> NOAA-ERSST-v2 Ts minus UAH T2LT | +0.0405 | (+/-0.0403) | 25 | (51.02%) |
| >> NOAA-ERSST-v3 Ts minus UAH T2LT | +0.0175 | (+/-0.0370) | 15 | (30.60%) |
| >> Multi-model ensemble mean | -0.0846 | | | |

>>

>> Things to note:

>>

>> 1) For all "pairs" involving RSS T2LT data, the multi-model ensemble
>> mean trend is well within even the 1-sigma statistical uncertainty of
>> the observed trend.

>>

>> 2) For all "pairs" involving RSS T2LT data, there are very few
>> statistically-significant differences between the observed and
>> model-simulated "differential warming" of the tropical surface and
>> lower troposphere.

>>

>> 3) For all "pairs" involving UAH T2LT data, there are
>> statistically-significant differences between the observed and

>> model-simulated "differential warming" of the tropical surface and
>> lower troposphere. Even in these cases, however, rejection of the null
>> hypothesis is not universal: rejection rates range from 30% to 57%.
>> Clearly, not all models are inconsistent with the observational
>> estimate of "differential warming" inferred from UAH data.
>>
>> These results contradict the "model inconsistent with data" claims of
>> Douglass et al.
>>
>> The attached Figure is analogous to the Figure we currently show in
>> the paper for T2LT trends. Now, however, results are for trends in the
>> surface-minus-T2LT difference series. Rather than showing all six
>> "pairs" of observational results in the top panel, I've chosen to show
>> two pairs only in order to avoid unnecessarily complicating the
>> Figure. I propose, however, that we provide results from all six pairs
>> in a Table.
>>
>> As is visually obvious from the Figure, trends in 46 of the 49
>> simulated surface-minus-T2LT difference series pairs are within the
>> 2-sigma confidence intervals of the NOAA-ERSST-v3 Ts minus RSS T2LT
>> trend (the light grey bar). And as is obvious from Panel B, even the
>> Douglass et al. " $\sigma\{SE\}$ " encompasses the difference series trend
>> from the NOAA-ERSST-v3 Ts/RSS T2LT pair.
>>
>> I think we should show these results in our paper.
>>
>> The bottom line: Use of newer T2LT datasets (RSS) and Ts datasets
>> (NOAA-ERSST-v3, HADISST1) largely removes the discrepancy between
>> tropical surface and tropospheric warming rates. We need to explain
>> why the observational estimates of tropical SST changes are now
>> smaller than they were at the time of the CCSP Report. We will need
>> some help from Dick Reynolds with this.
>>
>> With best regards,
>>
>> Ben
>> -----
>>
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>> -----

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>>
>

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>
>
>
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>
> program growlandmergeetc
> dimension lnd(72,36),nlnd(72,36),ivsst(72,36),jcov(72,36)
> dimension icmb(72,36),alcov(72,36),ascov(72,36),iysst(72,36)
> dimension isdvar(72,36,12),neigsd(72,36,12)
> dimension iorigt(72,36),icount(72,36)
> dimension ash(12),anh(12),ashp(12),anhp(12)
> dimension np(12),npch(12),npinf(12),npchan(12),npsst(12)
> rad=57.2958
> ir=13
> c calculate maximum % coverage of hemisphere in cos units
> xnh=0.0
> do 20 j=1,18
> w=cos((92.5-j*5)/rad)
> do 19 i=1,72
> 19 xnh=xnh+w
> 20 continue
> c read in land fraction in %
> read(1,21)i1,i2
> 21 format(2i6)
> do 22 j=1,36
> 22 read(1,23)(jcov(i,j),i=1,72)
> 23 format(72i6)

```

> c set coverage of land to % of at least 25% and less than 75%
> c ocean percent is then simply the rest
>   do 24 j=1,36
>   do 24 i=1,72
>   alcov(i,j)=0.01*jcov(i,j)
>   if(alcov(i,j).le.24.9)alcov(i,j)=25.0
>   if(alcov(i,j).ge.75.1)alcov(i,j)=75.0
>   ascov(i,j)=100.0 - alcov(i,j)
> 24 continue
> c read in the sd of the land only dataset (var corected) to assess
> c whether the neighbour check can legitimately correct values
>   do 901 k=1,12
>   read(4,27)ii
>   do 902 j=1,36
> 902 read(4,29)(isdvar(i,j,k),i=37,72),(isdvar(ii,j,k),ii=1,36)
> 901 continue
> c read in neighbouring sd calculated from at least 4 of the
> c neighbouring 8 5 degree squares around each grid box
>   do 903 k=1,12
>   read(18,27)ii
>   do 904 j=1,36
> 904 read(18,29)(neigsd(i,j,k),i=37,72),(neigsd(ii,j,k),ii=1,36)
> 903 continue
> c skip the first 19 years of the variance corrected land data
> c as the variance corrected SST data only starts in
> c also skip the first 19 years of the original gridded temps
> c so later can check the number of stations available per gridbox
> c per month
>   do 25 k=1851,1869
>   do 26 kk=1,12
>   read(2,27)i1,i2
> 27 format(2i5)
>   read(ir,27)i1,i2
>   do 28 j=1,36
> 28 read(2,29)(lnd(i,j),i=37,72),(lnd(ii,j),ii=1,36)
> 29 format(12i5)
>   do 128 j=1,36
> 128 read(ir,29)(iorigt(i,j),i=37,72),(iorigt(ii,j),ii=1,36)
>   do 129 j=1,36
> 129 read(ir,29)(icount(i,j),i=37,72),(icount(ii,j),ii=1,36)
> 26 continue
> 25 continue
> c read in the land and sst data (both variance corrected)

```

```

> c  reading in the land allow for the greenwich start of the land
> c  and the dateline start for the SST.  Output is from the dateline
>   do 31 k=1870,1999
>   ashy=0.0
>   anhy=0.0
>   if(k.ge.1901)ir=14
>   if(k.ge.1951)ir=15
>   if(k.ge.1991)ir=16
>   if(k.ge.1994)ir=17
>   do 32 kk=1,12
>   npch(kk)=0
>   npchan(kk)=0
>   np(kk)=0
>   npinf(kk)=0
>   npsst(kk)=0
> c  read in the original gridded land to get the station count
> c  per grid box
>   read(ir,27)i1,i2
>   do 131 j=1,36
> 131 read(ir,29)(iorigt(i,j),i=37,72),(iorigt(ii,j),ii=1,36)
>   do 132 j=1,36
> 132 read(ir,29)(icount(i,j),i=37,72),(icount(ii,j),ii=1,36)
> c  read in the variance corrected land
>   read(2,27)i1,i2
>   write(7,27)kk,k
>   do 33 j=1,36
> 33 read(2,29)(lnd(i,j),i=37,72),(lnd(ii,j),ii=1,36)
> c  copy lnd array to nlnd so that the growing doesn't use already
> c  infilled values
>   do 34 j=1,36
>   do 34 i=1,72
> 34 nlnd(i,j)=lnd(i,j)
> c  read in sst data
>   read(3,21)i1,i2
>   do 35 j=1,36
> 35 read(3,23)(ivsst(i,j),i=1,72)
> c  check land for extremes and fill in gaps (only one grid box away
> c  provided there are at least 4 of the 8 surrounding boxes)
>   do 41 j=1,36
>   j1=j-1
>   j2=j+1
>   if(j1.eq.0)j1=1
>   if(j2.eq.37)j2=36

```

```

> do 42 i=1,72
> sum=0.0
> nsum=0
> i1=i-1
> i2=i+1
> do 43 jj=j1,j2
> do 44 ii=i1,i2
> iii=ii
> if(iii.eq.73)iii=1
> if(iii.eq.0)iii=72
> if(jj.eq.j.and.iii.eq.i)go to 44
> if(lnd(iii,jj).eq.-9999)go to 44
> sum=sum+lnd(iii,jj)
> nsum=nsum+1
> 44 continue
> 43 continue
> if(lnd(i,j).ne.-9999)np(kk)=np(kk)+1
> if(nsum.le.3)go to 47
> sum=sum/nsum
> ndep=sum+0.5
> if(sum.lt.0.0)ndep=ndep-1
> nval=ndep
> if(lnd(i,j).eq.-9999)go to 46
> npch(kk)=npch(kk)+1
> ndep=lnd(i,j)-nval
> if(neigsd(i,j,kk).eq.-9999)go to 47
> if(iabs(ndep).le.225)go to 47
> if(iabs(ndep).lt.neigsd(i,j,kk)*2.0)go to 47
> if(icount(i,j).ge.2)go to 47
> nlnd(i,j)=nval
> npchan(kk)=npchan(kk)+1
> 48 write(6,202)k,kk,j,i,nval,lnd(i,j),ndep,isdvar(i,j,kk),
> >neigsd(i,j,kk),nlnd(i,j),nsum,icount(i,j),iorigt(i,j)
> 202 format(4i4,9i6)
> go to 47
> 46 nlnd(i,j)=nval
> npinf(kk)=npinf(kk)+1
> 47 continue
> 42 continue
> 41 continue
> c merge with marine using the weighting factors
> do 51 j=1,36
> do 52 i=1,72

```

```

> wx=0.0
> xx=0.0
> if(nlnd(i,j).eq.-9999)go to 55
> wx=wx+alcov(i,j)
> xx=xx+alcov(i,j)*nlnd(i,j)
> 55 if(ivsst(i,j).eq.-32768)go to 56
> wx=wx+ascov(i,j)
> xx=xx+ascov(i,j)*ivsst(i,j)
> 56 if(wx.ge.0.001)go to 59
> icmb(i,j)=-9999
> go to 57
> 59 aa=xx/wx
> ia=aa+0.5
> if(xx.lt.0.0)ia=ia-1
> icmb(i,j)=ia
> c writing out the land/sst merging checking when both are present
> c   if(wx.ge.99.9)write(6,203)kk,j,i,ia,nlnd(i,j),ivsst(i,j),
> c   >wx,alcov(i,j),ascov(i,j)
> c 203 format(6i6,3f7.1)
> 57 continue
> 52 continue
> 51 continue
> c write out the new merged file
>   do 53 j=1,36
> 53 write(7,54)(icmb(i,j),i=1,72)
> 54 format(12i5)
> c calculate the hemispheric averages
>   anh(kk)=0.0
>   ash(kk)=0.0
>   ashp(kk)=0.0
>   anhp(kk)=0.0
>   wx=0.0
>   xx=0.0
>   do 61 j=1,18
>   w=cos((92.5-j*5.0)/rad)
>   do 62 i=1,72
>   if(icmb(i,j).eq.-9999)go to 62
>   wx=wx+w
>   xx=xx+w*icmb(i,j)
> 62 continue
> 61 continue
>   anh(kk)=xx*0.01/wx
>   anhp(kk)=wx*100.0/xnh

```

```
> wx=0.0
> xx=0.0
> do 63 j=19,36
> w=cos((j*5.0-92.5)/rad)
> do 64 i=1,72
> if(icmb(i,j).eq.-9999)go to 64
> wx=wx+w
> xx=xx+w*icmb(i,j)
> 64 continue
> 63 continue
> ash(kk)=xx*0.01/wx
> ashp(kk)=wx*100.0/xnh
> anhy=anhy+anh(kk)
> ashy=ashy+ash(kk)
> 32 continue
> anhy=anhy/12.0
> ashy=ashy/12.0
> write(8,89)k,anh,anhy
> 89 format(i4,12f6.2,f7.2)
> write(8,90)k,anhp
> 90 format(i4,12f6.0)
> write(9,89)k,ash,ashy
> write(9,90)k,ashp
> write(10,91)k,np
> write(10,91)k,npch
> write(10,91)k,npchan
> write(10,91)k,npinf
> write(10,92)
> 92 format(/)
> 91 format(i4,12i6)
> 31 continue
> stop
> end
>
>
```

--

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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Yan Zhongwei" <yzw@mail.tea.ac.cn>
Subject: Re: Adjusting Beijing temperature series
Date: Fri Feb 22 10:14:36 2008

Zhongwei,

Will read soon !

Attached is what I finally submitted to JGR.

Don't pass on to anyone else.

I have also received a paper from Li, Q, but have yet to read that. He only sent it yesterday.

Cheers

Phil

At 09:55 22/02/2008, you wrote:

Hi, Phil,

Attached please find a draft paper about site-changes and urbanization at Beijing. It may be regarded as an extension of our early work (Yan et al 2001 AAS) and therefore I would be happy to ask you to join as a co-author.

Regarding your recent paper about UHI effect in China (no doubt upon a large-scale warming in the region), I hope the Beijing case may serve as a helpful rather than a contradictory (as it may appear so) reference.

The urbanization-bias at BJ was considerable but could hardly be quantified. I suspect it was somehow overestimated by a recent work (Ren et al 2007). Please feel free to comment and revise.

I'll check and complete the reference list, while you may also add in new references

Cheers

Zhongwei

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From: Ben Santer <santer1@llnl.gov>
To: Melissa Free <Melissa.Free@noaa.gov>
Subject: Re: IJOC paper
Date: Fri, 29 Feb 2008 15:03:43 -0800
Reply-to: santer1@llnl.gov
Cc: John Lanzante <John.Lanzante@noaa.gov>, "Philip D. Jones" <p.jones@uea.ac.uk>

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Dear Melissa,

Thanks for your comments on the IJoC paper. Here are a few quick responses.

Melissa Free wrote:

> Hi Ben,
> I've looked through the draft and have some comments:
> 1. I don't feel completely comfortable with the use of SSTs rather than
> combined land-sea surface temperatures for the lapse-rate analysis. Are
> we sure we have thought through the implications of this approach? If
> you show that the relationship between SSTs and tropical mean
> tropospheric temperatures is consistent between models and observations,
> that seems to imply that they are not so consistent for land
> surface-troposphere lapse rates. Could this be used to support the
> Pielke-Christy theory that (land) surface temperature trends are
> overestimated in the existing observational datasets?

I do feel comfortable with use of SSTs (rather than combined land+ocean temperatures) to estimate changes in tropical lapse rates. As Isaac Held pointed out, the temperature of the free troposphere in the deep tropics follows a moist adiabat which is largely set by the warmest SSTs in areas experiencing convection. The temperature of the free troposphere in the deep tropics is not set by temperatures over land. So if you want to see whether observations and models show lapse-rate changes that are in accord with a moist adiabatic lapse rate theory, it makes sense to look at SSTs rather than combined land+ocean surface temperatures. Admittedly, the focus of this paper is NOT on amplification behavior. Still, it does make sense to look at tropical lower tropospheric lapse rates in terms of their primary physical driver: SSTs.

As I tried to point out in the text of the IJoC paper, models and RSS-based estimates of lapser-rate changes are consistent, even if lapse-rate changes are inferred from combined land+ocean surface temperatures. The same same does not hold for lapse rate changes

estimated from HadCRUT3v and UAH data. I must admit that I don't fully understand the latter result. If you look at Table 1, you'll see that the multi-model ensemble-mean temporal standard deviation of $T\{SST\}$ is 0.243 degrees C, while the multi-model ensemble-mean temporal standard deviation of $T\{L+O\}$ is higher (0.274 degrees C). This makes good physical sense, since noise is typically higher over land than over ocean. Yet in the HadCRUT3v data, the temporal standard deviation of $T\{L+O\}$ (0.197 degrees C) is very similar to that of $T\{SST\}$ for the HadISST1 and HadISST2 data (HadISST2 is the SST component of HadCRUT3v). The fact that HadCRUT3v appears to have very similar variability over land and ocean seems counter-intuitive to me. Could it indicate a potential problem in the tropical land 2m temperatures in HadCRUT3v? I don't know. I'll let Phil address that one. The point is that we've done - at least in my estimation - a thorough job of looking at the sensitivity of our significance test results to current observational uncertainties in surface temperature changes.

> 2. The conclusion seems like too much of a dissertation on past history
> of the controversy.

As I pointed out in my email of Feb. 26th, I had a specific concern about the "Summary and Conclusions" section. I think that many readers of the paper will skip all the statistical stuff, and just read the Abstract and the "Summary and Conclusions". I did want the latter section to be relatively self-contained. We could have started by saying: "Here are the errors in Douglass et al., and here is what we found". But on balance, I thought that it would be more helpful to provide some scientific context. As I mentioned this morning, the Douglass et al. paper has received attention in high places. Not everyone who reads our response will be apprised of the history and context.

> 3. Regarding the time scale invariance of model amplification and the
> effects of volcanic eruptions on the trend comparisons, I am attaching a
> draft of my paper with John Lanzante comparing volcanic signals in sonde
> datasets v. models. I'm not sure if the statements on page 45 of the
> IJOC paper are consistent with my findings. (I thought about sending you
> this paper before, but it seemed like you were probably too busy with
> the IJOC paper to look at it.)

I'll look at your paper this weekend. I'm not quite sure which statements on page 45 you are referring to.

> 4. I suspect the statement in the last sentence of the conclusion won't

> represent the view of all authors-although it's certainly Dian's view. I
> don't think it is my view quite yet.

Others have also queried this final paragraph. At present, it looks like
it might be tough to accommodate the divergent views on this subject.
But I'll certainly try my best!

> I'm investigating an expedited internal review process and will let you
> know how it looks.

Thanks for looking into the expedited review!

> -Melissa

With best regards,

Ben

(P.S.: I hope you don't mind that I've copied my reply to Phil. I'm
hoping he can chime in on the issue of land surface temperature
variability in the HadCRUT3v data.)

--

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From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Past Millennia Climate Variability - Review Paper
Date: Thu, 13 Mar 2008 08:58:49 -0400
Reply-to: mann@psu.edu

Hi Phil,

Sorry, one other point. In item #4 below, the point that is being made, as shown (and discussed) elsewhere, applies both to the MBH method and the the canonical regression method (the latter is demonstrated in experiments by Wahl and Ammann not shown but referred to elsewhere in the text). So to be accurate and fair, the sentence in question on page 50 really has to be rephrased as follows:

Examinations of this kind are shown in Figures 3a,b (and parallel experiments not shown) demonstrating that, at least for the truncated-EOF CFR method used by MBH98 (employing inverse regression) and the canonical regression method that has been widely used by many other paleoclimate researchers, there is some degree of sensitivity to the climatological information available in calibration.

I realize there are many co-authors on the paper that have used the canonical regression method before, so perhaps there is pressure to focus the criticism on the MBH method. But that is simply not fair, as the other analyses by Wahl and Ammann not shown clearly demonstrates this applies to canonical regression as well--we can debate the relative sensitivity of the two methods, but it is similar.

This is an absolutely essential issue from my point of view, and I'm afraid I cannot sign my name to this paper w/out this revision.

I'm sure you understand--thanks for your help,
mike

Michael Mann wrote:

Phil,

Looks mostly fine to me now. I'm in Belgium (w/ the Louvain crowd) and only intermittent internet access, so will be difficult to provide much more feedback than the below. I hope that is ok? Here are my remaining minor comments:

1) the author list is a bit front-loaded w/ CRU folks. You should certainly be the first author, but the remaining order makes this paper look more like a "CRU" effort than a "Wengen" effort, and perhaps that will have an unintended impact on the way the paper is received by the broader community. I was also wondering how I ended up so far down the list :(

I think I was one of the first to provide a substantive contribution to the paper. Was my contribution really so minor compared to those others? The mechanism behind the author list is unclear, partially alphabetical (towards the end), but partly not. You are of course the best judge of peoples' relative contributions, and if the current author order indeed represents that according to your judgment, then I'm fine w/ that. Just thought I'd check

though.

2) page 45, 2nd paragraph, should substitute "(e.g. Shindell et al, 2001; Collins et al 2002)" for "Collins et al 2002"

3) page 48, 2nd paragraph, 3rd sentence, should substitute "RegEM (implemented with TTLS as described by Mann et al 2007) for "RegEM".

4) page 50, bottom paragraph, first sentence: I think that the use of "crucially" here is unnecessarily inflammatory and overly dramatic. This word can be removed without any detriment to the point being made, don't you think?

5) page 51, 2nd paragraph, logic does not properly follow in certain places as currently phrased (a frequent problem w/ Eugene's writing unfortunately!):

a. sentence beginning at end of line 9 of paragraph, should be rephrased as follows:

Mann et al. (2005) used pseudo-proxy experiments that apparently showed that this method did not underestimate the amplitude of the reconstructed NH temperature anomalies: however, Smerdon and Kaplan (2007) show that this may have been a false positive result arising from differences between the implementation of the RegEM algorithm in the pseudo-proxy experiments and in the real-proxy reconstructions which leads to a sensitivity of the pseudoproxy results to the calibration period used (also noted by Lee et al., 2008).

b. the sentence following the one above should be rephrased:

Mann et al. (2007; cf. their Figs. 3-4) demonstrate that a variant of the RegEM method that uses TTLS, rather than ridge regression produces an NH temperature reconstruction whose amplitude fidelity does not exhibit the calibration interval dependence of the previous implementation by Mann et al 2005, and yields reconstructions that do not suffer from amplitude loss for a wide range of signal-to-noise ratios and noise spectra (though Lee et al., 2008, suggest that an appropriately implemented ridge regression can also produce good results).

c. the sentence following the one above should be rephrased:

With TTLS as implemented by Mann et al (2007), RegEM performs without amplitude loss in model-based tests (versions without trend removal), including using the high-amplitude ECHO-G model output utilized by Bürger et al. (2006), von Storch et al. (2006), and Küttel et al. (2007) to examine truncated-EOF methods.

6) page 52, 1st paragraph, 7th line, the reference of "the MBH reconstruction" is erroneous, because the tests have nothing to do w/ the MBH reconstruction per se, only--potentially--the MBH method under certain circumstances. In fact, Mann et al (2007) [and Wahl and Amman(2007)] both show that the actual amplitude loss realized in the MBH reconstruction in reality is probably quite small. This very point is made at the top of page 53! So the reference to "the MBH reconstruction" needs to be eliminated here. It is already clear by context what this is actually referring to (idealized experiments using both the MBH and canonical applied to surrogate proxy networks).

7) Re, Caspar--well he seems to be in his "non-responsible" phase right now, hasn't replied to my messages either. Will keep on trying, let me know if any of the above needs further elaboration. we're travelling for the weekend but will still have intermittent email access,
mike

Phil Jones wrote:

Dear All,

Attached is the penultimate draft of the Wengen paper. If you have time can you look

through this. If you've not much time, can you look through your sections and the intro/conclusions. I hope we in CRU have got all your comments in. We have been through them all - including Gene's which came last night and Francis' the night before.

WE URGENTLY NEED CASPAR TO REpond. Can Gene, Mike and anyone who can get Caspar to respond to emails tell him that there are a few questions in this draft we need him to respond to. We need better versions of Figure 3, plus there are some flagged points in Sections 3 and 4.

Juerg - is Figure 5 OK. If not resend separately - don't embed as this screwed up last time.

Plan A is for us to submit this to The Holocene next Wednesday. So we need by then, from each of you a quick email to say you've got this and any comments by next Monday - March 17. Submission will be March 19. There is no Plan B.

With the Feb 20 email, there were no responses from Peck, Eystein and Nick. If we don't hear from you three by next week, we will remove you from the author list! If anyone knows if any of these three are in the field please let me know?

Things to check:

1. Everybody happy with the author order. The idea here was the three us in CRU, the main authors of the sections in section order, then others in alphabetical order.
2. If you have time also look at sections 2.5 and 2.6. Issue here is - is there enough there. Thanks to Juerg for some of these sections.
3. There are a couple of refs (Juerg) we need - Buntgen et al. and D'Arrigo et al. Next week, we (CRU) will be working on the alterations- using IPCC rules. These are - if you want a change justify it, and if you say this is unbalanced, or just European, or emphasizes Lee et al. (2008), then gives us the additional text to make alterations. We've left a few comments in where these sorts of comments were made last time.

There will be time to make alterations while The Holocene reviews it. It will also be better to read it later when there is time after submission.

I've not read this version yet, so apologies if there are any pieces of poor English. I will be reading again this weekend.

Finally, Thorsten, if you think I've missed anybody off this email, forward and let me know.

Juerg needs to send on to the others within Bern.

Cheers

Phil

Prof. Phil Jones

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--

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References

1. <mailto:p.jones@uea.ac.uk>
2. <mailto:mann@psu.edu>
3. <http://www.met.psu.edu/dept/faculty/mann.htm>
4. <mailto:mann@psu.edu>
5. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: David Parker <david.parker@metoffice.gov.uk>

To: "Mann, Michael" <mann@virginia.edu>

Subject: Heads up

Date: Wed, 26 Mar 2008 12:45:42 +0000

Cc: "Folland, Chris" <chris.folland@metoffice.gov.uk>, "Kennedy, John" <john.kennedy@metoffice.gov.uk>, "Jones, Phil" <p.jones@uea.ac.uk>, "Karl, Tom" <Thomas.R.Karl@noaa.gov>

Mike

Yes it was based on only Jan+Feb 2008 and padding with that final value but John Kennedy has changed / shortly will change this misleading plot!

Regards

David

-----Original Message-----

From: Michael Mann [mailto:mann@meteo.psu.edu]

Sent: 26 March 2008 11:19

To: Folland, Chris

Cc: Phil Jones; Thomas R Karl

Subject: heads up

Hi Chris (and Tom and Phil),

I hope you're all doing well. Just wanted to give you a heads up on something. Have you seen this?

http://hadobs.metoffice.com/hadcrut3/diagnostics/global/nh+sh/annual_s21.png

apparently the contrarians are having a field day w/ this graph. My understanding that it is based on using only Jan+Feb 08 and padding w/ that final value.

Surely this can't be?? Is Fred Singer now running the UK Met Office website?

Would appreciate any info you can provide,

mike

--

Michael E. Mann

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--
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From: Phil Jones <p.jones@uea.ac.uk>
To: trenbert@ucar.edu,"Jonathan Overpeck" <jto@u.arizona.edu>
Subject: Re: Fwd: ukweatherworld
Date: Thu, 27 Mar 2008 10:28:38 +0000
Cc: mann@multiproxy.evsc.virginia.edu,santer1@llnl.gov, "Susan Solomon" <susan.solomon@noaa.gov>

<x-flowed>

Peck et al,

I recall meeting David Deeming at a meeting years ago (~10). He worked in boreholes then. I've seen his name on several of the skeptic websites.

Kevin's idea is a possibility. I wouldn't post on the website 'ukweatherworld'.

The person who sent you this is likely far worse. This is David Holland. He is a UK citizen who send countless letters to his MP in the UK, writes in Energy & Environment about the biased IPCC and has also been hassling John Mitchell about his role as Review Editor for Ch 6. You might want to talk to John about how he's responding. He has been making requests under our FOI about the letters Review Editors sent when signing off. I'm sure Susan

is aware of this. He's also made requests for similar letters re WG2 and maybe 3.

Keith has been in contact with John about this.

I've also seen the quote about getting rid of the MWP - it would seem to go back many years, maybe even to around the TAR. I've no idea where it came from. I didn't say it!

I've written a piece for RMS [popular journal Weather on the MWP and LIA - from a UK perspective. It is due out in June. I can send if you want.

I'm away all next week - with Mike. PaleoENSO meeting in Tahiti - you can't turn those sorts of meetings down!

Cheers
Phil

At 23:15 26/03/2008, Kevin Trenberth wrote:
>Hi Jon

>There is a lot to be said for ignoring such a thing. But I understand the
>frustration. An alternative approach is to write a blog on this topic of
>the medieval warm period and post it at a neutral site and then refer
>enquiries to that link. You would have a choice of directly confronting
>the statements or making a more general statement, presumably that such a
>thing is real but was more regional and not as warm as most recent times.
>This approach would not then acknowledge that particular person, except
>indirectly.

>
>A possible neutral site might be blogs.nature.com/climatefeedback/
>I posted a number of blogs there last year but not this year. I can send
>you the contact person if you are interested and you can make the case
>that they should post the blog.

>
>Good luck
>Kevin

>
>
>> Hi Phil, Kevin, Mike, Susan and Ben - I'm looking
>> for some IPCC-related advice, so thanks in
>> advance. The email below recently came in and I
>> googled "We have to get rid of the warm medieval
>> period" and "Overpeck" and indeed, there is a
>> person David Deeming that attributes the quote to
>> an email from me. He apparently did mention the
>> quote (but I don't think me) in a Senate hearing.
>> His "news" (often with attribution to me) appears
>> to be getting widespread coverage on the
>> internet. It is upsetting.

>>
>> I have no memory of emailing w/ him, nor any
>> record of doing so (I need to do an exhaustive
>> search I guess), nor any memory of him period. I
>> assume it is possible that I emailed w/ him long
>> ago, and that he's taking the quote out of
>> context, since know I would never have said what
>> he's saying I would have, at least in the context
>> he is implying.

>>
>> Any idea what my reaction should be? I usually
>> ignore this kind of misinformation, but I can
>> imagine that it could take on a life of it's own
>> and that I might want to deal with it now, rather
>> than later. I could - as the person below

> > suggests - make a quick statement on a web site
> > that the attribution to me is false, but I
> > suspect that this Deeming guy could then produce
> > a fake email. I would then say it's fake. Or just
> > ignore? Or something else?

> >
> > I googled Deeming, and from the first page of
> > hits got the sense that he's not your average
> > university professor... to put it lightly.

> >
> > Again, thanks for any advice - I'd really like
> > this to not blow up into something that creates
> > grief for me, the IPCC, or the community. It is
> > bogus.

> >
> > Best, Peck

> >
> >
> >>X-Sieve: CMU Sieve 2.3
> >>Reply-To: "David Holland" <d.holland@theiet.org>
> >>From: "David Holland" <d.holland@theiet.org>
> >>To: <jto@u.arizona.edu>
> >>Subject: ukweatherworld
> >>Date: Mon, 24 Mar 2008 08:39:10 -0000

> >>
> >>Dear Dr Overpeck,

> >>
> >>
> >>
> >>I recall David Deeming giving evidence to a
> >>Senate hearing to the effect that he had
> >>received an email including a remark to the
> >>effect "We have to get rid of the warm medieval
> >>period". I have now seen several comment web
> >>pages attribute the email to your. Some serious
> >>and well moderated pages like
> >>ukweatherworld would welcome a post from you if
> >>the attribution is untrue and would, I feel
> >>sure, remove it if you were to ask them to. I am
> >>sure that many other blogs would report your
> >>denial. Is there any reason you have not issued
> >>a denial?

> >>
> >>

>>>

>>>David Holland

>>

>>

>> --

>> Jonathan T. Overpeck

>> Director, Institute for the Study of Planet Earth

>> Professor, Department of Geosciences

>> Professor, Department of Atmospheric Sciences

>>

>> Mail and Fedex Address:

>>

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>> fax: +1 520 792-8795

>> <http://www.geo.arizona.edu/dgesl/>

>> <http://www.ispe.arizona.edu/>

>>

>

>

>_____

>Kevin Trenberth

>Climate Analysis Section, NCAR

>PO Box 3000

>Boulder CO 80307

>ph 303 497 1318

><http://www.cgd.ucar.edu/cas/trenbert.html>

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</x-flowed>

From: Michael Mann <mann@meteo.psu.edu>

To: "Folland, Chris" <chris.folland@metoffice.gov.uk>

Subject: Re: heads up

Date: Wed, 02 Apr 2008 13:43:47 -0400

Reply-to: mann@psu.edu

Cc: Phil Jones <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Richard.W.Reynolds@noaa.gov

<x-flowed>

Hey Chris,

In Tahiti (w/ Phil), limited email. Thanks so much for the detailed response. I also heard from David about this, who had similar. sounds like you guys are on top of this. The contrarians will cry conspiracy once the spurious plot is taken down and replaced w/ a corrected one, but what you can do.

I'm sorry to hear you're retiring from the Met Office, but sounds like you're going to remain active, which is great. lets catch up on things sometime soon more generally!

talk to you later,

mike

Folland, Chris wrote:

> Dear Mike and all

>

> First, thanks very much, Mike, for noticing this and preventing greater
> problems. The error arose from a pre-existing hidden software bug that
> the person updating the data had not realised was there. The software is
> a mixture of languages which makes it less than transparent. The bug is
> now fixed on all the smoothed graphs. It was made worse because the last
> point was not an average of several preceding years as it should have
> been but was just January 2008. So many apologies for any excitement
> this may have created in the hearts of the more ardent sceptics. Some
> are much on the warpath at present over the lack of recent global
> warming, fired in some cases by visions of a new solar Dalton Minimum.

>

> I'm retiring from full time work on 17th April but I will return part
> time semi-retired taking pension on 1 June. I've managed to keep my
> present grading. My Climate Variability and Forecasting group is being
> split (it's the largest in the Hadley Centre by a margin). The biggest
> part is becoming technically from today a new Climate Monitoring and
> Attribution group under Peter Stott as Head. He will bring two existing
> attribution staff to make a group of c.22. Most of the rest (12) will
> form the bulk of a new Seasonal to Decadal Forecasting group to be set
> up most likely this summer with a new Head. Finally Craig Donlon,

> Director of the GODAE GHRSSST sea surface temperature project, will go
> back to our National Centre for Ocean Forecasting (in the next wing of
> this building), but will work closely we hope with Nick Rayner in Peter
> Stott's new group on HadISST2.

>
> I will return to a new 3 day a week position in the Seasonal to Decadal
> Forecasting Group, a mixture of research, some strategy and advice, and
> importantly, operational seasonal, annual, and probably decadal,
> forecasting. The Met Office are putting more emphasis on this area,
> especially the seasonal at present, which is becoming high profile as
> seasonal success is perceived to have improved. No staff
> responsibilities! Tom Peterson will approve! I will keep my
> co-leadership with Jim Kinter of the Clivar Climate of the Twentieth
> Century modelling project for now as well.

>
> So quite a change, as I will be doing more computing work than I have
> had time for, moving into IDL this autumn which the Hadley Centre as a
> whole are moving over to about then.

>
> Mike, it's a fair time since we interacted so I'd be very interested in
> your activities and plans.

>
> With best regards

>
> Chris

>
> Prof. Chris Folland
> Head of Climate Variability and Forecasting Research

>
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> Kingdom

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> <<http://www.metoffice.gov.uk>>

> Fellow of the Met Office

> Hon. Professor of School of Environmental Sciences, University of East
> Anglia

>
>
> -----Original Message-----

> From: Michael Mann [<mailto:mann@meteo.psu.edu>]

> Sent: 26 March 2008 11:19

> To: Folland, Chris

> Cc: Phil Jones; Thomas R Karl

> Subject: heads up

>

> Hi Chris (and Tom and Phil),
>
> I hope you're all doing well. Just wanted to give you a heads up on
> something. Have you seen this?
> [http://hadobs.metoffice.com/hadcrut3/diagnostics/global/nh+sh/annual_s21](http://hadobs.metoffice.com/hadcrut3/diagnostics/global/nh+sh/annual_s21.png)
> .png
> apparently the contrarians are having a field day w/ this graph. My
> understanding that it is based on using only Jan+Feb 08 and padding w/
> that final value.
>
> Surely this can't be?? Is Fred Singer now running the UK Met Office
> website?
>
> Would appreciate any info you can provide,
>
> mike
>
> --
> Michael E. Mann
> Associate Professor
> Director, Earth System Science Center (ESSC)
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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>

To: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>, "Clare Goodess" <C.Goodess@uea.ac.uk>, "Anthony Footitt" <a.footitt@uea.ac.uk>, "Suraje Dessai" <s.dessai@uea.ac.uk>, "Mark New" <mark.new@ouce.ox.ac.uk>, "Jim Hall" <jim.hall@newcastle.ac.uk>, "C G Kilsby" <c.g.kilsby@newcastle.ac.uk>, <ana.lopez@ouce.ox.ac.uk>

Subject: Re: EA PQQ for review by 4pm

Date: Tue Apr 15 12:48:32 2008

Cc: "Arkell, Brian" <Brian.Arkell@atkinsglobal.com>, "Sene, Kevin" <Kevin.Sene@atkinsglobal.com>

Geoff,

Have had a look through. I hope all will read their own CVs and institution bits.

My caught one word in Suraje's paragraph. The word was 'severed'. It should be

'served' ! Also his promising suit of methods would read better as a 'suite'

Finally in Mark's he's a Principal Investigator.

Cheers

Phil

At 09:38 15/04/2008, Darch, Geoff J wrote:

Dear all,

Thanks to everyone for sending text etc, in particular to Jim and Chris for the succinct

answer to ET1.

Please find attached (1) the full PQQ, minus Experience and Technical (ET) text, for

information; (2) the ET text, for review.

I'd be grateful for your review of the ET text. In particular (a) please comment on my

draft table in ET2 - I have done my best to capture my knowledge of CRU and Tyndall

skills with respect to the criteria, but you are clearly better placed than me! (b) do

you think the CVs cover the technical areas adequately? We may be a little weak on

conservation and ecology. We have a good CV we can add here, and I'm sure Tyndall has

too (e.g. Andrew) but that would mean taking another out.

We are exploring a link with the specialist communications consultancy Futerra, but

apart from a brief mention, we leaving anything else on this to the full bid stage.

I'd be grateful if you would let me have any comments by 4pm today. This will give me

time to finalise the document and email it first thing tomorrow.

Best wishes,

Geoff

<<EA_PQQ_ET_Draft.doc>> <<EA-PQQ_Atkins-CRU-Tyn_Draft.DOC>>

Geoff Darch

Senior Consultant

Water and Environment

ATKINS

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P Consider the environment. Please don't print this e-mail unless you really need to.

Prof. Phil Jones

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References

1. <file:///www.atkinsglobal.com/climatechange>

2. http://www.atkinsglobal.com/terms_and_conditions/index.aspx

From: Ben Santer <santer1@llnl.gov>
To: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, "'Susan Solomon'" <ssolomon@al.noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>
Subject: [Fwd: JOC-08-0098 - International Journal of Climatology]
Date: Thu, 24 Apr 2008 19:34:37 -0700
Reply-to: santer1@llnl.gov

<x-flowed>

Dear folks,

I'm forwarding an email from Prof. Glenn McGregor, the IJoC editor who is handling our paper. The email contains the comments of Reviewer #1, and notes that comments from two additional Reviewers will be available shortly.

Reviewer #1 read the paper very thoroughly, and makes a number of useful comments. The Reviewer also makes some comments that I disagree with.

The good news is that Reviewer #1 begins his review (I use this personal pronoun because I'm pretty sure I know the Reviewer's identity!) by affirming the existence of serious statistical errors in DCPS07:

"I've read the paper under review, and also DCPS07, and I think the present authors are entirely correct in their main point. DCPS07 failed to account for the sampling variability in the individual model trends and, especially, in the observational trend. This was, as I see it, a clear-cut statistical error, and the authors deserve the opportunity to present their counter-argument in print."

Reviewer #1 has two major concerns about our statistical analysis. Here is my initial reaction to these concerns.

CONCERN #1: Assumption of an AR-1 model for regression residuals.

In calculating our "adjusted" standard errors, we assume that the persistence of the regression residuals is well-described by an AR-1 model. This assumption is not unique to our analysis, and has been made in a number of other investigations. The Reviewer would "like to see at least some sensitivity check of the standard error formula against alternative model assumptions." Effectively, the Reviewer is asking whether a more complex time series model is required to describe the persistence.

Estimating the order of a more complex AR model is a tricky business. Typically, something like the BIC (Bayesian Information Criterion) or AIC (Akaike Information Criterion) is used to do this. We could, of course, use the BIC or AIC to estimate the order of the AR model that

best fits the regression residuals. This would be a non-trivial undertaking. I think we would find that, for different time series, we would obtain different estimates of the "best-fit" AR model. For example, 20c3m runs without volcanic forcing might yield a different AR model order than 20c3m runs with volcanic forcing. It's also entirely likely (based on Rick Katz's experience with such AR model-fitting exercises) that the AIC- and BIC-based estimates of the AR model order could differ in some cases.

As the Reviewer himself points out, DCPS07 "didn't make any attempt to calculate the standard error of individual trend estimates and this remains the major difference between the two paper." In other words, our paired trends test incorporates statistical uncertainties for both simulated and observed trends. In estimating these uncertainties, we account for non-independence of the regression residuals. In contrast, the DCPS07 trend "consistency test" does not incorporate ANY statistical uncertainties in either observed or simulated trends. This difference in treatment of trend uncertainties is the primary issue. The issue of whether an AR-1 model is the most appropriate model to use for the purpose of calculating adjusted standard errors is really a subsidiary issue. My concern is that we could waste a lot of time looking at this issue, without really enlightening the reader about key differences between our significance testing testing procedure and the DCPS07 approach.

One solution is to calculate (for each model and observational time series used in our paper) the parameters of an AR(K) model, where K is the total number of time lags, and then apply equation 8.39 in Wilks (1995) to estimate the effective sample size. We could do this for several different K values (e.g., K=2, K=3, and K=4; we've already done the K=1 case). We could then very briefly mention the sensitivity of our "paired trend" test results to choice of order K of the AR model. This would involve some work, but would be easier to explain than use of the AIC and BIC to determine, for each time series, the best-estimate of the order of the AR model.

CONCERN #2: No "attempt to combine data across model runs."

The Reviewer is claiming that none of our model-vs-observed trend tests made use of data that had been combined (averaged) across model runs. This is incorrect. In fact, our two modified versions of the DCPS07 test (page 29, equation 12, and page 30, equation 13) both make use of the multi-model ensemble-mean trend.

The Reviewer argues that our paired trends test should involve the ensemble-mean trends for each model (something which we have not done) rather than the trends for each of 49 individual 20c3m realizations. I'm not sure whether the rationale for doing this is as "clear-cut" as the Reviewer contends.

Furthermore, there are at least two different ways of performing the paired trends tests with the ensemble-mean model trends. One way (which seems to be what the Reviewer is advocating) involves replacing in our equation (3) the standard error of the trend for an individual

realization performed with model A with model A's intra-ensemble standard deviation of trends. I'm a little concerned about mixing an estimate of the statistical uncertainty of the observed trend with an estimate of the sampling uncertainty of model A's trend.

Alternately, one could use the average (over different realizations) of model A's adjusted standard errors, or the adjusted standard error calculated from the ensemble-mean model A time series. I'm willing to try some of these things, but I'm not sure how much they will enlighten the reader. And they will not help to make an already-lengthy manuscript any shorter.

The Reviewer seems to be arguing that the main advantage of his approach #2 (use of ensemble-mean model trends in significance testing) relative to our paired trends test (his approach #1) is that non-independence of tests is less of an issue with approach #2. I'm not sure whether I agree. Are results from tests involving GFDL CM2.0 and GFDL CM2.0 temperature data truly "independent" given that both models were forced with the same historical changes in anthropogenic and natural external forcings? The same concerns apply to the high- and low-resolution versions of the MIROC model, the GISS models, etc.

I am puzzled by some of the comments the Reviewer has made at the top of page 3 of his review. I guess the Reviewer is making these comments in the context of the pair-wise tests described on page 2. Crucially, the comment that we should use "...the standard error if testing the average model trend" (and by "standard error" he means DCPS07's $\sigma\{SE\}$) IS INCONSISTENT with the Reviewer's approach #3, which involves use of the inter-model standard deviation in testing the average model trend.

And I disagree with the Reviewer's comments regarding the superfluous nature of Section 6. The Reviewer states that, "when simulating from a know (statistical) model... the test statistics should by definition give the correct answer. The whole point of Section 6 is that the DCPS07 consistency test does NOT give the correct answer when applied to randomly-generated data!

In order to satisfy the Reviewer's curiosity, I'm perfectly willing to repeat the simulations described in Section 6 with a higher-order AR model. However, I don't like the idea of simulation of synthetic volcanoes, etc. This would be a huge time sink, and would not help to illustrate or clarify the statistical mistakes in DCPS07.

It's obvious that Reviewer #1 has put a substantial amount of effort into reading and commenting on our paper (and even performing some simple simulations). I'm grateful for the effort and the constructive comments, but feel that a number of comments are off-base. Am I misinterpreting the Reviewer's comments?

With best regards,

Ben

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Attachment Converted: "c:\eudora\attach\ - santerreport.pdf"
X-Account-Key: account1
Return-Path: <g.mcgregor@auckland.ac.nz>
Received: from mail-1.llnl.gov ([unix socket])
by mail-1.llnl.gov (Cyrus v2.2.12) with LMTPA;
Thu, 24 Apr 2008 12:47:37 -0700
Received: from smtp.llnl.gov (nspiron-3.llnl.gov [128.115.41.83])
by mail-1.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.6 \$) with ESMT
id m3OJlZk7028016
for <santer1@mail.llnl.gov>; Thu, 24 Apr 2008 12:47:37 -0700
X-Attachments: - santerreport.pdf
X-IronPort-AV: E=McAfee;i="5200,2160,5281"; a="32776528"
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Received: from nsziron-3.llnl.gov ([128.115.249.83])
by smtp.llnl.gov with ESMT; 24 Apr 2008 12:47:36 -0700
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by nsziron-3.llnl.gov with ESMT; 24 Apr 2008 12:47:34 -0700
Received: from tsslbe0004 (tsslbe0004 [10.237.148.27])
by uranus.scholarone.com (Postfix) with SMTP id 8F0554F44D5
for <santer1@llnl.gov>; Thu, 24 Apr 2008 15:47:33 -0400 (EDT)
Message-ID: <379866627.1209066453582.JavaMail.wladmin@tsslbe0004>
Date: Thu, 24 Apr 2008 15:47:33 -0400 (EDT)
From: g.mcgregor@auckland.ac.nz
To: santer1@llnl.gov
Subject: JOC-08-0098 - International Journal of Climatology
Errors-To: masmith@wiley.co.uk
Mime-Version: 1.0
Content-Type: multipart/mixed;
boundary="-----_Part_678_379761858.1209066453554"
X-Errors-To: masmith@wiley.co.uk
Sender: onbehalf@scholarone.com

24-Apr-2008

JOC-08-0098 - Consistency of Modelled and Observed Temperature Trends in the Tropical Troposphere

Dear Dr Santer

I have received one set of comments on your paper to date. Although I would normally wait for all comments to come in before providing them to you, I thought in this case I would give you a head start in your preparation for revisions. Accordingly please find attached one set of comments. Hopefully I should have two more to follow in the near future.

Best,

Prof. Glenn McGregor

Attachment Converted: "c:\eudora\attach\ - santerreport1.pdf"

From: Ben Santer <santer1@llnl.gov>
To: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, "'Susan Solomon'" <ssolomon@al.noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Karl Taylor <taylor13@llnl.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>
Subject: [Fwd: Re: JOC-08-0098 - International Journal of Climatology]
Date: Fri, 25 Apr 2008 13:19:18 -0700
Reply-to: santer1@llnl.gov

<x-flowed>
Dear folks,

On April 11th, I received an email from Prof. Glenn McGregor at IJoC. I am now forwarding that email, together with my response to Prof. McGregor.

Prof. McGregor's email asks for my opinion of an "Addendum" to the original DCPS07 IJoC paper. The addendum is authored by Douglass, Christy, Pearson, and Singer. As you can see from my reply to Prof. McGregor, I do not think that the Addendum is worthy of publication. Since one part of the Addendum deals with issues related to the RAOBCORE data used by DCPS07 (and by us), Leo responded to Prof. McGregor on this point. I will forward Leo's response in a separate email.

The Addendum does not reference our IJoC paper. As far as I can tell, the Addendum represents a response to discussions of the original IJoC paper on RealClimate.org. Curiously, Douglass et al. do not give a specific source for the criticism of their original paper. This is rather bizarre. Crucially, the Addendum does not recognize or admit ANY ERRORS in the original DCPS07 paper.

I have not yet heard whether IJoC intends to publish the Addendum. I'll update you as soon as I have any further information from Prof. McGregor.

With best regards,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

Attachment Converted: "c:\eudora\attach\[Fwd Re JOC-08-0098 - Interna.pdf"
X-Account-Key: account1

Return-Path: <santer1@llnl.gov>
Received: from mail-1.llnl.gov ([unix socket])
by mail-1.llnl.gov (Cyrus v2.2.12) with LMTPA;
Fri, 11 Apr 2008 11:19:24 -0700
Received: from smtp.llnl.gov (nspiron-3.llnl.gov [128.115.41.83])
by mail-1.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.6 \$) with ESMTTP id
m3BIJN5F012995
for <santer1@mail.llnl.gov>; Fri, 11 Apr 2008 11:19:24 -0700
X-Attachments: None
X-IronPort-AV: E=McAfee;i="5200,2160,5272"; a="31695223"
X-IronPort-AV: E=Sophos;i="4.25,642,1199692800";
d="scan'208";a="31695223"
Received: from dione.llnl.gov (HELO [128.115.57.29]) ([128.115.57.29])
by smtp.llnl.gov with ESMTTP; 11 Apr 2008 11:14:37 -0700
Message-ID: <47FFAA8D.8040308@llnl.gov>
Date: Fri, 11 Apr 2008 11:14:37 -0700
From: Ben Santer <santer1@llnl.gov>
Reply-To: santer1@llnl.gov
Organization: LLNL
User-Agent: Thunderbird 1.5.0.12 (X11/20070529)
MIME-Version: 1.0
To: g.mcgregor@auckland.ac.nz
CC: Leopold Haimberger <leopold.haimberger@univie.ac.at>,
"Thorne, Peter" <peter.thorne@metoffice.gov.uk>
Subject: Re: JOC-08-0098 - International Journal of Climatology
References: <363780847.1207875178234.JavaMail.wladin@tsslbe0004>
In-Reply-To: <363780847.1207875178234.JavaMail.wladin@tsslbe0004>
Content-Type: text/plain; charset=ISO-8859-1; format=flowed
Content-Transfer-Encoding: 7bit

<x-flowed>

Dear Prof. McGregor,

Thank you for your email, and for your efforts to ensure rapid review of our paper.

Leo Haimberger (who has led the development of the RAOBCORE* datasets) and Peter Thorne would be best placed to comment on the first issue raised by the Douglass et al. "Addendum". As we show in Figure 6 of our IJoC paper, recently-developed radiosonde datasets which do not rely on reanalysis data for correction of inhomogeneities (such as the Sherwood et al. IUK product and the Haimberger et al. "RICH" dataset) yield vertical profiles of atmospheric temperature change that are in better agreement with model results, and quite different from the profiles shown by Douglass et al.

The second issue raised in the Douglass et al. "Addendum" is completely spurious. Douglass et al. argue that their "experimental design" involves involves "comparing like to like", and satisfying "the critical condition that the model surface temperatures match the observations". If this was indeed their experimental design, Douglass et al. should have have examined "AMIP" (Atmospheric Model Intercomparison Project) simulations, in which an atmospheric model is run with prescribed changes in observed time-varying sea-surface temperatures (SSTs) and sea-ice distributions. Use of AMIP simulations would allow an analyst to compare simulated and observed tropospheric temperature changes given the same underlying changes in SSTs.

But Douglass et al. did NOT consider results from AMIP simulations, even though AMIP data were freely available to them (AMIP data were in the same "CMIP-3" archive that Douglass et al. accessed in order to obtain the model results analyzed in their original IJoC paper). Instead, Douglass et al. examined results from coupled model simulations. As we discuss at length in Section 3 of our paper, coupled model simulations are fundamentally different from AMIP runs. A coupled model is NOT driven by observed changes in SSTs, and therefore would not have (except by chance) the same SST changes as the real world over a specific period of time.

Stratifying the coupled model results by the observed surface temperature changes is not a meaningful or useful thing to do, particularly given the small ensemble sizes available here. Again, if Douglass et al. were truly interested in imposing "the critical condition that the model surface temperatures match the observations", they should have examined AMIP runs, not coupled model results.

I also note that, although Douglass et al. stipulate their "critical condition that the model surface temperatures match the observations", they do not actually perform any stratification of the model trend results! In other words, Douglass et al. do NOT discard simulations with surface trends that differ from the observed trend. They simply note that the MODEL AVERAGE surface trend is close to the observed surface trend, and state that this agreement in surface trends allows them to evaluate whether the model average upper air trend is consistent with observed upper air trends.

The Douglass et al. "Addendum" does nothing to clarify the serious statistical flaws in their paper. Their conclusion - that modelled and observed upper air trends are inconsistent - is simply wrong. As we point out in our paper, Douglass et al. reach this incorrect conclusion by ignoring uncertainties in observed and modelled upper air trends arising from interannual variability, and by applying a completely inappropriate "consistency test". Our Figure 5 clearly shows that the Douglass et al. "consistency test" yields incorrect results. The "Addendum" does not suggest that the authors are capable of recognizing or understanding the errors inherent in either their "experimental method" or their "consistency test".

The Douglass et al. IJoC paper reached a radically different conclusion from the conclusions reached by Santer et al. (2005), the 2006 CCSP report, the 2007 IPCC report, and Thorne et al. (2007). It did so on the basis of essentially the same data used in previous work. Most scientists would have asked whether the "consistency test" which yielded such startlingly different conclusions was appropriate. They would have applied this test to synthetic data, to understand its behaviour in a controlled setting. They would have applied alternative tests. They would have done everything they possibly could to examine the robustness of their findings. Douglass et al. did none of these things.

I will ask Leo Haimberger and Peter Thorne to respond to you regarding the first issue raised in the Douglass et al. "Addendum".

Best regards,

Ben Santer

(* In their addendum, Douglass et al. erroneously refer to "ROABCORE" datasets. One would hope that they would at least be able to get the name of the dataset right.)

g.mcgregor@auckland.ac.nz wrote:

> 10-Apr-2008

>

> JOC-08-0098 - Consistency of Modelled and Observed Temperature Trends in the Tropical Troposphere

>

> Dear Dr Santer

>

> Just to let you know that I am trying to secure reviews of your paper asap.

>

> I have attached an addendum for the Douglass et al. paper recently sent to me by David Douglass. I would be interested to learn of your views on this

>

>

> Best,

>

> Prof. Glenn McGregor

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
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Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: [Fwd: Talk on Understanding 20th C surface temperature variability]
Date: Tue Apr 29 09:08:36 2008
Cc: Ben Santer <santer1@llnl.gov>

Tom,

Here's what I sent Kevin yesterday. Still don't have the proofs with Figures in. It is most odd how

this Cambridge seminar has been so widely publicised. Michael McIntyre seems to be sending it everywhere. Dave Thompson is on a sabbatical in the UK for 6 months (at Reading). Should be here soon for a visit to CRU.

The press release is very much work in progress. Appended the latest version at the end. This version still need some work. Maybe I'll get a chance later today.

cc'd Ben as if and when (hopefully) the 'where Douglass et al went wrong' paper comes out a press release then would be useful. In both cases, there is a need to say things in plain English and not the usual way we write.

For some reason the skeptics (CA) are revisiting the Douglass et al paper. A very quick look shows that a number think the paper is wrong!

There is also a head of steam being built up (thanks to a would be Australian astronaut who knows nothing about climate) about the drop in temperature due to La Nina. If you've time look at the HadCRUT3 plot for March08. It was the warmest ever for NH land. The snow cover plots at Rutgers are interesting also. Jan08 for Eurasia had the most coverage ever, but March08 had the least (for their respective months).

It seems we just need the La Nina to finally wind down and the oceans to warm up a little. The press release could be an issue, as it looks as though we are underestimating SST

with the buoys - by about 0.1 deg C.

Cheers

Phil

Using a novel technique to remove the effects of temporary fluctuations in global temperature due to El Niño and transient weather patterns, researchers at Colorado State University, the University of Washington, the UK Met Office and the University of East Anglia have highlighted a number of sudden drops in global temperature.

Most of these drops coincide with the eruptions of large tropical volcanoes and are also evident in air temperatures measured over the worlds land areas, but the largest, occurring towards the end of 1945, is unrelated to any known volcanic eruption and is not apparent over land. It appears to arise from an artificial and temporary cooling caused by an abrupt

change in the mix of US and UK ships reporting temperatures at the end of the Second World War.

The majority of sea temperature measurements available in international data bases between 1941 and 1945 are from US ships. Far fewer data are available in this period than in the 1930s and the 1950s. The crews of US ships measured the temperature of the water before it was used to cool the ships engine. Because of warmth coming from the ship, the water was often a little warmer than the true sea temperature. At the end of 1945 the number of US observations in the data base dropped rapidly. At the same time the number of UK observations increased. UK ships measured the temperature of water samples collected using special buckets. Wind blowing past the buckets as they were hauled onto the deck often caused these measurements to be cooler than the actual sea temperature. The sudden change from US (engine room) to UK (bucket) measurements from warmer to cooler is what caused the abruptness of the drop.

Although the drop in 1945 was large in climate-change terms about 0.3°C its full effect is likely to be limited to the period immediately after the Second World War, because by the 1960s better-insulated buckets were coming into use and there was a more varied mix of measurements from different national merchant shipping fleets. Because it occurs in the middle of the century it will have little effect on 20th Century warming trends, which are corroborated by independent records of air temperatures taken over both land and sea.

Climate researchers at the Met Office Hadley Centre are working to reduce the biases in the temperature datasets. In the past two years, many hundreds of thousands of observations have been keyed in from hand-written log books that were kept aboard ships in the UK navy, particularly for the periods of sparse marine coverage, such as the two World War periods.

Although fixing the drop is unlikely to radically alter our understanding of climate change, having a more accurate record of the real temperature change during the mid-20th century could provide insight into the more subtle mechanisms that caused the early rise in temperatures to the 1920s and the subsequent flattening of the temperature curve that lasted into the early 1970s.

Marine temperatures are much more prone to systematic biases arising from changes in the way the measurements are taken and the platforms used, than are land air temperatures. For example, since the 1970s, sea surface temperatures have been estimated from satellites, but these need considerable adjustment (sometimes in excess of 2 deg C) to be comparable with ship and buoy measurements. The satellite sees only the top millimetre of the ocean surface, while traditional ship-based sampling sees the top few metres. A change is gradually taking place across the world's oceans in the way sea surface temperature measurements are made during the last ten years: the number of ship-based measurements has reduced slightly, but there is a dramatic increase in the number of measurements coming from automatic measurements taken on fixed and drifting buoys. Work is underway to determine the size of the difference between the ships and buoys, as the bias between the two could be of the same order as that in the 1940s.

Kevin,

Odd how far and wide Cambridge seminars are advertised!

Dave Thompson has given this talk at Reading and will be here tomorrow for a similar talk. Here's an email I sent earlier to someone in London.

I'm on the Nature paper - due out end of May/early June.

Attached the draft press release as well.

Any thoughts welcome. I hope you'll see how all this could be misinterpreted!

Cheers

Phil

Chris,

David Thompson is giving a talk here tomorrow on this.

The essence of his talk will be in Nature in a few weeks time.

The skeptics will make a meal of this when it comes out, but if they did their job properly (I know this is impossible!) they would have found it. It relates to a problem with SST data in the late 1940s. The problem will get corrected for at some point. SSTs need adjusting as there must be from buckets for the period from Aug45 by about 0.3 gradually reducing to a zero adjustment by about the mid-1960s. The assumption was that after WW2 they were all intake measurements and didn't need adjusting.

This will reduce the 1940-1970 cooling in NH temps. Explaining the cooling with sulphates won't be quite as necessary. It won't change century-scale trends.

There is much more of an interesting thing going on now. With all the drifters now deployed measuring SST, the % of ships making measurements is now only about 40% of the total - whereas it was all in the late 1990s. In comparisons over the last 10 years it seems that ships measure SSTs about 0.1-0.2 higher than the drifters/buoys. As the 61-90 base period is ship based, it means recent anomalies are colder than they should be (by about 0.1 for global mean T in the last 2 years).

Working on a press release with MOHC about the Nature paper.

We've been through page proofs with Nature, but these don't yet include figs.

I can send these when we get them.

Cheers

Phil

At 15:02 28/04/2008, you wrote:

Phil

Any idea what this is about?

Kevin

----- Original Message -----

Subject: Talk on Understanding 20th C surface temperature variability

Date: Mon, 28 Apr 2008 12:00:36 +0100 (BST)

From: Leverhulme Climate Symposium [1]<climate@esc.cam.ac.uk>

Reply-To: [2]climate@esc.cam.ac.uk

To: [3]climate@esc.cam.ac.uk

Dear Colleagues,

David Thompson of Colorado State University will be speaking in Cambridge on 22 May on 'Understanding 20th century surface temperature variability'. His talk will 'highlight a glaring but previously overlooked error in the time series of global-mean temperatures', see full abstract below. (For those too far from Cambridge to attend, this is for information and interest).

The prevailing view of 20th century temperature variability is that the Earth warmed from ~1910 to 1940, cooled slightly from ~1940 to 1970, and warmed markedly from ~1970 onward. In this talk I will exploit a physically-based filtering methodology which provides an alternative interpretation of 20th century global-mean temperature variability. The results clarify the consistency between the century- long monotonic rise in greenhouse gases and global-mean temperatures, provide new insights into the climatic impact of volcanic eruptions, and highlight a glaring but previously overlooked error in the time series of global-mean temperatures.

Thursday 22 May, 2.15 pm in Meeting Room 2, Centre for Mathematical Sciences (between Clarkson and Madingley Roads)

--

Kevin E. Trenberth e-mail: [4]trenbert@ucar.edu
Climate Analysis Section, [5]www.cgd.ucar.edu/cas/trenbert.html
NCAR
P. O. Box 3000, (303) 497 1318
Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ

UK

References

1. <mailto:climate@esc.cam.ac.uk>
2. <mailto:climate@esc.cam.ac.uk>
3. <mailto:climate@esc.cam.ac.uk>
4. <mailto:trenbert@ucar.edu>
5. <http://www.cgd.ucar.edu/cas/trenbert.html>

From: Ben Santer <santer1@llnl.gov>
To: g.mcgregor@auckland.ac.nz
Subject: Re: JOC-08-0098 - International Journal of Climatology
Date: Mon, 05 May 2008 19:32:12 -0700
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Glenn,

This is a little disappointing. We decided to submit our paper to IJoC in order to correct serious scientific errors in the Douglass et al. IJoC paper. We believe that there is some urgency here. Extraordinary claims are being made regarding the scientific value of the Douglass et al. paper, in part by co-authors of that paper. One co-author (S. Fred Singer) has used the findings of Douglass et al. to buttress his argument that "Nature not CO2, rules the climate". The longer such erroneous claims are made without any form of scientific rebuttal, the more harm is caused.

In our communications with Dr. Osborn, we were informed that the review process would be handled as expeditiously as possible. Had I known that it would take nearly two months until we received a complete set of review comments, I would not have submitted our paper to IJoC.

With best regards,

Ben Santer

g.mcgregor@auckland.ac.nz wrote:

> 05-May-2008

>

> JOC-08-0098 - Consistency of Modelled and Observed Temperature Trends in the Tropical Troposphere

>

> Dear Dr Santer

>

> I am hoping to have the remaining set of comments with 2 weeks of so. As soon as I have these in hand I will pass them onto to you.

>

> Best,

>

> Prof. Glenn McGregor

>

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: g.mcgregor@auckland.ac.nz
Subject: Re: JOC-08-0098 - International Journal of Climatology
Date: Tue May 6 09:19:06 2008

Hi Glenn -- I hope the slow reviewer is not one that I suggested! Sorry if it is. I'm not sure what Ben Santer expects you to do about it at this stage; I guess you didn't expect such a lengthy article... I've not seen it, but Phil Jones told me it ran to around 90 pages! Hope all's well in NZ. Tim

At 03:32 06/05/2008, Ben Santer wrote:

Dear Glenn,

This is a little disappointing. We decided to submit our paper to IJoC in order to correct serious scientific errors in the Douglass et al. IJoC paper. We believe that there is some urgency here. Extraordinary claims are being made regarding the scientific value of the Douglass et al. paper, in part by co-authors of that paper. One co-author (S. Fred Singer) has used the findings of Douglass et al. to buttress his argument that "Nature not CO2, rules the climate". The longer such erroneous claims are made without any form of scientific rebuttal, the more harm is caused.

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With best regards,

Ben Santer

g.mcgregor@auckland.ac.nz wrote:

05-May-2008

JOC-08-0098 - Consistency of Modelled and Observed Temperature Trends in the Tropical Troposphere

Dear Dr Santer

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Best,

Prof. Glenn McGregor

--

Benjamin D. Santer

Program for Climate Model Diagnosis and Intercomparison

Lawrence Livermore National Laboratory

P.O. Box 808, Mail Stop L-103

Livermore, CA 94550, U.S.A.

Tel: (925) 422-2486

FAX: (925) 422-7675

email: santer1@llnl.gov

From: Phil Jones <p.jones@uea.ac.uk>

To: "Cater Sandra Mrs \(\FIN\)" <S.Cater@uea.ac.uk>, "Meardon Fiona Miss \(\RBS\)" <F.Meardon@uea.ac.uk>, "Meldrum Alicia Dr \(\RBS\)" <A.Meldrum@uea.ac.uk>

Subject: RE: Request for Cost date for DOE Grant

Date: Wed May 7 12:42:32 2008

Sandra,

These will be fine. Keep a note of these in the file to check against when the later claims are made.

Cheers

Phil

At 12:08 07/05/2008, Cater Sandra Mrs \(\FIN\) wrote:

Dear Phil,

I have reconciled the account to date and propose to send the following figures all in US\$

| | |
|-------------------------|--------------|
| Received to date | 1,589,632.00 |
| 2007/08 | |
| Staff buyout Jones | 71,708.00 |
| Cons actual to date | 9,650.00 |
| Travel actual to date | 6,940.00 |
| Indirect costs on above | 66,200.00 |
|
 | |
| Total to 30/04/08 | 1,744,130.00 |

April to June 08

| | |
|----------------|--|
| Staff Jones | 19,290.00 |
| Cons | 10,550.00 includes some of the previous year under spend |
| Travel | 3,840.00 as above |
| Indirect costs | 25,200.00 |
| Total | 58,880.00 |

July to Sep 08

| | |
|----------------|---|
| Staff Jones | 19,290.00 |
| Cons | 3,200.00 includes some previous under spend |
| Travel | 4,500.00 as above |
| Indirect costs | 20,200.00 |
| Total | 47,190.00 |

These figures keep within the allocated budget. Please let me know if you agree this I will e-mail Catherine.

Regards
Sandra

Sandra M Cater
Office Supervisor
Finance Research
Registry Building
University of East Anglia
Norwich
NR 4 7TJ
Tel : 0044-1603-593216
Fax : 0044-1603-593860
e-mail: s.cater@uea.ac.uk

From: Phil Jones [[1]mailto:p.jones@uea.ac.uk]
Sent: Thursday, May 01, 2008 9:44 AM
To: Meardon Fiona Miss (RBS); Meldrum Alicia Dr (RBS); Cater Sandra Mrs (FIN)
Subject: Fwd: Request for Cost date for DOE Grant

Alicia, Fiona, Sandra,

Hope this doesn't take too long to work out and send to Catherine.

If you need any help let me know.

Cheers

Phil

X-Server-Uid: F0E03B37-707C-4DCF-A928-7EECE47830F0

Subject: Request for Cost date for DOE Grant

Date: Wed, 30 Apr 2008 13:44:38 -0500

X-MS-Has-Attach:

X-MS-TNEF-Correlator:

Thread-Topic: Request for Cost date for DOE Grant

Thread-Index: Aciq8j7EoosKEL4QQ9OUgErATV9ppA==

From: "Richardson, Catherine" <Catherine.Richardson@ch.doe.gov>

To: p.jones@uea.ac.uk

X-OriginalArrivalTime: 30 Apr 2008 18:44:39.0681 (UTC)

FILETIME=[3F0EEF10:01C8AAF2]

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X-Bayes-Prob: 0.0001 (Score 0, tokens from: @ @RPTN, f028)

X-Spam-Score: 0.00 () [Tag at 5.00] HTML_MESSAGE

X-CanItPRO-Stream: UEA:f028 (inherits from UEA:10_Tag_Only,UEA:default,base:default)
X-Canit-Stats-ID: 2299780 - 2e3481b4882c (trained as not-spam)
X-Antispam-Training-Forget:
[2]https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=f
X-Antispam-Training-Nospam:
[3]https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=n
X-Antispam-Training-Spam: [4]https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=s
X-Scanned-By: CanIt (www . roaringpenguin . com) on 139.222.131.184
X-UEA-Spam-Score: 0.0
X-UEA-Spam-Level: /
X-UEA-Spam-Flag: NO
Fiona Meardon
East Anglia University

Dear Grantee:

SUBJECT: REQUEST FOR COST INFORMATION

In accordance with the Presidents Management Agenda, there has been and continues to be a Government-wide movement to ensure that the American people receive better results for their money. Thus, all government entities are striving to improve the quality, accuracy, and timeliness of financial information regarding the results of operations and overall performance. As we seek to accomplish this goal, we are requesting cost data from our Grant recipients that have received significant financial assistance monies from the Department of Energy Office of Science - Chicago Office. The requested information, summarized below, will assist in our continuing efforts to ensure that we produce accurate and timely financial information. We need your assistance in the following areas:

A. Providing Cumulative Cost Data:

For most of the awards administered by the Office of Science - Chicago Office, there is a financial reporting requirement to submit cost data on the Financial Status Report (SF-269) at the end of the project period. Currently, there is no requirement for you to submit cost data on a more frequent basis. However, in order to achieve our goal of improving the quality, accuracy, and timeliness of our financial information, the Departments external independent auditors have insisted that we confirm cumulative cost balances with Grantees that have received significant financial assistance monies at least annually. For each grant award listed, we request that you provide the following:

DOE Grant Award(s) No.

1.

Cumulative actual Cost through March 31, 2008
(from inception of the award):

2.

Your best estimate for costs to be incurred for April through June 30, 2008:

3.

Your best estimate for costs to be incurred for July through September 30, 2008:

We are not requiring a specific or formal format for the requested information. Instead, please e-mail your cost data as requested above for each identified grant award to Catherine Richardson at [5]catherine.richardson@ch.doe.gov. Please direct your comments and/or questions to Ms. Richardson at 630/252-6276.

B. Requesting Advances and Reimbursements:

Consistent with our efforts to improve the Departments financial information, we are reviewing significant unpaid balances on our financial assistance awards as well as any credit balances on the Quarterly Federal Cash Transactions Reports (SF-272) which would indicate a delay between the performance of the work and the requests for reimbursements submitted to us from your organization. The Departments external auditors and other users of financial information are concluding that these unpaid balances may not be used and possibly should be withdrawn. Therefore, we request that you:

- Review your existing procedures for requesting advances and reimbursements from DOE; and
- Ensure that the delay between the performance of work and subsequent reimbursements is as minimal as administratively possible.

If this situation does not apply to your organization, no action is required on your part.

We appreciate your support in this important initiative. If you have any questions, please call Cornell Williams at 630/252-2394 or e-mail him at [6]cornell.williams@ch.doe.gov.

Catherine Richardson
Staff Accountant
US Department of Energy
Office of Science - Chicago Office
(630)252-6276

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

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References

1. <mailto:p.jones@uea.ac.uk>
2. <https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=f>
3. <https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=n>
4. <https://canit.uea.ac.uk/b.php?i=2299780&m=2e3481b4882c&c=s>
5. <mailto:catherine.richardson@ch.doe.gov>
6. <mailto:cornell.williams@ch.doe.gov>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@meteo.psu.edu>, "raymond s. bradley" <rbradley@geo.umass.edu>
Subject: A couple of things
Date: Fri May 9 09:53:41 2008
Cc: "Caspar Ammann" <ammann@ucar.edu>

Mike, Ray, Caspar,

A couple of things - don't pass on either.

1. Have seen you're RC bet. Not entirely sure this is the right way to go, but it will drum up some discussion.

Anyway Mike and Caspar have seen me present possible problems with the SST data (in the 1940s/50s and since about 2000). The first of these will appear in Nature on May 29. There should be a News and Views item with this article by Dick Reynolds. The paper concludes by pointing out that SSTs now (or since about 2000, when the effect gets larger) are likely too low. This likely won't get corrected quickly as it really needs more overlap to increase confidence. Bottom line for me is that it appears SSTs now are about 0.1 deg C too cool globally. Issue is that the preponderance of drifters now (which measure SST better but between 0.1 and 0.2 lower than ships) mean anomalies are low relative to the ship-based 1961-90 base.

This also means that the SST base the German modellers used in their runs was likely too warm by a similar amount. This applies to all modellers, reanalyses etc. There will be a lot of discussion of the global T series with people saying we can't even measure it properly now.

The 1940s/50s problem with SSTs (the May 29 paper) also means there will be warmer SSTs for about 10 years. This will move the post-40s cooling to a little later - more in line with higher sulphate aerosol loading in the late 50s and 1960s/70s. The paper doesn't provide a correction. This will come, but will include the addition of loads more British SSTs for WW2, which may very slightly cool the WW2 years. More British SST data have also been digitized for the late 1940s. Budget constraints mean that only about half the RN log books have been digitized. Emphasis has been given to the South Atlantic and Indian Ocean log books.

As an aside, it is unfortunate that there are few in the Pacific. They have digitized all the logbooks of the ships journeys from the Indian Ocean south of Australia and NZ to Seattle for refits. Nice bit of history here - it turns out that most of the ships are US ones the UK got under the Churchill/Roosevelt deal in early 1940. All the RN bases in South Africa, India and Australia didn't have parts for these ships for a few years. So the German group would be stupid to take your bet. There is a likely ongoing negative volcanic event in the offing!

2. You can delete this attachment if you want. Keep this quiet also, but this is the person who is putting in FOI requests for all emails Keith and Tim

have written and received re Ch 6 of AR4. We think we've found a way around this.

I can't wait for the Wengen review to come out with the Appendix showing what that 1990 IPCC Figure was really based on.

The Garnaut review appears to be an Australian version of the Stern Report.

This message will self destruct in 10 seconds!

Cheers

Phil

Prof. Phil Jones

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School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich Email p.jones@uea.ac.uk

NR4 7TJ

UK

From: Phil Jones <p.jones@uea.ac.uk>
To: "raymond s. bradley" <rbradley@geo.umass.edu>
Subject: Re: A couple of things
Date: Fri May 9 17:04:16 2008

Hi Ray,

Press release has been being written!

I can't seem to find a meeting to go to when the paper comes out!

Moorea was good - hope you'll be able to get to Athens!

Cheers

Phil

At 16:56 09/05/2008, you wrote:

Hi Phil:

I think you should issue your own carefully-worded press release, stating explicitly what your results DO NOT mean, as well as what they do...otherwise you will spend the next few weeks trying to undo a lot of unwanted press coverage.

Hope all is well with you....we need to get together at some place...sorry I missed Tahiti!

ray

At 04:53 AM 5/9/2008, you wrote:

Mike, Ray, Caspar,

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1. Have seen you're RC bet. Not entirely sure this is the right way to go, but it will drum up some discussion.

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Phil

Prof. Phil Jones

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Raymond S. Bradley

Director, Climate System Research Center*

Department of Geosciences, University of Massachusetts

Morrill Science Center

611 North Pleasant Street

AMHERST, MA 01003-9297

Tel: 413-545-2120

Fax: 413-545-1200

*Climate System Research Center: 413-545-0659

< [1]<http://www.paleoclimate.org>>

Paleoclimatology Book Web Site: [2]<http://www.geo.umass.edu/climate/paleo/html>
Publications (download .pdf files):

[3]<http://www.geo.umass.edu/faculty/bradley/bradleypub.html>

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References

1. <http://www.paleoclimate.org/>
2. <http://www.geo.umass.edu/climate/paleo/html>
3. <http://www.geo.umass.edu/faculty/bradley/bradleypub.html>

From: David Helms <David.Helms@noaa.gov>
To: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: Second review of IJoC paper
Date: Tue, 13 May 2008 12:22:13 -0400
Cc: santer1@llnl.gov, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <>wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, ssolomon@frii.com, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>, Bruce Baker <Bruce.Baker@noaa.gov>, David Helms <David.Helms@noaa.gov>, William R Moninger <William.R.Moninger@noaa.gov>, Bradley Ballish <Bradley.Ballish@noaa.gov>, Ralph Petersen <ralph.petersen@ssec.wisc.edu>, "Grooters, Frank" <Frank.Grooters@knmi.nl>, Carl Weiss <Carl.Weiss@noaa.gov>, Michael Berechree <M.Berechree@bom.gov.au>

<x-flowed>

Hi Tom,

I believe NCEP has found that, generally speaking, the AMDAR/MDCRS and radiosonde temperatures are treated in a similar fashion in assimilation. Like radiosonde which has varying performance from vendor to vendor, there are differences in performance between aircraft/series and temperature probes. Brad Ballish just had a paper approved for publication (in BAMS?) that identifies the performance differences between air carriers, aircraft type, and aircraft series. Unfortunately, we only know how the data compare with the model guess, but not necessarily absolute "truth". Hopefully Brad can share his paper with this distribution. Bill Moninger and Ralph Petersen may also have published recent papers on this issue they can share. Ralph has published papers that compare near simultaneously launched of Vaisala RS-92 sondes with ascending/descending B-757 aircraft, showing good data agreement.

One should be mindful of the potential advantages of including AMDAR data as a climate resource in addition to radiosonde.

1. Data has been available in quantity since 1992
2. Data does not have the radiation issue as the TAT probe is shielded
3. Data are available at all local times, nearly 24*7*365, at hundreds of major airports internationally, thereby supporting the climate diurnal temperature problem
4. All NMCs keep databases of individual aircraft bias, based on recent performance of the each aircraft's data verses the model guess. These information would be very useful in considering candidate aircraft for a "climate quality" long term database for AMDAR temperature data

I suspect that the reason why AMDAR data have not been used to track atmospheric change is because no-one in the climate community has ever made an effort to use these data. Availability of radiosonde data in the tropics (e.g. South America and Africa) is problematic. In response,

EUCOS/E-AMDAR has been adding data collection over Africa using Air France, British Airways, and Lufthansa aircraft. I have proposed expanding the U.S. data collection to include the Caribbean and South America regions from United, Delta, Continental, etc, aircraft, but have not received support for this expansion. WMO AMDAR Panel is moving to add additional regional AMDAR Programs in the developing countries, similar to the successful expansion in eastern Asia.

AMDAR data are not a replacement for radiosonde, but these data certainly can add to the climate record if the data are properly processed/QC'd.

Regards,

Dave Helms

Thomas.R.Karl wrote:

> Ben,
>
> Regarding the last comment by Francis -- Commercial aircraft data have
> not been demonstrated to be very reliable w/r to tracking changes in
> temperatures in the US. A paper by Baker a few years ago focused on US
> data showed errors in the 1C range. Not sure about the tropics and how
> many flights you could get. I have copied Bruce Baker for a copy of
> that article.
>
> Recently David Helms has been leading an effort to improve this. He
> may have more info related to global aircraft data. I will ask Bruce
> to see what data we have, just for your info.
>
> Tom
>
> P.S. Nice review by Francis, especially like his idea w/r to stat
tests.
>
>
>
> Ben Santer said the following on 5/12/2008 9:52 PM:
>> Dear folks,
>>
>> I just received the second review of our IJoC paper (see appended PDF
>> file). This was sent to me directly by the Reviewer (Francis Zwiers).
>> Francis's comments are very thorough and constructive. They are also
>> quite positive. I don't see any show stoppers. I'll work on a
>> response this week.
>>
>> The third review is still outstanding. I queried Glenn McGregor about
>> this, and was told that we can expect the final review within the
>> next 1-2 weeks.
>>
>> With best regards,
>>
>> Ben

```
>> -----
-----
>>
>> Benjamin D. Santer
>> Program for Climate Model Diagnosis and Intercomparison
>> Lawrence Livermore National Laboratory
>> P.O. Box 808, Mail Stop L-103
>> Livermore, CA 94550, U.S.A.
>> Tel: (925) 422-2486
>> FAX: (925) 422-7675
>> email: santer1@llnl.gov
>> -----
-----
>
>
> --
>
> *Dr. Thomas R. Karl, L.H.D.*
>
> */Director/*//
>
> NOAA's National Climatic Data Center
>
> Veach-Baley Federal Building
>
> 151 Patton Avenue
>
> Asheville, NC 28801-5001
>
> Tel: (828) 271-4476
>
> Fax: (828) 271-4246
>
> Thomas.R.Karl@noaa.gov <mailto:Thomas.R.Karl@noaa.gov>
>
</x-flowed>
```

From: C.Goodess@uea.ac.uk
To: p.jones@uea.ac.uk, t.osborn@uea.ac.uk
Subject: [Fwd: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01]
Date: Sat, 17 May 2008 12:06:18 +0100 (BST)

----- Original Message -----

Subject: [Fwd: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01]
From: f034@uea.ac.uk
Date: Sat, May 17, 2008 12:04 pm
To: p.jones@uea.ac.u
t.osborn@uea.ac.uk

Can we meet on Monday to discuss this and hear from Phil what was decided at the London meeting? I'll be in late Monday (waiting for someone to look at my leaking roof) - so maybe early afternoon. I'm going down to London early evening and will be at Chelsea on tuesday. Good to see Saffron is getting some publicity!

Clare

----- Original Message -----

Subject: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01
From: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>
Date: Fri, May 16, 2008 9:06 am
To: "Jim Hall" <jim.hall@newcastle.ac.uk>
"C G Kilsby" <c.g.kilsby@newcastle.ac.uk>
"Mark New" <mark.new@ouce.ox.ac.uk>
ana.lopez@ouce.ox.ac.uk
"Anthony Footitt" <a.footitt@uea.ac.uk>
"Suraje Dessai" <s.dessai@uea.ac.uk>
"Phil Jones" <p.jones@uea.ac.uk>
"Clare Goodess" <C.Goodess@uea.ac.uk>
t.osborn@uea.ac.uk
Cc: "McSweeney, Robert" <Rob.Mcsweeney@atkinsglobal.com>
"Arkell, Brian" <Brian.Arkell@atkinsglobal.com>
"Sene, Kevin" <Kevin.Sene@atkinsglobal.com>

Dear all,

Please find attached the final tender pack for the Environment Agency bid. The tasks have been re-jigged, with the main change being a broadening of flood risk management to flood and coastal erosion risk management (FCERM). This means a wider audience to include all operating authorities, and the best practice guidance required (new Task 11) is now substantial element, to include evaluation of FCERM climate change adaptation, case studies and provision of evidence to help upgrade the FCDPAG3 Supplementary Note.

We have just one week to finish this tender, as it must be posted on Friday 23rd. We are putting together the bid document, which we'll circulate on Monday 19th, but in the meantime, and by the end of Tuesday 20th, I need everyone to send information (as indicated in brackets) to support the following structure:

- + Understanding of the tender
- + Methodology and programme (methodology for tasks / sub-tasks - see below - and timing)
- + Project team, including individual and corporate experience (who you are putting forward, pen portraits, corporate case studies)
- + Financial and commercial (day rates and number of days; please also highlight potential issues with the T&Cs e.g. IPR)
- + Health & Safety, Quality and Environmental Management
- + Appendices (full CVs, limited to 6 pages)

Please send to me and Rob McSweeney. The information I have already e.g. on day rates, core pen portraits etc will go straight into the version we're working on, so no need to re-send.

In terms of tasks (new nos.), the following organisation is suggested based on what has been noted to date:

Task 1 (Inception meeting and reporting) Atkins, supported by lead representatives of partners

Task 2 (Project board meetings) Atkins, supported by lead representatives of partners

Task 3 (Analysis of user needs) Atkins with Tyn@UEA and OUCE, plus Futerra depending on style

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Note that Futerra is a communications consultancy, specialising in sustainability, who will input on workshops and on the guidance documents.

I'll be in touch again early next week.

Best wishes,

Geoff

Geoff Darch

Senior Consultant
Water and Environment
ATKINS

Broadoak, Southgate Park, Bakewell Road, Orton Southgate, Peterborough,
PE2 6YS, UK

Tel: +44 (0) 1733 366969

Fax: +44 (0) 1733 366999

Mobile: +44 (0) 7834 507590

E-mail: geoff.darch@atkinsglobal.com

Web: www.atkinsglobal.com/climate_change

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Consider the environment. Please don't print this e-mail unless you really need to.

From: Phil Jones <p.jones@uea.ac.uk>
To: Clare Goodess <C.Goodess@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: [Fwd: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01]
Date: Mon May 19 12:36:47 2008

OK
Phil

At 11:59 19/05/2008, Clare Goodess wrote:

OK . 2 pm - my office?
Clare

At 08:59 19/05/2008, Phil Jones wrote:

OK for me too.

At 08:27 19/05/2008, Tim Osborn wrote:

Hi,
yes this PM is fine with me,
Tim

----- Original Message -----

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From: f034@uea.ac.uk

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To: "Jim Hall" <jim.hall@newcastle.ac.uk>
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ana.lopez@ouce.ox.ac.uk
"Anthony Footitt" <a.footitt@uea.ac.uk>
"Suraje Dessai" <s.dessai@uea.ac.uk>
"Phil Jones" <p.jones@uea.ac.uk>
"Clare Goodess" <C.Goodess@uea.ac.uk>
t.osborn@uea.ac.uk

Cc: "McSweeney, Robert" <Rob.Mcsweeney@atkinglobal.com>

"Arkell, Brian" <Brian.Arkell@atkinglobal.com>

"Sene, Kevin" <Kevin.Sene@atkinglobal.com>

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Best wishes,

Geoff

Geoff Darch

Senior Consultant

Water and Environment

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PE2 6YS, UK

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6. <http://www.cru.uea.ac.uk/~clareg/clare.htm>

From: Phil Jones <p.jones@uea.ac.uk>

To: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>, "Jim Hall" <jim.hall@newcastle.ac.uk>, "C G Kilby" <c.g.kilby@newcastle.ac.uk>, "Mark New" <mark.new@ouce.ox.ac.uk>, <ana.lopez@ouce.ox.ac.uk>, "Anthony Footitt" <a.footitt@uea.ac.uk>, "Surajje Dessai" <s.dessai@uea.ac.uk>, "Clare Goodess" <C.Goodess@uea.ac.uk>, <t.osborn@uea.ac.uk>

Subject: Re: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01

Date: Mon May 19 15:35:54 2008

Cc: "McSweeney, Robert" <Rob.Mcsweeney@atkinsglobal.com>, "Arkell, Brian" <Brian.Arkell@atkinsglobal.com>, "Sene, Kevin" <Kevin.Sene@atkinsglobal.com>

Geoff,

Clare is off to Chelsea - back late tomorrow. We (Clare, Tim and me)

have had a brief meeting. Here are some thoughts and questions we had.

1. Were we going to do two sets of costings?
2. Those involved in UKCIP08 (both doing the work and involved in the SG) have signed confidentiality texts with DEFRA. Not sure how these affect access to the headline messages in the drafts we're going to be looking at over the next few months. Also not sure how these will affect the UKCIP workshops that are coming up before the launch.
3. We then thought about costs for the CRU work. We decided on 25K for all CRU work. At £500 per day this comes to 50 days. We then split this into the tasks: 5 - 5 days, 6 - 5 days, 7 - 30 days, 10/11 - 5 days, which leaves 5 more days for meetings. Assumed the 25K was without travel to the meetings.
4. On CVs and pen portraits. Clare will send one before she leaves. Are what you have for Tim and me OK?
5. Some thoughts on Tasks 6 and 7

Task 6 - assumed this was mostly Newcastle. Tim's work on rainfall extremes could be fed in, and we can do something on non-rainfall variables. Assume also you expect us to do waves, but not sure what we can do. It seems as though sea level has become waves?

Task 7 - assumed here Newcastle (Chris/Hayley) would be doing something on blocking (large-scale variability). Oxford would do the final bit on conceptual representation

of emissions and climate system and sensitivities, so based on GCMs.

This leaves CRU for the other three, which we base mainly on the 11 RCM runs, which we can access through LINK. We could also use ENSEMBLES runs for the others, but these would be RCMs. They seem more relevant for the sorts of scales UKCOP08 is working at.

All just a few thoughts at this time.

Can you send the UKWIR bid that went off, so we have a copy?

Cheers

Phil

At 09:06 16/05/2008, Darch, Geoff J wrote:

Dear all,

Please find attached the final tender pack for the Environment Agency bid. The tasks have been re-jigged, with the main change being a broadening of flood risk management to flood and coastal erosion risk management (FCERM). This means a wider audience to include all operating authorities, and the best practice guidance required (new Task 11) is now substantial element, to include evaluation of FCERM climate change adaptation, case studies and provision of evidence to help upgrade the FCDPAG3 Supplementary Note.

We have just one week to finish this tender, as it must be posted on Friday 23rd. We are putting together the bid document, which we'll circulate on Monday 19th, but in the meantime, and by the end of Tuesday 20th, I need everyone to send information (as indicated in brackets) to support the following structure:

- + Understanding of the tender
- + Methodology and programme (methodology for tasks / sub-tasks - see below - and timing)
- + Project team, including individual and corporate experience (who you are putting forward, pen portraits, corporate case studies)
- + Financial and commercial (day rates and number of days; please also highlight potential issues with the T&Cs e.g. IPR)
- + Health & Safety, Quality and Environmental Management
- + Appendices (full CVs, limited to 6 pages)

Please send to me and Rob McSweeney. The information I have already e.g. on day rates, core pen portraits etc will go straight into the version we're working on, so no need to re-send.

In terms of tasks (new nos.), the following organisation is suggested based on what has been noted to date:

Task 1 (Inception meeting and reporting) Atkins, supported by lead representatives of partners

Task 2 (Project board meetings) Atkins, supported by lead representatives of partners

Task 3 (Analysis of user needs) Atkins with Tyn@UEA and OUCE, plus Futerra depending on style

Task 4 (Phase 2 programme) Atkins, supported by all

Task 5 (Interpret messages from UKCIP08 projections) CRU, OUCE and Newcastle, with Atkins advice on sectors

Task 6 (Development of business specific projections) Newcastle and CRU, with Atkins advice on policy and ops

Task 7 (Putting UKCIP08 in context) CRU, Newcastle and OUCE

Task 8 (User guidance) Atkins, Tyn@UEA, Futerra

Task 9 (Pilot studies) Atkins, Newcastle, OUCE, Tyn@UEA

Task 10 (Phase 3 programme) Atkins, supported by all

Task 11 (Best Practice Guidance for FCERM) Newcastle and Atkins, with CRU

Task 12 (Awareness raising events) Atkins, key experts, Futerra (perhaps as an option as EA are quite specific here)

Task 13 (Training events) Atkins and Futerra

Note that Futerra is a communications consultancy, specialising in sustainability, who will input on workshops and on the guidance documents.

I'll be in touch again early next week.

Best wishes,

Geoff

Geoff Darch

Senior Consultant

Water and Environment

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From: Phil Jones <p.jones@uea.ac.uk>
To: mann@psu.edu
Subject: Re: Thompson et al paper
Date: Thu May 22 09:28:52 2008
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Mike, Gavin,

OK - as long as you're not critical and remember the embargo. I'll expect Nature will be sending the paper around later today to the press embargoed till the middle of next week.

Attached is the pdf. This is the final one bar page and volume numbers. Also attached is our latest draft press release. This is likely OK except for the last paragraph which we're still working on. There will also be a News and Views item from Dick Reynolds and a Nature news piece from Quirin Schiermeier. I don't have either of these. I did speak to Quirin on Tuesday and he's also spoke to Dave and John. It took me a while to explain the significance of the paper. I hope to get these later two items before I might have to do any interviews early next week. We have a bank holiday on Monday in the UK. The press release will go out jointly from the Met Office and UEA - not sure exactly when.

Potentially the key issue is the final Nature sentence which alludes to the probable underestimation of SSTs in the last few years. Drifters now measuring SSTs dominate by over 2 to 1 cf ships. Drifters likely measure SSTs about 0.1 to 0.2 deg C cooler than ships, so we could be underestimating SSTs and hence global T. I hope Dick will discuss this more. It also means that the 1961-90 average SST that people use to force/couple with models is slightly too warm. Ship-based SSTs are in decline - lots of issues related to the shipping companies wanting the locations of the ships kept secret, also some minor issues of piracy as well. You might want to talk to Scott Woodruff more about this.

A bit of background. Loads more UK WW2 logs have been digitized and these will be going or have gone into ICOADS. These logs cover the WW2 years as well as the late 1940s up to about 1950. It seems that all of these require bucket corrections. My guess will be that the period from 1945-49 will get raised by up to 0.3 deg C for the SSTs, so about 0.2 for the combined. In digitizing they have concentrated on the South Atlantic/Indian Ocean log books.

[1]http://brohan.org/hadobs/digitised_obs/docs/ and click on SST to see some comparisons.

The periods mentioned here don't seem quite right as more later 1940s logs have also been digitized. There are more log books to digitize for WW2 - they have done about half of those not already done.

If anyone wonders where all the RN ships came from, many of those in the S. Atlantic/indian

oceans were originally US ships. The UK got these through the Churchill/Roosevelt deal in 1939/40.

Occasionally some ships needed repairs and the UK didn't have the major parts, so this will explain the voyages of a few south of OZ and NZ across the Pacific to Seattle and then back into the fray.

ICOADS are looking into a project to adjust/correct all their log books.

Also attaching a ppt from Scott Woodruff. Scott knows who signed this!

If you want me to look through anything then email me.

I have another paper just accepted in JGR coming out on Chinese temps and urbanization. This will also likely cause a stir. I'll send you a copy when I get the proofs from AGU. Some of the paper relates to the 1990 paper and the fraud allegation against Wei-Chyung Wang. Remind me on this in a few weeks if you hear nothing.

Cheers

Phil

PS CRU/Tyndall won a silver medal for our garden at the Chelsea Flower Show - the theme of the show this year was the changing climate and how it affects gardening. Clare Goodess was at the garden on Tuesday. She said she never stopped for her 4 hour stint of talking to the public - only one skeptic. She met the environment minister.

She was talking about the high and low emissions garden. The minister (Phil Woolas) seemed to think that the emissions related to the ability of the plants to extract CO2 from the atmosphere! He'd also not heard of the UHI! Still lots of education needed.

PPS Our web server has found this piece of garbage - so wrong it is unbelievable that Tim Ball wrote a decent paper in Climate Since AD 1500. I sometimes wish I'd never said this about the land stations in an email. Referring to Alex von Storch just shows how up to date he is.

[2]<http://canadafreepress.com/index.php/article/3151>

At 20:12 21/05/2008, Michael Mann wrote:

Hi Phil,

Gavin and I have been discussing, we think it will be important for us to do something on the Thompson et al paper as soon as it appears, since its likely that naysayers are going to do their best to put a contrarian slant on this in the blogosphere.

Would you mind giving us an advance copy. We promise to fully respect Nature's embargo (i.e., we wouldn't post any article until the paper goes public) and we don't expect to in any way be critical of the paper. We simply want to do our best to help make sure that the right message is emphasized.

thanks in advance for any help!

mike

--

Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075
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2. <http://canadafreepress.com/index.php/article/3151>
3. <mailto:mann@psu.edu>
4. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>
Subject: RE: Probabilistic information to inform EA decision making - Draft Bid
Date: Thu May 22 17:18:09 2008

Geoff,

Hopefully this will do. No narrative.

Off home now. I'll look through anything you send tomorrow.

Exam scripts to mark tonight.

Cheers

Phil

At 17:00 22/05/2008, you wrote:

Phil,

The only CV we have for you is a few years old. Can you send a more up to date one (6 pages max).

Thanks,

Geoff

From: Phil Jones [[1]mailto:p.jones@uea.ac.uk]

Sent: 22 May 2008 13:07

To: Darch, Geoff J

Cc: Clare Goodess; t.osborn@uea.ac.uk; McSweeney, Robert

Subject: RE: Probabilistic information to inform EA decision making - Draft Bid

Geoff, Rob,

Will you be sending another version around at some time?

I can't recall where the idea of two sets of costings came from.

Here are some more thoughts

Related EA work

Drought work

Jones, P.D., Leadbetter, A., Osborn, T.J. and Bloomfield, J.P., 2006: The impact of climate change on severe droughts: River-flow reconstructions and implied groundwater levels. Science Report: SC040068/SR2, Environment Agency, 58pp.

Wade, S., Jones, P.D. and Osborn, T.J., 2006: The impact of climate change on severe droughts: Implications for decision making. Science Report: SC040068/SR3, Environment Agency, 86pp.

These two bits of work related to historic records of drought on the Eden and the Ouse (Anglian).

Flows were reconstructed on a monthly basis back to 1800, and the disaggregated to daily using months with similar monthly flows in the modern record from the 1960s to the near present. The 200 years of daily flows were then put through water resource system models in the two areas to see how often drought restrictions occurred. The historic record was then perturbed for the future time slices using three different GCMs. The important aspect of this work is that for both regions the perturbed futures were no worse than the historic droughts.

On the Eden some recent droughts were the most severe and on the Ouse they were earlier in the 20th and in the 19th century. So, for all work, it is important to get a better handle on the scale of natural variability within each region.

Task 6 should not just consider the instrumental observations that UKCIP08 has looked at (i.e. since 1961).

This period will very likely cover all temperature extremes (if we forget the very cold ones), but it will be inadequate for rainfall (changes in daily, monthly and seasonal extremes).

The EA

work (above) showed a framework for dealing with the issue with respect to drought. The longer

daily precipitation record has been looked at by Tim Osborn and Douglas Maraun (see attached

pdf). Task emphasizes floods exclusively - maybe this is their responsibility and they leave

droughts up to the companies.

One aspect that we could develop within Task 6 is a simple soil moisture accounting model

using rainfall and PET and a measure of soil amount. The results from this could then be

linked with the heavy rainfall to determine different impacts depending on antecedent conditions and time of year.

CRU's work on Task 7

We will be able to use the 11 RCMs on which the whole of UKCIP08 are based - available through LINK. MOHC have used emulation of these to build up distributions. An important aspect

is to see for seasons and variables how the 11 span the probability domain of all the emulations (where do they sit in the pdfs).

Other GCMs - this should really be RCMs. In the ENSEMBLES project we are comparing trends in reality with trends from ERA-40-forced runs of 15 different RCMs across Europe.

This will be able to show that HadRM3 is within the range of the other RCMs for

measures

of extremes in temperatures and daily and 5-day precipitation amounts. The measures here

are trends (seasonal and annual) over the period from 1961-2000.

This will also show their ability to represent current climate (61-00) not just for the means

and trends, but some extreme measures and their trends. This is also past variability as well, but I suspect they are meaning further back. We will be able to use a HadCM3 simulation with historic forcing since 1500.

Back to other work. CRANIUM is the one to refer to. BETWIXT led to CRANIUM. The other thing to add in somewhere is that the UKCIP08 WG came from EARWIG, so attaching that paper as well. There is nothing else yet.

Jones, PD, Harpham, C and Kilsby, CK, 2008: Perturbing a weather generator using factors

developed from RCM simulations. Int J. Climatol (not yet submitted).

This will get submitted. It shows that the way we are perturbing the WG for UKCIP08 works.

We do this by fitting the WG to the model present. We then perturb by using differences between model future (2080s) and model control. These perturbations are monthly. We then

run the WG and look at the daily variability in the simulations compared to the model future at the daily timescale. It works in the sense that the RCM future run is within the

range the WG simulations.

Whether the RCM future is right is another matter but the WG does what the RCM does.

Hope this helps.

Phil

At 16:56 21/05/2008, Darch, Geoff J wrote:

Phil,

Great. From CRU we need in particular project experience (case studies). At the moment we have CRANIUM, but other relevant ones would be good e.g. BETWIXT, SKCC, EA Drought work. Key is those related to probabilistic scenarios, weather generators, working with users and those with EA or Defra (or CCW) as the client.

Any further thoughts or elaboration of your input would be useful, particularly for Task 7, where it may be best to spell out what you will do.

Do you have any preference for the allocation of days between you, Clare and Tim? Also, do you want to revise your rates (for reference Jim Hall is in at £950, Chris Kilsby at £750)? They should apply until the end of the contract i.e. December 2009 and we are asked whether any discounts are available e.g. over and above a certain number of days, which could be worked in if you increased your rates. However, this is entirely up to

you!

We are still waiting on input from Oxford, Newcastle and Futerra - all promised imminently. It will be a busy day tomorrow!

Many thanks,

Geoff

From: Phil Jones [[2]mailto:p.jones@uea.ac.uk]

Sent: 21 May 2008 16:16

To: McSweeney, Robert

Cc: Clare Goodess; t.osborn@uea.ac.uk; Darch, Geoff J

Subject: Re: Probabilistic information to inform EA decision making - Draft Bid
Geoff, Rob,

I can do some work tomorrow. Can you be a little more specific?

It looks as though you need a lot. Have you got anything from anyone else?

I assume this still has to be all off by the end of Friday.

Cheers

Phil

At 14:15 20/05/2008, McSweeney, Robert wrote:

All,

Attached is an outline draft of the bid. It sets out the information we need to include, some of which is already in place.

Please could you take a look at it and forward any of the outstanding information to Geoff and me, such as

- CVs and pen portraits if you haven't already sent them (NB, CVs are in the Appendix and aren't in the attached document)
- Any relevant (corporate) project experience, case studies, etc
- Thoughts and input to the methodology section (NB, each task has been given a lead group or groups)
- General comments and suggestions

Please send comments and information as soon as you have the opportunity, the deadline is rapidly approaching!

Many thanks,

Rob

<<EA Tender_Draft.doc>>

Rob McSweeney
Assistant Scientist
MEng (Hons) MSc
Water and Environment (Water Resources Management)

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8. <http://bluepages.wsatkins.co.uk/?6875772>
9. http://www.atkinsglobal.com/terms_and_conditions/index.aspx

From: C.Goodess@uea.ac.uk
To: P.Jones@uea.ac.uk
Subject: Re: EA bid - final draft - for review by 8am Tues 27th
Date: Mon, 26 May 2008 11:44:19 +0100 (BST)
Cc: "Darch, Geoff J" <geoff.darch@atkingglobal.com>, "Phil Jones" <p.jones@uea.ac.uk>, "Clare Goodess" <c.goodess@uea.ac.uk>, t.osborn@uea.ac.uk, a.footitt@uea.ac.uk, "Suraje Dessai" <s.dessai@uea.ac.uk>, "Jim Hall" <jim.hall@newcastle.ac.uk>, "C G Kilsby" <c.g.kilsby@newcastle.ac.uk>, mark.new@ouce.ox.ac.uk, ana.lopez@ouce.ox.ac.uk, "Ed Gillespie" <ed@futura.co.uk>, "Arkell, Brian" <brian.arkell@atkingglobal.com>, "McSweeney, Robert" <rob.mcsweeney@atkingglobal.com>

Hi Geoff

Like Phil, I've just given this a quick read through and there are only a very few minor comments on the attached.

My main concern is the cost - which I have to say is much higher than I was anticipating. But we are proposing a substantial amount of analysis and work....

Thanks for all your work on this and good luck getting it off tomorrow.

Best wishes, Clare

>
> Geoff,
> After a relatively quick read through of the meat of the
> proposal, I'm sending it back with a few minor changes.
> You've done a good job of getting a lot of information
> across. I did spend a little more time on the CRU tasks,
> and there is enough detail there for review purposes.
>
> ON costs do whatever you want to CRU costs to ensure
> apparent consistency. I just hope this hasn't been pitched
> too high - but if they want the job doing well, they should be
> paying the right price.
>
> I can't think of any IPR aspects, in addition to that which Chris
> has alluded to. Chris and I will likely need to be careful as
> to what is and what is not part of the UKCIP08 WG, but we
> can address that later. At some stage - way after launch, it is
> possible that the WG within UKCIP08 could be upgraded, a bit like
> we upgrade software, but nowhere near as frequently as Bill Gates
> makes us do.
>
> Cheers
> Phil
>
>
>> Dear all,
>>

>> Please find the draft final bid and costs attached. We are working on
a
>> programme and a couple of summary tables.
>>
>> Method
>> * Please read this through to check you are ok with what is being
>> offered
>> (we'll go through to improve style etc), particularly those tasks you
>> are
>> (co-)leading.
>>
>> Costs
>> * Having initially put these in as desired, the project totalled
>> >£350k,
>> so I have adjusted a few elements to get it to a perhaps more
acceptable
>> £330k. Please check this meets your needs while at the same time
please
>> ensure that we're not duplicating time effort on shared tasks. Note I
>> have applied the 10% discount for those days beyond 10 days of an
>> individual's time for Newcastle and Atkins in line with our cost
models.
>> * I have guessed at rates for Anthony and Claire Walsh.
>> * Note that we may need to increase CRU and OUCE rates to improve
>> consistency (whilst maintaining overall costs).
>>
>> Contract
>> * The only prior right I have identified is the batch running model
that
>> Newcastle have developed. Is this one, and are there others?
>>
>> Any comments (succinct and specific please!) must be back to me by 8am
>> on
>> Tuesday morning in order to make the print run and delivery by noon.
>>
>> Thanks,
>>
>> Geoff
>>
>> Geoff Darch
>>
>> Senior Consultant
>> Water and Environment
>> ATKINS
>>
>> Broadoak, Southgate Park, Bakewell Road, Orton Southgate,
Peterborough,
>> PE2 6YS, UK
>> Tel: +44 (0) 1733 366969
>> Fax: +44 (0) 1733 366999
>> Mobile: +44 (0) 7834 507590
>> E-mail: geoff.darch@atkinglobal.com
>> Web: www.atkinglobal.com/climate_change
>>

>>
>>
>> <<EA_Probabilistic_Costs_v2_Ex.xls>> <<EA Tender_FinalDraft.doc>>
>>
>>
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>> Registered in England No. 1885586. Registered Office Woodcote Grove,
>> Ashley Road, Epsom, Surrey KT18 5BW. A list of wholly owned Atkins
Group
>> companies registered in the United Kingdom can be found at
>> http://www.atkinglobal.com/terms_and_conditions/index.aspx
>>
>> Consider the environment. Please don't print this e-mail unless you
>> really
>> need to.
>>
>

Attachment Converted: "c:\eudora\attach\EA Tender_FinalDraft2.doc"

From: Ben Santer <santer1@llnl.gov>
To: David Douglass <douglass@pas.rochester.edu>
Subject: Re: Your manuscript with Peter Thorne
Date: Tue, 27 May 2008 14:01:26 -0700
Reply-to: santer1@llnl.gov
Cc: Christy John <christy@nsstc.uah.edu>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>

<x-flowed>

Dr. Douglass:

I assume that you are referring to the Santer et al. paper which has been submitted to the International Journal of Climatology (IJoc). Despite your claims to the contrary, the Santer et al. IJoC paper is not essential reading material in order to understand the arguments advanced by Peter Thorne (in his "News and View" piece on the Allen and Sherwood "Nature Geosciences" article).

I note that you did not have the professional courtesy to provide me with any advance information about your 2007 IJoC paper, which was basically a commentary on previously-published work by myself and my colleagues. Neither I nor any of the authors of those previously-published works (the 2005 Santer et al. Science paper and the 2006 Karl et al. CCSP Report) had the opportunity to review your 2007 IJoC paper prior to its publication - presumably because you specifically requested that we should be excluded from consideration as possible reviewers.

I see no conceivable reason why I should now send you an advance copy of my IJoC paper. Collegiality is not a one-way street, Professor Douglass.

Sincerely,

Dr. Ben Santer

David Douglass wrote:

> Dear Dr Santer

>

> In a recent paper by Peter Thorne in Nature Geoscience he references a
> paper that you and he (and others) have written.

> I can not understand some parts of the Thorne paper without reading the
> Santer/Thorne reference.

> Would you please send me a copy?

>

> Sincerely;
> David Douglass

--

Benjamin D. Santer
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FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: Caspar Ammann <ammann@ucar.edu>
To: t.osborn@uea.ac.uk
Subject: Re: request for your emails
Date: Tue, 27 May 2008 17:36:26 -0600
Cc: "keith Briffa" <k.briffa@uea.ac.uk>, p.jones@uea.ac.uk

Oh MAN! will this crap ever end??

Well, I will have to properly answer in a couple days when I get a chance digging through emails. I don't recall from the top of my head any specifics about IPCC.

I'm also sorry that you guys have to go through this BS. You all did an outstanding job and the IPCC report certainly reflects that science and literature in an accurate and balanced way.

So long,

Caspar

On May 27, 2008, at 5:03 PM, Tim Osborn wrote:

Dear Caspar,

I hope everything's fine with you.

Our university has received a request, under the UK Freedom of Information law, from someone called David Holland for emails or other documents that you may have sent to us that discuss any matters related to the IPCC assessment process.

We are not sure what our university's response will be, nor have we even checked whether you sent us emails that relate to the IPCC assessment or that we retained any that you may have sent.

However, it would be useful to know your opinion on this matter. In particular, we would like to know whether you consider any emails that you sent to us as confidential.

Sorry to bother you with this,

Tim (cc Keith & Phil)

Caspar M. Ammann
National Center for Atmospheric Research
Climate and Global Dynamics Division - Paleoclimatology
1850 Table Mesa Drive
Boulder, CO 80307-3000
email: [1]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

References

1. mailto:ammann@ucar.edu

From: Phil Jones <p.jones@uea.ac.uk>
To: t.osborn@uea.ac.uk,"Palmer Dave Mr \ (LIB\)" <David.Palmer@uea.ac.uk>
Subject: Re: FW: Your Ref: FOI_08-23 - IPCC, 2007 WGI Chapter 6 Assessment Process [FOI_08-23]
Date: Wed, 28 May 2008 17:13:35 +0100
Cc: "Briffa Keith Prof \" <k.briffa@uea.ac.uk>, "Mcgarvie Michael Mr \" <m.mcgarvie@uea.ac.uk>

Dave,

Although requests (1) and (2) are for the IPCC, so irrelevant to UEA, Keith (or you Dave) could say that for (1) Keith didn't get any additional comments in the drafts other than those supplied by IPCC. On (2) Keith should say that he didn't get any papers through the IPCC process.either. I was doing a different chapter from Keith and I didn't get any. What we did get were papers sent to us directly - so not through IPCC, asking us to refer to them in the IPCC chapters. If only Holland knew how the process really worked!! Every faculty member in ENV and all the post docs and most PhDs do, but seemingly not Holland.

So the answers to both (1) and (2) should be directed to IPCC, but Keith should say that he didn't get anything extra that wasn't in the IPCC comments.

As for (3) Tim has asked Caspar, but Caspar is one of the worse responders to emails known. I doubt either he emailed Keith or Keith emailed him related to IPCC.

I think this will be quite easy to respond to once Keith is back.

From looking at these questions and the Climate Audit web site, this all relates to two papers in the journal Climatic Change. I know how Keith and Tim got access to these papers and it was nothing to do with IPCC.

Cheers

Phil

At 23:47 27/05/2008, Tim Osborn wrote:

Dear Dave,

re. David Holland's follow-up requests...

These follow-up questions appear directed more towards Keith than to me. But Keith may be unavailable for a few days due to family illness, so I'll attempt a brief response in case Keith doesn't get a chance to.

Items (1) and (2) concern requests that were made by the IPCC Technical Support Unit (hosted by UCAR in the USA) and any responses would have been sent direct to the IPCC Technical Support Unit, to the email address specified in the quote included in item (2). These requests are, therefore, irrelevant to UEA.

Item (3): we'll send the same enquiry to Ammann as we sent to our other colleagues, and let you know his response.

Item (3) also asks for emails from "the journal Climatic Change that discuss any matters in relation to the IPCC assessment process". I can confirm that I have not received any such emails or other documents. I expect that a similar answer will hold for Keith, since I cannot imagine that the editor of a journal would be contacting us about the IPCC process.

Best wishes

Tim

On Tue, May 27, 2008 6:30 pm, Palmer Dave Mr \ (LIB\) wrote:

- > Gents,
- > Please note the response received today from Mr. Holland. Could you
- > provide input as to his additional questions 1, and 2, and check with
- > Mr. Ammann in question 3 as to whether he believes his correspondence
- > with us to be confidential?
- >
- > Although I fear/anticipate the response, I believe that I should inform
- > the requester that his request will be over the appropriate limit and
- > ask him to limit it - the ICO Guidance states:
- >
- > 12. If an authority estimates that complying with a request will exceed
- > the cost limit, can advice and assistance be offered with a view to the
- > applicant refocusing the request?
- >
- > In such cases the authority is not obliged to comply with the request
- > and will issue a refusal notice. Included within the notice (which must
- > state the reason for refusing the request, provide details of complaints
- > procedure, and contain particulars of section 50 rights) could be advice
- > and assistance relating to the
- >
- > refocusing of the request, together with an indication of the
- > information that would be available within the cost limit (as required
- > by the Access Code).
- >
- > This should not preclude other 'verbal' contact with the applicant,
- > whereby the authority can ascertain the requirements of the applicant,
- > and the normal customer service standards that the authority usually
- > adopts.
- >
- >
- > And... our own Code of Practice states (Annex C, point 5)
- >
- > 5. Where the UEA is not obliged to supply the information requested
- > because the cost of doing so would exceed the "appropriate limit" (i.e.

> cost threshold), and where the UEA is not prepared to meet the
> additional costs itself, it should nevertheless provide an indication of
> what information could be provided within the cost ceiling.
>
> This is based on the Lord Chancellors Code of Practice which contains a
> virtually identical provision....
>
> In effect, we have to help the requester phrase the request in such a
> way as to bring it within the appropriate limit - if the requester
> disregards that advice, then we don't provide the information and allow
> them to proceed as they wish....
>
> I just wish to ensure that we do as much as possible 'by the book' in
> this instance as I am certain that this will end up in an appeal, with
> the statutory potential to end up with the ICO.

>
> Cheers, Dave

>
> _____

>
> From: David Holland [[1] <mailto:d.holland@theiet.org>]
> Sent: Tuesday, May 27, 2008 5:37 PM
> To: David Palmer
> Subject: Your Ref: FOI_08-23 - IPCC, 2007 WGI Chapter 6 Assessment
> Process

>
>
> Please find attached a response to your letter of 19th May 2008

>
> David Holland

>
>
>
>

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. <mailto:d.holland@theiet.org>

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>, Steven Sherwood
<Steven.Sherwood@yale.edu>
Subject: Re: David Douglass
Date: Wed May 28 17:25:27 2008
Cc: santer1@llnl.gov, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, ssolomon@frii.com, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Frank Wentz <frank.wentz@remss.com>

Ben et al,

Definitely the right response - so agree with Tom.
I have been known to disagree with him, and he's not
always right.

Submit asap !!

Cheers

Phil

At 23:48 27/05/2008, Tom Wigley wrote:

Steve et al.,

Sorry, but I agree with quick submission, but not with giving
anything to Douglass until the paper appears in print.

I guess the reason John likes 1.2 is because it agrees best
with UAH MSU -- which, as we all know, has been inspired by
and blessed by God, and so MUST be right.

Tom.

+++++

Steven Sherwood wrote:

Hi Ben,

I for one am happy with submission pronto, leaving to your
discretion the comments I
sent earlier.

I wouldn't feel too threatened by the likes of Douglass. This paper
will likely be

accepted as is upon resubmission, given the reviews, so why not just
send him a copy too

once it is ready and final.

On a related note I've heard from John Christy who stated his
opposition to the new

Allen+Sherwood article/method (who would've thought). He argues
that Leo's v1.2 dataset

is the "best" version because the later ones are contaminated by
artifacts in ERA-40 due

to Pinatubo. This argument made no sense to me on several levels
(one of which:

Pinatubo erupted almost exactly in the middle of the time period of
interest, thus

should have no impact on any linear trend). But there it is.

SS

On May 27, 2008, at 5:41 PM, Ben Santer wrote:

Dear folks,

I just wanted to alert you to an issue that has arisen in the last few days. As you

probably know, a paper by Robert Allen and Steve Sherwood was published last week in

"Nature Geoscience". Peter Thorne was asked to write a "News and Views" piece

on the Allen and Sherwood paper. Peter's commentary on Allen and Sherwood briefly

referenced our joint International Journal of Climatology (IJoC) paper. Peter discussed

this with me about a month ago, and I saw no problem with including a reference to our

IJoC paper. The reference in Peter's "News and Views" contribution is very general, and

gives absolutely no information on the substance of our IJoC paper.

At the time Peter I discussed this issue, I had high hopes that our IJoC manuscript

would now be very close to publication. I saw no reason why publication of Peter's "News

and Views" piece should cause us any concern. Now, however, it is obvious that David

Douglass has read the "News and Views" piece and wants a copy of our IJoC paper in

advance of its publication - in fact, before a final editorial decision on the paper has

been reached. Dr. Douglass has written to me and to Peter, requesting a copy of our IJoC

paper. In his letter to Peter, Dr. Douglass has claimed that failure to provide him

(Douglass) with a copy of our IJoC paper would contravene the ethics policies of the

journal "Nature".

As you can see from my reply to Dr. Douglass, I feel strongly that we should not give

him an advance copy of our paper. However, I think we should resubmit our revised

manuscript to IJoC as soon as possible. The sooner we receive a final editorial decision

on our paper, the less likely that it is that Dr. Douglass will be able to cause

problems. With your permission, therefore, I'd like to resubmit our revised manuscript

by no later than close of business tomorrow. I've incorporated most of the suggested

changes I've received from you in the past few days. My personal feeling is that we've

now reached the point of diminishing returns, and that it's more important to get the

manuscript resubmitted than to engage in further iterations about relatively minor

details. I will circulate a final version of the revised paper and the response to the reviewers later this evening.

Please let me know if resubmission by C.O.B. tomorrow is not acceptable to you.

With best regards,
Ben

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Steven Sherwood
Steven.Sherwood@yale.edu <[2]mailto:Steven.Sherwood@yale.edu>
Yale University ph:
203 432-3167
P. O. Box 208109 fax:
203 432-3134
New Haven, CT 06520-8109
[3]http://www.geology.yale.edu/~sherwood

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Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. <mailto:santer1@llnl.gov>
2. <mailto:Steven.Sherwood@yale.edu>
3. <http://www.geology.yale.edu/~sherwood>

From: Tom Wigley <wigley@ucar.edu>
To: santer1@llnl.gov
Subject: Re: Our d3* test
Date: Wed, 28 May 2008 21:58:34 -0600
Cc: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, "'Susan Solomon'" <ssolomon@al.noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>

<x-flowed>

Dear all,

Just to add a bit to Ben's notes. The conceptual problem is how to account for two different types of uncertainty in comparing a single observed trend (with temporal uncertainty) with the average of a bunch of model trends (where the uncertainty is from inter-model differences). The "old" d3 tried to do this, but failed the synthetic data test. The new d3 does this a different way (in the way that the inter-model uncertainty term is quantified). This passes the synthetic data test very well.

The new d3 test differs from DCSP07 only in that it includes in the denominator of the test statistic an observed noise term. This is by far the bigger of the two denominator terms. Ignoring it is very wrong, and this is why the DCSP07 method fails the synthetic data test.

Tom.

+++++

Ben Santer wrote:

> Dear folks,

>

> Just wanted to let you know that I did not submit our paper to IJoC.

> After some discussions that I've had with Tom Wigley and Peter Thorne,

I

> applied our d1*, d2*, and d3* tests to synthetic data, in much the same

> way that we applied the DCPS07 d* test and our original "paired trends"

> test (d) to synthetic data. The results are shown in the appended

Figure.

>

> Relative to the DCPS07 d* test, our d1*, d2*, and d3* tests of

> hypothesis H2 yield rejection rates that are substantially

> closer to theoretical expectations (compare the appended Figure with

> Figure 5 in our manuscript). As expected, all three tests show a

> dependence on N (the number of synthetic time series), with rejection

> rates decreasing to near-asymptotic values as N increases. This is

> because the estimate of the model-average signal (which appears in the

> numerator of $d1^*$, $d2^*$, and $d3^*$) has a dependence on N , as does the
> estimate of $s\{\langle b_{\{m\}} \rangle\}$, the inter-model standard deviation of trends
> (which appears in the denominator of $d2^*$ and $d3^*$).
>
> The worrying thing about the appended Figure is the behavior of $d3^*$.
> This is the test which we thought Reviewers 1 and 2 were advocating. As
> you can see, $d3^*$ produces rejection rates that are consistently LOWER
> (by a factor of two or more) than theoretical expectations. We do not
> wish to be accused by Douglass et al. of devising a test that makes it
> very difficult to reject hypothesis $H2$, even when there is a
significant
> difference between the trends in the model average signal and the
> 'observational signal'.
>
> So the question is, did we misinterpret the intentions of the
Reviewers?
> Were they indeed advocating a $d3^*$ test of the form which we used? I
will
> try to clarify this point tomorrow with Francis Zwiers (our Reviewer
2).
>
> Recall that our current version of $d3^*$ is defined as follows:
>
> $d3^* = (b_{\{o\}} - \langle\langle b_{\{m\}} \rangle\rangle) / \sqrt{ (s\{\langle b_{\{m\}} \rangle\} ** 2) + (s\{b_{\{o\}}\} ** 2) }$
>
> where
>
> $b_{\{o\}}$ = Observed trend
> $\langle\langle b_{\{m\}} \rangle\rangle$ = Model average trend
> $s\{\langle b_{\{m\}} \rangle\}$ = Inter-model standard deviation of ensemble-mean trends
> $s\{b_{\{o\}}\}$ = Standard error of the observed trend (adjusted for
> autocorrelation effects)
>
> In Francis's comments on our paper, the first term under the square
root
> sign is referred to as "an estimate of the variance of that average"
> (i.e., of $\langle\langle b_{\{m\}} \rangle\rangle$). It's possible that Francis was referring to
> $\sigma\{SE\}$, which IS an estimate of the variance of $\langle\langle b_{\{m\}} \rangle\rangle$. If one
> replaces $s\{\langle b_{\{m\}} \rangle\}$ with $\sigma\{SE\}$ in the equation for $d3^*$, the
> performance of the $d3^*$ test with synthetic data is (at least for large
> values of N) very close to theoretical expectations. It's actually even
> closer to theoretical expectations than the $d2^*$ test shown in the
> appended Figure (which is already pretty close). I'll produce the
> "revised $d3^*$ " plot tomorrow...
>
> The bottom line here is that we need to clarify with Francis the exact
> form of the test he was requesting. The "new" $d3^*$ (with $\sigma\{SE\}$ as
the
> first term under the square root sign) would lead to a simpler
> interpretation of the problems with the DCPS07 test. It would show that
> the primary error in DCPS07 was in the neglect of the observational
> uncertainty term. It would also simplify interpretation of the results
> from Section 6.
>

> I'm sorry about the delay in submission of our manuscript, but this is
> an important point, and I'd like to understand it fully. I'm still
> hopeful that we'll be able to submit the paper in the next few days.
> Many thanks to Tom and Peter for persuading me to pay attention to this
> issue. It often took a lot of persuasion...

>
> With best regards,

> Ben

>

> -----

>
> Benjamin D. Santer
> Program for Climate Model Diagnosis and Intercomparison
> Lawrence Livermore National Laboratory
> P.O. Box 808, Mail Stop L-103
> Livermore, CA 94550, U.S.A.
> Tel: (925) 422-2486
> FAX: (925) 422-7675
> email: santer1@llnl.gov

> -----

>

</x-flowed>

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: IPCC & FOI
Date: Thu, 29 May 2008 08:12:02 -0400
Reply-to: mann@psu.edu

<x-flowed>

Hi Phil,

laughable that CA would claim to have discovered the problem. They would have run off to the Wall Street Journal for an exclusive were that to have been true.

I'll contact Gene about this ASAP. His new email is: generwahl@yahoo.com

talk to you later,

mike

Phil Jones wrote:

>

>> Mike,

> Can you delete any emails you may have had with Keith re AR4?

> Keith will do likewise. He's not in at the moment - minor family crisis.

>

> Can you also email Gene and get him to do the same? I don't have his new email address.

>

> We will be getting Caspar to do likewise.

>

> I see that CA claim they discovered the 1945 problem in the Nature paper!!

>

> Cheers

> Phil

>

>

>

>>

>

> Prof. Phil Jones

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> School of Environmental Sciences Fax +44 (0) 1603 507784
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> UK
> -----
>

--

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Director, Earth System Science Center (ESSC)

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The Pennsylvania State University email: mann@psu.edu
University Park, PA 16802-5013

<http://www.met.psu.edu/dept/faculty/mann.htm>

</x-flowed>

From: Peter Thorne <peter.thorne@metoffice.gov.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: Our d3* test
Date: Thu, 29 May 2008 09:27:20 +0100
Cc: Ben Santer <santer1@llnl.gov>, Leopold Haimberger
<leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom
Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>,
"Susan Solomon" <ssolomon@al.noaa.gov>, Melissa Free
<melissa.free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, Phil Jones
<p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein
<klein21@mail.llnl.gov>, Carl Mears <mears@remss.com>, Doug Nychka
<nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steve Sherwood
<Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>

One more addendum:

We still need to be aware that this ignores two sources of uncertainty that will exist in the real world that are not included in Section 6 which is effectively 1 perfect obs and finite number of runs of a perfect model:

1. Imperfect models
2. Observational uncertainty related to dataset construction choices (parametric and structural)

Of course, with the test construct given #1 becomes moot as this is the thing we are testing for with H2. This is definitely not the case for #2 which will be important and is poorly constrained.

For Amplification factors we are either blessed or cursed by the wealth of independent estimates of the observational record. One approach, that I would advocate here because I'm lazy / because its more intuitive* (*=delete as appropriate) is that we can take the obs error term outside the explicit uncertainty calculation by making comparisons to each dataset in turn. However, the alternative approach would be to take the range of dataset estimates, make the necessary poor-mans assumption that this is the 1 sigma or 2 sigma range depending upon how far you think they span the range of possible answers and then incorporate this as an extra term in the denominator to d3. As with the other two it would be orthogonal error so still SQRT of sum of squares. Such an approach would have advantages in terms of universal applicability to other problems where we may have less independent observational estimates, but a drawback in terms of what we should then be using as our observational yardstick in testing H2 (the mean of all estimates, the median, something else?).

Anyway, just a methodological quirk that logically follows if we are worried about ensuring universal applicability of approach which with the increasingly frequent use of CMIP3 archive for these types of applications is something we maybe should be considering. I don't expect us to spend very much time, if any, on this issue as I agree that key is submitting ASAP.

Peter

On Wed, 2008-05-28 at 21:58 -0600, Tom Wigley wrote:

> Dear all,

>

> Just to add a bit to Ben's notes. The conceptual problem is how to
> account for two different types of uncertainty in comparing a single
> observed trend (with temporal uncertainty) with the average of a
> bunch of model trends (where the uncertainty is from inter-model
> differences). The "old" d3 tried to do this, but failed the synthetic
> data test. The new d3 does this a different way (in the way that the
> inter-model uncertainty term is quantified). This passes the synthetic
> data test very well.

>

> The new d3 test differs from DCSP07 only in that it includes in the
> denominator of the test statistic an observed noise term. This is by
> far the bigger of the two denominator terms. Ignoring it is very
> wrong, and this is why the DCSP07 method fails the synthetic data
> test.

>

> Tom.

>

> ++++++

>

> Ben Santer wrote:

> > Dear folks,

> >

> > Just wanted to let you know that I did not submit our paper to IJoC.

> > After some discussions that I've had with Tom Wigley and Peter
Thorne, I

> > applied our d1*, d2*, and d3* tests to synthetic data, in much the
same

> > way that we applied the DCPS07 d* test and our original "paired
trends"

> > test (d) to synthetic data. The results are shown in the appended
Figure.

> >

> > Relative to the DCPS07 d* test, our d1*, d2*, and d3* tests of

> > hypothesis H2 yield rejection rates that are substantially

> > closer to theoretical expectations (compare the appended Figure with

> > Figure 5 in our manuscript). As expected, all three tests show a

> > dependence on N (the number of synthetic time series), with rejection

> > rates decreasing to near-asymptotic values as N increases. This is

> > because the estimate of the model-average signal (which appears in

the

> > numerator of d1*, d2*, and d3*) has a dependence on N, as does the

> > estimate of $s_{\{b_m\}}$, the inter-model standard deviation of trends

> > (which appears in the denominator of d2* and d3*).

> >

> > The worrying thing about the appended Figure is the behavior of d3*.

> > This is the test which we thought Reviewers 1 and 2 were advocating.

As

> > you can see, d3* produces rejection rates that are consistently LOWER

> > (by a factor of two or more) than theoretical expectations. We do not

> > wish to be accused by Douglass et al. of devising a test that makes it
> > very difficult to reject hypothesis H2, even when there is a significant
> > difference between the trends in the model average signal and the
> > 'observational signal'.
> >
> > So the question is, did we misinterpret the intentions of the Reviewers?
> > Were they indeed advocating a d3* test of the form which we used? I will
> > try to clarify this point tomorrow with Francis Zwiers (our Reviewer 2).
> >
> > Recall that our current version of d3* is defined as follows:
> >
> >
$$d3^* = (b\{o\} - \langle\langle b\{m\}\rangle\rangle) / \sqrt{ (s\{\langle b\{m\}\rangle\} ** 2) + (s\{b\{o\}\} ** 2) }$$

> >
> > where
> >
> > b{o} = Observed trend
> > $\langle\langle b\{m\}\rangle\rangle$ = Model average trend
> > s{<b{m}>} = Inter-model standard deviation of ensemble-mean trends
> > s{b{o}} = Standard error of the observed trend (adjusted for autocorrelation effects)
> >
> > In Francis's comments on our paper, the first term under the square root
> > sign is referred to as "an estimate of the variance of that average"
> > (i.e., of $\langle\langle b\{m\}\rangle\rangle$). It's possible that Francis was referring to
> > sigma{SE}, which IS an estimate of the variance of $\langle\langle b\{m\}\rangle\rangle$. If one
> > replaces s{<b{m}>} with sigma{SE} in the equation for d3*, the
> > performance of the d3* test with synthetic data is (at least for large
> > values of N) very close to theoretical expectations. It's actually even
> > closer to theoretical expectations than the d2* test shown in the
> > appended Figure (which is already pretty close). I'll produce the
> > "revised d3*" plot tomorrow...
> >
> > The bottom line here is that we need to clarify with Francis the exact
> > form of the test he was requesting. The "new" d3* (with sigma{SE} as the
> > first term under the square root sign) would lead to a simpler
> > interpretation of the problems with the DCPS07 test. It would show that
> > the primary error in DCPS07 was in the neglect of the observational
> > uncertainty term. It would also simplify interpretation of the results
> > from Section 6.
> >

> > I'm sorry about the delay in submission of our manuscript, but this
is
> > an important point, and I'd like to understand it fully. I'm still
> > hopeful that we'll be able to submit the paper in the next few days.
> > Many thanks to Tom and Peter for persuading me to pay attention to
this
> > issue. It often took a lot of persuasion...

> >
> > With best regards,

> >
> > Ben

> > -----

> >
> > Benjamin D. Santer
> > Program for Climate Model Diagnosis and Intercomparison
> > Lawrence Livermore National Laboratory
> > P.O. Box 808, Mail Stop L-103
> > Livermore, CA 94550, U.S.A.
> > Tel: (925) 422-2486
> > FAX: (925) 422-7675
> > email: santer1@llnl.gov

> > -----

> >

>
--

Peter Thorne Climate Research Scientist
Met Office Hadley Centre, FitzRoy Road, Exeter, EX1 3PB
tel. +44 1392 886552 fax +44 1392 885681
www.metoffice.gov.uk/hadobs

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@meteo.psu.edu>
Subject: IPCC & FOI
Date: Thu May 29 11:04:11 2008

Mike,

Can you delete any emails you may have had with Keith re AR4?
Keith will do likewise. He's not in at the moment - minor family crisis.

Can you also email Gene and get him to do the same? I don't
have his new email address.

We will be getting Caspar to do likewise.
I see that CA claim they discovered the 1945 problem in the Nature paper!!

Cheers

Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: Our d3* test
Date: Thu May 29 15:13:35 2008

Ben,

Hopefully the email to Francis will help to resolve this quickly. It would seem from Tom's email that the new d3 approaches the expected result for largish N. A test ought to do this as Tom says.

You'll need to change the response a little as although you may have misinterpreted Francis, you may not have Rev 1.

Hope this is out of your hair as soon as feasible.

Climate Audit are an odd crowd. McIntyre is claiming that he spotted the problem in 1945 in the marine data - and refers to a blog page from late last year! We were already on to it by then and he didn't really know what he was talking about anyway. Maybe this paper and the various press coverage (especially Dick Reynold's N&V as he spelt it out) will allow them to realize that what is really robust in all this is the land record. I suspect it won't though. One day they may finally realize the concept of effective spatial degrees of freedom. John Christy doesn't understand this!

Cheers

Phil

At 04:46 29/05/2008, you wrote:

Dear folks,

Just wanted to let you know that I did not submit our paper to IJoC. After some discussions that I've had with Tom Wigley and Peter Thorne, I applied our d1*, d2*, and d3* tests to synthetic data, in much the same way that we applied the DCPS07 d* test and our original "paired trends" test (d) to synthetic data. The results are shown in the appended Figure.

Relative to the DCPS07 d* test, our d1*, d2*, and d3* tests of hypothesis H2 yield rejection rates that are substantially closer to theoretical expectations (compare the appended Figure with Figure 5 in our manuscript). As expected, all three tests show a dependence on N (the number of synthetic time series), with rejection rates decreasing to near-asymptotic values as N increases. This is because the estimate of the model-average signal (which appears in the numerator of d1*, d2*, and d3*) has a dependence on N, as does the estimate of $s\{\langle b_m \rangle\}$, the inter-model standard deviation of trends (which appears in the denominator of d2* and d3*).

The worrying thing about the appended Figure is the behavior of d3*. This is the test which we thought Reviewers 1 and 2 were advocating. As you can see, d3* produces rejection rates that are consistently LOWER (by a factor of two or more) than theoretical expectations. We do not wish to be accused by Douglass et al. of devising a

test that makes it very difficult to reject hypothesis H2, even when there is a significant difference between the trends in the model average signal and the 'observational signal'.

So the question is, did we misinterpret the intentions of the Reviewers? Were they indeed advocating a $d3^*$ test of the form which we used? I will try to clarify this point tomorrow with Francis Zwiers (our Reviewer 2).

Recall that our current version of $d3^*$ is defined as follows:

$$d3^* = (b\{o\} - \langle\langle b\{m\}\rangle\rangle) / \sqrt{ (s\{\langle b\{m\}\rangle\} ** 2) + (s\{b\{o\}\} ** 2) }$$

where

$b\{o\}$ = Observed trend

$\langle\langle b\{m\}\rangle\rangle$ = Model average trend

$s\{\langle b\{m\}\rangle\}$ = Inter-model standard deviation of ensemble-mean trends

$s\{b\{o\}\}$ = Standard error of the observed trend (adjusted for autocorrelation effects)

In Francis's comments on our paper, the first term under the square root sign is referred to as "an estimate of the variance of that average" (i.e., of $\langle\langle b\{m\}\rangle\rangle$). It's possible that Francis was referring to $\sigma\{SE\}$, which IS an estimate of the variance of $\langle\langle b\{m\}\rangle\rangle$. If one replaces $s\{\langle b\{m\}\rangle\}$ with $\sigma\{SE\}$ in the equation for $d3^*$, the performance of the $d3^*$ test with synthetic data is (at least for large values of N) very close to theoretical expectations. It's actually even closer to theoretical expectations than the $d2^*$ test shown in the appended Figure (which is already pretty close). I'll produce the "revised $d3^*$ " plot tomorrow...

The bottom line here is that we need to clarify with Francis the exact form of the test he was requesting. The "new" $d3^*$ (with $\sigma\{SE\}$ as the first term under the square root sign) would lead to a simpler interpretation of the problems with the DCPS07 test. It would show that the primary error in DCPS07 was in the neglect of the observational uncertainty term. It would also simplify interpretation of the results from Section 6. I'm sorry about the delay in submission of our manuscript, but this is an important point, and I'd like to understand it fully. I'm still hopeful that we'll be able to submit the paper in the next few days. Many thanks to Tom and Peter for persuading me to pay attention to this issue. It often took a lot of persuasion...

With best regards,

Ben

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University of East Anglia

Norwich Email p.jones@uea.ac.uk

NR4 7TJ

UK

From: Caspar Ammann <ammann@ucar.edu>
To: t.osborn@uea.ac.uk
Subject: Re: request for your emails
Date: Fri, 30 May 2008 10:14:46 -0600
Cc: "keith Briffa" <k.briffa@uea.ac.uk>, p.jones@uea.ac.uk

Hi Tim,

in response to your inquiry about my take on the confidentiality of my email communications with you, Keith or Phil, I have to say that the intent of these emails is to reply or communicate with the individuals on the distribution list, and they are not intended for general 'publication'. If I would consider my texts to potentially get wider dissemination then I would probably have written them in a different style. Having said that, as far as I can remember (and I haven't checked in the records, if they even still exist) I have never written an explicit statement on these messages that would label them strictly confidential.

Not sure if this is of any help, but it seems to me that it reflects our standard way of interaction in the scientific community.

Caspar

On May 27, 2008, at 5:03 PM, Tim Osborn wrote:

Dear Caspar,

I hope everything's fine with you.

Our university has received a request, under the UK Freedom of Information law, from someone called David Holland for emails or other documents that you may have sent to us that discuss any matters related to the IPCC assessment process.

We are not sure what our university's response will be, nor have we even checked whether you sent us emails that relate to the IPCC assessment or that we retained any that you may have sent.

However, it would be useful to know your opinion on this matter. In particular, we would like to know whether you consider any emails that you sent to us as confidential.

Sorry to bother you with this,

Tim (cc Keith & Phil)

Caspar M. Ammann
National Center for Atmospheric Research
Climate and Global Dynamics Division - Paleoclimatology
1850 Table Mesa Drive
Boulder, CO 80307-3000
email: [1]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

References

1. <mailto:ammann@ucar.edu>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Re: request for your emails
Date: Fri May 30 12:58:34 2008
Cc: "Keith Briffa" <k.briffa@uea.ac.uk>, p.jones@uea.ac.uk

Hi again Caspar,

I don't think it is necessary for you to dig through any emails you may have sent us to determine your answer.

Our question is a more general one, which is whether you generally consider emails that you sent us to have been sent in confidence. If you do, then we will use this as a reason to decline the request.

Cheers

Tim

At 00:36 28/05/2008, Caspar Ammann wrote:

Oh MAN! will this crap ever end??

Well, I will have to properly answer in a couple days when I get a chance digging through emails. I don't recall from the top of my head any specifics about IPCC.

I'm also sorry that you guys have to go through this BS. You all did an outstanding job and the IPCC report certainly reflects that science and literature in an accurate and balanced way.

So long,

Caspar

On May 27, 2008, at 5:03 PM, Tim Osborn wrote:

Dear Caspar,

I hope everything's fine with you.

Our university has received a request, under the UK Freedom of Information law, from someone called David Holland for emails or other documents that you may have sent to us that discuss any matters related to the IPCC assessment process.

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Sorry to bother you with this,

Tim (cc Keith & Phil)

Caspar M. Ammann

National Center for Atmospheric Research
Climate and Global Dynamics Division - Paleoclimatology
1850 Table Mesa Drive
Boulder, CO 80307-3000
email: [1]ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

References

1. <mailto:ammann@ucar.edu>

From: Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: RE: [Fwd: of buckets and blogs...]
Date: Sat, 31 May 2008 19:24:29 -0400 (EDT)
Reply-to: gschmidt@giss.nasa.gov
Cc: Phil Jones <P.Jones@uea.ac.uk>, mann@psu.edu

<x-flowed>

Phil - here's the text minus figures and links... It's subject to a little revision, but let me know if there are any factual or emphasis issues that are perhaps misplaced.

Thanks

Gavin

=====

Of buckets and blogs

This last week has been an interesting one for observers of how climate change is covered in the media and online. On Wednesday an interesting paper (Thompson et al) was published in Nature, pointing to a clear artifact in the sea surface temperatures in 1945 and associating it with the changing mix of fleets and measurement techniques at the end of World War II. The mainstream media by and large got the story right - puzzling anomaly tracked down, corrections in progress after a little scientific detective work, consequences minor - even though a few headline writers got a little carried away in equating a specific dip in 1945 ocean temperatures with the more gentle 1940s-1970s cooling that is seen in the land measurements. However, some blog commentaries have gone completely overboard on the implications of this study in ways that are very revealing of their underlying biases.

The best commentary came from John Nielsen-Gammon's new blog where he described very clearly how the uncertainties in data - both the known unknowns and unknown unknowns - get handled in practice (read this and then come back). Stoa, quite sensibly, suggested that it's a bit early to be expressing an opinion on what it all means. But patience is not one of the blogosphere's virtues and so there was no shortage of people extrapolating wildly to support their pet hobbyhorses. This in itself is

not so unusual; despite much advice to the contrary, people (the media and bloggers) tend to weight individual papers that make the news far more highly than the balance of evidence that really underlies assessments like the IPCC. But in this case, the addition of a little knowledge made the usual extravagances a little more scientific-looking and has given it some extra steam.

Like almost all historical climate data, ship-board sea surface temperatures (SST) were not collected with long term climate trends in mind. Thus practices varied enormously among ships and fleets and over time. In the 19th Century, simple wooden buckets would be thrown over the side to collect the water (a non-trivial exercise when a ship is moving, as many novice ocean-going researchers will painfully recall). Later on, special canvas buckets were used, and after WWII, insulated 'buckets' became more standard - though these aren't really buckets in the colloquial sense of the word as the photo shows (pay attention to this because it comes up later).

The thermodynamic properties of each of these buckets are different and so when blending data sources together to get an estimate of the true anomaly, corrections for these biases are needed. For instance, the canvas buckets give a temperature up to 1C cooler in some circumstances (that depend on season and location) than the modern insulated buckets. Insulated buckets have a slight cool bias compared to temperature measurements that are taken at the inlet for water in the engine room which is the most used method at present. Automated buoys which became more common in recent decades tend to be cooler than the engine intake measures as well. The recent IPCC report had a thorough description of these issues (section 3.B.3) fully acknowledging that these corrections were a work in progress.

And that is indeed the case. The collection and digitisation of the ship logbooks is a huge undertaking and continues to add significant amounts of 20th Century and earlier data to the records. This dataset (ICOADS) is continually growing, and the impacts of the bias adjustments are continually being assessed. The biggest transitions in measurements occurred at the beginning of WWII between 1939 and 1941 when the sources of data switched from European fleets to almost exclusively US fleets (and who tended to use engine inlet temperatures rather than canvas buckets). This offset was large and dramatic and was identified more than ten years ago from comparisons of simultaneous measurements of night-time marine air temperatures (NMAT) which did not show such a shift. The experimentally based adjustment to account for the canvas bucket cooling brought the sea

surface temperatures much more into line with the NMAT series (Folland and Parker, 1995). (Note that this reduced the 20th Century trends in SST).

More recent work (for instance, at this workshop in 2005), has focussed on refining the estimates and incorporating new sources of data. For instance, the 1941 shift in the original corrections, was reduced and pushed back to 1939 with the addition of substantial and dominant amounts of US Merchant Marine data (which mostly used engine inlets temperatures).

The version of the data that is currently used in most temperature reconstructions is based on the work of Rayner and colleagues (reported in 2006). In their discussion of remaining issues they state:

Using metadata in the ICOADS it is possible to compare the contributions made by different countries to the marine component of the global temperature curve. Different countries give different advice to their observing fleets concerning how best to measure SST. Breaking the data up into separate countries' contributions shows that the assumption made in deriving the original bucket correction—that is, that the use of uninsulated buckets ended in January 1942—is incorrect. In particular, data gathered by ships recruited by Japan and the Netherlands (not shown) are biased in a way that suggests that these nations were still using uninsulated buckets to obtain SST measurements as late as the 1960s. By contrast, it appears that the United States started the switch to using engine room intake measurements as early as 1920.

They go on to mention the modern buoy problems and the continued need to work out bias corrections for changing engine inlet data as well as minor issues related to the modern insulated buckets. For example, the differences in co-located modern bucket and inlet temperatures are around 0.1 deg C:

(from John Kennedy).

However it is one thing to suspect that biases might remain in a dataset (a sentiment shared by everyone), it is quite another to show that they are really there. The Thompson et al paper does the latter quite effectively by removing variability associated with some known climate modes (including ENSO) and seeing the 1945 anomaly pop out clearly. In doing this in fact, they show that the previous adjustments in the pre-war period were probably ok (though there is substantial additional evidence of that in any case - see the references in Rayner et al, 2006). The

Thompson anomaly seems to coincide strongly with the post-war shift back to a mix of US, UK and Dutch ships, implying that post-war bias corrections are indeed required and significant. This conclusion is not much of a surprise to any of the people working on this since they have been saying it in publications and meetings for years. The issue is of course quantifying and validating the corrections, for which the Thompson analysis might prove useful. The use of canvas buckets by the Dutch, Japanese and some UK ships is most likely to blame, and given the mix of national fleets shown above, this will make a noticeable difference in 1945 up to the early 1960s maybe - the details will depend on the seasonal and areal coverage of those sources compared to the dominant US information. The schematic in the Independent is probably a good first guess at what the change will look like (remember that the ocean changes are constrained by the NMAT record shown above).

So far, so good. The fun for the blog-watchers is what happened next. What could one do to get the story all wrong? First, you could incorrectly assume that scientists working on this must somehow be unaware of the problems (that is belied by the frequent mention of post WWII issues in workshops and papers since at least 2005, but never mind). Next, you could conflate the 'buckets' used in recent decades (as seen in the graphs in Kent et al 2007's discussion of the ICOADS meta-data) with the buckets in the pre-war period (see photo above). If you do make that mistake however, you can extrapolate to get some rather dramatic (if erroneous) conclusions. For instance, that the effect of the 'corrections' would be to halve the SST trend from the 1970s. Gosh! (The mismatch this would create with the independent NMAT data series should not be mentioned). But there is more! You could take the (incorrect) prescription based on the bucket confusion, apply it to the full global temperatures (land included, hmm) and think that this merits a discussion on whether the whole IPCC edifice had been completely undermined (Answer: no). And it goes on - the bucket confusion was pointed out but the complaint switches to the scandal that it wasn't properly explained.

All this shows is wishful thinking overcoming logic. However many times there is a similar rush to judgment that is subsequently showed to be based on nothing, it still adds to the vast array of similar 'evidence' that keeps getting trotted out by by the ill-informed. The excuse that these are just exploratory exercises in what-if thinking wears a little thin when the 'what if' always leads to the same (desired) conclusion. This week's play-by-play was quite revealing on that score.

| Gavin Schmidt NASA/Goddard Institute for Space Studies |
| 2880 Broadway |
| Tel: (212) 678 5627 New York, NY 10025 |
| |
| gschmidt@giss.nasa.gov <http://www.giss.nasa.gov/~gavin> |

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From: Ben Santer <santer1@llnl.gov>
To: Carl Mears <mears@sonic.net>
Subject: Re: Our d3* test
Date: Mon, 02 Jun 2008 09:32:01 -0700
Reply-to: santer1@llnl.gov
Cc: Steven Sherwood <Steven.Sherwood@yale.edu>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, "'Susan Solomon'" <ssolomon@al.noaa.gov>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Frank Wentz <frank.wentz@remss.com>

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Dear Carl,

This issue is now covered in the version of the manuscript that I sent out on Friday. The d2* and d3* statistics have been removed. The new d1* statistic DOES involve the standard error of the model average trend in the denominator (together with the adjusted standard error of the observed trend; see equation 12 in revised manuscript). The slight irony here is that the new d1* statistic essentially reduces to the old d1* statistic, since the adjusted standard error of the observed trend is substantially larger than the standard error of the model average trend...

With best regards,

Ben

Carl Mears wrote:

> Hi

>

> I think I agree (partly, anyway) with Steve S.

>

> I think that d3* partly double counts the uncertainty.

>

> Here is my thinking that leads me to this:

>

> Assume we have a "perfect model". A perfect model means in this context

> 1. Correct sensitivities to all forcing terms

> 2. Forcing terms are all correct

> 3. Spatial temporal structure of internal variability is correct.

>

> In other words, the model output has exactly the correct "underlying" trend, but

> different realizations of internal variability and this variability has the right

> structure.

>

> We now run the model a bunch of times and compute the trend in each case.
> The spread in the trends is completely due to internal variability.
>
> We compare this to the "perfect" real world trend, which also has
> uncertainty due
> to internal variability (but nothing else).
>
> To me either one of the following is fair:
>
> 1. We test whether the observed trend is inside the distribution of
> model trends. The uncertainty in the
> observed trend is already taken care of by the spread in modeled
> trends,
> since the representation of
> internal uncertainty is accurate.
>
> 2. We test whether the observed trend is equal to the mean model
> trend,
> within uncertainty. Uncertainty here is
> the uncertainty in the observed trend $s\{b\{o\}\}$, combined with the
> uncertainty in the mean model trend ($SE\{b\{m\}\}$).
>
> If we use $d3^*$, I think we are doing both these at once, and thus double
> counting the internal variability
> uncertainty. Option 2 is what Steve S is advocating, and is close to
> $d1^*$, since $SE\{b\{m\}\}$ is so small.
> Option 1 is $d2^*$.
>
> Of course the problem is that our models are not perfect, and a
> substantial portion of the spread in
> model trends is probably due to differences in sensitivity and forcing,
> and the representation
> of internal variability can be wrong. I don't know how to separate the
> model trend distribution into
> a "random" and "deterministic" part. I think $d1^*$ and $d2^*$ above get at
> the problem from 2 different angles,
> while $d3^*$ double counts the internal variability part of the
> uncertainty. So it is not surprising that we
> get some funny results for synthetic data, which only have this kind of
> uncertainty.
>
> Comments?
>
> -Carl
>
>
>
>
> On May 29, 2008, at 5:36 AM, Steven Sherwood wrote:
>
>>
>> On May 28, 2008, at 11:46 PM, Ben Santer wrote:
>>>

```

>>> Recall that our current version of d3* is defined as follows:
>>>
>>> d3* = ( b{o} - <<b{m}>> ) / sqrt[ ( s{<b{m}>} ** 2) + ( s{b{o}} ** 2)
]
>>>
>>> where
>>>
>>> b{o}          = Observed trend
>>> <<b{m}>>       = Model average trend
>>> s{<b{m}>}     = Inter-model standard deviation of ensemble-mean trends
>>> s{b{o}}      = Standard error of the observed trend (adjusted for
>>>                autocorrelation effects)
>>>
>> Shouldn't the first term under sqrt be the standard deviation of the
>> estimate of <<b(m)>> -- e.g., the standard error of <b(m)> -- rather
>> than the standard deviation of <b(m)>?  d3* would I think then be
>> equivalent to a z-score, relevant to the null hypothesis that models
>> on average get the trend right.  As written, I think the distribution
>> of d3* will have less than unity variance under this hypothesis.
>>
>> SS
>>
>>
>> -----
>> Steven Sherwood
>> Steven.Sherwood@yale.edu <mailto:Steven.Sherwood@yale.edu>
>> Yale University                                ph: 203
>> 432-3167
>> P. O. Box 208109                               fax: 203
>> 432-3134
>> New Haven, CT 06520-8109
>> http://www.geology.yale.edu/~sherwood
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Benjamin D. Santer
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P.O. Box 808, Mail Stop L-103
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Tel: (925) 422-2486
FAX: (925) 422-7675
email: santer1@llnl.gov
-----
---
```

</x-flowed>

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: nomination: materials needed!
Date: Mon, 02 Jun 2008 15:44:28 -0400
Reply-to: mann@psu.edu

Hi Phil,

This is coming along nicely. I've got 5 very strong supporting letter writers lined up to support your AGU Fellowship nomination (confidentially: Ben Santer, Tom Karl, Jean Jouzel, and Lonnie Thompson have all agreed, waiting to hear back from one more individual, maximum is six letters including mine as nominator).

Meanwhile, if you can pass along the following information that is needed for the nomination package that would be very helpful. thanks in advance!

mike

Selected bibliography

- * Must be no longer than 2 pages.
- * Begin by briefly stating the candidate's total number and types of publications and specifying the number published in AGU journals.
- * Do not just select the most recent publications; choose those that best support your argument for Fellowship.

Curriculum Vitae

- * Must be no longer than 2 pages.
- * List the candidate's name, address, history of employment, degrees, research experience, honors, memberships, and service to the community through committee work, advisory boards, etc.

--

Michael E. Mann
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Director, Earth System Science Center (ESSC)

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503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [1]mann@psu.edu
University Park, PA 16802-5013

[2]<http://www.met.psu.edu/dept/faculty/mann.htm>

References

1. <mailto:mann@psu.edu>
2. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: A couple of things
Date: Wed, 04 Jun 2008 09:47:02 -0400
Reply-to: mann@psu.edu
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Hi Phil,

Seems to me that CRU should charge him a fee for the service. He shouldn't be under the assumption that he has the right to demand reports be scanned in for him on a whim. CRU should require reasonable monetary compensation for the labor, effort (and postage!). If this were a colleague acting in good faith, I'd say do it at no cost. But of, course, he's not. He's not interested in the truth here, he's just looking for another way to try to undermine confidence in our science.

Henry's review looks helpful and easy to deal w/. Will be interesting to see the other reviews. I guess you're going to get your moneys' worth out of your scanner,
mike

Phil Jones wrote:

Gavin, Mike,

1. This email came to CRU last night.

From: Steve McIntyre [[1] <mailto:stephen.mcintyre@utoronto.ca>]

Sent: Tuesday, June 03, 2008 5:09 PM

To: [2] alan.ovenden@uea.ac.uk

Subject: Farmer et al 1989

Dear Sir, Can you please send me a pdf of the Farmer et al 1989, cited in Folland and Parker 1995, which, in turn is cited in the IPCC Fourth Assessment Report. Thanks,
Steve McIntyre

Farmer, G., Wigley, T. M. L., Jones, P. D. and Salmon, M., 1989 'Documenting and explaining recent global-mean temperature changes'. Climatic Research Unit, Norwich, Final Report to NERC, UK, Contract GR3/6565 (unpublished)

CRU has just the one copy of this! We've just got a new scanner for a project, so someone here

is going to try this out - and scan the ~150pp. I'm doing this as this is one of the project

reports that I wished I'd written up. It's got all the bucket equations, assessments of the accuracy of the various estimates for the parameters that have to be made. It also includes discussion of the shapes (seasonal cycles) of the residual seasonal cycles you get from different types of buckets prior to WW2 relative to intakes. It also includes a factor

they haven't considered at all yet - ship speed and its changes over time. This turns

out

to important. It has a lot more than Folland and Parker (1995). Doubt it will shut them up for

long - but it will justify your faith in those doing the SST work that we have considered everything

we could think of. We'll also put it up on our web site at the same time.

2. Reviews of the Holocene epic.

Got this today - so a journal still working by post! Here is Henry's review.

Possibly the other two might involve hand-written comments on hard copies.

Will get these scanned when they arrive and send around if necessary.

Dear Phil

I have today posted two referees' reports to you and the verdict of accepted subject to taking account of referees' comments. These two reports do not include the report of Henry Diaz which has just been sent to you directly. Please take his comments into account too.

John A Matthews

Emeritus Professor of Physical Geography

Editor, The Holocene

Department of Geography

School of the Environment and Society

University of Wales Swansea

Singleton Park

SWANSEA SA2 8PP

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich

Email [3]p.jones@uea.ac.uk

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--

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[5]<http://www.met.psu.edu/dept/faculty/mann.htm>

References

1. <mailto:stephen.mcintyre@utoronto.ca>
2. <mailto:alan.ovenden@uea.ac.uk>
3. <mailto:p.jones@uea.ac.uk>
4. <mailto:mann@psu.edu>
5. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Phil Jones <p.jones@uea.ac.uk>
To: Christoph Kull <christoph.kull@scnat.ch>, <bo@gfy.ku.dk>, <thompson.4@osu.edu>, <EWWO@bas.ac.uk>, <jan.esper@wsl.ch>, Janice Lough <j.lough@aims.gov.au>, Juerg Luterbacher <juerg@giub.unibe.ch>, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Ricardo Villalba <ricardo@lab.cricyt.edu.ar>, Kim Cobb <kcobb@eas.gatech.edu>, Heinz Wanner <wanner@giub.unibe.ch>, Jonathan Overpeck <jto@u.arizona.edu>, Michael Schulz <mschulz@palmod.uni-bremen.de>, Eystein Jansen <Eystein.Jansen@geo.uib.no>, Nick Graham <ngraham@hrc-lab.org>, Francis Zwiers <francis.zwiers@ec.gc.ca>, Caspar Ammann <ammann@ucar.edu>, "Michael E. Mann" <mann@meteo.psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Sandy Tudhope <sandy.tudhope@ed.ac.uk>, Tas van Ommen <tas.van.ommen@utas.edu.au>, "Wahl, Eugene R" <wahl@alfred.edu>, Brendan Buckley <bmb@ldeo.columbia.edu>, Hugues Goosse <hugues.goosse@uclouvain.be>
Subject: Review Comments on the Wengen paper
Date: Thu Jun 5 13:18:47 2008
Cc: <larry.williams@targetedgrowth.com>, Thorsten Kiefer <thorsten.kiefer@pages.unibe.ch>, Naresh Kumar <NKumar@epri.com>

Dear All (especially Peck!),

Attached are three sets of reviews of the paper - 2 in the pdf file and one in the small doc file.

As you'll be able to see, there isn't that much to do and the reviews have been

good. All three reviewers seem to be in awe of the group! I've had a brief

discussion with Keith as to who should do what. You're all welcome to help

but I only think most of you will need go through the revised version when we get that

out - hopefully asap. John Matthews is still hopeful of a 2008 publication date,

and you'll see we won't be going out for any further reviews - just John checking.

Many of the comments relate to the tree-ring section and Keith will

deal with these. They involve some re-organization and some additional refs

on dendro isotope work.

The coral and isotope sections get praised for organization - so well done!

I'll need some help with the one coral comment on 'vital effects', so can

Janice, Kim and Sandy work on that. I think it only needs a few sentences

and maybe extra refs. I know some of you are in Trieste next week, so maybe

you can work on it there.

I'll work on the documentary section a bit and liaise with Juerg. This shouldn't

involve

much extra work.

I'll also look at the borehole section together with what was in Ch 6 of AR4.

The major bit of new text we need is on the high-res varves and laminated lake records,

so this is why I highlighted Peck. They aren't used in large-area high-freq climate

reconstructions, so emphasis there and to a few key review papers. Is this doable in

the next couple of weeks, Peck? I don't think more than a page or two is required.

Related to the issue of the different proxies use or potential use in high-freq

reconstructions, I'll work on trying to bring that out in the Introduction. I'll

bring out the issues of the maturity of the different proxy disciplines.

Sections 3 and 4 just seem to need some minor wording changes and some clarification - possibly in a revised introduction. We're hoping that Tim

here will be able to do that. Note that although the reviewer suggested dropping

the forcing section, John Matthews would like that kept.

In conclusion, we are nearly there. CRU will be able to find the colour costs

envisaged.

To those in Trieste - enjoy the week and I hope it will as fruitful as Wengen was.

If anyone is going to be out of contact during the second half of June and early July

can you let me know.

I've reattached the submission as a word file.

Cheers

Phil

Prof. Phil Jones

Climatic Research Unit

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School of Environmental Sciences

Fax +44 (0) 1603 507784

University of East Anglia

Norwich

Email p.jones@uea.ac.uk

NR4 7TJ

UK

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: request for some additional info.
Date: Sun, 08 Jun 2008 07:32:00 -0400
Reply-to: mann@psu.edu

Hi Phil,

I'm continuing to work on your nomination package (here in my hotel room in Trieste--the weather isn't any good!). If its possible for a case to be too strong, we may have that here! Lonnie is also confirmed as supporting letter writer, along w/ Kevin, Ben, Tom K, and Jean J. (4 of the 5 are already AGU fellows, which I'm told is important! Surprisingly, Ben is not yet, nor am I. But David Thompson is (quite young for one of these). I'm guessing Mike Wallace and Susan Solomon might have had something to do w/ that ;)

Anyway, I wanted to check w/ you on two things:

1. One thing that people sometimes like to know is the maximum value of "N" where "N" is the number of papers an individual authored/co-authored that have more than N citations. N=40 (i.e., an individual has published at least 40 papers that have each been cited at least 40 times) is supposedly an important threshold for admission in the U.S. National Academy of Sciences. I'm guessing your N is significantly greater than that, and it would be nice to cite that if possible. Would you mind figuring out that number and sending--I think it would be useful is really sealing the case.

2. Would you mind considering a minor revision of your 2 page bibliography. In my nomination letter, I'm trying to underscore the diverse areas where you've made major contributions, and I think its well known and obvious to many that two of these are instrumental data and paleoclimate reconstructions. But it occurs to me that it is equally important to stress your work in detection of anthropogenic impacts on climate w/ both models and observations. For example, your early Nature papers w/ Wigley. in '80 and '81 seem to be among the earliest efforts to try to do this (though I don't have copies of the papers, so can't read them!), and that seems very much worth highlighting to me. My suggestion is that you add a category on "Anthropogenic Climate Signal" detection and include this work (say, 8 or so of the key papers in this area including the two early Nature one's w/ Wigley) as well as some of your later work w/

Santer/Tett/Thorne/Hegerl/Barnett. I realize that most of your work in this area isn't as primary author, but I do think it would be helpful to show this side of your research, and I'd like to incorporate that into my nomination letter (i.e. how critical your efforts have been to developments in areas such as D&A). You could still fit this onto 2 pages by making the font smaller for the references (10pt rather than 11 pt) while keeping the headings at 11 pt, and if necessary you could probably sacrifice a few of the surface temperature record references to make space for the additional references.

Also, if you happen to have pdfs of the two early Wigley papers, or even just the text for the abstracts, it would be great to have a little more detail about those papers so I can appropriately work them into the narrative of my letter.

thanks for any help,
mike

p.s. please tell Keith I was very sorry he was unable to make it here to Trieste, I was really looking forward to seeing him (as were Ed and many others here). I hope all is well w/ his daughter.

--

Michael E. Mann
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References

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2. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Michael Mann To: P.Jones@uea.ac.uk Subject: Re: request for some additional info. Date: Wed, 11 Jun 2008 12:24:41 -0400 Reply-to: mann@psu.edu thanks Phil--yes, that's perfect. I just wanted to have some idea of the paper, that's more than enough info. I wouldn't bother worrying about scanning in, etc. I should have a draft letter for you to comment on within a few days or so, after I return from Trieste, talk to you later, mike [1]P.Jones@uea.ac.uk wrote: Mike, Thanks. The 1980/1981 papers. I don't have the pdfs. 1980: This paper looked (spatially) at temperatures and precipitation for the 5 warmest years during the 20th century and the 5 coldest. We then differenced these to produce what might happen. We expanded this in a DoE Tech Report to look at the warmest/coldest 20-year periods. This latter effort didn't make much difference. 1981: This looked at statistics of annual/winter/summer Temperatures for the NH and zones of the NH to see what signals might you be able to detect. SNR problem really. Showed that best place to detect was NH annual and also Tropics in summer. Last place to look was the Arctic because variability was so high. I did look a while ago to see if Nature had back scanned these papers, but they hadn't. Is the above enough? I have hard copies of these two papers - in Norwich Cheers Phil Hi Phil, thanks---yes, revised bibliography looks great. I'll can send you a copy of my nominating letter for comment/suggestions when done. also--can you provide one or two sentences about the '80 and '81 Nature articles w/ Wigley so that I might be able to work this briefly into the narrative of my letter? thanks, mike [2]P.Jones@uea.ac.uk wrote: Mike. Will this do? Have added in a section on D&A. You didn't send the narrative. Will I have to alter that? Hope to get out of AVL at 5pm tonight - thunderstorms permitting. Cheers Phil HI Phil,

OK--thanks, I'll just go w/ the H=62. That is an impressive number and almost certainly higher than the vast majority of AGU Fellows.

I've attached the 2 page bibliography. I think it would be good to add some some of the more prominent D&A type papers, especially those early ones because they seem to be ahead of their time, and it is a high profile topic (more so than hydrology!). but its your call.

Enjoy Asheville--say hi to Tom for me.

talk to you later,

mike

[4]P.Jones@uea.ac.uk wrote:

Mike,

Off to the US tomorrow for 1.5 days in Asheville.

On 1, this is what people call the H index. I've tried working this out and there is software for it on the web of science.

Problem is my surname. I get a number of 62 if I just use the software, but I have too many papers. I then waded through and deleted those in journals I'd never heard of and got 52. I think this got rid of some biologist from the 1970s/1980s, so go with 52.

I don't have pdfs of the early papers. I won't be able to do anything for a few days either. When do you want this in, by the way? Can you email me the piece I wrote for you, as I don't have this on my lap top. I can then pick it up tomorrow at some airport.

The D&A work has always been with others. There is another area on hydrology that I omitted as well.

Keith's daughter is OK. She had the operation last Tuesday. He should be over in Birmingham this weekend.

Cheers
Phil

Hi Phil,

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references

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thanks for any help,

mike

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-- Michael E. Mann Associate Professor Director, Earth System Science Center (ESSC) Department of Meteorology Phone: (814) 863-4075 503 Walker Building FAX: (814) 865-3663

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-- Michael E. Mann Associate Professor Director, Earth System Science Center (ESSC) Department of Meteorology Phone: (814) 863-4075 503 Walker Building FAX: (814) 865-3663 The Pennsylvania State University email: [\[14\]mann@psu.edu](mailto:[14]mann@psu.edu) University Park, PA 16802-5013 [\[15\]http://www.met.psu.edu/dept/faculty/mann.htm](http://www.met.psu.edu/dept/faculty/mann.htm) -- Michael E. Mann Associate Professor Director, Earth System Science Center (ESSC) Department of Meteorology Phone: (814) 863-4075 503 Walker Building FAX: (814) 865-3663 The Pennsylvania State University email: [\[16\]mann@psu.edu](mailto:[16]mann@psu.edu) University Park, PA 16802-5013 [\[17\]http://www.met.psu.edu/dept/faculty/mann.htm](http://www.met.psu.edu/dept/faculty/mann.htm) References 1. <mailto:P.Jones@uea.ac.uk> 2. <mailto:P.Jones@uea.ac.uk> 3. <mailto:P.Jones@uea.ac.uk> 4. <mailto:P.Jones@uea.ac.uk> 5. <mailto:mid:1079.87.113.67.115.1212941466.squirrel@webmail.uea.ac.uk> 6. <mailto:menn@psu.edu> 7. <mailto:menn@psu.edu>

edu 8. <http://www.met.psu.edu/dept/faculty/mann.htm> 9. <http://www.met.psu.edu/dept/faculty/mann.htm> 10. <mailto:mann@psu.edu> 11. <mailto:mann@psu.edu> 12. <http://www.met.psu.edu/dept/faculty/mann.htm> 13. <http://www.met.psu.edu/dept/faculty/mann.htm> 14. <mailto:mann@psu.edu> 15. <http://www.met.psu.edu/dept/faculty/mann.htm> 16. <mailto:mann@psu.edu> 17. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Ben Santer <santer1@llnl.gov>
To: amlibpub@gmail.com
Subject: Your website
Date: Fri, 13 Jun 2008 15:59:06 -0700
Reply-to: santer1@llnl.gov

<x-flowed>

To the Editor
American Liberty Publishers
Minneapolis, MN 55418

Dear Sir,

Your website (http://www.amlibpub.com/top/contact_us.html) was recently brought to my attention. On this site, you make the following claims:

"In the Second Assessment Report, Benjamin Santer, lead author of a crucial study, falsified a chart to make it appear to support global warming—a conclusion not supported at all by the original data. But two climatologists, Knappenberger and Michaels, looked up the data and exposed the fraud. Santer said he adjusted the data to make it agree with political policy."

These claims have no factual basis whatsoever, and are demonstrably libelous. I did not falsify data. I did not commit fraud. I did not - nor have I ever - "adjusted" scientific data "to make it agree with political policy." Nor did I ever state that I had made data adjustments in order to conform to political policy.

I request that you retract these claims immediately. They are completely fictitious, and are harmful to my scientific reputation. If you do not retract these claims immediately, I will transfer this matter to the attention of legal staff at Lawrence Livermore National Laboratory.

Sincerely,

Dr. Benjamin Santer
U.S. Dept. of Energy Distinguished Scientist (2006)
Ernest Orlando Lawrence Award (2002)
John D. and Catherine T. MacArthur Fellow (1998)

Benjamin D. Santer

Program for Climate Model Diagnosis and Intercomparison

Lawrence Livermore National Laboratory

P.O. Box 808, Mail Stop L-103

Livermore, CA 94550, U.S.A.

Tel: (925) 422-2486

FAX: (925) 422-7675

email: santer1@llnl.gov

</x-flowed>

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: nomination letter
Date: Thu, 19 Jun 2008 09:39:01 -0400
Reply-to: mann@psu.edu

<x-flowed>
thanks Phil--fixed!

waiting on two more letters, then I'll send in the package to AGU.
Should be a no-brainer!

talk to you later,

mike

Phil Jones wrote:

>
> Mike,
> There is one type in your nomination letter. I missed it first
> time I read it.
>
> In the second paragraph, second line remove the first 'surface'. You
> have
> two one before and one after (CRU). Just the one after needed.
>
> Cheers
> Phil

>
>
> At 16:59 18/06/2008, you wrote:
>> hey Phil, at Dulles waiting for flight to Orlando Florida.
>>
>> IUGG is the first time I ever met you. but I believe I had already
>> corresponded w/ you about some of the work I was doing w/ Ray w/
>> proxy records. But the thing we talked about was the quality of the
>> early Trenberth and Paolino SLP gridbox data. you alerted me to some
>> of the early problems w/ that dataset. It was very helpful. I was
>> young and naive!
>> anyway, it made a very positive impression on me that you were so
>> approachable. im' sure many others agree.
>>

>> got to run to my flight now. talk later,

>>

>> mike

>>

>> Phil Jones wrote:

>>>

>>> Mike,

>>> This is fine. I don't remember talking to you at IUGG in Boulder !

>>> I am approachable though and have talked to lots of people. I get

>>> people

>>> coming up to me now saying we met in 199? and have no recall

>>> of our meeting - sometime no recall of even going to the meeting

>>> where I was supposed to have met them!

>>>

>>> Another thanks for putting this all together.

>>>

>>> Cheers

>>> Phil

>>>

>>>

>>> At 22:04 14/06/2008, you wrote:

>>>> Hi Phil,

>>>>

>>>> I've attached a copy of my nomination letter. I just want to make

>>>> sure I've got all my facts right--please let me know if there is

>>>> anything I've gotten wrong or should be changed. I would be shocked

>>>> is this doesn't go through--you're a no-brainer, and long overdue

>>>> for this.

>>>>

>>>> I've got letters from 3 of the 5 other letter writers now, waiting

>>>> on the 2 last ones, then will submit the package.

>>>>

>>>> talk to you alter,

>>>>

>>>> mike

>>>>

>>>> --

>>>> Michael E. Mann

>>>> Associate Professor

>>>> Director, Earth System Science Center (ESSC)

>>>>

>>>> Department of Meteorology

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>>>> 503 Walker Building

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>>>> Prof. Phil Jones
>>>> Climatic Research Unit Telephone +44 (0) 1603 592090
>>>> School of Environmental Sciences Fax +44 (0) 1603 507784
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>>>> Norwich Email p.jones@uea.ac.uk
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</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tim Osborn <t.osborn@uea.ac.uk>, P.Jones@uea.ac.uk, "Caspar Ammann" <ammann@ucar.edu>
Subject: Re: Fwd: IPCC FOIA Request
Date: Mon Jun 23 09:47:54 2008

Caspar

I have been of the opinion right from the start of these FOI requests, that our private , inter-collegial discussion is just that - PRIVATE . Your communication with individual colleagues was on the same basis as that for any other person and it discredits the IPCC process not one iota not to reveal the details. On the contrary, submitting to these "demands" undermines the wider scientific expectation of personal confidentiality . It is for this reason , and not because we have or have not got anything to hide, that I believe none of us should submit to these "requests". Best wishes

Keith

At 09:01 23/06/2008, Tim Osborn wrote:

Hi Caspar,

I've just had a quick look at CA. They seem to think that somehow it is an advantage to send material outside the formal review process. But **anybody** could have emailed us directly. It is in fact a disadvantage! If it is outside the formal process then we could simply ignore it, whereas formal comments had to be formally considered. Strange that they don't realise this and instead argue for some secret conspiracy that they are excluded from!

I'm not even sure if you sent me or Keith anything, despite McIntyre's conviction! But I'd ignore this guy's request anyway. If we aren't consistent in keeping our discussions out of the public domain, then it might be argued that none of them can be kept private. Apparently, consistency of our actions is important.

Best wishes

Tim

At 07:37 23/06/2008, P.Jones@uea.ac.uk wrote:

Caspar,

In Zurich at MeteoSwiss for a meeting this week.

It doesn't discredit IPCC!

Cheers

Phil

> FYI, more later.

> Caspar

>

>

> Begin forwarded message:

>

>> From: Brian Lynch <killballyowen2003@yahoo.co.uk>
>> Date: June 21, 2008 3:30:28 PM MDT
>> To: ammann@ucar.edu
>> Subject: IPCC FOIA Request
>> Reply-To: killballyowen2003@yahoo.co.uk
>>
>> Dear Sir,
>>
>> I have read correspondence on web about your letter to the in
>> relation to expert comments on IPCC chapter 6 sent directly by you
>> to Keith Briffa, sent outside the formal review process.
>>
>> The refusal to give these documents tends to discredit you and the
>> IPCC in the eyes of the public,
>>
>> Could I suggest that you make your letter and documents pubic. I
>> would be very glad if you gave me a copy and oblige,
>>
>> Yours faithfully,
>>
>> Brian Lynch
>> Galway
>>
>> Sent from Yahoo! Mail.
>> A Smarter Email.

>
> Caspar M. Ammann
> National Center for Atmospheric Research
> Climate and Global Dynamics Division - Paleoclimatology
> 1850 Table Mesa Drive
> Boulder, CO 80307-3000
> email: ammann@ucar.edu tel: 303-497-1705 fax: 303-497-1348

>
>
>
>

Dr Timothy J Osborn, Academic Fellow
Climatic Research Unit
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ, UK
e-mail: t.osborn@uea.ac.uk

phone: +44 1603 592089

fax: +44 1603 507784

web: [1]<http://www.cru.uea.ac.uk/~timo/>

suncllock: [2]<http://www.cru.uea.ac.uk/~timo/suncllock.htm>

--

Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909

Fax: +44-1603-507784

[3]<http://www.cru.uea.ac.uk/cru/people/briffa/>

References

1. <http://www.cru.uea.ac.uk/~timo/>
2. <http://www.cru.uea.ac.uk/~timo/suncllock.htm>
3. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: P.Jones@uea.ac.uk, k.briffa@uea.ac.uk, ammann@ucar.edu
Subject: Re: CA
Date: Mon Jun 23 09:54:03 2008

Hi Phil, Keith and "Confidential Agent Ammann",
At 17:00 21/06/2008, P.Jones@uea.ac.uk wrote:

This is a confidential email

So is this.

Have a look at Climate Audit. Holland has put all the responses and letters up.

There are three threads - two beginning with Fortress and a third later one.

Worth saving the comments on a Jim Edwards - can you do this Tim?

I've saved all three threads as they now stand. No time to read all the comments, but I did note in "Fortress Met Office" that someone has provided a link to a website that helps you to submit FOI requests to UK public institutions, and subsequently someone has made a further FOI request to Met Office and someone else made one to DEFRA. If it turns into an organised campaign designed more to inconvenience us than to obtain useful information, then we may be able to decline all related requests without spending ages on considering them. Worth looking out for evidence of such an organised campaign.

Tim

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: "Andrew Revkin" <anrevk@nytimes.com>
Subject: Re: clearing up climate trends sans ENSO and perhaps PDO?
Date: Mon, 7 Jul 2008 20:33:44 -0600 (MDT)
Reply-to: trenbert@ucar.edu
Cc: gschmidt@giss.nasa.gov, mann@psu.edu, davet@atmos.colostate.edu, p.jones@uea.ac.uk, david.parker@metoffice.gov.uk, wpatzert@jpl.nasa.gov, ackerman@atmos.washington.edu, wallace@atmos.washington.edu, barnett-ul@ucsd.edu, sarachik@atmos.washington.edu, peter.thorne@metoffice.gov.uk, john.kennedy@metoffice.gof.uk, cwunsch@mit.edu

Andy
Here's some further results, based on the time series for 1900 to 2007

Results:

- (0) correlation between ENSO and PDO: for the smoothed IPCC decadal filter: 0.490662
- (0) correlation between ENSO and PDO: for the annual means: 0.527169
- (0) regression coef for PDO with global T : 0.0473447
- (0) regression coef for N34 with global T : 0.0664886

Data sources:

```
-----  
: PDO: http://www.jisao.washington.edu/pdo/  
: http://jisao.washington.edu/pdo/PDO.latest  
-----  
: N34: http://www.cgd.ucar.edu/cas/catalog/climind/Nino_3_3.4_indices.html  
: http://www.cgd.ucar.edu/cas/catalog/climind/TNI_N34/index.html#Sec5  
-----  
: CRU: http://www.cru.uea.ac.uk/cru/data/temperature/  
: Hadcrut: http://www.cru.uea.ac.uk/cru/data/temperature/hadcrut3vgl.txt  
=====
```

: Files were manually stripped for 1900 to 2007
:=====

These numbers mean that for a one standard deviation in the ENSO index there is 0.066C change in global T, or from PDO: 0.047C, but that much of the latter comes from the ENSO index. Very roughly, since the correlation is 0.5 between PDO and ENSO, half of the 0.066 or 0.033C of the 0.047 is from ENSO. Strictly one should do this properly using screening regression.

Kevin

> dear all,
> re-sending because of a glitch.
>
> finally got round to posting on an earlier inquiry I made to some of
> you about whether there was a 'clean' graph of multi-decades
> temperature trends with ENSO wiggles removed -- thanks to gavin (and
> david thompson) posting on realclimate.
> here's Dot Earth piece with link to Realclimate etc..
> <http://dotearth.blogs.nytimes.com/2008/07/07/climate-trends-with-some-noise-removed/?ex=1216094400&en=a57177d93165cba3&ei=5070>
>
> next step is PDO. has anyone characterized how much impact (if any)
> PDO has on hemispheric or global temp trends, and if so is there a
> graph showing what happens when that's accounted for?
>
> as you are doubtless aware, this is another bone of contention with a
> lot of the anti-greenhouse-limits folks and some scientists (the post
> 1970s change is a PDO thing, etc etc), hoping to show a bit of how
> that works.
>
> thanks for any insights.
> and i encourage you to comment and provide links etc with the current
> post to add context etc.
>
> --
> Andrew C. Revkin
> The New York Times / Science
> 620 Eighth Ave., NY, NY 10018
> Tel: 212-556-7326 Mob: 914-441-5556
> Fax: 509-357-0965
> www.nytimes.com/revkin

Kevin Trenberth
Climate Analysis Section, NCAR
PO Box 3000
Boulder CO 80307
ph 303 497 1318
<http://www.cgd.ucar.edu/cas/trenbert.html>

From: Ben Santer <santer1@llnl.gov>

To: P.Jones@uea.ac.uk

Subject: Re: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]

Date: Thu, 10 Jul 2008 13:56:40 -0700

Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

The wedding was really very moving and beautiful. I had a great time. I'm sending along a picture of Tom and Helen which was taken at Granite Island (near Victor Harbor). I don't know whether I've ever seen Tom as happy as he is now...

Myles (if it is Myles) was a bit pedantic in his second review. Karl (who is a very-mild-mannered guy) described the tone of the review as "whining". It seems like the Reviewer was saying, "I'm a lot smarter than you, and I could do all of this stuff much better than you've done". I was very unhappy about the "wilfully ignoring" bit. That was completely uncalled for.

Have a great time at Lake Constance, Phil. It's a beautiful part of the world.

Best regards, and best wishes to Ruth,

Ben

P.Jones@uea.ac.uk wrote:

- > Ben,
- > Will read the comments in detail tomorrow, when at CRU.
- > I presume the wedding went well and a good time was had
- > by all.
- >
- > I'm in CRU tomorrow, but away next week. I'm off to one
- > your old hunting grounds - Friedrichshafen. I am going to
- > a summer school on the other side of the Lake near Konstanz.
- > Can't recall the village name - something like Treffpunkt.
- >
- > Only gone a week, back Friday week.
- >
- > From a quick scan below Myles does seem to be a pain!

> As we both know he can be.

>

> Cheers

> Phil

>

>

>> Dear folks,

>>

>> I just returned from my trip to Australia - I had a great time there.

>> Now (sadly) it's back to the reality of Douglass et al. I'm forwarding

>> the second set of comments from the two Reviewers. As you'll see,

>> Reviewer 1 was very happy with the revisions we've made to the paper.

>> Reviewer 2 was somewhat crankier. The good news is that the editor

>> (Glenn McGregor) will not send the paper back to Reviewer 2, and is

>> requesting only minor changes in response to the Reviewer's comments.

>>

>> Once again, Reviewer 2 gets hung up on the issue of fitting higher-order

>> autoregressive models to the temperature time series used in our paper.

>> As noted in our response to the Reviewer, this is a relatively minor

>> technical point. The main point is that we include an estimate of the

>> standard error of the observed trend. DCPS07 do not, which is the main

>> error in their analysis.

>>

>> In calculating modeled and observed standard errors, we assume an AR-1

>> model of the regression residuals. This assumption is not unreasonable

>> for many meteorological time series. We and others have made it in a

>> number of previous studies.

>>

>> Reviewer 2 would have liked us to fit higher-order autoregressive models

>> to the T2, T2LT, and TS-T2LT time series. This is a difficult business,

>> particularly given the relatively short length of the time series

>> available here. There is no easy way to reliably estimate the parameters

>> of higher-order AR models from 20 to 30 years of data. The same applies

>> to reliable estimation of the spectral density at frequency zero (since

>> we have only 2-3 independent samples for estimating the spectral density

>> at frequency zero). Reviewer 2's comments are not particularly relevant

>> to the specific problem we are dealing with here.

>>

>> It's also worth mentioning that use of higher-order AR models for

>> estimating trend standard errors would likely lead to SMALLER effective

>> sample sizes and LARGER standard errors, thus making it even more

>> difficult to find significant differences between modelled and observed

>> trends! Our use of an AR-1 model makes it easier for us to obtain

>> "DCPS07-like" results, and to find significant differences between
>> modelled and observed trends. DCPS cannot claim, therefore, that our
>> test somehow stacks the deck in favor of obtaining a non-significance
>> trend difference - which they might claim if we used a
>> (poorly-constrained) higher-order AR model for estimating standard
>> errors.
>>
>> The Reviewer does not want to "see the method proposed in this paper
>> become established as the default method of estimating standard errors
>> in climatological time series". We do not claim universal applicability
>> of our approach. There may well be circumstances in which it is more
>> appropriate to use higher-order AR models in estimating standard errors.
>> I'd be happy to make a statement to this effect in the revised paper.
>>
>> I have to confess that I was a little ticked off by Reviewer 2's
>> comments. The bit about "wilfully ignoring" time series literature was
>> uncalled for. Together with my former MPI colleague Wolfgang
>> Brueggemann, I've fooled around with a lot of different methods of
>> estimating standard errors, in both the time domain and frequency
>> domain. One could write a whole paper on this subject alone. Such a
>> paper would not help us to expose the statistical deficiencies in
>> DCPS07. Nor would in-depth exploration of this issue lead to the shorter
>> paper requested by the Reviewer.
>>
>> It should take me a few days to revise the paper and draft a response to
>> Reviewer 2's comments. I'll send you the revised paper and draft
>> response early next week. Slowly but surely, we are getting there!
>>
>> With best regards,
>>
>> Ben
>> -----
>> Benjamin D. Santer
>> Program for Climate Model Diagnosis and Intercomparison
>> Lawrence Livermore National Laboratory
>> P.O. Box 808, Mail Stop L-103
>> Livermore, CA 94550, U.S.A.
>> Tel: (925) 422-3840
>> FAX: (925) 422-7675
>> email: santer1@llnl.gov
>> -----
>>
>>

>
>
>

--

Benjamin D. Santer
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Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
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Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

Attachment Converted: "c:\eudora\attach\DSCN2786.JPG"

From: Ben Santer <santer1@llnl.gov>
To: Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>
Subject: [Fwd: Re: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]]
Date: Thu, 10 Jul 2008 14:18:35 -0700
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Glenn,

I thought you might be interested in this email exchange with Francis Zwiers. It's directly relevant to the third criticism raised by Reviewer 2.

With best regards,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

X-Account-Key: account1

Return-Path: <francis.zwiers@ec.gc.ca>

Received: from mail-1.llnl.gov ([unix socket])

by mail-1.llnl.gov (Cyrus v2.2.12) with LMTPA;

Thu, 10 Jul 2008 13:08:08 -0700

Received: from nspiron-2.llnl.gov (nspiron-2.llnl.gov [128.115.41.82])

by mail-1.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.7 \$) with ESMTP id m6AK864P023034

for <santer1@mail.llnl.gov>; Thu, 10 Jul 2008 13:08:07 -0700

X-Attachments: None

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X-IronPort-AV: E=Sophos;i="4.30,340,1212390000";

d="scan'208";a="21284881"

Received: from nsziron-2.llnl.gov ([128.115.249.82])

by nspiron-2.llnl.gov with ESMTP; 10 Jul 2008 13:08:06 -0700

X-Attachments: None
X-IronPort-Anti-Spam-Filtered: true
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d="scan'208";a="42743336"
Received: from ecdow130.tor.ec.gc.ca (HELO OntExch1.ontario.int.ec.gc.ca) ([199.212.19.130])
by nsziron-2.llnl.gov with ESMTP; 10 Jul 2008 13:07:46 -0700
Received: from OntExch3.ontario.int.ec.gc.ca ([142.97.202.217]) by OntExch1.ontario.int.ec.gc.ca with Microsoft SMTPSVC(6.0.3790.3959);
Thu, 10 Jul 2008 16:07:45 -0400
Content-class: urn:content-classes:message
MIME-Version: 1.0
Content-Type: text/plain;
charset="us-ascii"
X-MimeOLE: Produced By Microsoft Exchange V6.5
Subject: RE: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]
Date: Thu, 10 Jul 2008 16:07:45 -0400
Message-ID: <33F9E32CDB0917428758DD583E747CC804095CEA@OntExch3.ontario.int.ec.gc.ca>
In-Reply-To: <487663E3.1040309@llnl.gov>
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]
Thread-Index: Acjiw9lJw91pKfupQQOFebAg5s2/SgAAHtnA
References: <48764B2C.5050004@llnl.gov> <33F9E32CDB0917428758DD583E747CC804095CB7@OntExch3.ontario.int.ec.gc.ca> <487663E3.1040309@llnl.gov>
From: "Zwiers,Francis [Ontario]" <francis.zwiers@ec.gc.ca>
To: <santer1@llnl.gov>
X-OriginalArrivalTime: 10 Jul 2008 20:07:45.0611 (UTC) FILETIME=[9E3BB9B0:01C8E2C8]

Hi Ben, sure, that would be fine.

Cheers, Francis

Francis Zwiers
Director, Climate Research Division, Environment Canada
4905 Dufferin St., Toronto, Ont. M3H 5T4
Phone: 416 739 4767, Fax 416 739 5700

-----Original Message-----

From: Ben Santer [mailto:santer1@llnl.gov]
Sent: July 10, 2008 3:33 PM
To: Zwiers,Francis [Ontario]
Subject: Re: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]

Dear Francis,

Thanks - this information will be extremely helpful in responding to Reviewer 2. I really do feel that the Reviewer is getting overly exercised about a relatively minor technical point. As you note, the key issue is that, in terms of the statistical significance testing, we are making it easier to get a "Douglass-like" result by using an AR-1 model for calculating the adjusted standard errors.

I'm concerned that going down the road proposed by Reviewer 2 could leave us open to unjustified criticism. It would be a shame if Douglass et al. argued (erroneously) that our failure to find significant differences between modelled and observed trends was spurious, and arose primarily from use of higher-order autoregressive models for calculating the adjusted standard errors.

Would it be o.k. to share your email with Glenn McGregor and with my other coauthors on the paper? Since you've looked at these issues in detail in your previous papers with Thiebaut and with Hans, your comments would be very useful background information for Glenn.

With best regards,

Ben

Zwiers,Francis [Ontario] wrote:

> Hi Ben,
>
> Sorry the 2nd reviewer is being a pain. As you say, there is already
> quite a bit of literature on dealing with dependence in tests of the
> mean (and this referee would have been critical if this paper had
> gone over that ground again :)).
>
> Regardless, you might be interested in the attached papers. Both
> contain relevant information and might help to formulate a response to

> the editor.
>
> Thiebaut and Zwiers show that the equivalent sample size is hard to
> estimate well, particularly from small samples. The approach proposed
> by the reviewer is what we termed the "ARMA" method, and it produces
> equivalent sample size estimates that have unacceptably large RMSE's

- > when the sample is small, even when the time series in question is not

- > very persistent (see Table 6).
- >
- > Zwiers and von Storch show the performance of an estimator of
- > equivalent sample size using the approach you use (i.e., assume the
- > data are AR(1)). They show that the equivalent sample size tends to be

- > over-estimated (Table 1) particularly when samples are small, and that

- > the corresponding t-test tends to operate at significance levels above

- > the nominal level (i.e., rejects too frequently - Table 2). So using
- > such a test in effect gives those who would like to reject the null
- > hypothesis a small leg up.
- >
- > Directly comparable results are not shown in the two papers, but you
- > can infer, from the comparison between equivalent sample size results
- > (Table
- > 6 in TZ, Table 2 in ZvS) that the "ARMA" approach for estimating
- > equivalent sample size would be much less reliable than the approach
- > that you are using (and thus, the sampled series would have to be very

- > far from being AR(1) for the ARMA approach to be beneficial). The
- > absolute key is to keep things as parsimonious as possible - there is
- > simply not enough data to entertain complex models of the
- > auto-covariance structure.
- >
- > Cheers, Francis
- >
- >
- > Francis Zwiers
- > Director, Climate Research Division, Environment Canada
- > 4905 Dufferin St., Toronto, Ont. M3H 5T4
- > Phone: 416 739 4767, Fax 416 739 5700
- >
- > -----Original Message-----
- > From: Ben Santer [mailto:santer1@llnl.gov]
- > Sent: July 10, 2008 1:47 PM
- > To: Thorne, Peter; Leopold Haimberger; Karl Taylor; Tom Wigley; John
- > Lanzante; ssolomon@frii.com; Melissa Free; peter gleckler; 'Philip D.
- > Jones'; Thomas R Karl; Steve Klein; carl mears; Doug Nychka; Gavin
- > Schmidt; Steven Sherwood; Frank Wentz

> Subject: [Fwd: JOC-08-0098.R1 - Decision on Manuscript]
>
> Dear folks,
>
> I just returned from my trip to Australia - I had a great time there.
> Now (sadly) it's back to the reality of Douglass et al. I'm forwarding

> the second set of comments from the two Reviewers. As you'll see,
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>
> In calculating modeled and observed standard errors, we assume an AR-1

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> frequency zero (since we have only 2-3 independent samples for
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> effective sample sizes and LARGER standard errors, thus making it even

> more difficult to find significant differences between modelled and
> observed trends! Our use of an AR-1 model makes it easier for us to
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> that our test somehow stacks the deck in favor of obtaining a
> non-significance trend difference - which they might claim if we used
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> (poorly-constrained) higher-order AR model for estimating standard
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>
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>
> I'd be happy to make a statement to this effect in the revised paper.
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> comments. The bit about "wilfully ignoring" time series literature was

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> Brueggemann, I've fooled around with a lot of different methods of
> estimating standard errors, in both the time domain and frequency
> domain. One could write a whole paper on this subject alone. Such a
> paper would not help us to expose the statistical deficiencies in
> DCPS07. Nor would in-depth exploration of this issue lead to the
> shorter paper requested by the Reviewer.

>
> It should take me a few days to revise the paper and draft a response
> to Reviewer 2's comments. I'll send you the revised paper and draft
> response early next week. Slowly but surely, we are getting there!

>
> With best regards,

>
> Ben

> -----

> --

> ----

> Benjamin D. Santer

> Program for Climate Model Diagnosis and Intercomparison Lawrence
> Livermore National Laboratory P.O. Box 808, Mail Stop L-103 Livermore,

> CA 94550, U.S.A.
> Tel: (925) 422-3840
> FAX: (925) 422-7675
> email: santer1@llnl.gov

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> --
> ----
>

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Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison Lawrence
Livermore National Laboratory P.O. Box 808, Mail Stop L-103 Livermore,
CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

From: Tim Osborn <t.osborn@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: A long and rocky road...
Date: Tue Jul 22 15:12:59 2008

Dear Ben,

well, thanks for your thanks. I'm not sure that I did all that much, but glad that the small amount is appreciated. It's a shame that the process couldn't have been quicker still, but hopefully the final production stage will pass smoothly.

Thanks for the copy of the paper, which I've skim read already -- looks very carefully done and therefore convincing (I'm sure you already heard that from others).

I note that you also provide some supporting online material (SOM). Provision of SOM is a relatively new facility for IJoC to offer and it may be suffering from teething problems.

A paper of mine (Maraun et al.) that appeared online in IJoC back in February still has its SOM missing! Hopefully this is a one-off omission, but I'll now email Glenn to remind him of this in relation to my paper and also point out that your paper has SOM. I think this is a problem on the publisher's side of things rather than an editorial problem.

Because of our absent SOM, we've temporarily posted a copy of the SOM on our personal website. If your SOM was delayed, and if you think that critics might complain if the paper appears without the SOM, you might want to post a copy of the SOM on your own website when the paper appears online. But hopefully there'll be no problem with it!

I heard you had a recent trip to Australia for Tom's wedding -- hope that was fun!

Best regards

Tim

At 22:28 21/07/2008, you wrote:

Dear Tim,

Our response to the Douglass et al. IJoC paper has now been formally accepted, and is "in press" at IJoC. I've appended a copy of the final version of the manuscript. It's been a long and rocky road, and I'll be quite glad if I never have to write another MSU paper again - ever!

I'd be grateful if you handled the paper in confidence at present. Since IJoC now has online publication, we're hoping that the paper will appear in the next 4-6 weeks.

Hope you are well, Tim. Thanks for all your help with the tricky job of brokering the submission of the paper to IJoC.

With best regards,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
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Tel: (925) 422-3840

FAX: (925) 422-7675

email: santer1@llnl.gov

From: Mike MacCracken <mmaccrac@comcast.net>
To: Jason Lowe <jason.lowe@metoffice.gov.uk>, Jerry Meehl <meehl@ucar.edu>
Subject: Re: Proposed experiment design for CMIP5
Date: Wed, 30 Jul 2008 11:25:01 -0400
Cc: "Cox, Peter" <P.M.Cox@exeter.ac.uk>, Karl Taylor <taylor13@llnl.gov>, <bryant.mcavaney@lmd.jussieu.fr>, Curtis Covey <covey1@llnl.gov>, "Mitchell, John FB (Chief Scientist)" <john.f.mitchell@metoffice.gov.uk>, <mlatif@ifm-geomar.de>, <Tom.Delworth@noaa.gov>, Andreas Hense <ahense@uni-bonn.de>, Asgeir Sorteberg <asgeir.sorteberg@bjerknes.uib.no>, Erich Roeckner <roeckner@dkrz.de>, Evgeny Volodin <volodin@im.ras.ru>, "Gary L. Russell" <Gary.L.Russell@nasa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, <GFDL.Climate.Model.Info@noaa.gov>, Greg Flato <gflato@ec.gc.ca>, Helge Drange <helge.drange@nersc.no>, Jean-Francois Royer <jean-francois.royer@meteo.fr>, Jean-Louis Dufresne <Jean-Louis.Dufresne@lmd.jussieu.fr>, Jozef Syktus <jozef.syktus@qld.gov.au>, Julia Slingo <J.M.Slingo@reading.ac.uk>, Kimoto Masahide <kimoto@ccsr.u-tokyo.ac.jp>, Peter Gent <gent@ucar.edu>, Qingquan Li <liqq@cma.gov.cn>, Seita Emori <emori@nies.go.jp>, Seung-Ki Min <seung-ki.min@ec.gc.ca>, Shan Sun <ssun@giss.nasa.gov>, Shoji Kusunoki <skusunok@mri-jma.go.jp>, Shuting Yang <shuting@dmu.dk>, Silvio Gualdi <gualdi@bo.ingv.it>, Stephanie Legutke <legutke@dkrz.de>, Tongwen Wu <twwu@cma.gov.cn>, Tony Hirst <Tony.Hirst@csiro.au>, Toru Nozawa <nozawa@nies.go.jp>, Wilhelm May <wm@dmu.dk>, Won-Tae Kwon <wontk@metri.re.kr>, Ying Xu <xuying@cma.gov.cn>, Yong Luo <ylo@cma.gov.cn>, Yongqiang Yu <yyq@lasg.iap.ac.cn>, Kamal Puri <K.Puri@bom.gov.au>, Tim Stockdale <Tim.Stockdale@ecmwf.int>, Gabi Hegerl <hegerl@duke.edu>, James Murphy <james.murphy@metoffice.gov.uk>, Marco Giorgetta <marco.giorgetta@zmaf.de>, George Boer <George.Boer@ec.gc.ca>, Myles Allen <m.allen1@physics.ox.ac.uk>, claudia tebaldi <claudia.tebaldi@gmail.com>, Ben Santer <santer1@llnl.gov>, Tim Barnett <tbarnett-ul@ucsd.edu>, Nathan Gillett <n.gillett@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, David Karoly <dkaroly@unimelb.edu.au>, DBithø Stone <stoned@atm.ox.ac.uk>, "Stott, Peter" <peter.stott@metoffice.gov.uk>, Francis Zwiers <Francis.Zwiers@ec.gc.ca>, Ken Sperber <sperber1@llnl.gov>, Dave Bader <bader2@llnl.gov>, <boyle5@llnl.gov>, Stephen Klein <klein21@llnl.gov>, "A. Pier Siebesma" <siebesma@knmi.nl>, William Rossow <wbrossow@gmail.com>, Chris Bretherton <breth@atmos.washington.edu>, George Tselioudis <gtselioudis@giss.nasa.gov>, Mark Webb <mark.webb@metoffice.gov.uk>, Sandrine Bony <Sandrine.Bony@lmd.jussieu.fr>, James Hack <jhack@cgd.ucar.edu>, Martin Miller <Martin.Miller@ecmwf.int>, Ken Kunkel <kkunkel@uiuc.edu>, Christian Jakob <c.jakob@bom.gov.au>, Kathy Hibbard <kathyh@cgd.ucar.edu>, "Eyring, Veronika" <veronika.eyring@dlr.de>, <pasb@dsm-mail.saclay.cea.fr>, <giorgi@ictp.trieste.it>, <c.lequere@uea.ac.uk>, <naki@eeg.tuwien.ac.at>, <stephen.griffies@noaa.gov>, Pierre Friedlingstein <pierre.friedlingstein@cea.fr>, Olivier Boucher <olivier.boucher@metoffice.gov.uk>, Bala Govindasamy <balal@llnl.gov>, Jonathan Gregory <j.m.gregory@reading.ac.uk>, Chris Jones <chris.d.jones@metoffice.gov.uk>, "Jones, Gareth S" <gareth.s.jones@metoffice.gov.uk>, David Lobell <dlobell@stanford.edu>, peter gleckler <gleckler1@llnl.gov>, Cath Senior

<cath.senior@metoffice.gov.uk>, Keith Williams
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Collins <wcollins@ucar.edu>, Ken Caldeira <kcaldeira@stanford.edu>, Dave
Randall <randall@atmos.colostate.edu>, Joyce Penner <Penner@umich.edu>,
Anna Pirani <anna.pirani@noc.soton.ac.uk>, Bjorn Stevens
<bstevens@atmos.ucla.edu>, Ronald Stouffer <Ronald.Stouffer@noaa.gov>

Dear Jason and Jerry (and Karl and Ron)--One of my suggestions on an
earlier round was such a simulation--to determine how models might do and
compare with a declining concentration (optimistic as such a scenario
might
be). The one you are doing would seem to have an overshoot on the
forcing,
but probably not (or not much) on the global average temperature due to
lag
effects in the system. It seems to me it would be worthwhile figuring out
such a run that also got the temperature decreasing, so maybe returned to
below the equivalent concentration we have now (so below something like
375
ppm when counting aerosol effects). In that such scenarios would likely
lead
to sharp cuts in CO2 emissions, they would also presumably lead to sharp
reductions in the SO2/SO4 offset, we are really already at about 450 ppm
CO2
equivalent for GHGs alone--and so to really get cooling started, the run
would likely have to go back to 350 ppm or below--so basically to the
level
Jim Hansen has been arguing is required to get back near 1990s climatic
conditions.

I would also note that the CO2 equivalence calculations are being done
using
the 100-year GWPs. While there is not much difference for N2O and most
halocarbons, the 20-year GWP for methane is about 3 times the 100-year
value
and so over the near-term methane changes (from stringent methane
control,
or additional release from thawing tundra) could have a very large effect
on
the short-term forcing and so on temperature change over the next several
decades, so when the peak occurs and how one comes back thereafter. While
CO2 control may well take time, methane control is very cost effective
and
should be being pushed very hard as a strategy (along with soot and air
pollutants contributing to tropospheric ozone--a point made several years
ago by Jim Hansen). In any case, it seems to me it is not implausible to
imagine that we could get to conditions where radiative forcing is coming
down, and that type of run needs to be explored--so having some sort of
standard run that groups could try if they have resources would make good
sense.

Mike MacCracken

On 7/29/08 4:48 PM, "Jason Lowe" <jason.lowe@metoffice.gov.uk> wrote:

> Hi Peter,

> I seem to be the only person not in Snowmass!

>

> In addition to the Japanese proof of concept the EU Ensembles project
> is also running a model intercomparison with a low end scenario that
> peaks at a little over 500ppm CO₂eq before declining to an eventual
> 450ppm. Emissions will be diagnosed and, hopefully, many of
> the groups with C-C cycle feedback will also diagnose the feedback!
> It will be interesting to see the spread.

>

> Regards,

>

> Jason

>

> On Tue, 2008-07-29 at 11:48 -0600, Jerry Meehl wrote:

>> Hi Peter,

>>

>> How long will you be in Snowmass? I get there tomorrow late afternoon
>> and will be there for the sessions Thursday and Friday. Ron and I
were

>> planning on re-visiting the experimental design more then, and if you
>> could join in that would be great.

>>

>> Regarding your point in favor of using the RCPs for carbon cycle
>> feedback, I think Ron and I arrived at this conclusion independently
>> while we both attended a US-Japan workshop in Colorado a few weeks
ago.

>> The Japanese have performed a proof-of-concept experiment using two
>> idealized mitigation scenarios and basically computed numbers for the
>> Aspen experiments you originally proposed in 2006. There were two key
>> additional points that we noted--one was that they started from a
>> pre-industrial control run so they had 20th and 21st century in the
>> "climate-carbon feedback" contrasted to "no-climate carbon feedback"
>> allowable emissions plots. Second, they had some kind of 20th century
>> "observations" of carbon emissions they plotted on their allowable
>> emissions graphs to show that their model with carbon-climate feedback
>> actually tracked those observations for 20th century. Since there are
>> so few observations to compare carbon cycle feedback to, this seemed
>> like a fairly compelling reason to use RCPs, which is what you also
note

>> below.

>>

>> I think Karl and Ron had lumped the carbon cycle feedback experiments
in

>> the 1% runs both because this had come up as a possibility in the
>> post-Aspen WGCM meeting in Victoria in 2006, and because it could
>> possibly present a more pleasing context to evaluate all feedbacks,
>> carbon cycle and all others. However, on further review, in addition
to

>> the points you raised, deriving allowable emissions from RCPs allows a

>> check to what the IAMs used for emissions in the first place (and used
>> to derive concentrations used in the ESMS). Also, it seems to me that
>> carbon cycle feedback falls into a new category of feedback that we in
>> the AOGCM world are not used to evaluating. We must depend on the
>> advice from you and others in that community. Though it's tempting to
>> think that everything can be boiled out of 1% runs, I think those are
>> most useful for feedbacks basically "managed" by the atmosphere (like
>> clouds, water vapor, etc.). The original Aspen concept for carbon
cycle
>> feedback always depended on using actual mitigation scenarios, and I
>> think we're coming around again to agreeing on that.
>>
>> Another point is that the cloud feedback community will make a
proposal
>> to WGCM to enlarge the idealized 1% feedback experiment list, so that
>> makes separating out the carbon cycle feedback experiments in a
separate
>> category using RCPs more compelling.
>>
>> Hopefully we can discuss this more Thursday.
>>
>> Jerry
>>
>> Cox, Peter wrote:
>>> Dear Karl and Ron
>>>
>>> Thanks for this very thorough document.
>>>
>>> Generally speaking I think we should be focusing much more on
realistic
>>> policy relevant scenarios rather than 1% per year type experiments.
There
>>> are two reasons for this:
>>> 1) Most now consider a ("business as usual") 1% per year scenario not
to
>>> represent a viable future. So detailed information on these scenarios
is
>>> less and less relevant to people outside of the GCM modeling
community.
>>> 2) More realistic scenarios allow us to utilize observations to
validate
>>> models/reduce uncertainties in a way that idealized scenarios do not.
>>>
>>> So I am in favour of diagnosing feedbacks in the more policy-relevant
RCP
>>> scenarios wherever possible. I say this even though Ron, who is
sitting
>>> beside me here now in Snowmass, has told me that this makes
identifying
>>> model differences more difficult. Ron also tells me that this is a
fight not
>>> worth fighting, but I can't resist commenting anyway..:-)
>>>

>>> More usefully I would like to respond to your PS. regarding the diagnosis of
>>> carbon cycle feedbacks. I strongly believe these should be diagnosed
>>> relative to the RCP scenarios. Carbon cycle feedbacks cannot easily be
>>> reduced to an equilibrium response plus a timescale. Carbon uptake
>>> essentially relies on disequilibrium and is therefore dependent on scenario,
>>> so I don't think it is very helpful to define c cycle feedback relative to
>>> idealised 1% per year runs. There are also the potential for significant
>>> "cold-start" problems with the carbon cycle (as land and ocean uptake are
>>> both highly dependent on history). So I vote for diagnosing carbon cycle
>>> feedbacks (at least) relative to the RCP scenarios.

>>>

>>> All the best

>>>

>>> Peter

>>>

>>> PLEASE NOTE NEW MOBILE NUMBER

>>> Prof Peter Cox,

>>> Met Office Chair in Climate System Dynamics,

>>> Room 336, Harrison Building,

>>> School of Engineering, Computing and Mathematics,

>>> University of Exeter,

>>> Exeter,

>>> EX4 4QF,

>>>

>>> Email: P.M.Cox@exeter.ac.uk,

>>> Tel (univ): 01392 269220,

>>> Tel (mob) : 07827 412572

>>>

>>>

>>>

>>> -----Original Message-----

>>> From: Karl Taylor [mailto:taylor13@llnl.gov]

>>> Sent: Tue 22-Jul-08 09:25 AM

>>> To: bryant.mcavaney@lmd.jussieu.fr; Curtis Covey; Jerry Meehl;

Mitchell,

>>> John FB (Chief Scientist); mlatif@ifm-geomar.de;

Tom.Delworth@noaa.gov;

>>> Andreas Hense; Asgeir Sorteberg; Erich Roeckner; Evgeny Volodin; Gary

L.

>>> Russell; Gavin Schmidt; GFDL.Climate.Model.Info@noaa.gov; Greg Flato;

Helge

>>> Drange; Jason Lowe; Jean-Francois Royer; Jean-Louis Dufresne; Jozef

Syktus;

>>> Julia Slingo; Kimoto Masahide; Peter Gent; Qingquan Li; Seita Emori;

>>> Seung-Ki Min; Shan Sun; Shoji Kusunoki; Shuting Yang; Silvio Gualdi;

>>> Stephanie Legutke; Tongwen Wu; Tony Hirst; Toru Nozawa; Wilhelm May;

Won-Tae

>>> Kwon; Ying Xu; Yong Luo; Yongqiang Yu; Kamal Puri; Tim Stockdale;
Gabi
>>> Hegerl; James Murphy; Marco Giorgetta; George Boer; Myles Allen;
claudia
>>> tebaldi; Ben Santer; Tim Barnett; Nathan Gillett; Phil Jones; David
Karloly;
>>> Döithö Stone; Stott, Peter; Francis Zwiers; Toru Nozawa; Ken Sperber;
Dave
>>> Bader; Mike MacCracken; boyle5@llnl.gov; Stephen Klein; A. Pier
Siebesma;
>>> William Rossow; Chris Bretherton;
>> George Tselioudis; Mark Webb; Sandrine Bony; James Hack; Martin
Miller; Ken
>> Kunkel; Christian Jakob; Kathy Hibbard; Eyring, Veronika;
>> pasb@lsce.saclay.cea.fr; giorgi@ictp.trieste.it; c.lequere@uea.ac.uk;
>> naki@eeg.tuwien.ac.at; stephen.griffies@noaa.gov; Cox, Peter; Pierre
>> Friedlingstein; Olivier Boucher; Bala Govindasamy; Jonathan Gregory;
Chris
>> Jones; Jones, Gareth S; David Lobell; peter gleckler; Cath Senior;
Keith
>> Williams; stephen e. schwartz; David Easterling; Inez Fung; Duane
Waliser;
>> William Collins; Ken Caldeira; Dave Randall; Joyce Penner; Anna
Pirani; Bjorn
>> Stevens
>>> Cc: Ronald Stouffer
>>> Subject: Proposed experiment design for CMIP5
>>>
>>> Dear all,
>>>
>>> As most of you know, plans are well underway for a coordinated set of
>>> climate model experiments, which will constitute the Fifth phase of
>>> CMIP. Attached is a description of the proposed experiments. As
>>> members of the CMIP panel, which was established by the WCRP's
Working
>>> Group on Coupled Modelling (WGCM) to help coordinate this activity,
we
>>> are seeking your comments. Considerable thought and input from a
wide
>>> community of scientists have already contributed to the CMIP5 design,
>>> and therefore major changes are not envisioned. Competing interests
and
>>> various tradeoffs have been carefully considered before coming up
with
>>> the proposed suite of experiments. Please keep in mind that modeling
>>> groups have limited resources and the experiment must represent a
>>> compromise among various priorities. We will not be able to please
everyone.
>>>
>>> The CMIP panel must present a final design plan for CMIP5 to the WGCM
at
>>> its annual meeting in September, just two months from now. Given
this
>>> tight deadline (which cannot slip if the CMIP5 results are to be

From: Phil Jones <p.jones@uea.ac.uk>
To: "Darch, Geoff J" <Geoff.Darch@atkinsglobal.com>
Subject: RE: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01
Date: Mon Aug 18 12:54:55 2008

At 13:35 20/05/2008, you wrote:

Phil,
Thanks for this.

In response:

1. I can't remember the thinking behind this - can you?
2. I don't think we'll be doing anything with UKCIP08 material, or briefing people; initially at least it will be about user needs without people thinking about how they might use UKCIP08, if that makes sense!
3. This is fine, although we may want some consistency between us e.g. Newcastle rates have been revised and are substantially larger than yours.
4. We need a pen portrait for Tim.
5. Thanks - we'll use this in with the other text.

Best wishes,
Geoff

-----Original Message-----

From: Phil Jones [[1]mailto:p.jones@uea.ac.uk]
Sent: 19 May 2008 15:36
To: Darch, Geoff J; Jim Hall; C G Kilsby; Mark New; ana.lopez@ouce.ox.ac.uk; Anthony Footitt; Suraje Dessai; Clare Goodess; t.osborn@uea.ac.uk
Cc: McSweeney, Robert; Arkell, Brian; Sene, Kevin
Subject: Re: EA 21389 - Probabilistic information to inform EA decision making on climate change impacts - PCC(08)01

Geoff,

Clare is off to Chelsea - back late tomorrow. We (Clare, Tim and me) have had a brief meeting. Here are some thoughts and questions we had.

1. Were we going to do two sets of costings?
2. Those involved in UKCIP08 (both doing the work and involved in the SG) have signed confidentiality texts with DEFRA. Not sure how these affect access to the headline messages in the drafts we're going to be looking at over the next few months. Also not sure how these will affect the UKCIP workshops that are coming up before the launch.
3. We then thought about costs for the CRU work. We decided on 25K for all CRU work. At £500 per day this comes to 50 days. We then split this into the tasks: 5 - 5 days, 6 - 5 days, 7 - 30 days, 10/11 - 5 days, which leaves 5 more days for meetings. Assumed the 25K was without travel to the meetings.
4. On CVs and pen portraits. Clare will send one before she leaves. Are what you have for Tim and me OK?
5. Some thoughts on Tasks 6 and 7
Task 6 - assumed this was mostly Newcastle.

Tim's work on rainfall extremes could be

fed in, and we can do something on non-rainfall variables. Assume also you expect us to

do waves, but not sure what we can do. It seems as though sea level has become waves?

Task 7 - assumed here Newcastle (Chris/Hayley) would be doing something on blocking (large-scale variability). Oxford would do the final bit on conceptual representation

of emissions and climate system and sensitivities, so based on GCMs.

This leaves CRU for the other three, which we base mainly on the 11 RCM runs, which we can access through LINK. We could also use ENSEMBLES runs for the others, but these would be RCMs. They seem more relevant for the sorts of scales UKCOP08 is working at.

All just a few thoughts at this time.

Can you send the UKWIR bid that went off, so we have a copy?

Cheers

Phil

At 09:06 16/05/2008, Darch, Geoff J wrote:

>Dear all,

>

>Please find attached the final tender pack for the Environment Agency

>bid. The tasks have been re-jigged, with the main change being a

>broadening of flood risk management to flood and coastal erosion risk

>management (FCERM). This means a wider audience to include all

>operating authorities, and the best practice guidance required (new

>Task

>11) is now substantial element, to include evaluation of FCERM climate

>change adaptation, case studies and provision of evidence to help

>upgrade the FCDPAG3 Supplementary Note.

>

>We have just one week to finish this tender, as it must be posted on

>Friday 23rd. We are putting together the bid document, which we'll

>circulate on Monday 19th, but in the meantime, and by the end of

>Tuesday 20th, I need everyone to send information (as indicated in

>brackets) to support the following structure:

>

>+ Understanding of the tender

>+ Methodology and programme (methodology for tasks / sub-tasks - see

>below - and timing)

>+ Project team, including individual and corporate experience (who you

>are putting forward, pen portraits, corporate case studies)

>+ Financial and commercial (day rates and number of days; please also

>highlight potential issues with the T&Cs e.g. IPR)

>+ Health & Safety, Quality and Environmental Management Appendices

>+ (full CVs, limited to 6 pages)

>

>Please send to me and Rob McSweeney. The information I have already

>e.g. on day rates, core pen portraits etc will go straight into the

>version we're working on, so no need to re-send.

>

>In terms of tasks (new nos.), the following organisation is suggested

>based on what has been noted to date:

>

>Task 1 (Inception meeting and reporting) Atkins, supported by lead

>representatives of partners Task 2 (Project board meetings) Atkins,

>supported by lead representatives of partners Task 3 (Analysis of user
>needs) Atkins with Tyn@UEA and OUCE, plus Futerra depending on style
>Task 4 (Phase 2 programme) Atkins, supported by all Task 5 (Interpret
>messages from UKCIP08 projections) CRU, OUCE and Newcastle, with Atkins
>advice on sectors Task 6 (Development of business specific projections)
>Newcastle and CRU, with Atkins advice on policy and ops Task 7 (Putting
>UKCIP08 in context) CRU, Newcastle and OUCE Task 8 (User guidance)
>Atkins, Tyn@UEA, Futerra Task 9 (Pilot studies) Atkins, Newcastle,
>OUCE, Tyn@UEA Task 10 (Phase 3 programme) Atkins, supported by all Task
>11 (Best Practice Guidance for FCERM) Newcastle and Atkins, with CRU
>Task 12 (Awareness raising events) Atkins, key experts, Futerra
>(perhaps as an option as EA are quite specific here) Task 13 (Training
>events) Atkins and Futerra

>
>Note that Futerra is a communications consultancy, specialising in
>sustainability, who will input on workshops and on the guidance
>documents.

>
>I'll be in touch again early next week.

>
>Best wishes,

>
>Geoff

>
>Geoff Darch

>
>Senior Consultant
>Water and Environment
>ATKINS

>
>Broadoak, Southgate Park, Bakewell Road, Orton Southgate, Peterborough,
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>
>
>
>
>

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>
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References

1. <mailto:p.jones@uea.ac.uk>
2. http://www.atkinsglobal.com/climate_change
3. http://www.atkinsglobal.com/terms_and_conditions/index.aspx

From: Phil Jones <p.jones@uea.ac.uk>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: Re: Revised version the Wengen paper
Date: Wed Aug 20 09:32:52 2008
Cc: Michael Mann <mann@meteo.psu.edu>

Gavin,

Almost all have gone in. Have sent an email to Janice re the regional freshening. On the boreholes I've used mostly Mike's revised text, with bits of yours making it read a little better.

Thinking about the final bit for the Appendix. Keith should be in later, so I'll check with him - and look at that vineyard book. I did rephrase the bit about the 'evidence' as Lamb refers to it. I wanted to use his phrasing - he used this word several times in these various papers. What he means is his mind and its inherent bias(es).

Your final sentence though about improvements in reviewing and traceability is a bit of a hostage to fortune. The skeptics will try to hang on to something, but I don't want to give them something clearly tangible.

Keith/Tim still getting FOI requests as well as MOHC and Reading. All our FOI officers have been in discussions and are now using the same exceptions not to respond - advice they got from the Information Commissioner. As an aside and just between us, it seems that Brian Hoskins has withdrawn himself from the WG1 Lead nominations. It seems he doesn't want to have to deal with this hassle.

The FOI line we're all using is this. IPCC is exempt from any countries FOI - the skeptics have been told this. Even though we (MOHC, CRU/UEA) possibly hold relevant info the IPCC is not part our remit (mission statement, aims etc) therefore we don't have an obligation to pass it on.

Cheers

Phil

At 18:07 19/08/2008, you wrote:

Phil, here are some edits - mostly language, a couple of bits of logic, an attempt to soothe Mike on the borehole bit, and a paragraph for consideration in the Appendix. Two questions require a little thinking - the reference to 'regional freshening' on the coral section needs to be more specific - I doubt it is a global phenomena, second there is an 'in prep' reference to some new work by van Ommen - I don't think this is appropriate and should either be removed and put as a personal communication.

Having looked over the tropical trees section, I think that's fine.

The fig A1 does need labelling though.

Gavin

On Tue, 2008-08-19 at 09:11, Phil Jones wrote:

> Mike,

> Peck didn't do the speleothem bit either.

> Cheers

> Phil

>

> Mike,

> Have your text in - just need to read the borehole section again.

> Noted your comment re the final Appendix figure. Will look at more

> when Tim back.

> Peck's bit is 2.5 and the terrestrial part of 2.6 - except for the

> borehole text.

>

> Next time I co-ordinate anything I'll get the GB cycling coach

> involved. We've just one our 7th gold medal on two wheels. Only

> one short of Phelps.

>

> Cheers

> Phil

>

>

> At 13:52 19/08/2008, Michael Mann wrote:

>> thanks Phil--which part is Peck's? I'd like to read it over

>> carefully,

>>

>> mike

>>

>> Phil Jones wrote:

>>> Mike, Gavin,

>>> On the final Appendix plot, the first and last 12 years of

>>> the annual CET record

>>> were omitted from the smoothed plot. Tim's away, but when he did

>>> this with

>>> them in the light blue line goes off the plot at the end. The

>>> purpose of the piece

>>> was to show that the red/black lines were essentially the same.

>>> It wasn't

>>> to show the current light blue smoothed line was above the

>>> red/blue lines,

>>> as they are crap anyway.

>>> The y-axis scale of the plot is constrained by what was in

>>> the IPCC
>>> diagram from the first report. What we'll try is adding it fully
>>> back in or
>>> dashing the first/last 12 years. The 50-year smoother includes
>>> quite
>>> a bit of padding - we're using your technique Mike. The issue is
>>> that CET
>>> has been so warm the last 20 years or so.
>>> Normal people in the UK think the weather is cold and the
>>> summer is
>>> lousy, but the CET is on course for another very warm year.
>>> Warmth
>>> in winter/spring doesn't seem to count in most people's minds
>>> when it comes to warming.
>>>
>>> Will mod the borehole section now. Because this had been
>>> written
>>> by Juerg initially, I added in a paraphrased section from AR4. I
>>> will
>>> mod this accordingly. Hope you noticed Peck's stuff.
>>>
>>> Cheers
>>> Phil
>>>
>>> At 17:28 18/08/2008, Michael Mann wrote:
>>>> Hi Phil,
>>>>
>>>> traveling, and only had brief opportunity to look this over.
>>>> only 2 substantial comments:
>>>>
>>>> 1. I don't know who wrote the first paragraph of section 3.3
>>>> (bottom of page 52/page 53), but the lack of acknowledgement
>>>> here in this key summary that we actually introduced the idea of
>>>> 'pseudoproxies' into the climate literature is very troubling.
>>>> the end of the first sentence:
>>>> e.g., Zorita and González-Rouco, 2002, Küttel et al., 2007),
>>>> should be changed to:
>>>> e.g., Mann and Rutherford, 2002; Zorita and González-Rouco,
>>>> 2002, Rutherford et al, 2003; Küttel et al., 2007),
>>>>
>>>> 2. I'm also a bit confused and very concerned about the
>>>> description of smoothing in Appendix A Figure 1. It sounds like
>>>> the last 12 years were removed from the end of the series? If

>>>> so, that's not a fair comparison because its really the past
>>>> decade that takes us into 'unprecedented' territory. I would
>>>> suggest one of two alternative approaches:
>>>> a. show the full smoothed curve without removing end data (I
>>>> don't see any objective justification for doing that) or
>>>> b. show the raw annual data through 2006 so readers can see how
>>>> the most recent values compare w/ the MWP peak.
>>>>
>>>> By the way, I have a revised version of Mann [2004] now in press
>>>> in GRL, I've attached. Please don't distribute or cite prior to
>>>> publication (which should be one or two weeks from now).
>>>>
>>>> thanks,
>>>>
>>>> mike
>>>>
>>>>
>>>> Phil Jones wrote:
>>>>> Dear All,
>>>>> Here's the revised version of the paper, together with
>>>>> the responses to the reviewers.
>>>>> We have told John Matthews, that we will get this back to him
>>>>> by the beginning
>>>>> of next week. To us in the UK this means Aug 26/27 as next
>>>>> Monday is a national
>>>>> holiday. So, to those not away at the moment, can you look
>>>>> through your
>>>>> parts and get any comments back to us by the end of this week
>>>>> or over the
>>>>> weekend?
>>>>> Can you also look at the references - those in yellow and
>>>>> let me know of
>>>>> any that have come out, or are able to correct those that I
>>>>> think just look
>>>>> wrong?
>>>>> I hope you'll think of this as an improvement.
>>>>>
>>>>> Cheers
>>>>> Phil
>>>>>
>>>>>
>>>>> Prof. Phil Jones
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>>>> --

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>>>>
>>>> website:
>>>>
>>>>
>>>> [1]<http://www.met.psu.edu/dept/faculty/mann.htm>
>>>> "Dire Predictions" book site:

>>>>
>>>>
>>>> [2]<http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.html>
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>> [3]<http://www.met.psu.edu/dept/faculty/mann.htm>
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>>
>> [4]<http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.html>
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References

1. <http://www.met.psu.edu/dept/faculty/mann.htm>
2. <http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.html>
3. <http://www.met.psu.edu/dept/faculty/mann.htm>
4. <http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.html>

From: Gabi Hegerl <gabi.hegerl@ed.ac.uk>
To: tbarnett-ul@ucsd.edu
Subject: Re: comments on AR5 experimental design - reply by Aug 28
(thursday)
Date: Wed, 27 Aug 2008 09:33:33 +0100
Cc: dpierce@ucsd.edu, JKenyon <kenyon@duke.edu>, Myles Allen
<m.allen1@physics.ox.ac.uk>, Nathan <n.gillett@uea.ac.uk>, Phil Jones
<p.jones@uea.ac.uk>, David Karoly <dkaroly@unimelb.edu.au>, Knutti Reto
<reto.knutti@env.ethz.ch>, Toru Nozawa <nozawa@nies.go.jp>, Tom Knutson
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<rls@email.unc.edu>, Daithi Stone <stoned@atm.ox.ac.uk>, "Stott, Peter"
<peter.stott@metoffice.gov.uk>, Michael Wehner <mfwehner@lbl.gov>,
Francis Zwiers <francis.zwiers@ec.gc.ca>, Hans von Storch
<hvonstorch@web.de>

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Thanks Tim! We'll have another round later, confirmed by Tim, when we discuss storage and documentation - probably should try before WGCM meeting so that David can present results.

the 'near term prediction' is a mip all by itself, so there will be some guidance coming up hopefully!

In terms of ensemble size: for the stuff I was involved in, even one run from a model was good since it increased the overall ensemble size for multi model means and estimates of variance - did you analyze models individually? I would be keen to hear from the group:

is say a single 20th c run, single natural only run, single ghg run
a) useless
b) much better than nothing?

| vouch for b) for things I was involved in but it would be good to know for which applications its a!
Gabi

Tim Barnett wrote:

> hi gabi..in real haste....people will use the AR5 data set for impact
> studies no doubt about it. so what will they find when they jump
> in....same as we did trying to do the western D&A work with AR4....a
very
> disparate set of numbers.
> 1.some models don't give the data one would like.
> 2.some models have only 1 realization...which makes them useless. we
> found that with multiple realizations one can do statistics with
ensemble
> techniques which give a lot more statistical power. suggesting 10
member
> ensembles. with less the S/N can be small...e.g. we could not use the
> GFDL runs very well as they were so noisy and had few (5)
realizations)

> 3. daily data is required. storage is cheap these days so at least daily
> data for order 100 years is desired. otherwise it is finageled a la the
> current downscaling methods (save one).
> 4. the 20th century runs need to go to 2015 as suggested by IDAG. we had
> to stop at 1999 and lost 8 years we would well like to have studies.
> 5. some of the variables we needed to compare with satellite obs were
> largely missing, e.g. clouds information.
> 6. to Mike's point....just what data is going to be saved?
> 7. i hope potential users of the data aside from the modeling groups get
> a say in what is archived. we are to the point now where policy makers
> want our best guesses as to what will happen in the next 20 years. the
> people who will make those 'guesses' are most likely not in the major
> model centers.
>
> I invite David Pierce to chip in here as he spend alot of time in the
> details of the data sets and associated problems.
>
> sorry to be so hasty but such is life at the moment. best, tim
>
>
>
>
>> Hi IDAG'ies,
>>
>> As you probably know, a proposal for the AR5 experiments is being
>> circulated in the moment, with comments due by September 1. This will
>> then be presented at the working group for coupled modelling (WGCM)
>> meeting in Paris, which David Karoly will attend.
>> Peter Stott and I discussed the draft when I visited last week, and we
>> drafted a response and suggestions from IDAG (attached) Please let me
>> know if you are ok with this (if I dont hear back I assume you are),
>> if you suggest changes and if you want us to add another
topic/concern.
>>
>> I would need this by next thursday to add it to a comment 'from IDAG'
>> to be sent in time, and then hopefully David can present this also in
>> Paris at the WGCM meeting.
>>
>> hope you all had a nice summer, and still remember our next meeting in
>> planning, and your IDAG tasks :))
>>
>> Gabi
>>
>>
>> p.s. we were wondering also about forcing, and if the forcing issue
>> (how stored, synchronized?) should be added. However, given even some
>> 'rich' modelling groups worry about getting the mandatory experiments
>> through we should however not hope that groups will run more than 1
>> single forcing set for the 20th century, and arguments against
>> synchronizing are that its not feasible for many forcings (eg
>> aerosols) and that we loose quite a bit of information if only a

>> single, for example, set of solar forcings were used and with this
>> open the AR5 up for criticism. Ideally, of course, one center would
>> systematically explore all the forcings - but I am not sure somebody
>> is planning to do this - in that case, a common set of 20th century
>> forcings may be an advantage. Based on some EU project, forcings are
>> synchronized for some European modeling centers - we could draw
>> attention to that if you feel strongly about this...anyway, I hesitate
>> to start a discussion about this...

>>

>>

>> --

>> Gabriele Hegerl

>> School of GeoSciences

>> University of Edinburgh

>> <http://www.geos.ed.ac.uk/people/person.html?indv=1613>

>>

>> --

>> The University of Edinburgh is a charitable body, registered in

>> Scotland, with registration number SC005336.

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Scotland, with registration number SC005336.

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From: Phil Jones <p.jones@uea.ac.uk>
To: Caspar Ammann <ammann@ucar.edu>
Subject: Re: New Wengen Draft -- including changes to accommodate new Figure 3
Date: Wed Aug 27 14:31:48 2008
Cc: Eugene Wahl <Eugene.R.Wahl@noaa.gov>, t.osborn@uea.ac.uk

Caspar,
Thanks.

Phil

At 14:16 27/08/2008, Caspar Ammann wrote:

Phil,
I worked on the figures yesterday and sent them off to Gene for double check. Will be one panel each (6), much improved legibility and significantly reduced "footprint" in the appearance of the text. You should have them before the end of your day.
Thanks for all your work on this paper! (Tim too!)

Cheers,
Caspar

On Aug 27, 2008, at 2:42 AM, Phil Jones wrote:

Caspar, Gene,

We're going to send the manuscript back tomorrow. If we get a revised diagram we'll include - otherwise we won't.

Have had a few more comments, but nothing substantial. All yours Gene are in, as are those from Gavin, Mike, Juerg and the coral people. There is a completely revised tropical dendro section and Peck finally came through with a section on less-resolved proxies and varves.

All in all it reads very well and the recommendations should prove very useful for PAGES.

Cheers
Phil

At 04:52 26/08/2008, Caspar Ammann wrote:

Hey Gene,
I'll see how I can adjust the figures to fit.

Caspar

On Aug 25, 2008, at 8:30 PM, Eugene Wahl wrote:

Hi Phil and Tim, and Caspar:

Here are my full set of comments on the entirety of section 3, the figures relevant to section 3, the authors' address, and abstract (none there). I made slight changes in the portion of the text already sent last night, sorry that I could not avoid that!

Caspar, please note that I've operated here on the assumption that Figure 3 is simplified to one panel for each section, according to the suggestions we have talked

about, but does contain all 6 portions, A-F.

There are two versions: one with just the relevant portions of the text, and the full amended text document. The changes noted should be identical in each version.

Peace, Gene

Dr. Eugene R. Wahl

Physical Scientist

NOAA/NESDIS/NCDC/Paleoclimate Branch

325 Broadway Street

Boulder, CO 80305

303-497-6297

[1]<http://www.ncdc.noaa.gov/paleo/paleo.html>

[2]P.Jones@uea.ac.uk wrote:

Gene,

Thanks. Today is a holiday here. We'll all be back in CRU tomorrow. So, we'll begin revising Section 3 then. Have had quite a few comments so far, and all are in.

New Figure 3 most appreciated. We must send this off on Thursday or Friday.

Hope you're settling in to Boulder life. At least you should be able to contact Caspar more easily!

Cheers

Phil

----- Original Message -----

Subject: New Wengen Draft

From: [3]Eugene.R.Wahl@noaa.gov

Date: Mon, August 25, 2008 2:45 am

To: [4]p.jones@uea.ac.uk

Hi Phil:

I've had to wait to the weekend to get to this, due to several other matters that had to be attended to here at NOAA this week and in relation to a report required by a funder that was due Friday.

I've looked over about half of section 3 (up to the start of section 3.4.2), and also the abstract and the authors' address section.

Attached are my comments on those sections. I will be getting to the rest of section 3 tonight and tomorrow and will send anything else to you. Everything is done in WORD with "Track Changes" turned on.

HIGHLIGHTS

1) My address information has been updated to include my NOAA information, which is now appropriate. The original Alfred information is kept, as also appropriate. I've condensed it all to not change the overall page spacing of the address citations.

2) The addition to the results description of the Riedwyl et al.

(2008) paper across pp 10-11 here (near the top of p 56 in the text you

sent this week). It is NECESSARY to keep this addition, as the text as it was "overemphasized" the differential quality of the RegEM results in this study. Their graphs 4 and 6 clearly show the results I added, in which RegEM for winter adds quite problematic artifacts at the highest levels of noise added. The white-noise SNR at which this happens (0.25), while low, is not outside of what reality might bring. [NB: I have talked with Juerg about this situation, and he is clearly aware of my sense that RegEM is given too high marks in this context.]

3) I added very brief descriptions how the CFRs actually come up with a reconstruction to the descriptions of them in section 3.2. If you feel these three sentences cannot be included I understand, but I think they are useful for the readers to know HOW the covariance information we are talking about there is actually used.

TO COME: Caspar and I are working out a much simplified version of Figure 3 (one panel per each section A-F), which I think will be much better than what is there now. We communicated on that Friday and yesterday, and are now close to having a new graphic. I will adapt the references to Figure 3 in section 3.4.2 and in the figure caption in my next message accordingly, which I plan will come either tonight or tomorrow.

Peace, and again thanks!

Gene

----- Original Message -----

From: From Phil Jones New Wengen Draft

Dear All,

Here's the revised version of the paper, together with the responses to the reviewers.

We have told John Matthews, that we will get this back to him by the beginning

of next week. To us in the UK this means Aug 26/27 as next

Monday

is a national

holiday. So, to those not away at the moment, can you look

through

your

parts and get any comments back to us by the end of this week or

over

the

weekend?

Can you also look at the references - those in yellow and let

me

know of

any that have come out, or are able to correct those that I

think

just look
wrong?

I hope you'll think of this as an improvement.

Cheers

Phil

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<wengendraft_version_18Aug_Wahl_review_SHORT_b.doc><wengendraft_version_18Aug_Wahl_revi
w.doc>

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References

1. <http://www.ncdc.noaa.gov/paleo/paleo.html>
2. <mailto:P.Jones@uea.ac.uk>
3. <mailto:Eugene.R.Wahl@noaa.gov>
4. <mailto:p.jones@uea.ac.uk>
5. <mailto:p.jones@uea.ac.uk>
6. <mailto:ammann@ucar.edu>
7. <mailto:p.jones@uea.ac.uk>
8. <mailto:ammann@ucar.edu>

From: Michael Mann <mann@meteo.psu.edu>
To: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: paper on smoothing
Date: Fri, 29 Aug 2008 15:53:41 -0400
Reply-to: mann@psu.edu
Cc: Kevin Trenberth <trenbert@ucar.edu>, Curtis Covey <coveyl@llnl.gov>, mann@psu.edu, "Folland, Chris" <chris.folland@metoffice.gov.uk>, Ben Santer <santer1@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>

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yeah, its statistically real, but an artifact almost certainly of natural variability. As Josh Willis nicely pointed out in a recent interview, anyone citing this as a reason to doubt the reality of anthropogenic climate change is like a vegas roller thinking he can beat the system because he's on a momentary winning streak...

m

Thomas.R.Karl wrote:

> Curt,
>
> At this point the leveling off is more of a Blog myth than any change
> point scientific analysis
>
> Tom
> Kevin Trenberth said the following on 8/29/2008 3:47 PM:
>> No
>> Kevin
>>
>> Curtis Covey wrote:
>>> Very interesting. Does it mean that the apparent leveling-off of
>>> global mean surface temperature since the turn of the century is due
>>> to "artificial suppression of trends near the time series boundaries"
>>> ?
>>>
>>> - Curt
>>>
>>> Michael Mann wrote:
>>>> dear all,
>>>>
>>>> attached is a paper of mine (GRL) on time series smoothing that
>>>> might be of interest.
>>>>
>>>> best regards,
>>>>
>>>> mike
>>>>
>>
>

--

Michael E. Mann
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Director, Earth System Science Center (ESSC)

| | |
|-----------------------------------|-----------------------|
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| 503 Walker Building | FAX: (814) 865-3663 |
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website: <http://www.met.psu.edu/dept/faculty/mann.htm>

"Dire Predictions" book site:

<http://www.pearsonhighered.com/academic/product/0,3110,0136044352,00.html>

</x-flowed>

From: P.Jones@uea.ac.uk

To: trenbert@ucar.edu

Subject: Re: Climate

Date: Wed, 17 Sep 2008 16:39:07 +0100 (BST)

Cc: Wibjörn Karlén <wibjorn.karlen@kultgeog.uu.se>, "Phil Jones" <p.jones@uea.ac.uk>

Wibjorn,

I'm in Athens at the moment. Unless you're referring specifically to the Arctic the temperature curves in IPCC Ch 3 all include the oceans.

Fennoscandia is just a small part of the NH. When I'm back next week, I'll be able to calculate the boxes that encompass Fennoscandia, so you can compare with this region. As you're aware Anders did lots of the update work in 2001-2002 and he included all the NORDKLIM data. I can send you a list of the Fennoscandian data if you want - either the sites used or their data as well.

I guess your attachments are in your direct email, which I come to later.

One final thing - we are getting SST data in from some of the new sea-ice free parts of the Arctic. We are not using these as we've yet to figure out how to as we don't have normals for these 'mostly covered by sea ice in the 1961-90' areas.

Cheers

Phil

> Hi Wibjorn

> It appears that your concern is mainly with the surface temperature record,
> and my co lead author in IPCC, Phil Jones, is best able to address those
> questions. However the IPCC only uses published data plus their
> extensions and in our Chapter the sources of the data are well documented,
> along with their characteristics. I offer a few more comments below (my
> comments are limited as I am on vacation and away from my office).

>

>

>>

>>

>> Uppsala 17 September 2008,

>>

>>

>>

>> Dear Kevin,

>>

>>

>>

>> In short, the problem is that I cannot find data supporting the
>> temperature

>> curves in IPCC and also published in e.g. Forster, P. et al. 2007:

> Assessing uncertainty in climate simulation. Nature 4: 63-64.

>>

>>

>>

>> In attempts to reconstruct the temperature I find an increase from the
> early

>> 1900s to ca 1935, a trend down until the mid 1970s and so another

> increase

>> to about the same temperature level as in the late 1930s.

>>

>>

>>

>> A distinct warming to a temperature about 0.5 deg C above the level 1940
>> is

>> reported in the IPCC diagrams. I have been searching for this recent

> increase, which is very important for the discussion about a possible

> human

>> influence on climate, but I have basically failed to find an increase

>> above

>> the late 1930s.

>>

>

> This region, as I am sure you know, suffers from missing data and large
> gaps spatially. How one covered both can greatly influence the outcome.

> In IPCC we produce an Arctic curve and describe its problems and

> character. In IPCC the result is very conservative owing to lack of

> inclusion of the Arctic where dramatic decreases in sea ice in recent

> years have taken place: 2005 was lowest at the time we did our assessment

> but 2007 is now the record closely followed by 2008. Anomalies of over 5C

> are evident in some areas in SSTs but the SSTs are not established if

> there was ice there previously. These and other indicators show that

> there is no doubt about recent warming; see also chapter 4 of IPCC.

>

>>

>>

>> In my letter to "Klass V" I included diagram showing the mean annual
> temperature of the Nordic countries (1890-ca 2001) presented on the net
> by

>> the database NORDKLIM, a joint project between the meteorological
> institutes

>> in the Nordic countries. Except for Denmark, the data sets show an
>> increase

>> after the 1970s to the same level as in the late 1930s or lower. None
> demonstrates the distinct increase IPCC indicates. The trends of these 6
> areas are very similar except for a few interesting details.

>>

>>

> Results will also depend on the exact region.

>

>>

>> I have in my studies of temperatures also checked a number of areas
> using

>> data from NASA. One, in my mind interesting study, includes all the 13
> stations with long and decent continuously records north of 65 deg N.

> The
>> pattern is the same as for the Nordic countries. This diagram only shows
> 11-yr means of individual stations. A few stations such as Verhojansk
> and

>> Svalbard indicate a recent mean 11-year temperature increase up to 0.5
> deg

>> C
>> above the late 1930s. Verhojansk, shows this increase but the
> temperature

>> has after the peak temperature decreased with about 0.3 deg C during the
> last few years. The majority of the stations show that the recent
> temperatures are similar to the one in the late 1930s.

>>

>>

>> In preparation of some talks I have been invited to give, I have
> expanded

>> the Nordic area both west and east. The area of similar change in
> climate

>> is
>> vast. Only a few stations near Bering Strait deviates (e.g. St Paul,

> Kodiak,
>> Nome, located south of 65 deg. N).
>>
>>
>>
>> My studies include Africa, a study which took me most of a summer
> because
>> there are a large number of stations in the NASA records. I found 11
> stations including data from 1898-1975 and 16 stations including
> 1950-2003.
>> The data sets could in a convincing way be spliced. However, I noticed
>> that
>> some persons were not familiar with “splicing” technique so I have
>> accepted
>> to reduce the study to the 7 stations including data from the whole
> period
>> between 1898-2003. The results are similar as to the spiced data set and
> also, surprisingly similar to the variability of the Nordic data.
> Regression
>> indicates a minor (if any) decrease in temperature (I have used all
> stations
>> independent of location, city location or not).
>>
> Africa is notorious for missing and inaccurate data and needs careful
> assessment.
>>
>>
>> Another example is Australia. NASA only presents 3 stations covering the
> period 1897-1992. What kind of data is the IPCC Australia diagram based
> on?
>> If any trend it is a slight cooling. However, if a shorter period
> (1949-2005) is used, the temperature has increased substantially.
>>
>>
>
> The Australians have many stations and have published more detailed maps
> of changes and trends.
>
>
>>
>> There are more examples, but I think this is much enough for my present
> point:
>>

>>
>>
>> How has the laboratories feeding IPCC with temperature records selected
> stations?
>>
> See our chapter and the appendices.
>>
>>
>> I have noticed that major cities often demonstrate a major urban effect
> (Buenos Aires, Osaka, New York Central Park, etc). Have data from major
> cities been used by the laboratories sending data to IPCC? Lennart
> Bengtsson and other claims that the urban effect is accounted for but
> from
>> what I read, it seems like the technique used has been a simplistic
>>
>
> Major inner cities are excluded: their climate change is real but very
> local.
>
>>
>>
>> Next step has been to compare my results with temperature records in the
> literature. One interesting figures is published by you in:
>>
>>
>>
>> Trenberth, K., 2005: Uncertainty in Hurricanes and Global Warming.
>> Science
>> 308: 1753-1754.
>>
>>
>>
>> As you obviously know, the recent increase in temperature above the
> 1940s
>> is
>> minor between 10 deg N and 20 deg N and only slightly larger above the
> temperature maximum in the early 1950s. Booth the increases in
> temperature
>> in the 1930s and in the 1980s to 1990s is of similar amplitude and
> similar
>> steepness, if any difference possibly slightly less steep in the
> northern
>> area than in the southern (the eddies slow down the warm water

>> transport?).

>> Your diagram describes a limited area of the North Atlantic because you

>> are

>> primarily interested in hurricanes. The complexity of sea surface

> temperature increases and decreases is seen in e.g. Cabanes, C, et al.

> 2001

>> (Science 294: 840-842).

>>

>

> As we discuss, there is a lot of natural variability in the North Atlantic

> but there is also a common component that relates to global changes. See

> my GRL article with Shea for more details.

> Trenberth, K. E., and D. J. Shea, 2006: Atlantic hurricanes and natural

> variability in 2005. Geophys. Res. Lett., 33, L12704,

> doi:10.1029/2006GL026894.

>

>>

>>

>> One example of sea surface temperature is published by:

>>

>>

>>

>> Goldenberg, S.B., Landsea, C.W., Mestas-Nuñez, A.M. and Gray, W.M.,

> 2001:

>> The recent increases in Atlantic hurricane activity: causes and

>> implications. Science 293: 474-479.

>>

>>

>>

>> Again, there is a marked increase in temperature in the 1930s and 1950s

> (about 1 deg C), a decrease to approximately the level in the 1910s and

> thereafter a new increase to a temperature slightly below the level in

> the 1940s.

>>

>>

>> One example of published data not supporting a major temperature

> increase

>> during recent time is:

>>

>>

>> Polyakov, I.V., Bekryaev, R.V., Alekseev, G.H., Bhatt, U.S., Colony,

> R.L.,

>> Johnson, M.A., Maskhtas, A.P. and Walsh, D., 2003: Variability and

> Trends

>> of Air Temperature and Pressure in the Maritime Arctic, 1875–2000.

> Journal

>> of Climate: Vol. 16 (12): 2067–2077.

>>

>>

>>

>>

>> He included many more stations than I did in my calculation of
>> temperatures

>> N 65 N, but the result is similar. It is hard to find evidence of a
>> drastic

>> warming of the Arctic.

>>

>>

>>

>> It is also difficult to find evidence of a drastic warming outside urban
> areas in a large part of the world outside Europe. However the increase
> in

>> temperature in Central Europe may be because the whole area is urbanised
> (see

>> e.g. Bidwell, T., 2004: Scotobiology – the biology of darkness. Global
> change News Letter No. 58 June, 2004).

>>

>>

>>

>>

>>

>> So, I find it necessary to object to the talk about a scaring
> temperature

>> increase because of increased human release of CO₂. In fact, the warming
> seems to be limited to densely populated areas. The often mentioned
> correlation between temperature and CO₂ is not convincing. If there is a
> factor explaining a major part of changes in the temperature, it is
> solar

>> irradiation. There are numerous studies demonstrating this correlation
> but

>> papers are not accepted by IPCC. Most likely, any reduction of CO₂
> release

>> will have no effect whatsoever on the temperature (independent of how
> expensive).

>>

>

> You can object all you like but you are not looking at the evidence and
> you need to have a basis, which you have not established. You seem to
> doubt that CO2 has increased and that it is a greenhouse gas and you are
> very wrong. But of course there is a lot of variability and looking at
> one spot narrowly is not the way to see the big picture.

>

>

>>

>>

>> In my mind, we have to accept that it is great if we can reduce the
>> release

>> of CO2 because we are using up a resource the earth will be short of in
>> the

>> future, but we are in error if we claims a global warming caused by CO2.

>>

> I disagree.

>>

>>

>> I also think we had to protest when erroneous data like the claim that
> winter temperature in Abisko increased by 5.5 deg C during the last 100

> years. The real increase is 0.4 deg C. The 5.5 deg C figure has been

> repeated a number of times in TV-programs. This kind of exaggerations is
> not

>> supporting attempts to save fossil fuel.

>>

>>

>>

>> I have numerous diagrams illustrating the discussion above. I don't

>> include

>> these in an e-mail because my computer can only handle a few at a time.

> If

>> you would like to see some, I can send them by air mail.

>>

>>

>>

>> I am often asked about why I don't publish about my views. I have. Just
>> one

>> example of among 100 other I could select is: Karlén, W., 2001: Global
> temperature forces by solar irradiation and greenhouse gases? Ambio

> 30(6):

>> 349-350.

>>

>>

>>
>> Yours sincerely
>>
>>
>>
>> Wibjörn
>>
>>
>>
>> Geografiska Annaler
>>
>> Professor em Wibjörn Karlén
>>
>> Department of Social and Economic Geography
>>
>>
>> Geografiska Annaler Ser. A
>>
>> Box 513
>>
>> SE-751 20 Uppsala
>>
>> SWEDEN
>>
>>
>>
>> Wibjorn.Karlen@kultgeog.uu.se
>>
>
> I trust that Phil Jones may also respond
> Regards
> Kevin Trenberth
>
>
> _____
> Kevin Trenberth
> Climate Analysis Section, NCAR
> PO Box 3000
> Boulder CO 80307
> ph 303 497 1318
> <http://www.cgd.ucar.edu/cas/trenbert.html>
>
>

>
>

From: Clare Goodess <C.Goodess@uea.ac.uk>
To: R.L.Wilby@lboro.ac.uk,c.harpham@uea.ac.uk,M.agnew@uea.ac.uk, s.busby@uea.ac.uk
Subject: Fwd: RE: AXA Research Fund: launch of a new call for projects
Date: Thu, 18 Sep 2008 08:55:24 +0100
Cc: P.Jones@uea.ac.uk,k.briffa@uea.ac.uk

Dear all

Jacque had sounded very positive about this back in August, but it sounds like CSERGE are as stretched as much as people in CRU.

I'm afraid it's looking like we're not going to be able to get anything together on this unless Rob is able to take a lead. But I think that we would still be lacking the interdisciplinary research team that AXA are stressing.

Clare

PS Rob - sorry not to have been in touch with you sooner about this, but I didn't know until Tuesday that you were interested/had been approached.

Subject: RE: AXA Research Fund: launch of a new call for projects
Date: Thu, 18 Sep 2008 08:32:25 +0100
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: AXA Research Fund: launch of a new call for projects
Thread-Index: AckXVyDtvdpNCFYaR+WQsE/hzBjNYgCCW77g
From: "Burgess Jacquelin Prof \ (ENV)" <Jacquie.Burgess@uea.ac.uk>
To: "Goodess Clare Dr \ (ENV)" <C.Goodess@uea.ac.uk>
Hi Clare I dont think weve got the capacity to take this on at this stage. Never mind there will always be other opportunities.
Best wishes
Jacquie

From: Clare Goodess [[1] <mailto:C.Goodess@uea.ac.uk>]
Sent: 15 September 2008 18:19
To: Burgess Jacquelin Prof (ENV)
Cc: Alexander Jan Dr (ENV); Agnew Maureen Dr (ENV); Harpham Colin Dr (ENV); Busby Simon Mr (ENV)
Subject: RE: AXA Research Fund: launch of a new call for projects

Dear Jacquie

I'm afraid that I've not had time to do anything about this call since returning from holiday. The deadline is rapidly approaching - 3 October and after this week, I'm away at meetings until after the deadline. I also have two ARCC proposals and a DCMS tender to get sorted out this week.

So, I am not going to be able to take any kind of a lead on this even if we think its worth trying to get a last minute proposal together. No-one else from CRU has time to take a leading role, but Colin and Maureen are interested. Colin has been working on the CRU weather generator which will be an integral part of the UKCIP08 user interface and

Maureen has a broader impacts perspective and is lead author on the climate chapter in the forthcoming CII report. Simon Busby might also be interested - and has good experience of working with climate model outputs (although for a rather different purpose). One task for CRU would be to extend some of the validation work of the ENSEMBLES RCM runs. I should also be able to read and comment on material and provide some short draft sections of text (e.g., on ENSEMBLES, PRUDENCE, MICE and STARDEX) - I will have at least sporadic email access while away I hope.

But I think this is only going to be viable if somebody from CSERGE or the decision-making group is able to co-ordinate things. And we don't have the capacity for hydrological modelling in CRU - so again, this would need input from others. Though there is also the requirement in the call to assess the quality of flood modelling tools currently licensed by insurers - about which I know nothing. If it would be helpful to have a quick meeting this week, let me know.

Best wishes, Clare

At 16:30 12/08/2008, you wrote:

Dear Clare,

Many thanks for this I think it would be an excellent opportunity for a CRU + other parts of the School response. I know Jan Alexander has already got a European bid through to second stage on floods. We could certainly put something together with the environmental decision-making components too. Lets discuss when you get back from holiday.

Best wishes

Jacquie

From: Clare Goodess [[2]mailto:C.Goodess@uea.ac.uk]

Sent: 12 August 2008 14:58

To: Burgess Jacquelin Prof (ENV)

Cc: Jones Philip Prof (ENV); Osborn Timothy Dr (ENV); Agnew Maureen Dr (ENV); Harpham Colin Dr (ENV)

Subject: Fwd: AXA Research Fund: launch of a new call for projects

Dear Jacquie

CRU is interested in putting in a proposal under this call. As you can see, as well as the climate science aspects, there is also a need to work on economic issues - so this could be a good opportunity for putting in a joint proposal with people in CSERGE or other parts of ENV. There are also additional collaborators on the climate and flooding aspects that we could involve both in the UK and Germany.

I'm away from tomorrow for a couple of weeks, but the CRU people copied in on this email are also all interested in a potential proposal. Though currently we're not sure which if any of us has time to lead on this at least immediately.

Best wishes, Clare

Subject: AXA Research Fund: launch of a new call for projects

Date: Tue, 22 Jul 2008 19:18:02 +0200

X-MS-Has-Attach: yes

X-MS-TNEF-Correlator:

Thread-Topic: AXA Research Fund: launch of a new call for projects

Thread-Index: AcjsHuVgY1R8ndbHSHiv/kWz02+NeQ==

From: "CHOUX Mathieu" <mathieu.choux@axa.com>

To: <C.Goodess@uea.ac.uk>

Cc: "appelprojets" <appelprojets@axa.com>

X-Canit-CHI2: 0.00

X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f034)

X-Spam-Score: 4.10 (****) [Tag at 5.00] DEAR_SOMETHING,HTML_MESSAGE,MIME_QP_LONG_LINE

X-CanItPRO-Stream: UEA:f034 (inherits from UEA:10_Tag_Only,UEA:default,base:default)

X-Canit-Stats-ID: 6808857 - c6a2c2ad9106

X-Antispam-Training-Forget:

[3]<https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=f>

X-Antispam-Training-Nospam:

[4]<https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=n>

X-Antispam-Training-Spam: [5]<https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=s>

X-Scanned-By: CanIt (www . roaringpenguin . com) on 139.222.131.185

Hello Clare,

AXA recently launched a call for projects to academic institutions focused on the flooding risk and the impacts of climate change. The Climatic Research Unit may have been approached with the email reproduced below, and I just wanted to make sure you received the information.

Sincerely Yours,

Mathieu Choux

Dear Madam/Sir,

The AXA Research Fund has been created in order to encourage research in a number of disciplines that touch on the risks, challenges and major transformations that affect our rapidly changing world. The Fund will award 100 million Euros over five years to finance innovative research.

The AXA Research Fund team is delighted to announce the launch of a new call for projects on climate change impacts on the risk of flooding in <?xml:namespace prefix = st1 ns = "urn:schemas-microsoft-com:office:smarts" />Europe (see attached document) .

All the information needed to apply can be found on our internet site:

[6]<http://researchfund.axa.com/en/research-funding/calls-projects/>

Please make sure this information is communicated within your institution. The results of the selection process will be communicated to them as of January 15, 2009 .

Sincerely,

The AXA Research Fund Team

[7]appelprojets@axa.com

Mathieu CHOUX
Risk Analyst - Catastrophe Modeling Department
AXA Group
GIE AXA - 9 av. de Messine - Paris, France
[8]mathieu.choux@axa.com
Tel. : +33 1 40 75 55 68 - Fax : +33 1 40 75 58 27
AXA redefining / standards
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not

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sender

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diately.

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[14]<http://www.cru.uea.ac.uk/~clareg/clare.htm>

References

1. <mailto:C.Goodess@uea.ac.uk>
2. <mailto:C.Goodess@uea.ac.uk>
3. <https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=f>
4. <https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=n>
5. <https://canit.uea.ac.uk/b.php?i=6808857&m=c6a2c2ad9106&c=s>
6. [blocked:http://researchfund.axa.com/en/research-funding/calls-projects/](http://researchfund.axa.com/en/research-funding/calls-projects/)
7. <mailto:appelaprojets@axa.com>
8. <mailto:mathieu.choux@axa.com>
9. <http://www.cru.uea.ac.uk/>
10. <http://www.cru.uea.ac.uk/~clareg/clare.htm>
11. <http://www.cru.uea.ac.uk/>
12. <http://www.cru.uea.ac.uk/~clareg/clare.htm>
13. <http://www.cru.uea.ac.uk/>
14. <http://www.cru.uea.ac.uk/~clareg/clare.htm>

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: Status of IJoC manuscript
Date: Fri Sep 19 15:11:41 2008

Ben,

Good news. Endnote types is a much better option than in the text - not as good as footnotes.

Yes the paper you attached does look crap. I will read it though even if the journal is even worse.

This paper has come out. The plot of London and Vienna temps, although an aside, is something I need to follow up more. London has a UHI, but it doesn't mean any more warming in the 20th century!

Hope all is well with you.

Cheers

Phil

PS Attached another paper - has some nice photos!

At 17:12 18/09/2008, you wrote:

Dear folks,

I just wanted to give you a brief update on the status of our IJoC manuscript.

I received the page proofs about three weeks ago. Unfortunately, IJoC did not allow us to employ footnotes. You may recall that we made liberal use of footnotes in order to present technical information that would have interfered with the "flow" of the main text. The IJoC copy editors simply folded all footnotes into the main text. This was done without any regard for context. It made the main text very difficult to read. After lengthy negotiations with IJoC editors, we decided on a compromise solution. While IJoC was unwilling to accept footnotes (for reasons that are still unclear to me), they did agree to accept endnotes. The footnotes have now been transferred to an Appendix 2 entitled "Technical Notes". While this is not an optimal solution, it's a heck of a lot better than IJoC's original "assimilate in main text" solution.

Now that the footnote issue has been resolved, I'm hoping that online publication of our paper will happen within the next several weeks. I'll let you know as soon as I receive a publication date from IJoC. LLNL (and probably NOAA, too) will be working on press releases for the paper. I'll also be drafting a one-page, plain English "fact sheet", which will address why we initiated this study, what we learned, why I'll never do this again, etc. I'll circulate this fact sheet for your comments early next week.

With best regards,

Ben

(P.S.: David Douglass and John Christy continue to publish crappy papers. For their

latest science fiction, please see:

[1] <http://arxiv.org/ftp/arxiv/papers/0809/0809.0581.pdf>)

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Norwich Email p.jones@uea.ac.uk
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References

1. <http://arxiv.org/ftp/arxiv/papers/0809/0809.0581.pdf>

From: "Jenkins, Geoff" <geoff.jenkins@metoffice.gov.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: London UHI
Date: Wed, 24 Sep 2008 15:37:34 +0100
Cc: "Wilby, Robert" <r.wilby@lancaster.ac.uk>

Hi Phil

Thanks for the comments on the Briefing report. You say "There is no evidence with London of any change in the amount of the UHI over the last 40 years. The UHI is clear, but it's not getting any worse" and sent a paper to show this. By coincidence I also got recently a paper from Rob which says "London's UHI has indeed become more intense since the 1960s esp during spring and summer". Its not something I need to sort out for UKCIP08, but I thought you both might like to be aware of each others findings. I didn't keep a copy of Rob's PDF after I printed it off but I am sure you can swap papers. I don't need to be copied in to any discussion.

Cheers
Geoff

From: Ben Santer <santer1@llnl.gov>
To: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Peter.Thorne@noaa.gov, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan.Solomon@noaa.gov, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>
Subject: Next version of press release
Date: Wed, 01 Oct 2008 18:43:45 -0700
Reply-to: santer1@llnl.gov
Cc: Anne Stark <stark8@llnl.gov>, "Parker, David (Met Office)" <david.parker@metoffice.gov.uk>, "David C. Bader" <bader2@llnl.gov>, "Bamzai, Anjuli" <Anjuli.Bamzai@science.doe.gov>

<x-flowed>

Dear folks,

Here is the next version of the press release for our IJoC paper. I received a number of comments from you (many thanks!), and have tried hard to incorporate them without increasing the length of the release.

Peter Thorne suggested that it might be useful to delete the explicit reference to the UR/UAH group, and instead refer to the Douglass et al. IJoC paper in a footnote. After some internal debate, I have not done that. Anne Stark advised me that footnotes are not often used in press releases (they tend to get ignored by reporters). Furthermore, I couldn't see an easy way of getting rid of the "UR/UAH" acronym, yet still making a clear distinction between their results and our results, their test and our test, etc., etc.

I've tried to capture the spirit if not the letter of your suggested edits. Unfortunately, I don't think we have the time to iterate for days on the press release - we really need to finalize this tomorrow. We will have a little more time to finalize the "fact sheet".

So please let me know as soon as possible if there's anything you can't live with in the press release.

One final point. Peter also asked whether it might be useful to include the telephone numbers of co-authors in the final paragraph of the press release. Anne and I would prefer not to do that. If you are agreeable to fielding press inquiries about the paper, please let me know, and send me a telephone number under which you can be reached in the next few days. We'll then compile a list (with contact information) of co-authors willing to discuss the paper with interested reporters.

I hope to send you a revised version of the fact sheet later tomorrow.

With best regards,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
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P.O. Box 808, Mail Stop L-103
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FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

Attachment Converted: "c:\eudora\attach\Santer_IJC_Sept_2008_v7.doc"

From: Keith Briffa <k.briffa@uea.ac.uk>

To: Tim Osborn <t.osborn@uea.ac.uk>, Clare Goodess <C.Goodess@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "Douglas Maraun" <d.maraun@uea.ac.uk>, "Janice Darch" <J.Darch@uea.ac.uk>

Subject: Re: potential DfID funding for climate centre

Date: Mon, 13 Oct 2008 12:33:01 +0100

<x-flowed>

have not been approached - but I think it really
does sound like the sort of initiative CRU/ENV are looking for.

I get the feeling this is the sort of potential
contact ENV would wish to take over.

Keith

At 11:31 13/10/2008, Tim Osborn wrote:

>Hi CRU Board,

>

>I just had an interesting chat with Jack Newnham
>from the International Development Team at Price
>Waterhouse Cooper. They get lots of DfID
>(Douglas: DfID is the UK Government Department
>for International Development) funding.

>

>They've heard that DfID are likely to call for
>expressions of interest for a new centre
>focussing on international climate
>change. Their idea is to fund a centre that
>would be the first point of call for advice and
>for commissioning research related to climate
>change and development or to climate change in countries where DfID operate.

>

>He was talking about £15 million per year for 5
>years! Not sure how much would be from DfID and
>how much raised from other donors (and hence
>uncertain), nor how much would be given up-front
>versus how much spent later on specific research
>projects organised via this centre.

>

>Nevertheless, sounds big enough to be worth getting involved in.

>

>He was clearly just testing the water with us,
>so not sure that they definitely wish to involve
>us. He may want to meet to talk through things,
>if they decide to ask us to join their
>proposal. He said he'd email me later -- I'll
>forward this when it arrives. They're also
>contacting the Tyndall Centre, and no doubt a number of other institutes.

>

>Has anyone else in CRU been approached?

>

>Presumably, if this call for tenders is actually

>issued, this is likely to interest Tyndall

>greatly. But CRU can offer a significant

>contribution -- especially data and scenarios

>developed for specific (developing) countries --

>and this should be seen as independent from

>Tyndall rather than part of Tyndall

>contribution. There's also Declan/DEV, so UEA as a whole has much to offer.

>

>Any thoughts on this?

>

>Tim

>

>

>

>

>Dr Timothy J Osborn, Academic Fellow

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>

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>

--

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From: Ben Santer <santer1@llnl.gov>
To: David Douglass <douglass@pas.rochester.edu>
Subject: Response
Date: Tue, 14 Oct 2008 13:30:21 -0700
Reply-to: santer1@llnl.gov
Cc: "Peter W. Thorne" <peter.thorne@metoffice.gov.uk>, Peter.Thorne@noaa.gov, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, ssolomon@frii.com, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "Philip D. Jones" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>, "David C. Bader" <bader2@llnl.gov>

<x-flowed>
Prof. Douglass,

You have access to EXACTLY THE SAME radiosonde data that we used in our recently-published paper in the International Journal of Climatology (IJoC). You are perfectly within your rights to verify the calculations we performed with those radiosonde data. You are welcome to do so.

We used the IUK radiosonde data (the data mentioned in your email) to calculate zonal-mean temperature changes at different atmospheric levels. You should have no problem in replicating our calculation of zonal means. You can compare your results directly with those displayed in Figure 6 of our paper. You do not need our "numerical quantities" in order to determine whether we have correctly calculated zonal-mean trends, and whether the IUK data show tropospheric amplification of surface temperature changes.

Similarly, you should have no problem in replicating our calculation of "synthetic" MSU temperatures from radiosonde data. Algorithms for calculating synthetic MSU temperatures have been published by ourselves and others in the peer-reviewed literature. You have already demonstrated (in your own IJoC paper of 2007) that you are capable of computing synthetic MSU temperatures from climate model output. Furthermore, I note that in your 2007 IJoC paper, you have already successfully replicated our "model average" synthetic MSU temperature trends (which were published in the Karl et al., 2006 CCSP Report).

In summary, you have access to the same model and observational data that we used in our 2008 IJoC paper. You have all the information that you require in order to determine whether the conclusions reached in our IJoC paper are sound or unsound.

You are quick to threaten your intent to file formal complaints against me "with the journal and other scientific bodies". If I were you, Dr. Douglass, I would instead focus my energies on rectifying the serious error in the "robust statistical test" that you applied to compare modeled and observed temperature trends.

I am copying this email to all co-authors of the 2008 Santer et al. IJoC paper, as well as to Professor Glenn McGregor at IJoC. They deserve to be fully apprised of your threat to file formal complaints.

Please do not communicate with me in the future.

Ben Santer

David Douglass wrote:

> My request is not unreasonable. It is normal scientific discourse and
> should not be a personal matter.
> This is a scientific issue. You have published a paper with conclusions
> based upon certain specific numerical quantities. As another scientist,
> I challenge the value of those quantities. These values can not be
> authenticated by my calculating them because I have nothing to compare
> them to.
>
> If you will not give me the values of the IUK data in figure 6 then I
> will consider filing a formal complaint with the journal and other
> scientific bodies.
>
> David Douglass

Benjamin D. Santer
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From: Gabi Hegerl <Gabi.Hegerl@ed.ac.uk>
To: "Bamzai, Anjuli" <Anjuli.Bamzai@science.doe.gov>
Subject: RE: Meeting Jan 21-23
Date: Tue, 14 Oct 2008 21:51:24 +0100
Cc: Myles Allen <allen@atm.ox.ac.uk>, claudia tebaldi <claudia.tebaldi@gmail.com>, Knutti Reto <reto.knutti@env.ethz.ch>, "Stott, Peter" <peter.stott@metoffice.gov.uk>, "Zwiers, Francis [Ontario]" <francis.zwiers@ec.gc.ca>, Tim Barnett <tbarnett-ul@ucsd.edu>, Hans von Storch <hvonstorch@web.de>, Claudia Tebaldi <tebaldi@ucar.edu>, Phil Jones <p.jones@uea.ac.uk>, David Karoly <dkaroly@unimelb.edu.au>, Toru Nozawa <nozawa@nies.go.jp>, Ben Santer <santer1@llnl.gov>, Daithi Stone <stoned@atm.ox.ac.uk>, Richard Smith <rls@email.unc.edu>, Nathan Gillett <n.gillett@uea.ac.uk>, Michael Wehner <MFWehner@lbl.gov>, Doug Nychka <nychka@ucar.edu>, Xuebin Zhang <Xuebin.Zhang@ec.gc.ca>, Chris Miller <christopher.d.miller@noaa.gov>, Tom Knutson <Tom.Knutson@noaa.gov>, Tim Delsole <delsole@cola.iges.org>, Susan Solomon <Susan.Solomon@noaa.gov>, "Jones, Gareth S" <gareth.s.jones@metoffice.gov.uk>, Tara Torres <tara@ucar.edu>

<x-flowed>

Hi all, I assume this is general interest, not IDAG meeting - I think the meeting would be a bit too big and complicated if we would try to resolve IPCC type issues - on the other hand, involving Chris Field and maybe Tom Stocker may be an interesting way to vent the scientific issues in a relaxed setting. But I would suggest to avoid agency type things - can be convinced otherwise if you feel strongly. we do have a limited budget, too!

Gabi

Quoting "Bamzai, Anjuli" <Anjuli.Bamzai@science.doe.gov>:

> Myles,
>
> The Dept of State is the U.S. lead on IPCC, Conference of Party
> discussions, etc. USAID does the bulk of adaptation assistance at the
> international level. At the national level, there are various CCSP
> agencies, e.g. Dept of Agriculture, Dept of Interior, EPA, who are more
> on the 'application' side of the CCSP.
>
> I'd need to ask someone in those agencies on how they are approaching
> the issues you raise. Perhaps Chris Miller knows someone there...?
>
> Programs such as NOAA Climate Change Data Detection (CCDD), and DOE
> Climate Change Prediction Program(CCPP) focus almost exclusively on
> IPCC WG I type of questions.
>
> Anjuli
>
>
> -----Original Message-----
> From: Myles Allen [mailto:allen@atm.ox.ac.uk]
> Sent: Tuesday, October 14, 2008 5:00 AM
> To: claudia tebaldi; Gabi Hegerl

> Cc: Knutti Reto; Stott, Peter; Zwiers,Francis [Ontario]; Tim Barnett;
> Hans von Storch; Claudia Tebaldi; Phil Jones; David Karoly; Toru
Nozawa;
> Ben Santer; Daithi Stone; Richard Smith; Nathan Gillett; Michael
Wehner;
> Doug Nychka; Xuebin Zhang; Bamzai, Anjuli; Chris Miller; Tom Knutson;
> Tim Delsole; Susan Solomon; Jones, Gareth S; Tara Torres
> Subject: RE: Meeting Jan 21-23
>
> Hi All,
>
> That is a very good idea indeed. I was talking to Tom Stocker last
week,
> arguing that resolving the differences in the definition of attribution
> between WG1 and WG2 was going to be one of the key challenges for AR5,
> particularly as attribution of impacts becomes a live topic as
countries
> start to make the case for adaptation assistance. How about we invite
> the co-Chair of WG1 along as well?
>
> If we are going to invite Chris Field, we should definitely also invite
> someone from the "double attribution" community, or it will seem a bit
> like WG1 lecturing to the co-Chair of WG2. Any suggestions, David?
>
> Anjuli, has anyone in the US State Department (or whichever department
> will handle this) started addressing the question of how the US
> government will distinguish "impacts of climate change" from
> "vulnerability to natural climate variability" in allocating resources
> for adaptation assistance? If anyone has even started thinking about
> this problem, it would be very interesting to hear from them to know
> what questions they are likely to need answering. We could also try and
> find out if anyone in the European Commission is worrying about this.
>
> Regards,
>
> Myles
>
> -----Original Message-----
> From: claudia tebaldi [mailto:claudia.tebaldi@gmail.com]
> Sent: 13 October 2008 20:46
> To: Gabi Hegerl
> Cc: Myles Allen; Knutti Reto; Stott, Peter; Zwiers,Francis [Ontario];
> Tim Barnett; Hans von Storch; Claudia Tebaldi; Phil Jones; David
Karoly;
> Toru Nozawa; Ben Santer; stoned@csag.uct.ac.za; Richard Smith; Nathan
> Gillett; Michael Wehner; Doug Nychka; Xuebin Zhang; Bamzai, Anjuli;
> Chris Miller; Tom Knutson; Tim Delsole; Susan Solomon; Jones, Gareth S;
> Tara Torres
> Subject: Re: Meeting Jan 21-23
>
> Hi Gabi et al.
>
> I wonder if we could try to get Chris Field, who is going to be the
> chair of working group 2 for AR5...I don't know how likely it is to get

> him but it may be interesting to get his perspective on what was done
in
> AR4 WG2 and what he would like to see in AR5 WG2.
>
> c
>
> On Mon, Oct 13, 2008 at 10:51 AM, Gabi Hegerl <gabi.hegerl@ed.ac.uk>
> wrote:
>> Hi IDAG people,
>>
>> Its time to start planning our next IDAG meeting in detail. A
> provisional
>> coarse agenda is attached. Please feel free to email me suggestions
>> to improve/update this, and if there is a topic you would
> love
>> to see covered but that isn;t please get in touch as well.
>> Also, we should have one topic related to the impacts review paper
> that is
>> to be written in year 2 of the grant. Therefore, if you have a
>> suggestion of a guest that would help us elucidate the
> challenges in
>> impact attribution but also to move forward on this, please let me
>> know!
>> Tara Torres from UCAR (tara@ucar.edu) will help us to plan the
> meeting.
>> Also, I hope to hire a student helper at Duke to get our meeting
> webpage
>> going, keep track of agenda items etc, but please bear with me and
>> tolerate a bit of chaos before we have succeeded with this!
>>
>> What I need from you is to please
>> - let me know if you can make it, and what you would vaguely like to
> speak
>> about (you can do the first now and postpone the second)
>> - get in touch with Tara to book your travel - ideally, towards the
> end of
>> October / or in early November (she is a bit buried right now)
>> - get in touch with me when you have suggestions, or want to bring
> somebody
>>
>> Gabi
>>
>> --
>> Dr Gabriele Hegerl School of GeoSciences The University of Edinburgh
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>>
>> The University of Edinburgh is a charitable body, registered in
>> Scotland, with registration number SC005336.
>>
>>
>

>
>
> --
> Claudia Tebaldi
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>
>
>
>

--
Gabriele Hegerl
School of GeoSciences
University of Edinburgh
<http://www.geos.ed.ac.uk/people/person.html?indv=1613>

--
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Scotland, with registration number SC005336.

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From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Why are the temperature data from Hadley different from NASA?
Date: Thu, 16 Oct 2008 13:00:59 -0400
Cc: Judith Lean <jlean@ssd5.nrl.navy.mil>, Yousif K Kharaka <ykharaka@usgs.gov>

thanks Phil--this all makes sense. I'll be intrigued to hear more about how the melting sea ice issue is going to be dealt with. no question there is a lot of warming going on up there.

hope to see you one of these days,

mike

On Oct 16, 2008, at 6:52 AM, Phil Jones wrote:

Hi Mike, Judith and Yousif,

Mike has basically answered the question. The GISS group average surface T data into 80 equal area boxes across the world. The UK group (CRU/MOHC) grid the data into 5 by 5 degree lat/long boxes, as does NCDC. These griddings don't allow so much extrapolation of data - no extrapolation beyond the small grid box. The US groups also calculate the globe as one domain, whereas we in the UK use (NH+SH)/2. This also makes some difference as most of the missing areas are in the SH, and currently the NH is warmer than the SH with respect to 1961-90. Our rationale for doing what we do is that it is better to estimate the missing areas of the SH (which we do by tacitly assuming they are the average of the rest of the SH) from the rest of the SH as opposed to the rest of the world.

The Arctic is a problem now. With less sea ice, we are getting SST data in for regions for which we have no 1961-90 averages - because it used to sea ice (so had no measurements).

We are not using any of the SST from the central Arctic in summer.

So we are probably underestimating temperatures in the recent few years. We're working on what we can do about this. There are also more general SST issues in recent years. In 1990, for example, almost all SST values came from ships. By 2000 there were about 20% from Buoys and Drifters, but by 2008 this percentage is about 85%. We're also doing comparisons of the drifters with the ships where both are plentiful, as it is likely that drifters measure a tenth of one degree C cooler than ships, and the 1961-90 period is ship-based average.

New version of the dataset coming in summer 2009.

All the skeptics look at the land data to explain differences between datasets and say urbanization is responsible for some or all of the warming. The real problem is the marine data at the moment.

Attaching a recent paper on urbanization and effects in China.

Cheers

Phil

At 22:08 15/10/2008, Michael Mann wrote:

Hi Judith,

Its nice to hear from you, been too long (several years??). My understanding is that

the differences arise largely from how missing data are dealt with. For example, in Jim et al's record the sparse available arctic data are interpolated over large regions, whereas Phil and co. either use the available samples or in other versions (e.g. Brohan et al) use optimal interpolation techniques. The bottom line is that Hansen et al '05 I believe weights the high-latitude warming quite a bit more, which is why he gets a warmer '05, while Phil and co find '98 to be warmer.

But Phil can certainly provide a more informed and complete answer!

mike

p.s. see you at AGU this year??

On Oct 15, 2008, at 5:03 PM, Judith Lean wrote:

Hi Yousif,

Many apologies for not replying sooner to your email - but I've only just returned from travel and am still catching up with email.

Unfortunately, I am simply a "user" of the surface temperature data record and not an expert at all, so cannot help you understand the specific issues of the analysis of the various stations that produce the differences that you identify. I too would like to know the reason for the differences.

Fortunately, there are experts who can tell us, and I am copying this email to Mike Mann and Phil Jones who are such experts.

Mike and Phil (hi! hope you are both well!), can you please, please help us to understand these differences that Yousif points out in the GISS and Hadley Center surface temperature records (see two attached articles).

Many thanks, for even a brief answer, or some reference.

Judith

On Oct 8, 2008, at 1:50 PM, Yousif K Kharaka wrote:

Judith:

I hope you are doing well (these days OK would be good!) at work and personally.

Can you help me to understand the huge discrepancy (see below) between the temperature data from the Hadley Center and GISS? Any simple explanations, or references that I can read on this topic? I certainly would appreciate your help on this.

Best regards. Yousif Kharaka

Yousif Kharaka, Research Geochemist Phone: (650) 329-4535

U. S. Geological Survey, MS 427 Fax: (650) 329-4538

345, Middlefield Road Mail: [1]ykharaka@usgs.gov

Menlo Park, California 94025, USA

----- Forwarded by Yousif K Kharaka/WRD/USGS/DOI on 10/08/2008 10:42 AM -----

Yousif K Kharaka/WRD/USGS/DOI

10/06/2008 02:07 PM

To

"Dr David Jenkins" <[2]jenkins@chartwood.com >

cc

[3]allyson_anderson@energy.senate.gov, [4]drahovzal@uky.edu, [5]dvance@arcadis-us.com,

[6]ebarron@jsg.utexas.edu, "Gene Shinn" <[7]eshinn@marine.usf.edu>,

[8]jarmenrock@gmail.com, [9]jblank@aapg.org, [10]Jeffrey@LevineOnline.com,

[11]jjones@vanoperating.com, [12]julie.kupecz@shell.com, [13]pgrew@unlnotes.unl.edu,
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Subject

Why are the temperature data from Hadley different from NASA? [18]Link

David and all:

One advantage (or great disadvantage if you are very busy!) of membership in GCCC is that you are forced to investigate topics outside your areas of expertise. For some time now, I have been puzzled as to why global temperature data from the British Hadley Centre are different from those reported by NASA GISS, especially in the last 10 years. GISS reports that 2005 was the warmest year (see first attachment) on record, and that 2007 tied 1998 for the second place. The Hadley group continues reporting 1998 (a strong El Nino year) as having the highest global temperature, and then showing temperature decreases thereafter. The two groups report their temperatures relative to different time intervals (1951-1980 for GISS; 1961-1990 for Hadley), but much more important is the fact that GISS data include temperatures from the heating Arctic that are excluded by others (see second attachment). If you are interested in the topic of sun spots, the 11-year irradiance cycle, and solar forcing versus AGHGs, see the first attachment for what NASA has to say.

We may need help on this complex topic from a "true climate scientists", such as Judith Lean!

Cheers. Yousif Kharaka

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345, Middlefield Road Mail: [19]ykharaka@usgs.gov
Menlo Park, California 94025, USA

<GCC-Data @ NASA GISS_ GISS Surface Temperature Analysis_ 2007.pdf>
<GCC-2005 Warmest Year In A Century.pdf>

<GCC-Data @ NASA GISS_ GISS Surface Temperature Analysis_ 2007.pdf><GCC-2005 Warmest Year In A Century.pdf>

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"Dire Predictions" book site:
[22]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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"Dire Predictions" book site:
[26]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. <mailto:ykharaka@usgs.gov>
2. <mailto:jenkins@chartwood.com>
3. mailto:allyson_anderson@energy.senate.gov
4. <mailto:drahovzal@uky.edu>
5. <mailto:dvance@arcadis-us.com>
6. <mailto:ebarron@jsg.utexas.edu>
7. <mailto:eshinn@marine.usf.edu>
8. <mailto:jarmenrock@gmail.com>
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10. <mailto:Jeffrey@LevineOnLine.com>
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16. <mailto:tpaexpl@aol.com>
17. <mailto:w.a.morgan@conocophillips.com>
18. Notes:///8825668F00670ABE/DABA975B9FB113EB852564B5001283EA/A93F684FF508B452872574D90044850F
19. <mailto:ykharaka@usgs.gov>
20. <mailto:mann@psu.edu>
21. <http://www.meteo.psu.edu/~mann/Mann/index.html>
22. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
23. <mailto:p.jones@uea.ac.uk>
24. <mailto:mann@psu.edu>
25. <http://www.meteo.psu.edu/~mann/Mann/index.html>
26. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

27. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Mick Kelly <mick.tiempo@googlemail.com>
To: <P.Jones@uea.ac.uk>
Subject: RE: Global temperature
Date: Sun, 26 Oct 2008 09:02:00 +1300

Yeah, it wasn't so much 1998 and all that that I was concerned about, used to dealing with that, but the possibility that we might be going through a longer - 10 year - period of relatively stable temperatures beyond what you might expect from La Nina etc.

Speculation, but if I see this as a possibility then others might also. Anyway, I'll maybe cut the last few points off the filtered curve before I give the talk again as that's trending down as a result of the end effects and the recent cold-ish years.

Enjoy Iceland and pass on my best wishes to Astrid.

Mick

> -----Original Message-----
> From: P.Jones@uea.ac.uk [mailto:P.Jones@uea.ac.uk]
> Sent: 24 October 2008 20:39
> To: Mick Kelly
> Subject: Re: Global temperature
>
>
> Mick,
> They have noticed for years - mostly wrt
> the warm year of 1998. The recent coolish years
> down to La Nina. When I get this question I
> have 1991-2000 and 2001-2007/8 averages to hand.
> Last time I did this they were about 0.2 different,
> which is what you'd expect.
> In Iceland at a meeting that Astrid invited me to.
> Cold with snow on the ground, but things cheap as the
> currency has gone down 30-40% wrt even the pound.
>
>
> Cheers
> Phil
>
>> Hi Phil

>>
>> Just updated my global temperature trend graphic for a
> public talk and
>> noted
>> that the level has really been quite stable since 2000 or
> so and 2008
>> doesn't look too hot.
>>
>> Anticipating the sceptics latching on to this soon, if they
> haven't done
>> already, has anyone had a good look at the large-scale circulation
>> anomalies
>> over this period? I haven't noticed anything consistent
> coming up in the
>> annual climate reviews but then I wasn't really looking.
>>
>> Be awkward if we went through a early 1940s type swing!
>>
>> Hope all's well with you
>>
>> Mick
>>
>> _____
>>
>> Mick Kelly
>> PO Box 4260 Kamo
>> Whangarei 0141 New Zealand
>> email: mick.tiempo@gmail.com
>> web: www.tiempocyberclimate.org
>> _____
>>
>>
>
>

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: End of the road...
Date: Mon Oct 27 16:42:01 2008

Ben,

It seems that Climate Audit has been discussing the paper. I had a look whilst I was in Iceland as I had nothing better to do a few times. It was cold and snowy outside, there was internet.....

Seems as though they are making some poor assumptions; someone is trying to defend us, but gets rounded upon and one of the co-authors on the paper is in touch with McIntyre.

As it isn't me, and I can rule out a number of the others, my list of who it might be isn't that long....

Looking forward to next week !!

Cheers

Phil

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: Ben Santer <santer1@llnl.gov>
To: "'Philip D. Jones'" <p.jones@uea.ac.uk>
Subject: [Fwd: Re: [Fwd: Typo in equation 12 Santer.]]
Date: Thu, 30 Oct 2008 20:14:41 -0700
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

I thought you'd be interested in my reply to Gavin (see forwarded email).

Cheers,

Ben

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

X-Account-Key: account1

Return-Path: <santer1@llnl.gov>

Received: from mail-2.llnl.gov ([unix socket])
by mail-2.llnl.gov (Cyrus v2.2.12) with LMTPA;
Thu, 30 Oct 2008 20:10:53 -0700

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by mail-2.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.7 \$) with ESMTP id m9V3Arh7024023;
Thu, 30 Oct 2008 20:10:53 -0700

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by nspiron-1.llnl.gov with ESMTP; 30 Oct 2008 20:10:53 -0700

Message-ID: <490A773D.20807@llnl.gov>

Date: Thu, 30 Oct 2008 20:10:53 -0700
From: Ben Santer <santer1@llnl.gov>
Reply-To: santer1@llnl.gov
Organization: LLNL
User-Agent: Thunderbird 1.5.0.12 (X11/20070529)
MIME-Version: 1.0
To: Gavin Schmidt <gschmidt@giss.nasa.gov>
CC: Karl Taylor <taylor13@llnl.gov>
Subject: Re: [Fwd: Typo in equation 12 Santer.]
References: <1224543811.19301.2452.camel@isotope.giss.nasa.gov>
In-Reply-To: <1224543811.19301.2452.camel@isotope.giss.nasa.gov>
Content-Type: text/plain; charset=ISO-8859-1; format=flowed
Content-Transfer-Encoding: 7bit

<x-flowed>

Dear Gavin,

There is no typo in equation 12. The first term under the square root in equation 12 is a standard estimate of the variance of a sample mean (see, e.g., "Statistical Analysis in Climate Research", Zwiers and Storch, their equation 5.24, page 86). The second term under the square root sign is a very different beast - an estimate of the variance of the observed trend. As we point out, our $d1^*$ test is very similar to a standard Student's t-test of differences in means (which involves, in its denominator, the square root of two pooled sample variances).

In testing the statistical significance of differences between the model average trend and a single observed trend, Douglass et al. were wrong to use σ_{SE} as the sole measure of trend uncertainty in their statistical test. Their test assumes that the model trend is uncertain, but that the observed trend is perfectly-known. The observed trend is not a "mean" quantity; it is NOT perfectly-known. Douglass et al. made a demonstrably false assumption.

Bottom line: σ_{SE} is a standard estimate of the uncertainty in a sample mean - which is why we use it to characterize uncertainty in the estimate of the model average trend in equation 12. It is NOT appropriate to use σ_{SE} as the basis for a statistical test between two uncertain quantities (see our comments in our point #3, immediately before equation 12). The uncertainty in the estimates of both modeled AND observed trend needs to be explicitly incorporated in the design of any statistical test comparing modeled and observed trends. Douglass et al. incorrectly ignored uncertainties in observed trends.

Our Figure 6A is not a statistical test. It does not show the standard errors in the observed trends at discrete pressure levels (which would have made for a very messy Figure, given that we show results from 7 different observational datasets). Had we attempted to show the observed standard errors in Figure 6A, I suspect that standard errors from the RICH, IUK, RAOBCORE-v1.3, and RAOBCORE 1.4 datasets would have overlapped with the multi-model average trend at most pressure levels. I can easily produce such a Figure if necessary.

With best regards,

Ben

Gavin Schmidt wrote:

> Ben, Just thought I'd check with you first. I don't think there is a
> problem - but I think the question is really alluding to is our comment
> about Douglass et al 'being wrong' in using sigma_SE - since if we use
> it in the denominator in the $d1^*$ test, it can't be wrong, see?

>
> My response would be that we are testing a number of different things
> here: $d1^*$ tests whether the ensemble mean is consistent with the obs
> (given their uncertainty). Whereas our figure 6 and the error bars shown
> there are testing whether the real world obs are consistent with a
> distribution defined from the model ensemble members.

>
> gavin

>
> -----Forwarded Message-----

>
>> From: lucia liljegren <lucia@rankexploits.com>
>> To: gschmidt@giss.nasa.gov
>> Subject: Typo in equation 12 Santer.
>> Date: 20 Oct 2008 15:46:51 -0500

>>
>> Hi Gavin,
>>
>> Someone commenting at ClimateAudit is suggesting that equation 12
>> contains a typo. They are under the impression the $1/nm$ does not
>> belong in the circled term. Rather than going back and forth with "is
>> not a typo", "is so a typo", I figured I'd just ask you. Is there a
>> typo in equation 12 below.

>>

>> ----
>>
>
>>
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>>
>> BTW: I think Santer is pretty good paper.

>>
>> Thanks, Lucia

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>>
>> -----
>>

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Peter.Thorne@noaa.gov, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan.Solomon@noaa.gov, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>
Subject: [Fwd: Santer et al 2008]
Date: Fri, 31 Oct 2008 10:13:11 -0700
Reply-to: santer1@llnl.gov
Cc: "David C. Bader" <bader2@llnl.gov>

Dear folks, While on travel in Hawaii, I received a request from Steven McIntyre for all of the model data used in our IJoC paper (see forwarded email). After some conversation with my PCMDI colleagues, I have decided not to respond to McIntyre's request. If McIntyre repeats his request, I will provide him with the same answer that I gave to David Douglass - all model and observational data used in our IJoC paper are freely available to scientific researchers (as are algorithms for calculating synthetic MSU temperatures from climate model and radiosonde data). If Mr. McIntyre wishes to "audit" our analysis and findings, he has access to exactly the same raw data that we employed. He can compute synthetic MSU temperatures exactly the same way that we did. And he has full details of the statistical tests we applied to compare modeled and observed temperature trends. Recall that McIntyre is the guy who "audited" the temperature reconstructions of Mike Mann and colleagues. Now it appears as if McIntyre wants to audit us. McIntyre should have "audited" the methods and findings of Douglass et al. 2007 - not the methods and findings of Santer et al. 2008. I thought you should know about this development. With best regards, Ben

----- Benjamin D.
Santer Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National
Laboratory P.O. Box 808, Mail Stop L-103 Livermore, CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675 email: santer1@llnl.gov

----- X-Account-Key:

account1 Return-Path: Received: from mail-2.llnl.gov ([unix socket])
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(Cyrus v2.2.12) with LMTPA; Mon, 20 Oct 2008 10:29:15 -0700 Received:
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2008 10:29:15 -0700
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stephen.mcintyre@utoronto.ca using -f Received: from nspiron-
2.llnl.gov (nspiron-2.llnl.gov
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Oct 2008 13:29:11
-0400 From: "Steve McIntyre" To: Subject: Santer et al 2008 Date: Mon,
20 Oct 2008 13:29:11
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X-Priority: 3 (Normal) X-MSMail-Priority: Normal X-Mailer: Microsoft
Outlook, Build
10.0.2627 Importance: Normal X-MimeOLE: Produced By Microsoft MimeOLE
V6.00.2900.3350

Dear Dr Santer,

Could you please provide me either with the monthly model data (49 series) used for statistical analysis in Santer et al 2008 or a link to a URL. I understand that your version has been collated from PCMDI ; my interest is in a file of the data as you used it (I presume that the monthly data used for statistics is about 1-2 MB)

Thank you for your attention,

Steve McIntyre

From: "Cawley Gavin Dr \ (CMP\)" <G.Cawley@uea.ac.uk>

To: <santer1@llnl.gov>

Subject: RE: Possible error in recent IJC paper

Date: Fri, 31 Oct 2008 11:01:46 -0000

Cc: "Jones Philip Prof \ (ENV\)" <P.Jones@uea.ac.uk>, "Gavin Schmidt" <gschmidt@giss.nasa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, "Tom Wigley" <wigley@cgd.ucar.edu>

Dear Ben,

many thanks for the full response to my query. I think my confusion arose from the discussion on RealClimate (which prompted our earlier communication on this topic), which clearly suggested that the observed trend should be expected to lie within the spread of the models, rather than necessarily being close to the mean as the models are stochastic simulations (which seemed reasonable). I've just re-read that post, the key paragraph from [1]<http://www.realclimate.org/index.php/archives/2007/12/tropical-troposphere-trends/> is as follows:

"The interpretation of this is a little unclear (what exactly does the sigma refer to?), but the most likely interpretation, and the one borne out by looking at their Table IIa, is that sigma is calculated as the standard deviation of the model trends. In that case, the formula given defines the uncertainty on the estimate of the mean - i.e. how well we know what the average trend really is. But it only takes a moment to realise why that is irrelevant. Imagine there were 1000's of simulations drawn from the same distribution, then our estimate of the mean trend would get sharper and sharper as N increased. However, the chances that any one realisation would be within those error bars, would become smaller and smaller. Instead, the key standard deviation is simply sigma itself. That defines the likelihood that one realisation (i.e. the real world) is conceivably drawn from the distribution defined by the models."

I had therefore expected the test to use the standard deviations of both the models and the observations (which would give a flat plot in 5B and there would be an obvious overlap of the uncertainties in 6a at say 500hPa).

best regards

Gavin

-----Original Message-----

From: Ben Santer [[2]mailto:santer1@llnl.gov]

Sent: Fri 10/31/2008 4:06 AM

To: Cawley Gavin Dr (CMP)

Cc: Jones Philip Prof (ENV); Gavin Schmidt; Thorne, Peter; Tom Wigley

Subject: Re: Possible error in recent IJC paper

Dear Gavin,

Thanks very much for your email, and for your interest in our recent paper in the International Journal of Climatology (IJoC). There is no error in equation (12) in our IJoC paper. Let me try to answer the questions that you posed.

The first term under the square root in our equation (12) is a standard estimate of the variance of a sample mean - see, e.g., "Statistical Analysis in Climate Research", by Francis Zwiers and Hans von Storch, Cambridge University Press, 1999 (their equation 5.24, page 86). The second term under the square root sign is a very different beast - an estimate of the variance of the observed trend. As we point out, our $d1^*$ test is very similar to a standard Student's t-test of differences in

means (which involves, in its denominator, the square root of two pooled sample variances).

In testing the statistical significance of differences between the model average trend and a single observed trend, Douglass et al. were wrong to use σ_{SE} as the sole measure of trend uncertainty in their statistical test. Their test assumes that the model trend is uncertain, but that the observed trend is perfectly-known. The observed trend is not a "mean" quantity; it is NOT perfectly-known. Douglass et al. made a demonstrably false assumption.

Bottom line: σ_{SE} is a standard estimate of the uncertainty in a sample mean - which is why we use it to characterize uncertainty in the estimate of the model average trend in equation (12). It is NOT appropriate to use σ_{SE} as the basis for a statistical test between two uncertain quantities. The uncertainty in the estimates of both modeled AND observed trend needs to be explicitly incorporated in the design of any statistical test seeking to compare modeled and observed trends. Douglass et al. incorrectly ignored uncertainties in observed trends.

I hope this answers your first question, and explains why there is no inconsistency between the formulation of our $d1^*$ test in equation (12) and the comments that we made in point #3 [immediately before equation (12)]. As we note in point #3, "While σ_{SE} is an appropriate measure of how well the multi-model mean trend can be estimated from a finite sample of model results, it is not an appropriate measure for deciding whether this trend is consistent with a single observed trend."

We could perhaps have made point #3 a little clearer by inserting "imperfectly-known" before "observed trend". I thought, however, that the uncertainty in the estimate of the observed trend was already made very clear in our point #1 (on page 7, bottom of column 2).

To answer your second question, $d1^*$ gives a reasonably flat line in Figure 5B because the first term under the square root sign in equation (12) (the variance of the model average trend, which has a dependence on N , the number of models used in the test) is roughly a factor of 20 smaller than the second term under the square root sign (the variance of the observed trend, which has no dependence on N). The behaviour of $d1^*$ with synthetic data is therefore dominated by the second term under the square root sign - which is why the black lines in Figure 5B are flat.

In answer to your third question, our Figure 6A provides only one of the components from the denominator of our $d1^*$ test (σ_{SE}). Figure 6A does not show the standard errors in the observed trends at discrete pressure levels. Had we attempted to show the observed standard errors at individual pressure levels, we would have produced a very messy Figure, since Figure 6A shows results from 7 different observational datasets.

We could of course have performed our $d1^*$ test at each discrete pressure level. This would have added another bulky Table to an already lengthy paper. We judged that it was sufficient to perform our $d1^*$ test with the synthetic MSU T2 and T2LT temperature trends calculated from the seven radiosonde datasets and the climate model data. The results of such

tests are reported in the final paragraph of Section 7. As we point out, the d_1^* test "indicates that the model-average signal trend (for T2LT) is not significantly different (at the 5% level) from the observed signal trends in three of the more recent radiosonde products (RICH, IUK, and RAOBCORE v1.4)." So there is no inconsistency between the formulation of our d_1^* test in equation (12) and the results displayed in Figure 6.

Thanks again for your interest in our paper, and my apologies for the delay in replying to your email - I have been on travel (and out of email contact) for the past 10 days.

With best regards,

Ben

Cawley Gavin Dr (CMP) wrote:

>
>
> Dear Prof. Santer,
>
> I think there may be a minor problem with equation (12) in your paper
> "Consistency of modelled and observed temperature trends in the tropical
> trophosphere", namely that it includes the standard error of the models
> $1/n_m s_{\langle b_m \rangle}^2$ instead of the standard deviation $s_{\langle b_m \rangle}^2$. Firstly
> the current formulation of (12) seems at odds with objection 3 raised at
> the start of the first column of page 8. Secondly, I can't see how the
> modified test d_1^* gives a flat line in Figure 5B as the test statistic
> is explicitly dependent on the size of the model ensemble n_m . Thirdly,
> the equation seems at odds with the results depicted graphically in
> Figure 6 which would suggest the models are clearly inconsistent at
> higher levels (400-850 hPa) using the confidence interval based on the
> standard error. Lastly, (12) seems at odds with the very lucid
> treatment at RealClimate written by Dr Schmidt.
>
> I congratulate all 17 authors for an excellent contribution that I have
> found most instructive!
>
> I do hope I haven't missed something - sorry to have bothered you if
> this is the case.
>
> best regards
>
> Gavin
>
--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
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FAX: (925) 422-7675

email: santer1@llnl.gov

References

1. <http://www.realclimate.org/index.php/archives/2007/12/tropical-troposphere-trends/>
2. <mailto:santer1@llnl.gov>

From: Tom Wigley <wigley@ucar.edu>
To: Ben Santer <santer1@llnl.gov>, Phil Jones <p.jones@uea.ac.uk>
Subject: [Fwd: Re: Possible error in recent IJC paper]
Date: Sat, 01 Nov 2008 18:50:12 -0600

Hi Ben & Phil, No need to push this further, and you probably realize this anyhow, but the RealClimate criticism of Doug et al. is simply wrong. Ho hum. Tom. Return-Path: Received: from nscan2.ucar.edu (nscan2.ucar.edu [128.117.64.192]) by upham.cgd.ucar.edu (8.13.1/8.13.1) with ESMTP id m9VB1nbA017855 for ; Fri, 31 Oct 2008 05:01:49 -0600 Received: from localhost (localhost.localdomain [127.0.0.1]) by nscan2.ucar.edu (Postfix) with ESMTP id 215F8309C01C for ; Fri, 31 Oct 2008 05:01:49 -0600 (MDT) Received: from nscan2.ucar.edu ([127.0.0.1]) by localhost (nscan2.ucar.edu [127.0.0.1]) (amavisd-new, port 10024) with ESMTP id 24343-06 for ; Fri, 31 Oct 2008 05:01:48 -0600 (MDT) X-SMTP-Auth: no Received: from mailgate5.uea.ac.uk (mailgate5.uea.ac.uk [139.222.130.185]) by nscan2.ucar.edu (Postfix) with ESMTP id 7B9B2309C018 for ; Fri, 31 Oct 2008 05:01:47 -0600 (MDT) Received: from [139.222.130.203] (helo=UEAEXCHCLUS01.UEA.AC.UK) by mailgate5.uea.ac.uk with esmtp (Exim 4.50) id 1KvrlC-00006x-Sp for wigley@cgd.ucar.edu; Fri, 31 Oct 2008 11:01:46 +0000 X-MimeOLE: Produced By Microsoft Exchange V6.5 Content-class: urn:content-classes:message MIME-Version: 1.0 Content-Type: multipart/alternative; boundary="----=_NextPart_001_01C93B48.10CD099C" Subject: RE: Possible error in recent IJC paper Date: Fri, 31 Oct 2008 11:01:46 -0000 Message-ID: <63675957ADD2DF4D9E246871174BEF1EC901E1@UEAEXCHCLUS01.UEA.AC.UK> X-MS-Has-Attach: X-MS-TNEF-Correlator: Thread-Topic: Possible error in recent IJC paper Thread-Index: Ack7DrU3+LlgMjttS5+IB1r2EiTAKAANYJtF References: <63675957ADD2DF4D9E246871174BEF1EC901CE@UEAEXCHCLUS01.UEA.AC.UK> <490A8447.1010603@llnl.gov> From: "Cawley Gavin Dr \ (CMP)" To: Cc: "Jones Philip Prof \ (ENV)" , "Gavin Schmidt" , "Thorne, Peter" , "Tom Wigley" X-Virus-Scanned: amavisd-new at ucar.edu

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best regards

Gavin

-----Original Message-----

From: Ben Santer [[2]mailto:santer1@llnl.gov]

Sent: Fri 10/31/2008 4:06 AM

To: Cawley Gavin Dr (CMP)

Cc: Jones Philip Prof (ENV); Gavin Schmidt; Thorne, Peter; Tom Wigley

Subject: Re: Possible error in recent IJC paper

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Bottom line: σ_{SE} is a standard estimate of the uncertainty in a sample mean - which is why we use it to characterize uncertainty in the estimate of the model average trend in equation (12). It is NOT appropriate to use σ_{SE} as the basis for a statistical test between two uncertain quantities. The uncertainty in the estimates of both modeled AND observed trend needs to be explicitly incorporated in the design of any statistical test seeking to compare modeled and observed trends. Douglass et al. incorrectly ignored uncertainties in observed trends.

I hope this answers your first question, and explains why there is no inconsistency between the formulation of our $d1^*$ test in equation (12) and the comments that we made in point #3 [immediately before equation

(12)]. As we note in point #3, "While σ_{SE} is an appropriate measure of how well the multi-model mean trend can be estimated from a finite sample of model results, it is not an appropriate measure for deciding whether this trend is consistent with a single observed trend."

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To answer your second question, $d1^*$ gives a reasonably flat line in Figure 5B because the first term under the square root sign in equation (12) (the variance of the model average trend, which has a dependence on N , the number of models used in the test) is roughly a factor of 20 smaller than the second term under the square root sign (the variance of the observed trend, which has no dependence on N). The behaviour of $d1^*$ with synthetic data is therefore dominated by the second term under the square root sign - which is why the black lines in Figure 5B are flat.

In answer to your third question, our Figure 6A provides only one of the components from the denominator of our $d1^*$ test (σ_{SE}). Figure 6A does not show the standard errors in the observed trends at discrete pressure levels. Had we attempted to show the observed standard errors at individual pressure levels, we would have produced a very messy Figure, since Figure 6A shows results from 7 different observational datasets.

We could of course have performed our $d1^*$ test at each discrete pressure level. This would have added another bulky Table to an already lengthy paper. We judged that it was sufficient to perform our $d1^*$ test with the synthetic MSU T2 and T2LT temperature trends calculated from the seven radiosonde datasets and the climate model data. The results of such tests are reported in the final paragraph of Section 7. As we point out, the $d1^*$ test "indicates that the model-average signal trend (for T2LT) is not significantly different (at the 5% level) from the observed signal trends in three of the more recent radiosonde products (RICH, IUK, and RAOBCORE v1.4)." So there is no inconsistency between the formulation of our $d1^*$ test in equation (12) and the results displayed in Figure 6.

Thanks again for your interest in our paper, and my apologies for the delay in replying to your email - I have been on travel (and out of email contact) for the past 10 days.

With best regards,

Ben

Cawley Gavin Dr (CMP) wrote:

>

>

> Dear Prof. Santer,

>

> I think there may be a minor problem with equation (12) in your paper
> "Consistency of modelled and observed temperature trends in the tropical

> trophosphere", namely that it includes the standard error of the models
> $1/n_m s\{<b_m>\}^2$ instead of the standard deviation $s\{<b_m>\}^2$. Firstly
> the current formulation of (12) seems at odds with objection 3 raised at
> the start of the first column of page 8. Secondly, I can't see how the
> modified test d_1^* gives a flat line in Figure 5B as the test statistic
> is explicitly dependent on the size of the model ensemble n_m . Thirdly,
> the equation seems at odds with the results depicted graphically in
> Figure 6 which would suggest the models are clearly inconsistent at
> higher levels (400-850 hPa) using the confidence interval based on the
> standard error. Lastly, (12) seems at odds with the very lucid
> treatment at RealClimate written by Dr Schmidt.

>
> I congratulate all 17 authors for an excellent contribution that I have
> found most instructive!

>
> I do hope I haven't missed something - sorry to have bothered you if
> this is the case.

>
> best regards

>
> Gavin

>
--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

References

1. <http://www.realclimate.org/index.php/archives/2007/12/tropical-troposphere-trends/>
2. <mailto:santer1@llnl.gov>

From: Ben Santer <santer1@llnl.gov>
To: Steve McIntyre <stephen.mcintyre@utoronto.ca>
Subject: Re: FW: Santer et al 2008
Date: Mon, 10 Nov 2008 12:10:52 -0800
Reply-to: santer1@llnl.gov
Cc: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>

<x-flowed>

Dear Mr. McIntyre,

I gather that your intent is to "audit" the findings of our recently-published paper in the International Journal of Climatology (IJoC). You are of course free to do so. I note that both the gridded model and observational datasets used in our IJoC paper are freely available to researchers. You should have no problem in accessing exactly the same model and observational datasets that we employed. You will need to do a little work in order to calculate synthetic Microwave Sounding Unit (MSU) temperatures from climate model atmospheric temperature information. This should not pose any difficulties for you. Algorithms for calculating synthetic MSU temperatures have been published by ourselves and others in the peer-reviewed literature. You will also need to calculate spatially-averaged temperature changes from the gridded model and observational data. Again, that should not be too taxing.

In summary, you have access to all the raw information that you require in order to determine whether the conclusions reached in our IJoC paper are sound or unsound. I see no reason why I should do your work for you, and provide you with derived quantities (zonal means, synthetic MSU temperatures, etc.) which you can easily compute yourself.

I am copying this email to all co-authors of the 2008 Santer et al. IJoC paper, as well as to Professor Glenn McGregor at IJoC.

I gather that you have appointed yourself as an independent arbiter of the appropriate use of statistical tools in climate research. Rather than "auditing" our paper, you should be directing your attention to the 2007 IJoC paper published by David Douglass et al., which contains an egregious statistical error.

Please do not communicate with me in the future.

Ben Santer

Steve McIntyre wrote:

> Could you please reply to the request below, Regards, Steve McIntyre

>
> -----Original Message-----
> *From:* Steve McIntyre [mailto:stephen.mcintyre@utoronto.ca]
> *Sent:* Monday, October 20, 2008 1:29 PM
> *To:* ' (santer1@llnl.gov)'
> *Subject:* Santer et al 2008
>
> Dear Dr Santer,
>
> Could you please provide me either with the monthly model data (49
> series) used for statistical analysis in Santer et al 2008 or a link
> to
> a URL. I understand that your version has been collated from PCMDI ;
> my
> interest is in a file of the data as you used it (I presume that the
> monthly data used for statistics is about 1-2 MB) .
>
> Thank you for your attention,
>
> Steve McIntyre
>
>

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: [Fwd: FOI Request]
Date: Tue, 11 Nov 2008 19:57:22 -0800
Reply-to: santer1@llnl.gov
Cc: Karen Owen <Karen.Owen@noaa.gov>, Sharon Leduc
<Sharon.Leduc@noaa.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>, "David C. Bader" <bader2@llnl.gov>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>, "Bamzai, Anjuli" <Anjuli.Bamzai@science.doe.gov>

<x-flowed>

Dear Tom,

Thanks for your email regarding Steven McIntyre's twin requests under the Freedom of Information (FOI) Act. Regarding McIntyre's request (1), no "monthly time series of output from any of the 47 climate models" was "sent by Santer and/or other coauthors of Santer et al 2008 to NOAA employees between 2006 and October 2008".

As I pointed out to Mr. McIntyre in the email I transmitted to him yesterday, all of the raw (gridded) model and observational data used in the 2008 Santer et al. International Journal of Climatology (IJoC) paper are freely available to Mr. McIntyre. If Mr. McIntyre wishes to audit us, and determine whether the conclusions reached in our paper are sound, he has all the information necessary to conduct such an audit. Providing Mr. McIntyre with the quantities that I derived from the raw model data (spatially-averaged time series of surface temperatures and synthetic Microwave Sounding Unit [MSU] temperatures) would defeat the very purpose of an audit.

I note that David Douglass and colleagues have already audited our calculation of synthetic MSU temperatures from climate model data. Douglass et al. obtained "model average" trends in synthetic MSU temperatures (published in their 2007 IJoC paper) that are virtually identical to our own.

McIntyre's request (2) demands "any correspondence concerning these monthly time series between Santer and/or other coauthors of Santer et al 2008 and NOAA employees between 2006 and October 2008". I do not know how you intend to respond this second request. You and three other NOAA co-authors on our paper (Susan Solomon, Melissa Free, and John Lanzante) probably received hundreds of emails that I sent to you in the course of our work on the IJoC paper. I note that this work began in December 2007, following online publication of Douglass et al. in the IJoC. I have no idea why McIntyre's request for email correspondence has a

"start date" of 2006, and thus predates publication of Douglass et al.

My personal opinion is that both FOI requests (1) and (2) are intrusive and unreasonable. Steven McIntyre provides absolutely no scientific justification or explanation for such requests. I believe that McIntyre is pursuing a calculated strategy to divert my attention and focus away from research. As the recent experiences of Mike Mann and Phil Jones have shown, this request is the thin edge of wedge. It will be followed by further requests for computer programs, additional material and explanations, etc., etc.

Quite frankly, Tom, having spent nearly 10 months of my life addressing the serious scientific flaws in the Douglass et al. IJoC paper, I am unwilling to waste more of my time fulfilling the intrusive and frivolous requests of Steven McIntyre. The supreme irony is that Mr. McIntyre has focused his attention on our IJoC paper rather than the Douglass et al. IJoC paper which we criticized. As you know, Douglass et al. relied on a seriously flawed statistical test, and reached incorrect conclusions on the basis of that flawed test.

I believe that our community should no longer tolerate the behavior of Mr. McIntyre and his cronies. McIntyre has no interest in improving our scientific understanding of the nature and causes of climate change. He has no interest in rational scientific discourse. He deals in the currency of threats and intimidation. We should be able to conduct our scientific research without constant fear of an "audit" by Steven McIntyre; without having to weigh every word we write in every email we send to our scientific colleagues.

In my opinion, Steven McIntyre is the self-appointed Joe McCarthy of climate science. I am unwilling to submit to this McCarthy-style investigation of my scientific research. As you know, I have refused to send McIntyre the "derived" model data he requests, since all of the primary model data necessary to replicate our results are freely available to him. I will continue to refuse such data requests in the future. Nor will I provide McIntyre with computer programs, email correspondence, etc. I feel very strongly about these issues. We should not be coerced by the scientific equivalent of a playground bully.

I will be consulting LLNL's Legal Affairs Office in order to determine how the DOE and LLNL should respond to any FOI requests that we receive from McIntyre. I assume that such requests will be forthcoming.

I am copying this email to all co-authors of our 2008 IJoC paper, to my immediate superior at PCMDI (Dave Bader), to Anjuli Bamzai at DOE headquarters, and to Professor Glenn McGregor (the editor who was in charge of our paper at IJoC).

I'd be very happy to discuss these issues with you tomorrow. I'm sorry that the tone of this letter is so formal, Tom. Unfortunately, after today's events, I must assume that any email I write to you may be subject to FOI requests, and could ultimately appear on McIntyre's "ClimateAudit" website.

With best personal wishes,

Ben

Thomas.R.Karl wrote:

> FYI --- Jolene can you set up a conference call with all the parties
> listed below including Ben.

>

> Thanks

>

> ----- Original Message -----

> Subject: FOI Request

> Date: Mon, 10 Nov 2008 10:02:00 -0500

> From: Steve McIntyre <stephen.mcintyre@utoronto.ca>

> To: FOIA@noaa.gov

> CC: Thomas R Karl <Thomas.R.Karl@noaa.gov>

>

>

>

> Nov. 10, 2008

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> National Oceanic and Atmospheric Administration

>

> Public Reference Facility (OFA56)

>

> Attn: NOAA FOIA Officer

>

> 1315 East West Highway (SSMC3)

>

> Room 10730

>

> Silver Spring, Maryland 20910

>

>

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> Re: Freedom of Information Act Request

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> Dear NOAA FOIA Officer:

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> This is a request under the Freedom of Information Act.

>

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>

> Santer et al, Consistency of modelled and observed temperature trends
in

>

> the tropical troposphere, (Int J Climatology, 2008), of which NOAA

> employees J. R. Lanzante, S. Solomon, M. Free and T. R. Karl were

> co-authors, reported on a statistical analysis of the output of 47 runs
> of climate models that had been collated into monthly time series by
> Benjamin Santer and associates.

>

>

>

> I request that a copy of the following NOAA records be provided to me:
> (1) any monthly time series of output from any of the 47 climate models
> sent by Santer and/or other coauthors of Santer et al 2008 to NOAA
> employees between 2006 and October 2008; (2) any correspondence
> concerning these monthly time series between Santer and/or other
> coauthors of Santer et al 2008 and NOAA employees between 2006 and
> October 2008.

>

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>

> The primary sources for NOAA records are J. R. Lanzante, S. Solomon,
M.
> Free and T. R. Karl.

>

>

>

> In order to help to determine my status for purposes of determining the
> applicability of any fees, you should know that I have 5 peer-reviewed
> publications on paleoclimate; that I was a reviewer for WGI; that I
made

> a invited presentations in 2006 to the National Research Council Panel
> on Surface Temperature Reconstructions and two presentations to the
> Oversight and Investigations Subcommittee of the House Energy and
> Commerce Committee.

>

>

>

> In addition, a previous FOI request was discussed by the NOAA Science
> Advisory Board's Data Archiving and Access Requirements Working Group
> (DAARWG). [http:// www.
> joss.ucar.edu/daarwg/may07/presentations/KarL_DAARWG_NOAAArchivepolify-
v0514.pdf](http://www.joss.ucar.edu/daarwg/may07/presentations/KarL_DAARWG_NOAAArchivepolify-v0514.pdf).

>

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>

> I believe a fee waiver is appropriate since the purpose of the request
> is academic research, the information exists in digital format and the
> information should be easily located by the primary sources.

>

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>

> I also include a telephone number (416-469-3034) at which I can be
> contacted between 9 and 7 pm Eastern Daylight Time, if necessary, to
> discuss any aspect of my request.

>

>

>

> Thank you for your consideration of this request.
>
>
>
> I ask that the FOI request be processed promptly as NOAA failed to send
> me a response to the FOI request referred to above, for which Dr Karl
> apologized as follows:
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>
> due to a miscommunication between our office and our headquarters, the
> response was not submitted to you. I deeply apologize for this
> oversight, and we have taken measures to ensure this does not happen in
> the future.
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>
>
> Stephen McIntyre
>
> 25 Playter Blvd
>
> Toronto, Ont M4K 2W1
>
>
>

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Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: Tom Wigley <wigley@ucar.edu>
To: santer1@llnl.gov
Subject: Re: [Fwd: FOI Request]
Date: Tue, 11 Nov 2008 21:27:10 -0700
Cc: "Thomas.R.Karl" <Thomas.R.Karl@noaa.gov>, Karen Owen
<Karen.Owen@noaa.gov>, Sharon Leduc <Sharon.Leduc@noaa.gov>, "Thorne,
Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger
<leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom
Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>,
Susan Solomon <ssolomon@frii.com>, Melissa Free <Melissa.Free@noaa.gov>,
peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'"
<p.jones@uea.ac.uk>, Steve Klein <klein21@mail.llnl.gov>, carl mears
<mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt
<gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>,
Frank Wentz <frank.wentz@remss.com>, "David C. Bader" <bader2@llnl.gov>,
Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>, "Bamzai, Anjali"
<Anjali.Bamzai@science.doe.gov>

<x-flowed>

Hmmm. I note the following , , ,

"at which I can be contacted between 9 and 7 pm Eastern Daylight Time"

Is this a 22 hour, or, for people with time machine, a negative 2 hour
window?

Joking aside, it seems as a matter of principle (albeit a principle yet
to be set by the courts) that provision of primary data sources that are
sufficient to reproduce the results of a scientific analysis is all that
is necessary under FOI.

It also seems that judgment of what correspondence is central to the
analysis can only be made by the persons involved. As a participant in
many of these inter-author communications, I do not recall any that
would give information not already contained in the published paper.

Tom.

+++++

Ben Santer wrote:

> Dear Tom,

>

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> the Freedom of Information (FOI) Act. Regarding McIntyre's request (1),
> no "monthly time series of output from any of the 47 climate models"
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> "sent by Santer and/or other coauthors of Santer et al 2008 to NOAA
> employees between 2006 and October 2008".

>

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> yesterday, all of the raw (gridded) model and observational data used
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> Quite frankly, Tom, having spent nearly 10 months of my life addressing the serious scientific flaws in the Douglass et al. IJoC paper, I am unwilling to waste more of my time fulfilling the intrusive and frivolous requests of Steven McIntyre. The supreme irony is that Mr. McIntyre has focused his attention on our IJoC paper rather than the Douglass et al. IJoC paper which we criticized. As you know, Douglass et al. relied on a seriously flawed statistical test, and reached incorrect conclusions on the basis of that flawed test.

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> climate science. I am unwilling to submit to this McCarthy-style
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> how the DOE and LLNL should respond to any FOI requests that we receive
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> today's events, I must assume that any email I write to you may be
> subject to FOI requests, and could ultimately appear on McIntyre's
> "ClimateAudit" website.

>

> With best personal wishes,

>

> Ben

>

> Thomas.R.Karl wrote:

>> FYI --- Jolene can you set up a conference call with all the parties
>> listed below including Ben.

>>

>> Thanks

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>> Subject: FOI Request

>> Date: Mon, 10 Nov 2008 10:02:00 -0500

>> From: Steve McIntyre <stephen.mcintyre@utoronto.ca>

>> To: FOIA@noaa.gov

>> CC: Thomas R Karl <Thomas.R.Karl@noaa.gov>

>>

>>

>>

>> Nov. 10, 2008

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>> National Oceanic and Atmospheric Administration

>>
>> Public Reference Facility (OFA56)
>>
>> Attn: NOAA FOIA Officer
>>
>> 1315 East West Highway (SSMC3)
>>
>> Room 10730
>>
>> Silver Spring, Maryland 20910
>>
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>> Re: Freedom of Information Act Request
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>> Dear NOAA FOIA Officer:
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>>
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>> The primary sources for NOAA records are J. R. Lanzante, S. Solomon,
>> M. Free and T. R. Karl.
>>
>>
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>> In order to help to determine my status for purposes of determining
>> the applicability of any fees, you should know that I have 5
>> peer-reviewed publications on paleoclimate; that I was a reviewer for

>> WG1; that I made a invited presentations in 2006 to the National
>> Research Council Panel on Surface Temperature Reconstructions and two
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>> oversight, and we have taken measures to ensure this does not happen
>> in the future.

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>>

>> Stephen McIntyre

>>
>> 25 Playter Blvd
>>

>> Toronto, Ont M4K 2W1

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>>

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>

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: [Fwd: FOI Request]
Date: Wed Nov 12 09:31:31 2008

Ben,

Another point to discuss when you have your conference call - is why don't they ask Douglass for all his data. It is essentially the same.

You can also think of all this positively - they think a few of us do really important work, so they concentrate on what they think are the cutting edge pieces of work.

I have a big review on paleo coming out soon in *The Holocene* - with 20+ others. Won't be out till next year, but I can say for certain that it will feature strongly on CA. Not too much they can request via FOI, but they will think of something. This paper will explain where a Figure came from in the First IPCC Report - the infamous one that Chris Folland put together on the last 1000 years. CA will say they found this out - they had

a thread on it 9 months ago according to Gavin. I have the submission date of the article and more detail though - to show we found out first.

Cheers

Phil

At 03:57 12/11/2008, you wrote:

Dear Tom,

Thanks for your email regarding Steven McIntyre's twin requests under the Freedom of Information (FOI) Act. Regarding McIntyre's request (1), no "monthly time series of output from any of the 47 climate models" was "sent by Santer and/or other coauthors of Santer et al 2008 to NOAA employees between 2006 and October 2008".

As I pointed out to Mr. McIntyre in the email I transmitted to him yesterday, all of the raw (gridded) model and observational data used in the 2008 Santer et al. *International Journal of Climatology* (IJoC) paper are freely available to Mr. McIntyre. If Mr. McIntyre wishes to audit us, and determine whether the conclusions reached in our paper are sound, he has all the information necessary to conduct such an audit. Providing Mr. McIntyre with the quantities that I derived from the raw model data (spatially-averaged time series of surface temperatures and synthetic Microwave Sounding Unit [MSU] temperatures) would defeat the very purpose of an audit.

I note that David Douglass and colleagues have already audited our calculation of synthetic MSU temperatures from climate model data. Douglass et al. obtained "model average" trends in synthetic MSU temperatures (published in their 2007 IJoC paper) that are virtually identical to our own.

McIntyre's request (2) demands "any correspondence concerning these monthly time series between Santer and/or other coauthors of Santer et al 2008 and NOAA employees between

2006 and October 2008". I do not know how you intend to respond this second request. You and three other NOAA co-authors on our paper (Susan Solomon, Melissa Free, and John Lanzante) probably received hundreds of emails that I sent to you in the course of our work on the IJoC paper. I note that this work began in December 2007, following online publication of Douglass et al. in the IJoC. I have no idea why McIntyre's request for email correspondence has a "start date" of 2006, and thus predates publication of Douglass et al.

My personal opinion is that both FOI requests (1) and (2) are intrusive and unreasonable. Steven McIntyre provides absolutely no scientific justification or explanation for such requests. I believe that McIntyre is pursuing a calculated strategy to divert my attention and focus away from research. As the recent experiences of Mike Mann and Phil Jones have shown, this request is the thin edge of wedge. It will be followed by further requests for computer programs, additional material and explanations, etc., etc.

Quite frankly, Tom, having spent nearly 10 months of my life addressing the serious scientific flaws in the Douglass et al. IJoC paper, I am unwilling to waste more of my time fulfilling the intrusive and frivolous requests of Steven McIntyre. The supreme irony is that Mr. McIntyre has focused his attention on our IJoC paper rather than the Douglass et al. IJoC paper which we criticized. As you know, Douglass et al. relied on a seriously flawed statistical test, and reached incorrect conclusions on the basis of that flawed test.

I believe that our community should no longer tolerate the behavior of Mr. McIntyre and his cronies. McIntyre has no interest in improving our scientific understanding of the nature and causes of climate change. He has no interest in rational scientific discourse. He deals in the currency of threats and intimidation. We should be able to conduct our scientific research without constant fear of an "audit" by Steven McIntyre; without having to weigh every word we write in every email we send to our scientific colleagues.

In my opinion, Steven McIntyre is the self-appointed Joe McCarthy of climate science. I am unwilling to submit to this McCarthy-style investigation of my scientific research. As you know, I have refused to send McIntyre the "derived" model data he requests, since all of the primary model data necessary to replicate our results are freely available to him. I will continue to refuse such data requests in the future. Nor will I provide McIntyre with computer programs, email correspondence, etc. I feel very strongly about these issues. We should not be coerced by the scientific equivalent of a playground bully.

I will be consulting LLNL's Legal Affairs Office in order to determine how the DOE and LLNL should respond to any FOI requests that we receive from McIntyre. I assume that such requests will be forthcoming.

I am copying this email to all co-authors of our 2008 IJoC paper, to my immediate superior at PCMDI (Dave Bader), to Anjuli Bamzai at DOE headquarters, and to Professor Glenn McGregor (the editor who was in charge of our paper at IJoC).

I'd be very happy to discuss these issues with you tomorrow. I'm sorry that the tone of

this letter is so formal, Tom. Unfortunately, after today's events, I must assume that any email I write to you may be subject to FOI requests, and could ultimately appear on McIntyre's "ClimateAudit" website.

With best personal wishes,

Ben

Thomas.R.Karl wrote:

FYI --- Jolene can you set up a conference call with all the parties listed below including Ben.

Thanks

----- Original Message -----

Subject: FOI Request

Date: Mon, 10 Nov 2008 10:02:00 -0500

From: Steve McIntyre <stephen.mcintyre@utoronto.ca>

To: FOIA@noaa.gov

CC: Thomas R Karl <Thomas.R.Karl@noaa.gov>

Nov. 10, 2008

National Oceanic and Atmospheric Administration

Public Reference Facility (OFA56)

Attn: NOAA FOIA Officer

1315 East West Highway (SSMC3)

Room 10730

Silver Spring, Maryland 20910

Re: Freedom of Information Act Request

Dear NOAA FOIA Officer:

This is a request under the Freedom of Information Act.

Santer et al, Consistency of modelled and observed temperature trends in the tropical troposphere, (Int J Climatology, 2008), of which NOAA employees J. R. Lanzante, S. Solomon, M. Free and T. R. Karl were co-authors, reported on a statistical analysis of the output of 47 runs of climate models that had been collated into monthly time series by Benjamin Santer and associates.

I request that a copy of the following NOAA records be provided to me: (1) any monthly time series of output from any of the 47 climate models sent by Santer and/or other coauthors of Santer et al 2008 to NOAA employees between 2006 and October 2008; (2) any correspondence concerning these monthly time series between Santer and/or other coauthors of Santer et al 2008 and NOAA employees between 2006 and October 2008.

The primary sources for NOAA records are J. R. Lanzante, S. Solomon, M. Free and T. R. Karl.

In order to help to determine my status for purposes of determining the applicability of any fees, you should know that I have 5 peer-reviewed publications on paleoclimate; that I was a reviewer for WG1; that I made a invited presentations in 2006 to the National Research Council Panel on Surface Temperature Reconstructions and two presentations to the Oversight and Investigations Subcommittee of the House Energy and Commerce Committee.

In addition, a previous FOI request was discussed by the NOAA Science Advisory Boards Data Archiving and Access Requirements Working Group (DAARWG). [1][http:// www.joss.ucar.edu/daarwg/may07/presentations/KarL_DAARWG_NOAAArchivepolify-v0514.pdf](http://www.joss.ucar.edu/daarwg/may07/presentations/KarL_DAARWG_NOAAArchivepolify-v0514.pdf).

I believe a fee waiver is appropriate since the purpose of the request is academic research, the information exists in digital format and the information should be easily located by the primary sources.

I also include a telephone number (416-469-3034) at which I can be contacted between 9 and 7 pm Eastern Daylight Time, if necessary, to discuss any aspect of my request.

Thank you for your consideration of this request.

I ask that the FOI request be processed promptly as NOAA failed to send me a response to the FOI request referred to above, for which Dr Karl apologized as follows:

due to a miscommunication between our office and our headquarters, the response was not submitted to you. I deeply apologize for this oversight, and we have taken measures to ensure this does not happen in the future.

Stephen McIntyre
25 Playter Blvd
Toronto, Ont M4K 2W1

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103

Livermore, CA 94550, U.S.A.

Tel: (925) 422-3840

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Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich Email p.jones@uea.ac.uk

NR4 7TJ

UK

References

1. <http://>

From: Phil Jones <p.jones@uea.ac.uk>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: Re: GHCN
Date: Mon Nov 17 17:04:27 2008

Gavin,

First the figures are just for you - don't pass on!!! I don't normally see these. I just asked my MOHC contact - and he's seen the furore on the blogs. Why did the Daily Telegraph run with the story - it's all back to their readers thinking the UK is run by another country!

These 3 paras (below) are from the GHCN web site. They appear to be the only mention I can see of the WMO CLIMAT network on a web site. The rigorous QC that is being talked about is

done in retrospect. They don't do much in real time - except an outlier check.

Anyway - the CLIMAT network is part of the GTS. The members (NMSs) send their monthly averages/total around the other NMSs on the 4th and the 18-20th of the month afterwards. Few seem to adhere to these dates much these days, but the aim is to send the data around twice in the following month. Data comes in code like everything else on the GTS, so a few centres (probably a handful, NOAA/CPC, MOHC, MeteoFrance, DWD, Roshydromet, CMA, JMA and the Australians) that are doing analyses for weather forecasts have the software to pick out the CLIMAT data and put it somewhere.

At the same time these same centres are taking the synop data off the system and summing it to months - producing flags of how much was missing. At the MOHC they compare the CLIMAT message with the monthly calculated average/total. If they are close they accept the CLIMAT. Some countries don't use the mean of max and min (which the synops provide) to calculate the mean, so it is important to use the CLIMAT as this is likely to ensure continuity. If they don't agree they check the flags and there needs to be a bit of human intervention. The figures are examples for this October.

What often happens is that countries send out the same data for the following month. This happens mostly in developing countries, as a few haven't yet got software to produce the CLIMAT data in the correct format. There is WMO software to produce these from a wide variety of possible formats the countries might be using. Some seem to do this by overwriting the files from the previous month. They add in the correct data, but then forget to save the revised file. Canada did this a few years ago - but they sent the correct data around a day later and again the second time, after they got told by someone at MOHC.

My guess here is that NOAA didn't screw up, but that Russia did. For all countries except Russia, all data for that country comes out together. For Russia it comes out in regions - well it is a big place! Trying to prove this would need some Russian help - Pasha Groisman? - but there isn't much point. The fact that all the affected

data were from one Russian region suggests to me it was that region.

Probably not of much use to an FAQ!

Cheers

Phil

The Global Historical Climatology Network (GHCN-Monthly) data base contains historical temperature, precipitation, and pressure data for thousands of land stations worldwide. The period of record varies from station to station, with several thousand extending back to 1950 and several hundred being updated monthly via CLIMAT reports. The data are available without charge through NCDCs anonymous FTP service.

Both historical and near-real-time GHCN data undergo rigorous quality assurance reviews. These reviews include preprocessing checks on source data, time series checks that identify spurious changes in the mean and variance, spatial comparisons that verify the accuracy of the climatological mean and the seasonal cycle, and neighbor checks that identify outliers from both a serial and a spatial perspective.

GHCN-Monthly is used operationally by NCDC to monitor long-term trends in temperature and precipitation. It has also been employed in several international climate assessments, including the Intergovernmental Panel on Climate Change 4th Assessment Report, the Arctic Climate Impact Assessment, and the "State of the Climate" report published annually by the Bulletin of the American Meteorological Society.

At 12:56 17/11/2008, you wrote:

thanks.

Actually, I don't think that many people have any idea how the NWS's send out data, what data they send out, what they don't and how these things are collated. Perhaps you'd like to send me some notes on this that I could write up as a FAQ? Won't change anything much, but it would be a handy reference....

gavin

On Mon, 2008-11-17 at 07:53, Phil Jones wrote:

>> Gavin,

- > I may be getting touchy but the CA thread on the HadCRUt October 08
- > data seems full of snidey comments. Nice to see that they have very little
- > right. Where have they got the idea that the data each month come
- > from GHCN? There are the daily synops and the CLIMAT messages -
- > nothing to do with GHCN. All they have to do is read Brohan et al (2006)
- > and they can see this - and how we merge the land and marine! They
- > seem to have no idea about the Global Telecommunications System.
- > Anyway - expecting the proofs of the Wengen paper any day now.
- > Have already sent back loads of updated references and sorted out almost all
- > of the other reference problems.
- > When the paper comes out - not sure if The Holocene do online first -
- > happy for you to point out the publication dates (date first

> received etc) when
> they scream that they sorted out that diagram from the first IPCC Report.
>
> Don't know how you find the time to do all this responding- keep it up!
>
> Cheers
> Phil
>
>
>
>
> Prof. Phil Jones
> Climatic Research Unit Telephone +44 (0) 1603 592090
> School of Environmental Sciences Fax +44 (0) 1603 507784
> University of East Anglia
> Norwich Email p.jones@uea.ac.uk
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>

>

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University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: wigley@ucar.edu
To: santer1@llnl.gov
Subject: Re: Further fallout from our IJoC paper
Date: Tue, 2 Dec 2008 15:29:07 -0700 (MST)
Cc: santer1@llnl.gov, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, peter.thorne@noaa.gov, "Leopold Haimberger" <leopold.haimberger@univie.ac.at>, "Karl Taylor" <taylor13@llnl.gov>, "Tom Wigley" <wigley@cgd.ucar.edu>, "John Lanzante" <john.lanzante@noaa.gov>, susan.solomon@noaa.gov, "Melissa Free" <melissa.free@noaa.gov>, "peter gleckler" <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, "Thomas R Karl" <thomas.r.karl@noaa.gov>, "Steve Klein" <klein21@mail.llnl.gov>, "carl mears" <mears@remss.com>, "Doug Nychka" <nychka@ucar.edu>, "Gavin Schmidt" <gschmidt@giss.nasa.gov>, "Steven Sherwood" <steven.sherwood@yale.edu>, "Frank Wentz" <frank.wentz@remss.com>

Ben,

I support you on this. However, there is more to be said than what you give below. For instance, it would be useful to note that, in principle, an audit scheme could be a good thing if done properly. But an audit must start at square one (your point). So, one can appear to applaud McIntyre at first, but then go on to note that his modus operandi seems to be flawed.

In this case, as you have noted before, if Mc could not get the data from us, then he could have got it from Douglass. Given this, it is strange to keep hounding us. This would, of course, raise the issue of whether the Douglass data are the same as ours (and/or the same as in CCSP 1.1). I'm not sure whether Douglass et al. actually state that their data are the same as CCSP 1.1, but it would be good if they did -- because our IJoC data are the same as CCSP 1.1.

Mc could say that Douglass already effectively audited our calculations from the raw data, which is why he does not want to/need to repeat this step. But if he does say this then why not get the data from Douglass?

Have a go at writing something -- but try to pre-empt any come back from Mc or others. Also, don't just consider our case, but put it as an example of more general issues.

The issue of auditing is a tricky one. The auditors must, themselves, be able to demonstrate that they have no ulterior motives. One way to do this would be to audit papers on both sides of an issue. In other words, both us and Douglass should be audited together. In a sense, our paper is an audit of Douglass -- and we found his work to be flawed. A second opinion on this already exists, through the refereeing of our paper. I suppose a third opinion from the likes of Mc might be of value in a controversial area like this. But then, is Mc the right person to do this? Is he unbiased? Does he have the right credentials (as a statistician)?

One could argue that IPCC had an auditing system in place. This is

partly through the multiple levels of review -- but doesn't each chapter have another person(s) to sign off on the responses to review comments?

There are some interesting general issues here.

Tom.

+++++

I'm happy to co-author anything you write.

> Dear folks,

>

> There has been some additional fallout from the publication of our paper

> in the International Journal of Climatology. After reading Steven
> McIntyre's discussion of our paper on climateaudit.com (and reading
> about my failure to provide McIntyre with the data he requested), an
> official at DOE headquarters has written to Cherry Murray at LLNL,
> claiming that my behavior is bringing LLNL's good name into disrepute.
> Cherry is the Principal Associate Director for Science and Technology at

> LLNL, and reports to LLNL's Director (George Miller).

>

> I'm getting sick of this kind of stuff, and am tired of simply taking it

> on the chin.

>

> Accordingly, I have been trying to evaluate my options. I believe that
> one option is to write a letter to Nature, briefly outlining some of the

> events that have transpired subsequent to the publication of our IJoC
> paper. Nature would be a logical choice for such a letter, since they
> published a brief account of our findings in their "Research
Highlights"

> section. The letter would provide some public record of my position
> regarding McIntyre's data request, and would note that:

>

> "all of the raw (gridded) model and observational data used in the 2008
> Santer et al. International Journal of Climatology (IJoC) paper are
> freely available to Mr. McIntyre. If Mr. McIntyre wishes to audit us,
> and determine whether the conclusions reached in our paper are sound,
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> has all the information necessary to conduct such an audit. Providing
> Mr. McIntyre with the quantities that I derived from the raw model data
> (spatially-averaged time series of surface temperatures and synthetic
> Microwave Sounding Unit [MSU] temperatures) would defeat the very
> purpose of an audit." (email from Ben Santer to Tom Karl, Nov. 11,
2008).

>

> I think that some form of public record would be helpful, particularly
> if LLNL management continues to receive emails alleging that my
behavior

> is tarnishing LLNL's scientific reputation.

>
> Since it was my decision not to provide McIntyre with derived
quantities
> (synthetic MSU temperatures), I'm perfectly happy to be the sole author
> of such a letter to Nature.
>
> Your thoughts or advice in this matter would be much appreciated.
>
> With best regards,
>
> Ben
> -----

> Benjamin D. Santer
> Program for Climate Model Diagnosis and Intercomparison
> Lawrence Livermore National Laboratory
> P.O. Box 808, Mail Stop L-103
> Livermore, CA 94550, U.S.A.
> Tel: (925) 422-3840
> FAX: (925) 422-7675
> email: santer1@llnl.gov
> -----

>

From: Gavin Schmidt <gschmidt@giss.nasa.gov>
To: santer1@llnl.gov
Subject: Re: Further fallout from our IJoC paper
Date: 02 Dec 2008 17:58:34 -0500
Cc: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Peter.Thorne@noaa.gov, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan.Solomon@noaa.gov, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Steve Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>

Ben, there are two very different things going on here. One is technical and related to the actual science and the actual statistics, the second is political, and is much more concerned with how incidents like this can be portrayed. The second is the issue here.

The unfortunate fact is that the 'secret science' meme is an extremely powerful rallying call to people who have no idea about what is going on. Claiming (rightly or wrongly) that information is being hidden has a huge amount of resonance (as you know), much more so than whether Douglass et al know their statistical elbow from a hole in the ground.

Thus any increase in publicity on this - whether in the pages of Nature or elsewhere - is much more likely to bring further negative fallout despite your desire to clear the air. Whatever you say, it will still be presented as you hiding data.

The contrarians have found that there is actually no limit to what you can ask people for (raw data, intermediate steps, additional calculations, residuals, sensitivity calculations, all the code, a workable version of the code on any platform etc.), and like Somali pirates they have found that once someone has paid up, they can always shake them down again.

Thus, I would not advise any public statements on this. Instead, email you immediate superiors and the director with a short statement along the lines of what you suggest below (i.e. of course you want open science, the data *are* in the public domain (with links) and calls for more intermediate steps are just harassment to prevent scientists doing what they are actually paid too). I wouldn't put in anything specifically related to McIntyre.

A much more satisfying response would be to demonstrate how easy it is to replicate the analysis in the paper starting from scratch using openly available data (such as through Joe Sirott's portal) and the simplest published MSU weighting function. If you can show that this can be done in a couple of hours (or whatever), it makes the other side look like incompetent amateurs. Maybe someone has a graduate student available....?

Gavin

On Tue, 2008-12-02 at 15:52, Ben Santer wrote:

> Dear folks,

>

> There has been some additional fallout from the publication of our paper

> in the International Journal of Climatology. After reading Steven
> McIntyre's discussion of our paper on climateaudit.com (and reading
> about my failure to provide McIntyre with the data he requested), an
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> Your thoughts or advice in this matter would be much appreciated.

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> With best regards,

>

> Ben

> -----

> Benjamin D. Santer

> Program for Climate Model Diagnosis and Intercomparison

> Lawrence Livermore National Laboratory

> P.O. Box 808, Mail Stop L-103

> Livermore, CA 94550, U.S.A.

> Tel: (925) 422-3840

> FAX: (925) 422-7675

> email: santer1@llnl.gov

> -----

>

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov, Tom Wigley <wigley@ucar.edu>
Subject: Re: Schles suggestion
Date: Wed Dec 3 13:57:09 2008
Cc: mann <mann@psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Karl Taylor <taylor13@llnl.gov>, peter gleckler <gleckler1@llnl.gov>

Ben,

When the FOI requests began here, the FOI person said we had to abide by the requests. It took a couple of half hour sessions - one at a screen, to convince them otherwise showing them what CA was all about. Once they became aware of the types of people we were dealing with, everyone at UEA (in the registry and in the Environmental Sciences school - the head of school and a few others) became very supportive. I've got to know the FOI person quite well and the Chief Librarian - who deals with appeals. The VC is also aware of what is going on - at least for one of the requests, but probably doesn't know the number we're dealing with. We are in double figures.

One issue is that these requests aren't that widely known within the School. So I don't know who else at UEA may be getting them. CRU is moving up the ladder of requests at UEA though - we're way behind computing though. We're away of requests going to others in the UK - MOHC, Reading, DEFRA and Imperial College.

So spelling out all the detail to the LLNL management should be the first thing you do. I hope that Dave is being supportive at PCMDI.

The inadvertent email I sent last month has led to a Data Protection Act request sent by a certain Canadian, saying that the email maligned his scientific credibility with his peers!

If he pays 10 pounds (which he hasn't yet) I am supposed to go through my emails and he can get anything I've written about him. About 2 months ago I deleted loads of emails, so have very little - if anything at all. This legislation is different from the FOI -

it is supposed to be used to find out why you might have a poor credit rating !

In response to FOI and EIR requests, we've put up some data - mainly paleo data. Each request generally leads to more - to explain what we've put up. Every time, so far, that hasn't led to anything being added - instead just statements saying read what is in the papers and what is on the web site! Tim Osborn sent one such response (via the FOI person) earlier this week. We've never sent programs, any codes and manuals.

In the UK, the Research Assessment Exercise results will be out in 2 weeks time. These are expensive to produce and take too much time, so from next year we'll be moving onto a metric based system. The metrics will be # and amounts of grants, papers and citations etc. I did flippantly suggest that the # of FOI requests you get should be another.

When you look at CA, they only look papers from a handful of people. They will start on another coming out in The Holocene early next year. Gavin and Mike are on this with loads of others. I've told both exactly what will appear on CA once they get access to it!

Cheers

Phil

At 01:17 03/12/2008, Ben Santer wrote:

Dear Tom,

I think that the idea of a Commentary in Science or Nature is a good one. Steve Sherwood made a similar suggestion. I'd be perfectly happy NOT to be involved in such a Commentary. My involvement would look too self-serving.

One of the problems is that I'm caught in a real Catch-22 situation. At present, I'm damned and publicly vilified because I refused to provide McIntyre with the data he requested. But had I acceded to McIntyre's initial request for climate model data, I'm convinced (based on the past experiences of Mike Mann, Phil, and Gavin) that I would

have spent years of my scientific career dealing with demands for further explanations, additional data, Fortran code, etc. (Phil has been complying with FOIA requests from McIntyre and his cronies for over two years). And if I ever denied a single request for further information, McIntyre would have rubbed his hands gleefully and written: "You see - he's guilty as charged!" on his website.

You and I have spent over a decade of our scientific careers on the MSU issue, Tom. During much of that time, we've had to do science in "reactive mode", responding to the latest outrageous claims and inept science by John Christy, David Douglass, or S. Fred Singer. For the remainder of my scientific career, I'd like to dictate my own research agenda. I don't want that agenda driven by the constant need to respond to Christy, Douglass, and Singer. And I certainly don't want to spend years of my life interacting with the likes of Steven McIntyre.

I hope LLNL management will provide me with their full support. If they do not, I'm fully prepared to seek employment elsewhere.

With best regards,

Ben

Tom Wigley wrote:

Ben,

Re the idea Michael sent around (to Revkin et al.) this is something that Nature or Science might like as a Commentary. It might even be possible to include some indirect reference to the Mc audit issue. The notes I sent could be a starting point. One problem is that you could not be first author as this would look like garnering publicity for your own work (as the 2 key papers are both Santer et al.) Even having me as the first author may not work. An ideal person would be Tom Karl, who sent me a response saying "nice summary".

What do you think?

Tom.

--

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
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Tel: (925) 422-3840
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email: santer1@llnl.gov

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: Phil Jones <p.jones@uea.ac.uk>
To: wigley@ucar.edu
Subject: Re: Schles suggestion
Date: Thu Dec 4 12:40:29 2008

Tom,

Obviously don't pass on! These proofs have gone back with about 60 changes to be made. Should be out first issue of 2009.

The bet is that CA will say they found that the IPCC Figure from 1990 was a Lamb diagram 6 months ago. They did, but they didn't get the right source, and our paper was submitted in early 2008. CA will also comment on the section on pp21-31. The summary of where we are with the individual proxies is useful for most of them - but we didn't get anyone working with speleothems involved. I remain unconvinced they get the resolution claimed. Yet to see a speleothem paper which doesn't compare their (individual site) reconstruction with either the MBH series or a solar proxy.

I hope Ben gets the support from PCMDI and LLNL.

Cheers

Phil

Cheers

Phil

At 22:33 03/12/2008, you wrote:

Phil,

Thanks for all the information on the GISS etc. data.

Re below -- can you send me a preprint of the Holocene paper.

Tom.

+++++

>

> Ben,

> When the FOI requests began here, the FOI person said we had to abide

> by the requests. It took a couple of half hour sessions - one at a

> screen, to convince them otherwise

> showing them what CA was all about. Once they became aware of the

> types of people we were

> dealing with, everyone at UEA (in the registry and in the

> Environmental Sciences school

> - the head of school and a few others) became very supportive. I've

> got to know the FOI

> person quite well and the Chief Librarian - who deals with appeals.

- > The VC is also
- > aware of what is going on - at least for one of the requests, but
- > probably doesn't know
- > the number we're dealing with. We are in double figures.
- >
- > One issue is that these requests aren't that widely known within
- > the School. So
- > I don't know who else at UEA may be getting them. CRU is moving up
- > the ladder of
- > requests at UEA though - we're way behind computing though. We're away
- > of
- > requests going to others in the UK - MOHC, Reading, DEFRA and
- > Imperial College.
- >
- > So spelling out all the detail to the LLNL management should be
- > the first thing
- > you do. I hope that Dave is being supportive at PCMDI.
- >
- > The inadvertent email I sent last month has led to a Data
- > Protection Act request sent by
- > a certain Canadian, saying that the email maligned his scientific
- > credibility with his peers!
- > If he pays 10 pounds (which he hasn't yet) I am supposed to go
- > through my emails
- > and he can get anything I've written about him. About 2 months ago
- > I deleted loads of
- > emails, so have very little - if anything at all. This legislation
- > is different from the FOI -
- > it is supposed to be used to find out why you might have a poor
- > credit rating !
- >
- > In response to FOI and EIR requests, we've put up some data -
- > mainly paleo data.
- > Each request generally leads to more - to explain what we've put
- > up. Every time, so
- > far, that hasn't led to anything being added - instead just
- > statements saying read
- > what is in the papers and what is on the web site! Tim Osborn sent one
- > such
- > response (via the FOI person) earlier this week. We've never sent
- > programs, any codes
- > and manuals.
- >

> In the UK, the Research Assessment Exercise results will be out
> in 2 weeks time.
> These are expensive to produce and take too much time, so from next
> year we'll
> be moving onto a metric based system. The metrics will be # and
> amounts of grants,
> papers and citations etc. I did flippantly suggest that the # of
> FOI requests you get
> should be another.

>
> When you look at CA, they only look papers from a handful of
> people. They will start on another coming out in The Holocene early
> next year. Gavin
> and Mike are on this with loads of others. I've told both exactly
> what will appear on
> CA once they get access to it!

>
> Cheers
> Phil

>
>

> At 01:17 03/12/2008, Ben Santer wrote:

>>Dear Tom,

>>
>>I think that the idea of a Commentary in Science or Nature is a good
>>one. Steve Sherwood made a similar suggestion. I'd be perfectly
>>happy NOT to be involved in such a Commentary. My involvement would
>>look too self-serving.

>>
>>One of the problems is that I'm caught in a real Catch-22 situation.
>>At present, I'm damned and publicly vilified because I refused to
>>provide McIntyre with the data he requested. But had I acceded to
>>McIntyre's initial request for climate model data, I'm convinced
>>(based on the past experiences of Mike Mann, Phil, and Gavin) that I
>>would have spent years of my scientific career dealing with demands
>>for further explanations, additional data, Fortran code, etc. (Phil
>>has been complying with FOIA requests from McIntyre and his cronies
>>for over two years). And if I ever denied a single request for
>>further information, McIntyre would have rubbed his hands gleefully
>>and written: "You see - he's guilty as charged!" on his website.

>>
>>You and I have spent over a decade of our scientific careers on the
>>MSU issue, Tom. During much of that time, we've had to do science in

>>"reactive mode", responding to the latest outrageous claims and
>>inept science by John Christy, David Douglass, or S. Fred Singer.
>>For the remainder of my scientific career, I'd like to dictate my
>>own research agenda. I don't want that agenda driven by the constant
>>need to respond to Christy, Douglass, and Singer. And I certainly
>>don't want to spend years of my life interacting with the likes of
>>Steven McIntyre.

>>
>>I hope LLNL management will provide me with their full support. If
>>they do not, I'm fully prepared to seek employment elsewhere.

>>
>>With best regards,

>>
>>Ben

>>
>>Tom Wigley wrote:

>>>Ben,
>>>Re the idea Michael sent around (to Revkin et al.)
>>>this is something that Nature or Science might like
>>>as a Commentary. It might even be possible to include
>>>some indirect reference to the Mc audit issue. The
>>>notes I sent could be a starting point. One problem
>>>is that you could not be first author as this would
>>>look like garnering publicity for your own work (as
>>>the 2 key papers are both Santer et al.) Even having
>>>me as the first author may not work. An ideal person
>>>would be Tom Karl, who sent me a response saying "nice
>>>summary".

>>>What do you think?

>>>Tom.

>>
>>
>>--

>>-----
>>Benjamin D. Santer
>>Program for Climate Model Diagnosis and Intercomparison
>>Lawrence Livermore National Laboratory
>>P.O. Box 808, Mail Stop L-103
>>Livermore, CA 94550, U.S.A.
>>Tel: (925) 422-3840
>>FAX: (925) 422-7675
>>email: santer1@llnl.gov
>>-----

>
> Prof. Phil Jones
> Climatic Research Unit Telephone +44 (0) 1603 592090
> School of Environmental Sciences Fax +44 (0) 1603 507784
> University of East Anglia
> Norwich Email p.jones@uea.ac.uk
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> UK
> -----
>

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Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

From: David Thompson <davet@atmos.colostate.edu>
To: Phil Jones <p.jones@uea.ac.uk>, John Kennedy <john.kennedy@metoffice.gov.uk>, Mike Wallace <wallace@atmos.washington.edu>
Subject: the paper and a can of worms
Date: Tue, 9 Dec 2008 11:49:09 -0700

hi all, I plan on sending the 'penultimate' draft of the full paper later today, but thought I'd comment on the NH/SH comparison in a separate email. Anyway, I've been debating adding a comparison of the NH and SH, as per your suggestions. But I think I'm going to delay that discussion to a different paper. The current paper is already long. And I think looking at the differences between the hemispheres is going to open a can of worms. Here is an example that influenced my thinking: The time series in the attached figure show the differences between the NH and SH mean (0-90N minus 0-90S) for the raw data (top) and ENSO/COWL residual data (bottom). (COWL is removed only from the NH). Among many things, the difference time series show that the cooling in the 70s is largest in the NH, which we know from previous work. Maybe it's just my eye, but the differences between the time series in the 70s look almost discrete. It's as if the NH ratcheted downwards relative to the SH in a very short period ~1968, then crept upwards through the present. My thinking is that we will get a lot of mileage out of comparing the hemispheres, but that to do it right, it's going to take a fair bit more analysis. And at 27 pages I think we're pushing the attention span of the average reader. So I'm going to delay the analysis to our next paper. It gives us something to do in future! Paper will follow later... -Dave

----- David W. J. Thompson
www.atmos.colostate.edu/~davet i¼ Dept of Atmospheric Science Colorado State University
Fort Collins, CO 80523 USA Phone: 970-491-3338 Fax: 970-491-8449 hi all,

I plan on sending the 'penultimate' draft of the full paper later today, but thought I'd comment on the NH/SH comparison in a separate email.

Anyway, I've been debating adding a comparison of the NH and SH, as per your suggestions. But I think I'm going to delay that discussion to a different paper. The current paper is already long. And I think looking at the differences between the hemispheres is going to open a can of worms. Here is an example that influenced my thinking:

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Paper will follow later...

-Dave

David W. J. Thompson
www.atmos.colostate.edu/~davet

Attachment Converted: "c:\eudora\attach\NHandSHRawFullResidual.pdf"

Dept of Atmospheric Science
Colorado State University
Fort Collins, CO 80523
USA
Phone: 970-491-3338
Fax: 970-491-8449

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: A quick question
Date: Wed Dec 10 10:14:10 2008

Ben,

Haven't got a reply from the FOI person here at UEA. So I'm not entirely confident the numbers are correct. One way of checking would be to look on CA, but I'm not doing that. I did get an email from the FOI person here early yesterday to tell me I shouldn't be deleting emails - unless

this was 'normal' deleting to keep emails manageable! McIntyre hasn't paid his £10, so nothing looks likely to happen re his Data Protection Act email.

Anyway requests have been of three types - observational data, paleo data and who made IPCC changes and why. Keith has got all the latter - and there have been at least 4. We made Susan aware of these - all came from David Holland. According to the FOI Commissioner's Office, IPCC is an international organization, so is above any national FOI. Even if UEA holds anything about IPCC, we are not obliged to pass it on, unless it has anything to do with our core business - and it doesn't! I'm sounding like Sir Humphrey here!

McIntyre often gets others to do the requesting, but requests and responses all get posted up on CA regardless of who sends them.

On observational data, there have been at least 5 including a couple from McIntyre. Others here came from Eschenbach and also Douglas Keenan. The latter relate to Wei-Chyung Wang, and despite his being exonerated by SUNY, Keenan has not changed his web site since being told the result by SUNY!
[1]<http://www.informath.org/>

The paleo data requests have all been to Keith, and here Tim and Keith reply. The recent couple have come from McIntyre but there have been at least two others from Holland.

So since Feb 2007, CRU is in double figures. We never get any thanks for putting things up - only abuse and threats. The latest lot is up in the last 3-4 threads on CA.

I got this email over the weekend - see end of this email. This relates to what Tim sent back late last week. There was another one as well - a chatty one saying why didn't I respond to keep these people on CA quiet. I've ignored both.

Finally, I know that DEFRA receive Parliamentary Questions from MPs to answer. One of these 2 months ago was from a Tory MP asking how much money DEFRA has given to CRU over the last 5 years. DEFRA replied that they don't give money - they award grants based on open competition. DEFRA's system also told them there were no awards to CRU, as when we do get something it is

down as UEA!

I've occasionally checked DEFRA responses to FOI requests - all from Holland.

Cheers

Phil

Dear Mr Jones

What are you frightened of?

Is it that suddenly mugs like me who pay our taxes suddenly realise we are paying your wages.

Please respond to Climate Audit's valid queries otherwise I will contact my MP. Please see below.

Quote From CA

As it happens, I have experience in mining exploration programs and I can assure Phil Jones that, contrary to this experience enabling me to "understand why some samples are excluded", it gives me exactly the opposite perspective. It makes it virtually impossible for me to think up valid explanations for "excluding" some samples. It's illegal in the businesses that I know.

Anyhow, CRU answered as follows:

We have checked our files and no manuals, computer code, documents or correspondence are available. We can confirm, however, that we did not use a different Omoloyla data set and therefore there is no further data to provide.

Your behaviour is absolutely outrageous.

Best regards

Stuart Harmon

At 01:48 09/12/2008, you wrote:

Dear Phil,

I had a quick question for you: What is the total number of FOIA requests that you've received from Steven McIntyre?

With best regards,

Ben

Benjamin D. Santer

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Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. <http://www.informath.org/>

From: Tom Wigley <wigley@ucar.edu>
To: santer1@llnl.gov
Subject: Re: FOIA request
Date: Tue, 16 Dec 2008 18:01:07 -0700
Cc: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>, "David C. Bader" <bader2@llnl.gov>, Bill Goldstein <goldstein3@llnl.gov>, Tomas Diaz De La Rubia <delarubia@llnl.gov>, Hal Graboske <graboskel@llnl.gov>, Cherry Murray <murray38@llnl.gov>, mann <mann@psu.edu>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Bill Fulkerson <wfulk@utk.edu>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>, Luca Delle Monache <ldm@llnl.gov>, "Hack, James J." <jhack@ornl.gov>, Thomas C Peterson <Thomas.C.Peterson@noaa.gov>, vladeckd@law.georgetown.edu, miller21@llnl.gov, Michael Wehner <mfwehner@lbl.gov>, "Bamzai, Anjuli" <Anjuli.Bamzai@science.doe.gov>

<x-flowed>

Dear Ben,

This is a good idea. However, will you give only tropical (20N-20S) results? I urge you to give data for other zones as well, viz, SH, NH, GL, 0-20N, 20-60N, 60-90N, 0-20S, 20-60S, 60-90S (plus 20N-20S). To have these numbers on line would be of great benefit to the community. In other words, although prompted by McIntyre's request, you will actually be giving something to everyone.

Also, if you can give N3.4 SSTs and SOI data, this would be an additional huge boon to the community.

For the data, what period will you cover. Although for our paper we only use data from 1979 onwards, to give data for the full 20th century runs would be of great benefit to all. This, of course, raises the issue of drift. Even over 1979 to 1999 some models show appreciable drift. From memory we did not account for this in our paper -- but it is an important issue.

This is a lot of work -- but the benefits to the community would be truly immense.

Finally, I think you need to formally get McIntyre to list the 47 models that he wants the data for. The current request is ambiguous -- or, at least, ill defined. I think it is crucial for McIntyre to state specifically what he wants. Even if we think we know what he wants, this is not good

enough -- FOIA requests must be clear, complete and unambiguous. This, after all, is a legal issue, and no court of law would accept anything less.

Tom.

+++++

Ben Santer wrote:

> Dear co-authors,
>

> I just wanted to alert you to the fact that Steven McIntyre has now made

> a request to U.S. DOE Headquarters under the Freedom of Information Act
> (FOIA). McIntyre asked for "Monthly average T2LT values for the 47
> climate models (sic) as used to test the H1 hypothesis in Santer et
al.,

> Consistency of modelled and observed temperature trends in the tropical
> troposphere". I was made aware of the FOIA request earlier this
morning.

>

> McIntyre's request eventually reached the U.S. DOE National Nuclear
> Security Administration (NNSA), Livermore Site Office. The requested
> records are to be provided to the "FOIA Point of Contact" (presumably
at

> NNSA) by Dec. 22, 2008.

>

> McIntyre's request is poorly-formulated and misleading. As noted in the
> Santer et al. paper cited by McIntyre, we examined "a set of 49
> simulations of twentieth century climate change performed with 19
> different models". McIntyre confuses the number of 20th century
> realizations analyzed in our paper (49, not 47!) with the number of
> climate models used to generate those realizations (19). This very
basic

> mistake does not inspire one with confidence about McIntyre's
> understanding of climate models, or his ability to undertake meaningful
> analysis of climate model results.

>

> Over the past several weeks, I've had a number of discussions about the
> "FOIA issue" with PCMDI's Director (Dave Bader), with other LLNL
> colleagues, and with colleagues outside of the Lab. Based on these
> discussions, I have decided to "publish" all of the climate model
> surface temperature time series and synthetic MSU time series (for the
> tropical lower troposphere [T2LT] and the tropical mid- to
> upper-troposphere [T2]) that we used in our International Journal of
> Climatology (IJoC) paper. This will involve putting these datasets
> through an internal "Review and Release" procedure, and then placing
the

> datasets on PCMDI's publicly-accessible website. The website will also
> provide information on how synthetic Microwave Sounding Unit (MSU)
> temperatures were calculated, anomaly definition, analysis periods,
etc.

>

> After publication of the model data, we will inform the "FOIA Point of

> Contact" that the information requested by McIntyre is publicly
> available for bona fide scientific research.
>
> Unfortunately, we cannot guard against intentional or unintentional
> misuse of these datasets by McIntyre or others.
>
> By publishing the T2, T2LT, and surface temperature data, we will be
> providing far more than the "Monthly average T2LT values" mentioned in
> McIntyre's FOIA request to DOE. This will make it difficult for
McIntyre
> to continue making the bogus claim that he is being denied access to the
the
> climate model data necessary to evaluate the validity of our findings.
> All of the raw model output used in our IJoC paper are already
available
> to Mr. McIntyre (as I informed him several months ago), as are the
> algorithms required to calculate synthetic MSU temperatures from raw
> model temperature data.
>
> I hope that "publication" of the synthetic MSU temperatures resolves
> this matter to the satisfaction of NNSA, DOE Headquarters, and LLNL.
>

> With best regards,

> Ben

> -----

>
> Benjamin D. Santer
> Program for Climate Model Diagnosis and Intercomparison
> Lawrence Livermore National Laboratory
> P.O. Box 808, Mail Stop L-103
> Livermore, CA 94550, U.S.A.
> Tel: (925) 422-3840
> FAX: (925) 422-7675
> email: santer1@llnl.gov

> -----

>

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Allan Astrup Jensen" <aaj@force.dk>, "Stefan Reimann" <Stefan.Reimann@empa.ch>
Subject: RE: WP8 added text and additional person from CMA
Date: Fri Dec 19 13:53:15 2008
Cc: "lu xiaoxia" <luxx@urban.pku.edu.cn> "Brian Reid" <b.reid@uea.ac.uk>, <p.burton@uea.ac.uk>

Allan,
I was leaving that for Brian Reid or Paul Burton here.

Cheers

Phil

At 13:32 19/12/2008, Allan Astrup Jensen wrote:

Fine, do you know how status is with WP14?
Allan Astrup Jensen
Technical Vice President
Secretariat for Quality Management and Metrology
FORCE Technology, Brøndby
Park Allé 345
2605 Brøndby
Denmark
Phone: +45 43 26 70 00
Direct: +45 43 26 70 81
Mobile: +45 40 94 10 22
Fax: +45 43 26 70 11
e-mail: aaj@force.dk <[1]mailto:aaj@force.dk>

www: [2]www.forcetechnology.com <[3]http://www.forcetechnology.com/>

This email and any files transmitted with it may contain confidential information intended for the addressee(s) only. The information is not to be surrendered or copied to unauthorised persons. If you have received this communication in error, please notify us immediately by email at: info@forcetechnology.com

-----Original Message-----

From: Phil Jones [[4]mailto:p.jones@uea.ac.uk]
Sent: 19. december 2008 14:29
To: Allan Astrup Jensen; Stefan Reimann
Cc: lu xiaoxia
Subject: RE: WP8 added text and additional person from CMA

Stefan,

Can you contact your person, as they are more senior to mine?

I'll make modifications to WP8 and get it back to Allan.

Cheers

Phil

At 13:12 19/12/2008, Allan Astrup Jensen wrote:

>First you should contact them and hear if they
>would be interested, they may be occupied by
>another proposal. If they are ready, they should
>send me urgently their ½ pages descriptions of
>each and CMA, their PIC no., email and salary.
>May be Peking University know them. We add them then as partner no. 21.

>
>Yours truly,

>
>Allan Astrup Jensen

>
>Technical Vice President
>Secretariat for Quality Management and Metrology

>
>FORCE Technology, Brøndby

>Park Allé 345

>2605 Brøndby

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>
>Phone: +45 43 26 70 00

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>e-mail: aaj@force.dk <[5]mailto:aaj@force.dk>

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>
>*****

>This email and any files transmitted with it may contain confidential
>information intended for the addressee(s) only. The information is not to be
>surrendered or copied to unauthorised persons. If you have received
>this communication in error, please notify us
>immediately by email at: info@forcetechnology.com

>*****
>

>-----Original Message-----

>From: Stefan Reimann [[8]mailto:Stefan.Reimann@empa.ch]

>Sent: 19. december 2008 13:51

>To: Allan Astrup Jensen; Phil Jones

>Subject: WP8 added text and additional person from CMA

>

>Dear Allan, Phil and Bill,
>I have added some text concerning greenhouse gas and air pollution monitoring.
>
>I hope that this is precise enough.
>
>I also have an extremely good contact in CMA. Prof. Lingxi Zhou,
>CMA, CAWAS (Center for Atmosphere Watch and Services)
>Further,
>she has been newly elected into the bureau of
>the task force in National greenhouse gas inventories of IPCC
>
>[9]<http://www.ipcc-nggip.iges.or.jp/org/overview.html>
>
>I suggest that we have Phils and our contact
>from CMA included (Zhongwei Yan and Lingxi
>Zhou). Can you please tell me if this is ok?
>
>Stefan
>
>Stefan Reimann
>Empa - Materials Science & Technology
>Ueberlandstr.129
>8600 Duebendorf
>Switzerland
>Tel:0041 (0)44823 46 38
>Fax:0041 (0)44821 62 44
>e-mail stefan.reimann@empa.ch
>[10]http://www.empa.ch/climate_gases
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References

1. <mailto:aaj@force.dk>
2. <http://www.forcetechnology.com/>
3. <http://www.forcetechnology.com/>
4. <mailto:p.jones@uea.ac.uk>
5. <mailto:aaj@force.dk>
6. <http://www.forcetechnology.com/>
7. <http://www.forcetechnology.com/>
8. <mailto:Stefan.Reimann@empa.ch>
9. <http://www.ipcc-nggip.iges.or.jp/org/overview.html>
10. http://www.empa.ch/climate_gases

From: Ben Santer <santer1@llnl.gov>
To: lbutler@ucar.edu
Subject: Re: averaging
Date: Tue, 23 Dec 2008 12:08:14 -0800
Reply-to: santer1@llnl.gov
Cc: Tom Wigley <wigley@cgd.ucar.edu>, kevin trenberth <trenbert@ucar.edu>

<x-flowed>

Dear Lisa,

That's great news! I've confirmed with DOE that I can use up to \$10,000 of my DOE Fellowship to provide financial support for Tom's Symposium. I will check with Anjuli Bamzai at DOE to determine whether there are any strings attached to this money. I'm hopeful that we'll be able to use the DOE money for the Symposium dinner, and to defray some of the travel expenses of international participants who can't come up with their own travel money. I'll try to resolve this question in the next few days.

Best wishes to you and your family for a very Merry Christmas, and a happy, healthy, and peaceful 2009!

Ben

Lisa Butler wrote:

- > Hi Ben,
- > Sorry for the slow reply -- I had to check on a few things, but yes, now
- > I can agree that June 19th seems like a good bet for our Wigley
- > Symposium. CCSM in Breckenridge will adjourn sometime on Thursday
- > afternoon, 6/18.
- >
- > For June 19 I reserved the Main Seminar Room at the Mesa from 8:00 AM -
- > 5:30 PM and the Damon Room (for a reception) from 5:30 PM to 8:00 PM. Of
- > course we can tweak these times as we get closer if need be.
- >
- > After the holidays I work up a rough draft budget for the catering and
- > see what, if any, financial help we might be able to get from CGD
- > and/or NCAR Directorate.
- >
- > Best wishes for a Merry Christmas and Happy New Year!
- > Lisa
- >
- > Ben Santer wrote:

>> Dear Tom,

>>

>> I think we agreed that your symposium would be after the 2009 CCSM
>> Workshop in Breckenridge, which will take place during the week of
>> June 15th. I do not yet have the exact dates of the CCSM meeting - I
>> don't know whether it ends on Thursday, June 18th. I suspect it will.
>> In the past, CCSM Workshops have generally started on a Tuesday and
>> ended on a Thursday. So my guess is that Friday, June 19th would
>> probably be our best bet for your symposium. CCSM Workshops are
>> usually preceded by a Monday meeting of the CCSM Scientific Steering
>> Committee, CCSM Working Group Co-Chairs, and CCSM Advisory Board. As a
>> Co-Chair of the Climate Change Working Group, I would be involved in
>> this Monday meeting.

>>

>> I'm copying Lisa on this email, in order to check whether Friday, June
>> 19th is a good date for the symposium.

>>

>> Cheers,

>>

>> Ben

>> Tom Wigley wrote:

>>> Ben,

>>>

>>> Did you get my email about papers on averaging of
>>> model results? Do you want me to email the papers?

>>>

>>> Is there a date for my symposium? Have you invited
>>> anyone? Shall I make a priority list? This would/could
>>> be based on ...

>>>

>>> (1) A balance of sub-disciplines so as to have the
>>> potential to produce a useful book

>>>

>>> (2) Importance of topics, perhaps determined via
>>> citations of related papers by the invited participants

>>>

>>> (3) Closeness to me personally

>>>

>>> (4) Numbers of jointly authored papers

>>>

>>> -----

>>>

>>> So, e.g., there would have to be presentations by you

>>> and Phil. Also (as a close friend) Tim -- on paleoclimate
>>> in general I guess rather than just isotopes in speleothems.
>>> He could easily slot in some cool caving stuff.
>>>
>>> Jerry Meehl on AOGCMs. Malte and/or Sarah on UD EBMs.
>>> (But how to get some SCENGEN in? ... as this is almost
>>> totally my work.)
>>>
>>> Rob Wilby on downscaling.
>>>
>>> Niel Plummer would be nice to invite, but I'm not sure
>>> how he would fit in subject wise.
>>>
>>> Peter Foukal (or Claus Frohlich) on the Sun -- altho I've not
>>> worked much with them, this is an important subject area.
>>>
>>> Caspar on volcanoes.
>>>
>>> Also, Jean Palutikof on impacts and adaptation (her new Oz
>>> job is focussed on adaaptation).
>>>
>>> I'm just thinking out loud here. Might be good to talk about
>>> this soon.
>>>
>>> -----
>>>
>>> But in the meantime -- what is the proposed date?
>>>
>>>
>>
>>
>

--

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FAX: (925) 422-7675

email: santer1@llnl.gov

</x-flowed>

From: Tim Johns <tim.johns@metoffice.gov.uk>
To: "Folland, Chris" <chris.folland@metoffice.gov.uk>
Subject: Re: FW: Temperatures in 2009
Date: Mon, 05 Jan 2009 09:34:49 +0000
Cc: "Smith, Doug" <doug.smith@metoffice.gov.uk>, p.jones@uea.ac.uk, Tim Johns <tim.johns@metoffice.gov.uk>

Dear Chris, cc: Doug

Mike McCracken makes a fair point. I am no expert on the observational uncertainties in tropospheric SO2 emissions over the recent past, but it is certainly the case that the SRES A1B scenario (for instance) as seen by different integrated assessment models shows a range of possibilities. In fact this has been an issue for us in the ENSEMBLES project, since we have been running models with a new mitigation/stabilization scenario "E1" (that has large emissions reductions relative to an A1B baseline, generated using the IMAGE IAM) and comparing it with A1B (the AR4 marker version, generated by a different IAM). The latter has a possibly unrealistic secondary SO2 emissions peak in the early 21st C - not present in the IMAGE E1 scenario, which has a steady decline in SO2 emissions from 2000. The A1B scenario as generated with IMAGE also show a decline rather than the secondary emissions peak, but I can't say for sure which is most likely to be "realistic".

The impact of the two alternative SO2 emissions trajectories is quite marked though in terms of global temperature response in the first few decades of the 21st C (at least in our HadGEM2-AO simulations, reflecting actual aerosol forcings in that model plus some divergence in GHG forcing). Ironically, the E1-IMAGE scenario runs, although much cooler in the long term of course, are considerably warmer than A1B-AR4 for several decades! Also - relevant to your statement - A1B-AR4 runs show potential for a distinct lack of warming in the early 21st C, which I'm sure skeptics would love to see replicated in the real world... (See the attached plot for illustration but please don't circulate this any further as these are results in progress, not yet shared with other ENSEMBLES partners let alone published). We think the different short term warming responses are largely attributable to the different SO2 emissions trajectories.

So far we've run two realisations of both the E1-IMAGE and A1B-AR4 scenarios with HadGEM2-AO, and other partners in ENSEMBLES are doing similar runs using other GCMs. Results will start to be analysed in a multi-model way in the next few months. CMIP5 (AR5) prescribes similar kinds of experiments, but the implementation details might well be different from ENSEMBLES experiments wrt scenarios and their SO2 emissions trajectories (I haven't studied the CMIP5 experiment fine print to that extent).

Cheers,
Tim

On Sat, 2009-01-03 at 21:31 +0000, Folland, Chris wrote:
> Tim and Doug

>
> Please see McCrackens email.
>
> We are now using the average of 4 AR4 scenarios you gave us for GHG + aerosol. What is the situation likely to be for AR5 forcing, particularly anthropogenic aerosols. Are there any new estimates yet? Particularly, will there be a revision in time for the 2010 forecast? We do in the meantime have an explanation for the interannual variability of the last decade. However this fits well only when an underlying net GHG+aerosol warming of 0.15C per decade is fitted in the statistical models. In a sense the methods we use would automatically fit to a reduced net warming rate so Mike McCracken can be told that. In other words the method creates it own transient climate sensitivity for recent warming. But the forcing rate underlying the method nevertheless perhaps sits a bit uncomfortably with the absolute forcing figures we are using from AR4. However having said this, interestingly, the statistics and DePreSys are in remarkable harmony about the temperature of 2009.
>
> Any guidance welcome
>
> Chris
>
>
> Prof. Chris Folland
> Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)
>
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> <<http://www.metoffice.gov.uk>>
> Fellow of the Met Office
> Hon. Professor of School of Environmental Sciences, University of East Anglia
>
>
>
>
> -----Original Message-----
> From: Mike MacCracken [<mailto:mmaccrac@comcast.net>]
> Sent: 03 January 2009 16:44
> To: Phil Jones; Folland, Chris
> Cc: John Holdren; Rosina Bierbaum
> Subject: Temperatures in 2009
>
> Dear Phil and Chris--
>
> Your prediction for 2009 is very interesting (see note below for notice that went around to email list for a lot of US Congressional staff)--and I would expect the analysis you have done is correct. But, I have one nagging question, and that is how much SO2/sulfate is being generated by the rising emissions from China and India (I know that at least some

plants are using desulfurization--but that antidotes are not an inventory). I worry that what the western nations did in the mid 20th century is going to be what the eastern nations do in the next few decades--go to tall stacks so that, for the near-term, "dilution is the solution to pollution". While I understand there are efforts to get much better inventories of CO2 emissions from these nations, when I asked a US EPA representative if their efforts were going to also inventory SO2 emissions (amount and height of emission), I was told they were not. So, it seems, the scientific uncertainty generated by not having good data from the mid-20th century is going to be repeated in the early 21st century (satellites may help on optical depth, but it would really help to know what is being emitted).

>

> That there is a large potential for a cooling influence is sort of evident in the IPCC figure about the present sulfate distribution--most is right over China, for example, suggesting that the emissions are near the surface--something also that is, so to speak, 'clear' from the very poor visibility and air quality in China and India. So, the quick, fast, cheap fix is to put the SO2 out through tall stacks. The cooling potential also seems quite large as the plume would go out over the ocean with its low albedo--and right where a lot of water vapor is evaporated, so maybe one pulls down the water vapor feedback a little and this amplifies the sulfate cooling influence.

>

> Now, I am not at all sure that having more tropospheric sulfate would be a bad idea as it would limit warming--I even have started suggesting that the least expensive and quickest geoengineering approach to limit global warming would be to enhance the sulfate loading--or at the very least we need to maintain the current sulfate cooling offset while we reduce CO2 emissions (and presumably therefore, SO2 emissions, unless we manage things) or we will get an extra bump of warming. Sure, a bit more acid deposition, but it is not harmful over the ocean (so we only/mainly emit for trajectories heading out over the ocean) and the impacts of deposition may well be less that for global warming (will be a tough comparison, but likely worth looking at). Indeed, rather than go to stratospheric sulfate injections, I am leaning toward tropospheric, but only during periods when trajectories are heading over ocean and material won't get rained out for 10 days or so.

> Would be an interesting issue to do research on--see what could be done.

>

> In any case, if the sulfate hypothesis is right, then your prediction of warming might end up being wrong. I think we have been too readily explaining the slow changes over past decade as a result of variability--that explanation is wearing thin. I would just suggest, as a backup to your prediction, that you also do some checking on the sulfate issue, just so you might have a quantified explanation in case the prediction is wrong. Otherwise, the Skeptics will be all over us--the world is really cooling, the models are no good, etc. And all this just as the US is about ready to get serious on the issue.

>

> We all, and you all in particular, need to be prepared.

>

> Best, Mike MacCracken

>
>
> Researchers Say 2009 to Be One of Warmest Years on Record
>
> On December 30, climate scientists from the UK Met Office and the University of East Anglia projected 2009 will be one of the top five warmest years on record. Average global temperatures for 2009 are predicted to be 0.4°C above the 1961-1990 average of 14 °C. A multiyear forecast using a Met Office climate model indicates a rapid return of global temperature to the long-term warming trend, with an increasing probability of record temperatures after 2009. The fact that 2009, like 2008, will not break records does not mean that global warming has gone away What matters is the underlying rate of warming, said Dr. Phil Jones, the director of climate research at the University of East Anglia. The presence of La Nina during the last year partially masked this underlying rate. Phenomena such as El Nino and La Nina have a significant influence on global surface temperature, said Dr. Chris Folland of the Met Office Hadley Center.
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>
> For additional information see:
> <http://uk.reuters.com/article/topNews/idUKTRE4BT49920081230>
> <http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-set-to-be-colder-than-in-Iceland.html>
> <http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>
> <http://www.metoffice.gov.uk/corporate/pressoffice/2008/pr20081230.html>
>
>
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Attachment Converted: "c:\eudora\attach\wave.gif"

From: Phil Jones <p.jones@uea.ac.uk>
To: Tim Johns <tim.johns@metoffice.gov.uk>, "Folland, Chris" <chris.folland@metoffice.gov.uk>
Subject: Re: FW: Temperatures in 2009
Date: Mon Jan 5 16:18:24 2009
Cc: "Smith, Doug" <doug.smith@metoffice.gov.uk>, Tim Johns <tim.johns@metoffice.gov.uk>

Tim, Chris,

I hope you're not right about the lack of warming lasting till about 2020. I'd rather hoped to see the earlier Met Office press release with Doug's paper that said something like - half the years to 2014 would exceed the warmest year currently on record, 1998!

Still a way to go before 2014.

I seem to be getting an email a week from skeptics saying where's the warming gone. I know the warming is on the decadal scale, but it would be nice to wear their smug grins away.

Chris - I presume the Met Office continually monitor the weather forecasts. Maybe because I'm in my 50s, but the language used in the forecasts seems a bit over the top re the cold. Where I've been for the last 20 days (in Norfolk) it doesn't seem to have been as cold as the forecasts.

I've just submitted a paper on the UHI for London - it is 1.6 deg C for the LWC. It comes out to 2.6 deg C for night-time minimums. The BBC forecasts has the countryside 5-6 deg C cooler than city centres on recent nights. The paper shows the UHI hasn't got any worse since 1901 (based on St James Park and Rothamsted).

Cheers

Phil

At 09:34 05/01/2009, Tim Johns wrote:

Dear Chris, cc: Doug

Mike McCracken makes a fair point. I am no expert on the observational uncertainties in tropospheric SO₂ emissions over the recent past, but it is certainly the case that the SRES A1B scenario (for instance) as seen by different integrated assessment models shows a range of possibilities. In fact this has been an issue for us in the ENSEMBLES project, since we have been running models with a new mitigation/stabilization scenario "E1" (that has large emissions reductions relative to an A1B baseline, generated using the IMAGE IAM) and comparing it with A1B (the AR4 marker version, generated by a different IAM). The latter has a possibly unrealistic secondary SO₂ emissions peak in the early 21st C - not present in the IMAGE E1 scenario, which has a steady decline in SO₂ emissions from 2000. The A1B

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>

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>

>

> Prof. Chris Folland

> Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

>

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> <[1]<http://www.metoffice.gov.uk>>

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> Hon. Professor of School of Environmental Sciences, University of East Anglia

>

>

>

>

> -----Original Message-----

> From: Mike MacCracken [[2]<mailto:mmaccrac@comcast.net>]

> Sent: 03 January 2009 16:44

> To: Phil Jones; Folland, Chris

> Cc: John Holdren; Rosina Bierbaum

> Subject: Temperatures in 2009

>

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> ³Further warming to record levels is likely once a moderate El Nino develops.² The transition from a La Nina effect to an El Nino one is expected late next year.

>

> For additional information see:

> [3]<http://uk.reuters.com/article/topNews/idUKTRE4BT49920081230>

> [4]<http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-set-to-be-c-older-than-in-Iceland.html>

> [5]<http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>

> [6]<http://www.metoffice.gov.uk/corporate/pressoffice/2008/pr20081230.html>

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>

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Prof. Phil Jones

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References

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2. <mailto:mmaccrac@comcast.net>
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4. <http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-set-to-be-c>
5. <http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>
6. <http://www.metoffice.gov.uk/corporate/pressoffice/2008/pr20081230.html>
7. <http://www.metoffice.gov.uk/>
8. <http://www.metoffice.gov.uk/research/hadleycentre/google/>

From: "Folland, Chris" <chris.folland@metoffice.gov.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: RE: FW: Temperatures in 2009
Date: Tue, 6 Jan 2009 10:04:57 -0000

Phil

Maybe in your conclusions you should comment on the fact that some more general studies show relationships between the population or size of cities and the urban effect. This seems not to be true here. Is there any evidence from other studies of a "saturation effect" on urban warming in some cases? And why this might be so?

Chris

Prof. Chris Folland
Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

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Fellow of the Met Office

Hon. Professor of School of Environmental Sciences, University of East Anglia

-----Original Message-----

From: Phil Jones [mailto:p.jones@uea.ac.uk]

Sent: 05 January 2009 17:02

To: Folland, Chris

Subject: RE: FW: Temperatures in 2009

Chris,

Will look at later. Here is the UHI paper I submitted today to Weather.

Didn't take long to do. I started doing it as people kept on saying the UHI

in London (and this is only Central London) was getting worse. I couldn't

see it and Rothamsted and Wisley confirmed what I'd thought.

Any comments appreciated. Remember it is just Weather, and I tried to make it quite simple ! David did see it last month.

Cheers

Phil

At 16:46 05/01/2009, you wrote:

>Phil

>

>Strictly very much in confidence, this was submitted to Nature

>Geosciences just before Xmas after discussion with them.

>

>Night-time temperatures seem to have been rather underestimated here as
>well since the cold spell started. Daytime forecasts have been better,
>allowing for 1000 feet of elevation. Real cold would shock all under 30!

>

>Chris

>

>

>Prof. Chris Folland

>Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

>

>Met Office Hadley Centre, Fitzroy Rd, Exeter, Devon EX1 3PB United

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>Tel: +44 (0)1647 432978

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>

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>

>

>-----Original Message-----

>From: Phil Jones [<mailto:p.jones@uea.ac.uk>]

>Sent: 05 January 2009 16:18

>To: Johns, Tim; Folland, Chris

>Cc: Smith, Doug; Johns, Tim

>Subject: Re: FW: Temperatures in 2009

>

>

> Tim, Chris,

> I hope you're not right about the lack of warming lasting
> till about 2020. I'd rather hoped to see the earlier Met Office
> press release with Doug's paper that said something like -
> half the years to 2014 would exceed the warmest year currently on
> record, 1998!

> Still a way to go before 2014.

>

> I seem to be getting an email a week from skeptics saying
> where's the warming gone. I know the warming is on the decadal
> scale, but it would be nice to wear their smug grins away.

>

> Chris - I presume the Met Office

> continually monitor the weather forecasts.

> Maybe because I'm in my 50s, but the language used in the forecasts
seems

> a bit over the top re the cold. Where I've been for the last 20
> days (in Norfolk)
> it doesn't seem to have been as cold as the forecasts.
>
> I've just submitted a paper on the UHI for London - it is 1.6 deg
> C for the LWC.
> It comes out to 2.6 deg C for night-time minimums. The BBC forecasts
has
> the countryside 5-6 deg C cooler than city centres on recent nights.
> The paper
> shows the UHI hasn't got any worse since 1901 (based on St James Park
> and Rothamsted).
>
> Cheers
> Phil
>
>
>

>At 09:34 05/01/2009, Tim Johns wrote:

> >Dear Chris, cc: Doug
> >
> >Mike McCracken makes a fair point. I am no expert on the
> >observational uncertainties in tropospheric SO2 emissions over the
> >recent past, but it is certainly the case that the SRES A1B scenario
> >(for instance) as seen by different integrated assessment models
> >shows a range of possibilities. In fact this has been an issue for us
> >in the ENSEMBLES project, since we have been running models with a
> >new mitigation/stabilization scenario "E1" (that has large emissions
> >reductions relative to an A1B baseline, generated using the IMAGE
> >IAM) and comparing it with A1B (the AR4 marker version, generated by
> >a different IAM). The latter has a possibly unrealistic secondary SO2
> >emissions peak in the early 21st C - not present in the IMAGE E1
> >scenario, which has a steady decline in SO2 emissions from 2000. The
> >A1B scenario as generated with IMAGE also show a decline rather than
> >the secondary emissions peak, but I can't say for sure which is most
> >likely to be "realistic".
> >

> >The impact of the two alternative SO2 emissions trajectories is quite
> >marked though in terms of global temperature response in the first
> >few decades of the 21st C (at least in our HadGEM2-AO simulations,
> >reflecting actual aerosol forcings in that model plus some divergence
> >in GHG forcing). Ironically, the E1-IMAGE scenario runs, although
> >much cooler in the long term of course, are considerably warmer than
> >A1B-AR4 for several decades! Also - relevant to your statement -
> >A1B-AR4 runs show potential for a distinct lack of warming in the
> >early 21st C, which I'm sure skeptics would love to see replicated in
> >the real world... (See the attached plot for illustration but please
> >don't circulate this any further as these are results in progress,
> >not yet shared with other ENSEMBLES partners let alone published). We
> >think the different short term warming responses are largely
> >attributable to the different SO2 emissions trajectories.
> >

> >So far we've run two realisations of both the E1-IMAGE and A1B-AR4
> >scenarios with HadGEM2-AO, and other partners in ENSEMBLES are doing

> >similar runs using other GCMs. Results will start to be analysed in a
> >multi-model way in the next few months. CMIP5 (AR5) prescribes
> >similar kinds of experiments, but the implementation details might
> >well be different from ENSEMBLES experiments wrt scenarios and their
> >SO2 emissions trajectories (I haven't studied the CMIP5 experiment
> >fine print to that extent).
> >
> >Cheers,
> >Tim
> >
> >On Sat, 2009-01-03 at 21:31 +0000, Folland, Chris wrote:
> > > Tim and Doug
> > >
> > > Please see McCrackens email.
> > >
> > > We are now using the average of 4 AR4
> > scenarios you gave us for GHG + aerosol. What is the situation
> > likely to be for AR5 forcing, particularly anthropogenic aerosols.
> > Are there any new estimates yet? Particularly, will there be a
> > revision in time for the 2010 forecast? We do in the meantime have
> > an explanation for the interannual variability of the last decade.
> > However this fits well only when an underlying net GHG+aerosol
> > warming of 0.15C per decade is fitted in the statistical models. In
> > a sense the methods we use would automatically fit to a reduced net
> > warming rate so Mike McCracken can be told that. In other words the
> > method creates it own transient climate sensitivity for recent
> > warming. But the forcing rate underlying the method nevertheless
> > perhaps sits a bit uncomfortably with the absolute forcing figures we
are using from AR4.
> > However having said this, interestingly, the statistics and DePreSys
> > are in remarkable harmony about the temperature of 2009.
> > >
> > > Any guidance welcome
> > >
> > > Chris
> > >
> > >
> > > Prof. Chris Folland
> > > Research Fellow, Seasonal to Decadal Forecasting (from 2 June
> > > 2008)
> > >
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> > Sciences, University of East Anglia
> > >
> > >
> > >
> > >
> > >

> > > -----Original Message-----

> > > From: Mike MacCracken [mailto:mmaccrac@comcast.net]

> > > Sent: 03 January 2009 16:44

> > > To: Phil Jones; Folland, Chris

> > > Cc: John Holdren; Rosina Bierbaum

> > > Subject: Temperatures in 2009

> > >

> > > Dear Phil and Chris--

> > >

> > > Your prediction for 2009 is very interesting

> > (see note below for notice that went around to email list for a lot

> > of US Congressional staff)--and I would expect the analysis you have

> > done is correct. But, I have one nagging question, and that is how

> > much SO₂/sulfate is being generated by the rising emissions from

> > China and India (I know that at least some plants are using

> > desulfurization--but that antidotes are not an inventory). I worry

> > that what the western nations did in the mid 20th century is going

> > to be what the eastern nations do in the next few decades--go to

> > tall stacks so that, for the near-term, "dilution is the solution to

> > pollution". While I understand there are efforts to get much better

> > inventories of CO₂ emissions from these nations, when I asked a US

> > EPA representative if their efforts were going to also inventory

> > SO₂ emissions (amount and height of emission), I was told they were

> > not. So, it seems, the scientific uncertainty generated by not

> > having good data from the mid-20th century is going to be repeated

> > in the early 21st century (satellites may help on optical depth, but

> > it would really help to know what is being emitted).

> > >

> > > That there is a large potential for a cooling

> > influence is sort of evident in the IPCC figure about the present

> > sulfate distribution--most is right over China, for example,

> > suggesting that the emissions are near the surface--something also

> > that is, so to speak, 'clear' from the very poor visibility and air

> > quality in China and India. So, the quick, fast, cheap fix is to put

> > the SO₂ out through tall stacks. The cooling potential also seems

> > quite large as the plume would go out over the ocean with its low

> > albedo--and right where a lot of water vapor is evaporated, so maybe

> > one pulls down the water vapor feedback a little and this amplifies

> > the sulfate cooling influence.

> > >

> > > Now, I am not at all sure that having more

> > tropospheric sulfate would be a bad idea as it would limit

> > warming--I even have started suggesting that the least expensive and

> > quickest geoengineering approach to limit global warming would be to

> > enhance the sulfate loading--or at the very least we need to

> > maintain the current sulfate cooling offset while we reduce CO₂

> > emissions (and presumably therefore, SO₂ emissions, unless we manage

> > things) or we will get an extra bump of warming. Sure, a bit more

> > acid deposition, but it is not harmful over the ocean (so we

> > only/mainly emit for trajectories heading out over the ocean) and

> > the impacts of deposition may well be less that for global warming

> > (will be a tough comparison, but likely worth looking at). Indeed,

> > rather than go to stratospheric sulfate injections, I am leaning

> > toward tropospheric, but only during periods when trajectories are

> > heading over ocean and material won't get rained out for 10 days or so.
> > > Would be an interesting issue to do research on--see what could be done.
> > >
> > > In any case, if the sulfate hypothesis is
> > right, then your prediction of warming might end up being wrong. I
> > think we have been too readily explaining the slow changes over past
> > decade as a result of variability--that explanation is wearing thin.
> > I would just suggest, as a backup to your prediction, that you also
> > do some checking on the sulfate issue, just so you might have a
> > quantified explanation in case the prediction is wrong. Otherwise,
> > the Skeptics will be all over us--the world is really cooling, the
> > models are no good, etc.
> > And all this just as the US is about ready to get serious on the
issue.
> > >
> > > We all, and you all in particular, need to be prepared.
> > >
> > > Best, Mike MacCracken
> > >
> > >
> > > Researchers Say 2009 to Be One of Warmest Years on Record
> > >
> > > On December 30, climate scientists from the
> > UK Met Office and the University of East Anglia projected 2009 will
> > be one of the top five warmest years on record. Average global
> > temperatures for 2009 are predicted to be 0.4°C above the 1961-1990
> > average of 14 || C. A multiyear forecast using a Met Office climate
> > model indicates a |rapid return of global temperature to the
> > long-term warming trend, with an increasing probability of record
> > temperatures after 2009. |The fact that 2009, like 2008, will not
> > break records does not mean that global warming has gone away . . .
> > . What matters is the underlying rate of warming, said Dr. Phil
> > Jones, the director of climate research at the University of East
> > Anglia. The presence of La Nina during the last year partially
masked this underlying rate.
> > |Phenomena such as El Nino and La Nina have a significant influence
> > on global surface temperature, said Dr. Chris Folland of the Met
> > Office Hadley Center.
> > > |Further warming to record levels is likely
> > once a moderate El Nino develops. The transition from a La Nina
> > effect to an El Nino one is expected late next year.
> > >
> > > For additional information see:
> > > <http://uk.reuters.com/article/topNews/idUKTRE4BT49920081230>
> > >
> > > <http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-set>
> > > -t
> > > o-be-c
> > > [older-than-in-Iceland.html](http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs)
> > > <http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>
> > > <http://www.metoffice.gov.uk/corporate/pressoffice/2008/pr20081230>.
> > > ht

> > > ml

> > >

> > >

> > --

> > Tim Johns Manager Global Coupled Modelling

> > Met Office Hadley Centre

> > FitzRoy Rd Exeter Devon EX1 3PB United Kingdom

> > Tel: +44 (0)1392 886901 Fax: +44 (0)1392 885681

> > E-mail: tim.johns@metoffice.gov.uk <http://www.metoffice.gov.uk>

> >

> > Please note I work part time, normally Monday-Tuesday

> > Thursday-Friday

> >

> > Met Office climate change predictions can now be viewed on Google

> > Earth <http://www.metoffice.gov.uk/research/hadleycentre/google/>

> >

> >

>

>Prof. Phil Jones

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From: Stephen H Schneider <shs@stanford.edu>
To: santer1@llnl.gov
Subject: Re: [Fwd: data request]
Date: Tue, 6 Jan 2009 10:50:56 -0800 (PST)
Cc: "David C. Bader" <bader2@llnl.gov>, Bill Goldstein <goldstein3@llnl.gov>, Pat Berge <bergel@llnl.gov>, Cherry Murray <murray38@llnl.gov>, George Miller <millier21@llnl.gov>, Anjuli Bamzai <Anjuli.Bamzai@science.doe.gov>, Tomas Diaz De La Rubia <delarubia@llnl.gov>, Doug Rotman <rotman1@llnl.gov>, Peter Thorne <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "Philip D. Jones" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>

"Thanks" Ben for this, hi all and happy new year. I had a similar experience--but not FOIA since we at Climatic Change are a private institution--with Stephen McIntyre demanding that I have the Mann et al cohort publish all their computer codes for papers published in Climatic Change. I put the question to the editorial board who debated it for weeks. The vast majority opinion was that scientists should give enough information on their data sources and methods so others who are scientifically capable can do their own brand of replication work, but that this does not extend to personal computer codes with all their undocumented sub routines etc. It would be odious requirement to have scientists document every line of code so outsiders could then just apply them instantly. Not only is this an intellectual property issue, but it would dramatically reduce our productivity since we are not in the business of producing software products for general consumption and have no resources to do so. The NSF, which funded the studies I published, concurred--so that ended that issue with Climatic Change at the time a few years ago.

This continuing pattern of harassment, as Ben rightly puts it in my opinion, in the name of due diligence is in my view an attempt to create a fishing expedition to find minor glitches or unexplained bits of code--which exist in nearly all our kinds of complex work--and then assert that the entire result is thus suspect. Our best way to deal with this issue of replication is to have multiple independent author teams, with their own codes and data sets, publishing independent work on the same topics--like has been done on the "hockey stick". That is how credible scientific replication should proceed.

Let the lawyers figure this out, but be sure that, like Ben is doing now, you disclose the maximum reasonable amount of information so competent scientists can do replication work, but short of publishing undocumented personalized codes etc. The end of the email Ben attached shows their intent--to discredit papers so they have no "evidentiary value in public policy"--what you resort to when you can't win the intellectual battle scientifically at IPCC or NAS.

Good luck with this, and expect more of it as we get closer to international climate policy actions, We are witnessing the "contrarian battle of the bulge" now, and expect that all weapons will be used.

Cheers, Steve

PS Please do not copy or forward this email.

Stephen H. Schneider

Melvin and Joan Lane Professor for Interdisciplinary Environmental Studies,

Professor, Department of Biology and

Senior Fellow, Woods Institute for the Environment

Mailing address:

Yang & Yamazaki Environment & Energy Building - MC 4205

473 Via Ortega

Ph: 650 725 9978

F: 650 725 4387

Websites: climatechange.net

patientfromhell.org

----- Original Message -----

From: "Ben Santer" <santer1@llnl.gov>

To: "Peter Thorne" <peter.thorne@metoffice.gov.uk>, "Leopold Haimberger" <leopold.haimberger@univie.ac.at>, "Karl Taylor" <taylor13@llnl.gov>, "Tom Wigley" <wigley@cgd.ucar.edu>, "John Lanzante" <John.Lanzante@noaa.gov>, "Susan Solomon" <ssolomon@frii.com>, "Melissa Free" <Melissa.Free@noaa.gov>, "peter gleckler" <gleckler1@llnl.gov>, "Philip D. Jones" <p.jones@uea.ac.uk>, "Thomas R Karl" <Thomas.R.Karl@noaa.gov>, "Steve Klein" <klein21@mail.llnl.gov>, "carl mears" <mears@remss.com>, "Doug Nychka" <nychka@ucar.edu>, "Gavin Schmidt" <gschmidt@giss.nasa.gov>, "Steven Sherwood" <Steven.Sherwood@yale.edu>, "Frank Wentz" <frank.wentz@remss.com>

Cc: "David C. Bader" <bader2@llnl.gov>, "Bill Goldstein" <goldstein3@llnl.gov>, "Pat Berge" <bergel@llnl.gov>, "Cherry Murray" <murray38@llnl.gov>, "George Miller" <miller21@llnl.gov>, "Anjuli Bamzai" <Anjuli.Bamzai@science.doe.gov>, "Tomas Diaz De La Rubia" <delarubia@llnl.gov>, "Doug Rotman" <rotman1@llnl.gov>

Sent: Tuesday, January 6, 2009 9:23:41 AM GMT -08:00 US/Canada Pacific

Subject: [Fwd: data request]

Dear coauthors of the Santer et al. International Journal of Climatology paper (and other interested parties),

I am forwarding an email I received this morning from a Mr. Geoff Smith.

The email concerns the climate model data used in our recently-published International Journal of Climatology (IJoC) paper. Mr. Smith has requested that I provide him with these climate model datasets. This request has been made to Dr. Anna Palmisano at DOE Headquarters and to Dr. George Miller, the Director of Lawrence Livermore National Laboratory.

I have spent the last two months of my scientific career dealing with multiple requests for these model datasets under the U.S. Freedom of Information Act (FOIA). I have been able to do little or no productive

research during this time. This is of deep concern to me.

From the beginning, my position on this matter has been clear and consistent. The primary climate model data used in our IJoC paper are part of the so-called "CMIP-3" (Coupled Model Intercomparison Project) archive at LLNL, and are freely available to any scientific researcher. The primary observational (satellite and radiosonde) datasets used in our IJoC paper are also freely available. The algorithms used for calculating "synthetic" Microwave Sounding Unit (MSU) temperatures from climate model data (to facilitate comparison with actual satellite temperatures) have been documented in several peer-reviewed publications. The bottom line is that any interested scientist has all the scientific information necessary to replicate the calculations performed in our IJoC paper, and to check whether the conclusions reached in that paper were sound.

Neither Mr. Smith nor Mr. Stephen McIntyre (Mr. McIntyre is the initiator of the FOIA requests to the U.S. DOE and NOAA, and the operator of the "ClimateAudit.com" blog) is interested in full replication of our calculations, starting from the primary climate model and observational data. Instead, they are demanding the value-added quantities we have derived from the primary datasets (i.e., the synthetic MSU temperatures).

I would like a clear ruling from DOE lawyers - ideally from both the NNSA and DOE Office of Science branches - on the legality of such data requests. They are troubling, for a number of reasons.

1. In my considered opinion, a very dangerous precedent is set if any derived quantity that we have calculated from primary data is subject to FOIA requests. At LLNL's Program for Climate Model Diagnosis and Intercomparison (PCMDI), we have devoted years of effort to the calculation of derived quantities from climate model output. These derived quantities include synthetic MSU temperatures, ocean heat content changes, and so-called "cloud simulator" products suitable for comparison with actual satellite-based estimates of cloud type, altitude, and frequency. The intellectual investment in such calculations is substantial.

2. Mr. Smith asserts that "there is no valid intellectual property justification for withholding this data". I believe this argument is incorrect. The synthetic MSU temperatures used in our IJoC paper - and the other examples of derived datasets mentioned above - are integral components of both PCMDI's ongoing research, and of proposals we have submitted to funding agencies (DOE, NOAA, and NASA). Can any competitor simply request such datasets via the U.S. FOIA, before we have completed full scientific analysis of these datasets?

3. There is a real danger that such FOIA requests could (and are already) being used as a tool for harassing scientists rather than for valid scientific discovery. Mr. McIntyre's FOIA requests to DOE and NOAA are but the latest in a series of such requests. In the past, Mr. McIntyre has targeted scientists at Penn State University, the U.K. Climatic Research Unit, and the National Climatic Data Center in

Asheville. Now he is focusing his attention on me. The common denominator is that Mr. McIntyre's attention is directed towards studies claiming to show evidence of large-scale surface warming, and/or a prominent human "fingerprint" in that warming. These serial FOIA requests interfere with our ability to do our job.

Mr. Smith's email mentions the Royal Meteorological Society's data archiving policies (the Royal Meteorological Society are the publishers of the International Journal of Climatology). Recently, Prof. Glenn McGregor (the Chief Editor of the IJoC) provided Mr. McIntyre with the following clarification:

"In response to your question about data policy my position as Chief Editor is that the above paper has been subject to strict peer review, supporting information has been provided by the authors in good faith which is accessible online (attached FYI) and the original data from which temperature trends were calculated are freely available. It is not the policy of the International Journal of Climatology to require that data sets used in analyses be made available as a condition of publication."

As many of you may know, I have decided to publicly release the synthetic MSU temperatures that were the subject of Mr. McIntyre's FOIA request (together with additional synthetic MSU temperatures which were not requested by Mr. McIntyre). These datasets have been through internal review and release procedures, and will be published shortly on PCMDI's website, together with a technical document which describes how synthetic MSU temperatures were calculated. I agreed to this publication process primarily because I want to spend the next few years of my career doing research. I have no desire to be "taken out" as scientist, and to be involved in years of litigation.

The public release of the MSU data used in our IJoC paper may or may not resolve these problems. If Mr. McIntyre's past performance is a guide to the future, further FOIA requests will follow. I would like to know that I have the full support of LLNL management and the U.S. Dept. of Energy in dealing with these unwarranted and intrusive requests.

I do not intend to reply to Mr. Smith's email.

Sincerely,

Ben Santer

Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
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From: "Folland, Chris" <chris.folland@metoffice.gov.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: RE: FW: Temperatures in 2009
Date: Tue, 6 Jan 2009 17:01:37 -0000

Phil

Thanks. Bad news today. Nature Geosciences wont publish this because the Real Climate Blog mentions (more vaguely) the basic content of what we have written. That is indeed the reason Nature Geosciences have given. It seems blogs can now prevent publication! I have suggested to Jeff we try GRL but only after raising this issue with them.

Chris

Prof. Chris Folland
Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

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Fellow of the Met Office

Hon. Professor of School of Environmental Sciences, University of East Anglia

-----Original Message-----

From: Phil Jones [<mailto:p.jones@uea.ac.uk>]

Sent: 06 January 2009 14:56

To: Folland, Chris

Subject: RE: FW: Temperatures in 2009

Chris,

City population size and urban effects are not related that well. I think

a lot depends on where the city is in relation to the sea, large rivers and water bodies as well.

I did try and get population figures for London from various times during the 20th century.

I found these, but the area of London they referred to kept changing. Getting the

areas proved more difficult, as I though population density would be better. Those I

could find showed that the area was increasing, so I sort of gave up on it.

Whether London is saturated is not clear. The fact that LWC has a bigger UHI than SJP implies that if you did more development around SJP it could be raised. I doubt though that there will be any development in the Mall and on Horseguards Parade!

The Nature Geosciences paper looks good - so hope it gets reviewed favourably.

It will be a useful thing to refer to, but I can't see it cutting any ice with the skeptics.

They think the models are wrong, and can't get to grips with natural variability!

Thanks for the CV. I see I'm on an abstract for the Hawaii meeting! Only noticed as it was the last one on your list.

Cheers
Phil

At 10:04 06/01/2009, you wrote:

>Phil

>

>Maybe in your conclusions you should comment on the fact that some more general studies show relationships between the population or size of cities and the urban effect. This seems not to be true here. Is there any evidence from other studies of a "saturation effect" on urban warming in some cases? And why this might be so?

>

>Chris

>

>

>Prof. Chris Folland

>Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)

>

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>

>

>

>

>-----Original Message-----

>From: Phil Jones [<mailto:p.jones@uea.ac.uk>]

>Sent: 05 January 2009 17:02

>To: Folland, Chris
>Subject: RE: FW: Temperatures in 2009
>
>
> Chris,
> Will look at later. Here is the UHI paper I submitted today to
Weather.
> Didn't take long to do. I started doing it as people kept on saying
the UHI
> in London (and this is only Central London) was getting worse. I
couldn't
> see it and Rothamsted and Wisley confirmed what I'd thought.
>
> Any comments appreciated. Remember it is just Weather,
> and I tried to make it quite simple ! David did see it last month.
>
> Cheers
> Phil
>
>
>At 16:46 05/01/2009, you wrote:
> >Phil
> >
> >Strictly very much in confidence, this was submitted to Nature
> >Geosciences just before Xmas after discussion with them.
> >
> >Night-time temperatures seem to have been rather underestimated here
> >as well since the cold spell started. Daytime forecasts have been
> >better, allowing for 1000 feet of elevation. Real cold would shock all
under 30!
> >
> >Chris
> >
> >
> >Prof. Chris Folland
> >Research Fellow, Seasonal to Decadal Forecasting (from 2 June 2008)
> >
> >Met Office Hadley Centre, Fitzroy Rd, Exeter, Devon EX1 3PB United
> >Kingdom
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> >Tel: +44 (0)1647 432978
> >Fax: (in UK) 0870 900 5050
> > (International) +44 (0)113 336 1072)
> ><<http://www.metoffice.gov.uk>> Fellow of the Met Office Hon. Professor
> >of School of Environmental Sciences, University of East Anglia
> >
> >
> >
> >
> >-----Original Message-----
> >From: Phil Jones [<mailto:p.jones@uea.ac.uk>]
> >Sent: 05 January 2009 16:18
> >To: Johns, Tim; Folland, Chris
> >Cc: Smith, Doug; Johns, Tim

> >Subject: Re: FW: Temperatures in 2009
> >
> >
> > Tim, Chris,
> > I hope you're not right about the lack of warming lasting
> > till about 2020. I'd rather hoped to see the earlier Met Office
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> > half the years to 2014 would exceed the warmest year currently on
> > record, 1998!
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> > scale, but it would be nice to wear their smug grins away.
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> > Chris - I presume the Met Office continually monitor the weather
> > forecasts.
> > Maybe because I'm in my 50s, but the
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> > >Dear Chris, cc: Doug
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> > >On Sat, 2009-01-03 at 21:31 +0000, Folland, Chris wrote:
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> > > > Any guidance welcome

> > > >

> > > > Chris

> > > >

> > > >

> > > > Prof. Chris Folland

> > > > Research Fellow, Seasonal to Decadal Forecasting (from 2 June

> > > > 2008)

> > > >

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> > > >

> > > >

> > > >

> > > >

> > > > -----Original Message-----

> > > > From: Mike MacCracken [<mailto:mmaccrac@comcast.net>]

> > > > Sent: 03 January 2009 16:44

> > > > To: Phil Jones; Folland, Chris

> > > > Cc: John Holdren; Rosina Bierbaum

> > > > Subject: Temperatures in 2009

> > > >

> > > > Dear Phil and Chris--

> > > >

> > > > Your prediction for 2009 is very interesting

> > > > (see note below for notice that went around to email list for a

> > > > lot of US Congressional staff)--and I would expect the analysis

> > > > you have done is correct. But, I have one nagging question, and

> > > > that is how much SO2/sulfate is being generated by the rising

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> > > > next few decades--go to tall stacks so that, for the near-term,

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> > > > repeated in the early 21st century (satellites may help on optical

> > > > depth, but it would really help to know what is being emitted).

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> > > > That there is a large potential for a cooling

> > > influence is sort of evident in the IPCC figure about the present
> > > sulfate distribution--most is right over China, for example,
> > > suggesting that the emissions are near the surface--something also
> > > that is, so to speak, 'clear' from the very poor visibility and
> > > air quality in China and India. So, the quick, fast, cheap fix is
> > > to put the SO2 out through tall stacks. The cooling potential also
> > > seems quite large as the plume would go out over the ocean with
> > > its low albedo--and right where a lot of water vapor is
> > > evaporated, so maybe one pulls down the water vapor feedback a
> > > little and this amplifies the sulfate cooling influence.
> > > >
> > > > Now, I am not at all sure that having more
> > > tropospheric sulfate would be a bad idea as it would limit
> > > warming--I even have started suggesting that the least expensive
> > > and quickest geoengineering approach to limit global warming would
> > > be to enhance the sulfate loading--or at the very least we need to
> > > maintain the current sulfate cooling offset while we reduce CO2
> > > emissions (and presumably therefore, SO2 emissions, unless we
> > > manage
> > > things) or we will get an extra bump of warming. Sure, a bit more
> > > acid deposition, but it is not harmful over the ocean (so we
> > > only/mainly emit for trajectories heading out over the ocean) and
> > > the impacts of deposition may well be less that for global warming
> > > (will be a tough comparison, but likely worth looking at). Indeed,
> > > rather than go to stratospheric sulfate injections, I am leaning
> > > toward tropospheric, but only during periods when trajectories are
> > > heading over ocean and material won't get rained out for 10 days
or so.
> > > > Would be an interesting issue to do
> research on--see what could be done.
> > > >
> > > > In any case, if the sulfate hypothesis is
> > > right, then your prediction of warming might end up being wrong. I
> > > think we have been too readily explaining the slow changes over
> > > past decade as a result of variability--that explanation is wearing
thin.
> > > I would just suggest, as a backup to your prediction, that you
> > > also do some checking on the sulfate issue, just so you might have
> > > a quantified explanation in case the prediction is wrong.
> > > Otherwise, the Skeptics will be all over us--the world is really
> > > cooling, the models are no good, etc.
> > > And all this just as the US is about ready to get serious on the
issue.
> > > >
> > > > We all, and you all in particular, need to be prepared.
> > > >
> > > > Best, Mike MacCracken
> > > >
> > > >
> > > > Researchers Say 2009 to Be One of Warmest Years on Record
> > > >
> > > > On December 30, climate scientists from the
> > > UK Met Office and the University of East Anglia projected 2009
> > > will be one of the top five warmest years on record. Average

> > > global temperatures for 2009 are predicted to be 0.4°C above the
> > > 1961-1990 average of 14 °C. A multiyear forecast using a Met
> > > Office climate model indicates a rapid return of global
> > > temperature to the long-term warming trend, with an increasing
> > > probability of record temperatures after 2009. The fact that
> > > 2009, like 2008, will not break records does not mean that global
warming has gone away
> > > . What matters is the underlying rate of warming, said Dr. Phil
> > > Jones, the director of climate research at the University of East
> > > Anglia. The presence of La Nina during the
> last year partially masked this underlying rate.
> > > Phenomena such as El Nino and La Nina have a significant
> > > influence on global surface temperature, said Dr. Chris Folland
> > > of the Met Office Hadley Center.
> > > Further warming to record levels is likely
> > > once a moderate El Nino develops. The transition from a La Nina
> > > effect to an El Nino one is expected late next year.
> > > >
> > > > For additional information see:
> > > > <http://uk.reuters.com/article/topNews/idUKTRE4BT49920081230>
> > > >
> > > > <http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-s>
> > > > et
> > > > -t
> > > > o-be-c
> > > > older-than-in-Iceland.html
> > > > <http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>
> > > >
<http://www.metoffice.gov.uk/corporate/pressoffice/2008/pr20081230>.
> > > > ht
> > > > ml
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> > > >
> > > >--
> > > Tim Johns Manager Global Coupled Modelling
> > > Met Office Hadley Centre
> > > FitzRoy Rd Exeter Devon EX1 3PB United Kingdom
> > > Tel: +44 (0)1392 886901 Fax: +44 (0)1392 885681
> > > E-mail: tim.johns@metoffice.gov.uk <http://www.metoffice.gov.uk>
> > >
> > > Please note I work part time, normally Monday-Tuesday
> > > Thursday-Friday
> > >
> > > Met Office climate change predictions can now be viewed on Google
> > > Earth <http://www.metoffice.gov.uk/research/hadleycentre/google/>
> > >
> > >
> >
> >Prof. Phil Jones
> >Climatic Research Unit Telephone +44 (0) 1603 592090
> >School of Environmental Sciences Fax +44 (0) 1603 507784
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From: Phil Jones <p.jones@uea.ac.uk>
To: "Folland, Chris" <chris.folland@metoffice.gov.uk>
Subject: RE: FW: Temperatures in 2009
Date: Wed Jan 7 12:51:51 2009

Chris,

Apart from contacting Gavin and Mike Mann (just informing them) you should appeal.

In essence it means that Real Climate is a publication.

If you do go to GRL I wouldn't raise the issue with them. Happy to be a suggested reviewer if you do go to GRL.

Cheers

Phil

Chris,

Worth pursuing - even if only GRL.

Possibly worth sending a note to Gavin Schmidt at Real Climate to say what Nature have used as a refusal!

Cheers

Phil

At 17:01 06/01/2009, you wrote:

Phil

Thanks. Bad news today. Nature Geosciences wont publish this because the Real Climate Blog mentions (more vaguely) the basic content of what we have written. That is indeed the reason Nature Geosciences have given. It seems blogs can now prevent publication! I have suggested to Jeff we try GRL but only after raising this issue with them.

Chris

Prof. Chris Folland

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Fellow of the Met Office

Hon. Professor of School of Environmental Sciences, University of East Anglia

-----Original Message-----

From: Phil Jones [[2]mailto:p.jones@uea.ac.uk]

Sent: 06 January 2009 14:56

To: Folland, Chris

Subject: RE: FW: Temperatures in 2009

Chris,

City population size and urban effects are not related that well. I think a lot depends on where the city is in relation to the sea, large rivers and water bodies as well.

I did try and get population figures for London from various times during the 20th century.

I found these, but the area of London they referred to kept changing. Getting the areas proved more difficult, as I thought population density would be better. Those I could find showed that the area was increasing, so I sort of gave up on it.

Whether London is saturated is not clear. The fact that LWC has a bigger UHI than SJP implies that if you did more development around SJP it could be raised. I doubt though that there will be any development in the Mall and on Horseguards Parade!

The Nature Geosciences paper looks good - so hope it gets reviewed favourably. It will be a useful thing to refer to, but I can't see it cutting any ice with the skeptics.

They think the models are wrong, and can't get to grips with natural variability!

Thanks for the CV. I see I'm on an abstract for the Hawaii meeting! Only noticed as it was the last one on your list.

Cheers

Phil

At 10:04 06/01/2009, you wrote:

>Phil

>

>Maybe in your conclusions you should comment on the fact that some more
>general studies show relationships between the population or size of
>cities and the urban effect. This seems not to be true here. Is there
>any evidence from other studies of a "saturation effect" on urban
>warming in some cases? And why this might be so?

>

>Chris

>

>

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>Sent: 05 January 2009 17:02
>To: Folland, Chris
>Subject: RE: FW: Temperatures in 2009

>
>

> Chris,

> Will look at later. Here is the UHI paper I submitted today to Weather.
> Didn't take long to do. I started doing it as people kept on saying the UHI
> in London (and this is only Central London) was getting worse. I couldn't
> see it and Rothamsted and Wisley confirmed what I'd thought.

>
> Any comments appreciated. Remember it is just Weather,
> and I tried to make it quite simple ! David did see it last month.

>

> Cheers

> Phil

>
>

>At 16:46 05/01/2009, you wrote:

>>Phil

>>

>>Strictly very much in confidence, this was submitted to Nature
>>Geosciences just before Xmas after discussion with them.

>>

>>Night-time temperatures seem to have been rather underestimated here
>>as well since the cold spell started. Daytime forecasts have been
>>better, allowing for 1000 feet of elevation. Real cold would shock all under 30!

>>

>>Chris

>>

>>

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>>> suggesting that the emissions are near the surface--something also

>>> that is, so to speak, 'clear' from the very poor visibility and

>>> air quality in China and India. So, the quick, fast, cheap fix is

>>> to put the SO₂ out through tall stacks. The cooling potential also

>>> seems quite large as the plume would go out over the ocean with

>>> its low albedo--and right where a lot of water vapor is

>>> evaporated, so maybe one pulls down the water vapor feedback a

>>> little and this amplifies the sulfate cooling influence.

>>>>

>>>> Now, I am not at all sure that having more

>>> tropospheric sulfate would be a bad idea as it would limit

>>> warming--I even have started suggesting that the least expensive

>>> and quickest geoengineering approach to limit global warming would

>>> be to enhance the sulfate loading--or at the very least we need to

>>> maintain the current sulfate cooling offset while we reduce CO₂

>>> emissions (and presumably therefore, SO₂ emissions, unless we

>>> manage

>>> things) or we will get an extra bump of warming. Sure, a bit more

>>> acid deposition, but it is not harmful over the ocean (so we

>>> only/mainly emit for trajectories heading out over the ocean) and

>>> the impacts of deposition may well be less that for global warming

>>> (will be a tough comparison, but likely worth looking at). Indeed,

>>> rather than go to stratospheric sulfate injections, I am leaning

>>> toward tropospheric, but only during periods when trajectories are

>>> heading over ocean and material won't get rained out for 10 days or so.

>>>> Would be an interesting issue to do

> research on--see what could be done.

>>>>

>>>> In any case, if the sulfate hypothesis is

>>> right, then your prediction of warming might end up being wrong. I

>>> think we have been too readily explaining the slow changes over

>>> past decade as a result of variability--that explanation is wearing thin.

>>> I would just suggest, as a backup to your prediction, that you

>>> also do some checking on the sulfate issue, just so you might have

>>> a quantified explanation in case the prediction is wrong.

>>> Otherwise, the Skeptics will be all over us--the world is really

>>> cooling, the models are no good, etc.

>>> And all this just as the US is about ready to get serious on the issue.

>>>>

>>>> We all, and you all in particular, need to be prepared.

>>>>

>>>> Best, Mike MacCracken

>>>>

>>>>

>>>> Researchers Say 2009 to Be One of Warmest Years on Record

>>>>

>>>> On December 30, climate scientists from the

>>> UK Met Office and the University of East Anglia projected 2009

>>> will be one of the top five warmest years on record. Average

>>> global temperatures for 2009 are predicted to be 0.4°C above the

>>> 1961-1990 average of 14 ° C. A multiyear forecast using a Met

>>> Office climate model indicates a ³rapid return of global

>>> temperature to the long-term warming trend,² with an increasing

>>> probability of record temperatures after 2009. ³The fact that

>>> 2009, like 2008, will not break records does not mean that global warming has gone away . . .

>>> . What matters is the underlying rate of warming,² said Dr. Phil

>>> Jones, the director of climate research at the University of East

>>> Anglia. The presence of La Nina during the

> last year partially masked this underlying rate.

>>> ³Phenomena such as El Nino and La Nina have a significant

>>> influence on global surface temperature,² said Dr. Chris Folland

>>> of the Met Office Hadley Center.

>>>> ³Further warming to record levels is likely

>>> once a moderate El Nino develops.² The transition from a La Nina

>>> effect to an El Nino one is expected late next year.

>>>>

>>>> For additional information see:

>>>> [9]<http://uk.reuters.com/article/topNews/idUKTRE4BT49920081230>

>>>>

>>> [10]<http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-s>

>>> et

>>> -t

>>> o-be-c

>>>> older-than-in-Iceland.html

>>>> [11]<http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>

>>>> [12]<http://www.metoffice.gov.uk/corporate/pressoffice/2008/pr20081230>.

>>>> ht

>>>> ml

>>>>

>>>>

>>>--

>>> Tim Johns Manager Global Coupled Modelling
>>> Met Office Hadley Centre
>>> FitzRoy Rd Exeter Devon EX1 3PB United Kingdom
>>> Tel: +44 (0)1392 886901 Fax: +44 (0)1392 885681
>>> E-mail: tim.johns@metoffice.gov.uk [13]<http://www.metoffice.gov.uk>

>>>

>>> Please note I work part time, normally Monday-Tuesday
>>> Thursday-Friday

>>>

>>> Met Office climate change predictions can now be viewed on Google
>>> Earth [14]<http://www.metoffice.gov.uk/research/hadleycentre/google/>

>>>

>>>

>>

>>Prof. Phil Jones

>>Climatic Research Unit Telephone +44 (0) 1603 592090
>>School of Environmental Sciences Fax +44 (0) 1603 507784
>>University of East Anglia
>>Norwich Email p.jones@uea.ac.uk
>>NR4 7TJ
>>UK

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NR4 7TJ
UK

References

1. <http://www.metoffice.gov.uk/>
2. <mailto:p.jones@uea.ac.uk>
3. <http://www.metoffice.gov.uk/>
4. <mailto:p.jones@uea.ac.uk>
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6. <mailto:p.jones@uea.ac.uk>
7. <http://www.metoffice.gov.uk/>
8. <mailto:mmaccrac@comcast.net>
9. <http://uk.reuters.com/article/topNews/idUKTRE4BT49920081230>
10. <http://www.telegraph.co.uk/earth/earthnews/4030681/New-Years-Eve-s>
11. <http://www.bloomberg.com/apps/news?pid=20601072&sid=aTHzt5EA3UXs>
12. <http://www.metoffice.gov.uk/corporate/pressoffice/2008/pr20081230>
13. <http://www.metoffice.gov.uk/>
14. <http://www.metoffice.gov.uk/research/hadleycentre/google/>

From: Ben Santer <santer1@llnl.gov>
To: "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Karl Taylor <taylor13@llnl.gov>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, Melissa Free <Melissa.Free@noaa.gov>, peter gleckler <gleckler1@llnl.gov>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Steve Klein <klein21@mail.llnl.gov>, carl mears <mears@remss.com>, Doug Nychka <nychka@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>
Subject: Data published
Date: Thu, 15 Jan 2009 19:12:35 -0800
Reply-to: santer1@llnl.gov
Cc: "David C. Bader" <bader2@llnl.gov>, Bill Goldstein <goldstein3@llnl.gov>, Pat Berge <berge1@llnl.gov>, Janet Tulk <tulk1@llnl.gov>, Kathryn Craft Rogers <CraftRogers1@llnl.gov>, George Miller <miller21@llnl.gov>, Tomas Diaz De La Rubia <delarubia@llnl.gov>, Cherry Murray <murray38@llnl.gov>, Doug Rotman <rotman1@llnl.gov>, "Bamzai, Anjuli" <Anjuli.Bamzai@science.doe.gov>, mann <mann@psu.edu>, Anthony Socci <socci@ametsoc.org>, Bud Ward <wardbud@gmail.com>, "Peter U. Clark" <clarkp@onid.orst.edu>, "Michael C. MacCracken" <mmaccrac@comcast.net>, Professor Glenn McGregor <g.mcgregor@auckland.ac.nz>, Stephen H Schneider <shs@stanford.edu>, "Stott, Peter" <peter.stott@metoffice.gov.uk>, "'Francis W. Zwiers'" <francis.zwiers@ec.gc.ca>, Tim Barnett <tbarnett-ul@ucsd.edu>, "Verardo, David J." <dverardo@nsf.gov>, Branko Kosovic <kosovic1@llnl.gov>, Bill Fulkerson <wfulk@utk.edu>, Michael Wehner <mfwehner@lbl.gov>, Hal Graboske <graboske1@llnl.gov>, Tom Guilderson <tguilderson@llnl.gov>, Luca Delle Monache <ldm@llnl.gov>, "Celine J. W. Bonfils" <bonfils2@llnl.gov>, "Dean N. Williams" <williams13@llnl.gov>, Charles Doutriaux <doutriaux1@llnl.gov>, Anne Stark <stark8@llnl.gov>

<x-flowed>

Dear coauthors of the Santer et al. International Journal of Climatology paper (and other interested parties),

I have now publicly released the synthetic MSU tropical lower tropospheric temperatures that were the subject of Mr. Stephen McIntyre's request to the U.S. Dept. of Energy/National Nuclear Security Agency under the U.S. Freedom of Information Act (FOIA). I have also released additional synthetic MSU temperatures which were not requested by Mr. McIntyre. These synthetic MSU datasets are available on PCMDI's publicly-accessible website. The link to the datasets is:

<http://www-pcmdi.llnl.gov/projects/msu/index.php>

Technical information about the synthetic MSU datasets is provided in a document entitled:

"Information regarding synthetic Microwave Sounding Unit (MSU) temperatures calculated from CMIP-3 archive"

The link to the technical document is:

http://www-pcmdi.llnl.gov/projects/msu/MSU_doc.pdf

I hope that these datasets will prove useful for bona fide scientific research, and will be employed for such purposes only.

I am also hopeful that after publication of these datasets, I will be able to return to full-time research, unencumbered by further FOIA requests from Mr. McIntyre. In my opinion, Mr. McIntyre's FOIA requests are for the purpose of harassing Government scientists, and not for the purpose of improving our understanding of the nature and causes of climate change.

I'd like to thank Dave Bader, Bill Goldstein, and Pat Berge for helping me complete the process of reviewing, releasing, and publishing the synthetic MSU datasets and the technical document. And thanks to all of you for your support and encouragement over the past two months. It is deeply appreciated.

With best regards,

Ben

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Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: Good news! Plus less good news
Date: Thu, 29 Jan 2009 11:13:21 -0800
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

Yeah, I had already seen the stuff from McIntyre. Tom Peterson sent it to me. McIntyre has absolutely no understanding of climate science. He doesn't realize that, as the length of record increases and trend confidence intervals decrease, even trivially small differences between an individual observed trend and the multi-model average trend are judged to be highly significant. These model-versus-observed trend differences are, however, of no practical significance whatsoever - they are well within the structural uncertainties of the observed MSU trends.

It would be great if Francis and Myles got McIntyre's paper for review. Also, I see that McIntyre has put email correspondence with me in the Supporting Information of his paper. What a jerk!

I will write to Keith again. The Symposium wouldn't be the same without him. I think Tom would be quite disappointed.

Have fun in Switzerland!

With best regards,

Ben

P.Jones@uea.ac.uk wrote:

- > Ben,
- > I'm at an extremes meeting in Riederalp - near Brig. I'm too
- > old to go skiing. I'll go up the cable car to see the Aletsch Glacier
- > at some point - when the weather is good. Visibility is less than
- > 200m at the moment.
- >
- > It is good news that Rob can come. I'm still working on
- > Keith. It might be worth you sending him another email,
- > telling him what he'll be missing if he doesn't go. I think
- > Sarah will come, but I've not yet been in CRU when she has.

>
> With free wifi in my room, I've just seen that M+M have
> submitted a paper to IJC on your H2 statistic - using more
> years, up to 2007. They have also found your PCMDI data -
> laughing at the directory name - FOIA? Also they make up
> statements saying you've done this following Obama's
> statement about openness in government! Anyway you'll likely
> get this for review, or poor Francis will. Best if both
> Francis and Myles did this. If I get an email from Glenn I'll
> suggest this.

>
> Also I see Pielke Snr has submitted a comment on Sherwood's
> work. He is a prat. He's just had a response to a comment
> piece that David Parker, Tom Peterson and I wrote on a paper
> they had in 2007. Pielke wouldn't understand independence if it
> hit him in the face. Both papers in JGR online. Not worth you
> reading them unless interested.

>
> Cheers
> Phil

>
>
>

--

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email: santer1@llnl.gov

</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: Good news! Plus less good news
Date: Thu, 29 Jan 2009 12:16:33 -0800
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

Congratulations on the AGU Fellowship! That's great news. I'm really delighted. I hope that Mr. Mc "I'm not entirely there in the head" isn't there to spoil the occasion...

With best regards,

Ben

P.Jones@uea.ac.uk wrote:

- > Ben,
- > Meant to add - hope you're better! You were missed at
- > IDAG. Meeting went well though.
- >
- > I heard during IDAG that I've been made an AGU Fellow.
- > Will likely have to go to Toronto to Spring AGU to collect it.
- > I hope I don't see a certain person there!
- > Have to get out of a keynote talk I'm due to give in
- > Finland the same day!
- >
- > Cheers
- > Phil
- >
- >
- > Ben,
- > I'm at an extremes meeting in Riederalp - near Brig. I'm too
- > old to go skiing. I'll go up the cable car to see the Aletsch Glacier at
- > some point - when the weather is good. Visibility is less than 200m at
- > the moment.
- >
- > It is good news that Rob can come. I'm still working on
- > Keith. It might be worth you sending him another email,
- > telling him what he'll be missing if he doesn't go. I think
- > Sarah will come, but I've not yet been in CRU when she has.

>
> With free wifi in my room, I've just seen that M+M have
> submitted a paper to IJC on your H2 statistic - using more
> years, up to 2007. They have also found your PCMDI data -
> laughing at the directory name - FOIA? Also they make up
> statements saying you've done this following Obama's
> statement about openness in government! Anyway you'll likely
> get this for review, or poor Francis will. Best if both
> Francis and Myles did this. If I get an email from Glenn I'll
> suggest this.

>
> Also I see Pielke Snr has submitted a comment on Sherwood's
> work. He is a prat. He's just had a response to a comment
> piece that David Parker, Tom Peterson and I wrote on a paper
> they had in 2007. Pielke wouldn't understand independence if it
> hit him in the face. Both papers in JGR online. Not worth you
> reading them unless interested.

>
> Cheers
> Phil

--

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</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: Smithg <smithg49@starhub.net.sg>
Subject: Re: data request
Date: Fri, 30 Jan 2009 09:33:53 -0800
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Mr. Smith,

Please do not lecture me on "good science and replicability". Mr. McIntyre had access to all of the primary model and observational data necessary to replicate our results. Full replication of our results would have required Mr. McIntyre to invest time and effort. He was unwilling to do that.

Our results were published in a peer-reviewed publication (the International Journal of Climatology). These results were fully available for "independent testing and replication by others". Indeed, I note that David Douglass et al. performed such independent testing and replication in their 2007 International Journal of Climatology paper.

Douglass et al. used the same primary climate model data that we employed. They did what Mr. McIntyre was unwilling to do - they independently calculated estimates of "synthetic" Microwave Sounding Unit (MSU) temperatures from climate model data. The Douglass et al. "synthetic" MSU temperatures are very similar to our own. The scientific differences between the Douglass et al. and Santer et al. results are primarily related to the different statistical tests that the two groups employed in their comparisons of models and observations. Demonstrably, the Douglass et al. statistical test contains several serious flaws, which led them to reach incorrect inferences regarding the level of agreement between modeled and observed temperature trends.

Mr. McIntyre could easily have examined the appropriateness of the Douglass et al. statistical test and our statistical test with randomly-generated data (as we did in our paper). Mr. McIntyre chose not to do that. He preferred to portray himself as a victim of evil Government-funded scientists. A good conspiracy theory always sells well.

Mr. Smith, you chose to take the extreme step of writing to LLNL and DOE management to complain about my "unresponsiveness" and my failure to provide data to Mr. McIntyre. You made your complaint on the basis of

the information available on Mr. McIntyre's blog. You did not understand - and still do not understand - that the primary model data used in our paper have always been freely available to any scientific researcher, and are currently being used by many hundreds of scientists around the world. Any competent climate scientist could perform full replication of our calculation of "synthetic" MSU temperatures - as Douglass et al. have already done.

Your email to George Miller and Anna Palmisano was highly critical of my behavior in this matter. Your criticism was entirely unjustified, and damaging to my professional reputation. I therefore see no point in establishing a dialogue with you. Please do not communicate with me in the future. I do not give you permission to distribute this email or post it on Mr. McIntyre's blog.

Sincerely,

Dr. Ben Santer

Smithg wrote:

> Dear Dr. Santer,

>

> I'm pleased to see that the requested data is now available on line.

> Thank you for your efforts to make these materials available.

>

> My "dog in this fight" is good science and replicability. I note the following references:

>

> The American Physical Society on line statement reads (in part):

>

> "The success and credibility of science are anchored in the willingness of scientists to:

>

- > 1. Expose their ideas and results to independent testing and replication by others. This requires the open exchange of data, procedures and materials.
- > 2. Abandon or modify previously accepted conclusions when confronted with more complete or reliable experimental or observational evidence."

>

> Also I note the NAS booklet "On Being a Scientist: Responsible Conduct in Research" (2nd edition) states "After publication, scientists expect that data and other research materials will be shared with qualified

> colleagues upon request. Indeed, a number of federal agencies, journals,
> and professional societies have established policies requiring the
> sharing of research materials. Sometimes these materials are too
> voluminous, unwieldy, or costly to share freely and quickly. But in
> those fields in which sharing is possible, a scientist who is unwilling
> to share research materials with qualified colleagues runs the risk of
> not being trusted or respected. In a profession where so much depends on
> interpersonal interactions, the professional isolation that can follow a
> loss of trust can damage a scientist's work". I know that the 3rd
> edition is expected soon, but I cannot imagine this position will be
> weakened. Indeed, with electronic storage of data increasing
> dramatically, I expect that most of the exceptions are likely to be
> dropped.

>
> I understand that science is considered by some to be a "blood sport"
> and that there are serious rivalries and disputes. Nevertheless, the
> principles above are vital to the continuation of good science, wherever
> the results may lead.

>
> Again, I thank you for making the data available, and I wish you success
> in your future research.

>
> Kind regards,

>
> Geoff Smith

>
> ----- Original Message -----
> *From:* Smithg <mailto:smithg49@starhub.net.sg>
> *To:* santer1@llnl.gov <mailto:santer1@llnl.gov>
> *Sent:* Tuesday, January 06, 2009 11:23 PM
> *Subject:* data request

>
> Dear Dr. Santer

>
> ref: Santer, et. al.
> Consistency of modelled and observed temperature trends in the
> tropical troposphere
> International Journal of Climatology
> Volume 28, Issue 13, Date: 15 November 2008, Pages: 1703-1722

>
> As a courtesy, I would like to advise you that I have requested data
> to support the above paper (monthly model data (49 series) used for
> statistical analysis in Santer et al 2008 or a link to a URL with a

> file of the data as used in the paper) be made available to me via a
> request to Dr. Anna Palmisano of the DOE, Office of Science, the
> funding agency. This request is for "recorded factual material
> commonly accepted in the scientific community as necessary to
> validate research findings".

> This data is already the subject of an FOIA request, but I have
> asked Dr. Palisano to obtain and send me the data independently of
> the outcome of any FOIA review. My reasons are:

- > 1) further analysis of the data is important for public policy
- > 2) there is no valid intellectual property justification for
> withholding this data
- > 3) the data is readily available as obviously you (Dr. Santer) used
> the information in preparing the recently published paper

> My request has been copied to Dr. George Miller.

> Since I have not asked you directly for the data, I now request this
> data directly from you (monthly model data (49 series) used for
> statistical analysis in Santer et al 2008 or a link to a URL with a
> file of the data as used in the paper).

> Your reported replies to requests of other individuals that the
> datasets are publicly available is non-responsive to the request.

> You may be aware that the head of the Royal Meteorological Society
> (who are the publishers of the above referenced journal) has
> announced that their data archiving policies will be reviewed at
> their next general editors meeting. That may change things for the
> future, but a future change does not have retrospective force.
> Nevertheless, there is a high probability that requests for this
> data will continue until provided.

> In the absence of the requested data, it is very likely this
> publication will be judged "non-replicable" and therefore of no
> evidentiary value in public policy.

> Kind regards,

> Geoff Smith

--

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email: santer1@llnl.gov

</x-flowed>

From: Ben Santer <santer1@llnl.gov>
To: P.Jones@uea.ac.uk
Subject: Re: [Fwd: data availability]
Date: Mon, 02 Feb 2009 10:02:55 -0800
Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

Yes, this is the same Geoff Smith who wrote to me. Do you know who he is? From his comments about the RMS, he seems to be a Brit.

In his email to you, Mr. Smith notes that: "there is a strong case to be made that intermediate results, e.g., collation of such data and the relevant code should be made available in studies such as this one, since there is an important possibility of errors in trying to replicate such a collation".

This is a key point. Douglass et al. already audited our "collation" of the primary temperature data (i.e., our calculation of synthetic MSU temperatures). As I've already told Mr. Smith, Douglass et al. obtained synthetic MSU temperatures very similar to the ones published in our IJoC paper. Mr. Smith does not understand this. Nor does he understand that the algorithms used to calculate synthetic MSU temperatures from raw model temperature data have already been published and documented in the peer-reviewed literature.

I think it would be useful to raise these issues with Paul Hardaker.

Cheers,

Ben

P.Jones@uea.ac.uk wrote:

- > Ben,
- > Is this the Smith who has emailed? Why does he think
- > you've not informed your co-authors that you've made the
- > data available? Most odd - though he does accept that the
- > raw data was already there. Pity that loads of people on
- > CA including McIntyre didn't seem to accept or realise this.
- > I'm not on an RMS committee at the moment, but I could
- > try and contact Paul Hardaker if you think it might be useful.

> Possibly need to explain what is raw and what is intermediate.

>

> I wasn't going to give this guy Smith the satisfaction of a reply!

>

> Cheers

> Phil

>

> ----- Original Message -----

> Subject: data availability

> From: "Smithg" <smithg49@starhub.net.sg>

> Date: Sun, February 1, 2009 2:09 pm

> To: p.jones@uea.ac.uk

> -----

>

> Dear Prof. Jones,

>

> ref: Santer, et. al.

> Consistency of modelled and observed temperature trends in the tropical
> troposphere

> International Journal of Climatology

> Volume 28, Issue 13, Date: 15 November 2008, Pages: 1703-1722

>

> As you are a co-author of the referenced paper, you may be interested to
> know of developments (in case you have not heard already).

>

> You will be aware that intermediate data ("monthly model data (49 series)
> used for statistical analysis in Santer et al 2008 or a link to a URL with
> a file of the data as used it the paper") had been requested from the
> first author, Dr. Santer. A refusal has been posted on line, but in the
> meantime the data is now available at
> [http:// www-pcmdi.llnl.gov/projects/msu/index.php](http://www-pcmdi.llnl.gov/projects/msu/index.php) .

>

> Perhaps you had this data already, but other co-authors have reportedly
> claimed (earlier) they did not have the data. A typical reported response
> to a FOIA request was "I have examined my files and have no monthly time
> series from climate models used in the paper referred to, and no
> correspondence regarding said time series".

>

> No one disputes Dr. Santer's claim that the "primary model data" is
> publicly available, but there is a strong case to be made that
> intermediate results, e.g., collation of such data and the relevant code
> should be made available in studies such as this one, since there is an
> important possibility of errors in trying to replicate such a collation.

> The archiving of such intermediate results is required for econometrics
> journals, among others.

>
> It is further reported on line that the posting of the data was not
> pursuant to an FOIA order, but posted voluntarily (although likely at the
> request of the funding agency, the Department of Energy, Office of
> Science). I hope other scientists will take this type of voluntary action.
> You may have heard that Professor Hardaker, the CEO of the Royal
> Meteorological Society which publishes the International Journal of
> Climatology, has confirmed the issue of data archiving will be on the
> agenda for the next meeting of the Society's Scientific Publishing
> Committee. There is a need for journals as well as funding agencies, and
> publishing scientists themselves, to establish and enforce good data and
> code archiving policies. A more precise definition of "recorded factual
> material commonly accepted in the scientific community as necessary to
> validate research findings" is probably overdue.

>
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> I'll look at the progress on archiving, but in the meantime, no reply is
> necessary.

>
> Kind regards,
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> Geoff Smith

>
>
> -----
>

> Dear Prof. Jones,
>
> ref: Santer, et. al.
> Consistency of modelled and observed temperature trends in the tropical
> troposphere
> International Journal of Climatology
> Volume 28, Issue 13, Date: 15 November 2008, Pages: 1703-1722

>
> As you are a co-author of the referenced paper, you may be interested to
> know of developments (in case you have not heard already).

>
> You will be aware that intermediate data ("monthly model data (49
> series) used for statistical analysis in Santer et al 2008 or a link to
> a URL with a file of the data as used it the paper") had been requested
> from the first author, Dr. Santer. A refusal has been posted on line,

> but in the meantime the data is now available at
> <http://www-pcmdi.llnl.gov/projects/msu/index.php> .
>
> Perhaps you had this data already, but other co-authors have reportedly
> claimed (earlier) they did not have the data. A typical reported
> response to a FOIA request was "I have examined my files and have no
> monthly time series from climate models used in the paper referred to,
> and no correspondence regarding said time series".
>
> No one disputes Dr. Santer's claim that the "primary model data" is
> publicly available, but there is a strong case to be made that
> intermediate results, e.g., collation of such data and the relevant code
> should be made available in studies such as this one, since there is an
> important possibility of errors in trying to replicate such a collation.
> The archiving of such intermediate results is required for econometrics
> journals, among others.
>
> It is further reported on line that the posting of the data was not
> pursuant to an FOIA order, but posted voluntarily (although likely at
> the request of the funding agency, the Department of Energy, Office of
> Science). I hope other scientists will take this type of voluntary
> action. You may have heard that Professor Hardaker, the CEO of the Royal
> Meteorological Society which publishes the International Journal of
> Climatology, has confirmed the issue of data archiving will be on the
> agenda for the next meeting of the Society's Scientific Publishing
> Committee. There is a need for journals as well as funding agencies, and
> publishing scientists themselves, to establish and enforce good data and
> code archiving policies. A more precise definition of "recorded factual
> material commonly accepted in the scientific community as necessary to
> validate research findings" is probably overdue.
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> I hope the Hadley Centre will take a lead in this issue. From time to
> time I'll look at the progress on archiving, but in the meantime, no
> reply is necessary.
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> Kind regards,
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> Geoff Smith

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Benjamin D. Santer

Program for Climate Model Diagnosis and Intercomparison

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Tel: (925) 422-3840

FAX: (925) 422-7675

email: santer1@llnl.gov

</x-flowed>

From: "peter.thorne" <peter.thorne@metoffice.gov.uk>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Visit to Met Office
Date: Tue, 10 Feb 2009 09:54:16 +0000
Cc: David Parker <david.parker@metoffice.gov.uk>

Phil, David,

as David says I'll be away in Oklahoma first week in March. Antarctic data first piqued my interest with the Science paper on raobs trends which was clearly non-physical but hard to nail down how wrong it was. I did some minor digging into READER and found that in the UA domain it was qc'ed but not homogenised. I've made a rather rash assumption that this would also be the case for the surface data but am happy to be corrected.

Its clear to me that Antarctica is a uniquely difficult environment to collect long-term homogeneous data in. So I have substantial doubts that all the manned station pegs in Steig et al. are adequate. Does this really matter? I'm not sure.

What Steig et al., satellites, and potentially reanalyses does do is allow us, in principle, at least to get around the no-neighbours issue in assessing homogeneity away from the peninsula.

For example we could use a bootstrapping of the Steig et al approach by creating say 50 realisations of each station series using randomly seeded combinations of manned station pegs as the S et al. RegEM constraint (excluding the candidate station) to make a neighbour composite ensemble. We could then add in the available reanalysis field estimates and satellite estimates and make a reasonable punt about the existence and magnitude of any breaks based upon multiple lines of evidence (of course, we lose some of these before 1979 ...). We could use this information to assess in a more rigorous way than has been done to date the homogeneity of these sparse stations. Then cleaned up data could be fed back through Steig et al. afterwards to see how it impacts that analysis making for a nice clean self-contained study.

My understanding from the blog discussion of Steig et al. is that the analysis step is fairly trivial so such an ensemble realisation approach should be plausible with a humble PC so long as it has the coding platform available.

Of course, this doesn't resolve any fundamental methodological concerns about the S et al. approach that may exist but it does give us a reasonable chance of creating a much more homogeneous READER manned station dataset for next IPCC AR and our future products.

My suspicion is that actually changing the manned station data in this way may make S et al. more different to the straight average of the READER data as used (effectively) in AR5 and point to the importance of the long-term homogeneity of the data pegs in RegEM ... this may, of course, be felt to be a can of worms too far ...

Peter

On Mon, 2009-02-09 at 16:53 +0000, Phil Jones wrote:

- > David,
- > I think I misinterpreted your email when in Switzerland. I think I thought
- > you wanted a talk and a possible project. Now I read it and it is just a
- > possible project.
- > I've done a lot with the Antarctic temperature data - I also have an
- > archive of MSLP data for most sites (for some it is station level pressure).
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- > (and be confident about anything) as the stations are too far apart. There is
- > an issue I could ask Adrian - whether ERA-INTERIM is good enough since
- > 1988? This could also assess the AVHRR, but this may be circular.
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- > data are OK, all you're assuming is that covariance structure
- > remains the same.
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- > that relevant.
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> Anyway - I do need to come down to see Ian. Possibilities would be coming
> mid week, say Feb 25/26 or March 4/5. How do these dates suit? I'd need to
> spend the night - maybe that Travel-lodge near you, it is only one night!

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> Cheers
> Phil

>
>
> At 16:04 30/01/2009, David Parker wrote:

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>>>Thanks. I hope the GCOS meeting goes well: Roger Saunders will be there.

>>>We look forward to your thoughts on the Antarctic data, and to your
>>>visit whenever that may be convenient for you,

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>>>On Fri, 2009-01-30 at 15:56 +0000, P.Jones@uea.ac.uk wrote:

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>>>> As I don't, I've been on 60 or 90 mins walks along snow covered
>>>> trails. Snow is 1m deep off the trails.

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>>>> be here now and Geneva next week.

>>>> Have a good weekend - if a little cold!

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>>>> future to set up a project in which CRU would homogenise the "Reader"
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>>>> at Imperial on 12-13 Feb when you aren't available.

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>>>> Hope you've had good meetings in Geneva

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> School of Environmental Sciences Fax +44 (0) 1603 507784
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> UK

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Peter Thorne Climate Research Scientist
Met Office Hadley Centre, FitzRoy Road, Exeter, EX1 3PB
tel. +44 1392 886552 fax +44 1392 885681
www.metoffice.gov.uk/hadobs

From: Phil Jones <p.jones@uea.ac.uk>
To: David Parker <david.parker@metoffice.gov.uk>
Subject: Re: Visit to Met Office
Date: Tue Feb 10 16:42:03 2009
Cc: Peter Thorne <peter.thorne@metoffice.gov.uk>, "Simpson, Ian.R" <ian.r.simpson@metoffice.gov.uk>

David, Peter, Ian,

Let's go for the week with Feb 25/26 in it. I could come down for late on the 25th then spend most of the 26th discussing Ian's work and also the Antarctic ideas. Presumably John Prior and others will be available at some point on the 26th.

The Antarctic surface T data that are in CRUTEM3 have come from my searches over the years and also from READER. Much of the early stuff in READER has come from the archives here, except where BAS have got the original digitized data from the Antarctic Institutes in all the countries.

I also have some files of when some of the manned stations on the ice have moved. These are forced moves, as the station moves, but they have never been accounted for. Halley and Casey are affected.

There are issues to discuss about the AWSs and also, as David knows from AOPC, work that Wisconsin are doing in putting together all the historic US series. I've talked to them about this - mainly to try and stop them calculating mean T a different way. If they do this it will screw their series up. It all relates to them saying that the mean of min and max is not a great way in the Antarctic to calculate mean T. They say they can now do the mean of every 3 hours, but it needs the historic series and the routine updating to change at the same time - which is unlikely to happen.

Cheers

Phil

At 18:13 09/02/2009, David Parker wrote:

Phil

Thanks. I think Feb 25-26 is better as Peter, who suggested the Reader-data project, will be away in the first week of March. Ian will be here except, I think, on Feb 27th when he is going to a chess tournament. The hotel next to the Met Office should be OK but I haven't checked availability - that can be done when the date is chosen.

David

On Mon, 2009-02-09 at 16:53 +0000, Phil Jones wrote:

> David,

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References

1. <http://www.metoffice.gov.uk/>

From: Phil Jones <p.jones@uea.ac.uk>

To: Michael Mann <mann@meteo.psu.edu>, Jean Jouzel <jean.jouzel@lsce.ipsl.fr>

Subject: Re: EGU2009 - Presentation Selection

Date: Mon Feb 16 17:06:35 2009

Mike,

It would be good to get some fresh blood.

Caspar and Pascal would be good choices. Discuss with Jean in Hawaii.

The meeting in Il Ciocco was a very good one - but so was the one in Wengen. It is just a matter of getting the right people and the right venue. The EGU and AGU meetings don't really work.

Cheers

Phil

At 15:41 15/02/2009, Michael Mann wrote:

thanks Jean,

yes, I've heard much about the legendary Il Ciocco meeting, sadly it was before I got into this field. I understand how you might want to discontinue being a co-convenor of this session, since its somewhat disconnected from the recent directions of your research. In fact, perhaps we should consider recruiting entirely new, more junior scientist conveners to take this over. Perhaps e.g. Caspar and Pascal.

Phil--interested in your thoughts on this.

Jean--looking forward to seeing you in Hawaii!

mike

On Feb 15, 2009, at 6:07 AM, Jean Jouzel wrote:

Dear mike and Phil,

This looks quite good (including poster presentations).

I confirm that I will be unable to attend this year (IPCC plenary in Turkey this same week). I hope that it will be better next year.

As you can see, I'am less and less involved in studies dealing with the last millenium. Obviously, I have still a lot of interest since the NATO meeting we organized at Il Ciocco with Ray Bradley and Phil about the climate of the 2000 years (and a great pleasure to interact with both of you). But, as far as our session, it may be wise to think of someone more directly invoved for the coming years.

You certainly have names in mind and this would be very welcome (one of my suggestion could be Pascal Yiou).

I'am sorry not to be with you in Vienna but I will be in Hawaii (Mike I feel that you will be there too).

Cheers Jean

At 9:07 +0000 13/02/09, Phil Jones wrote:

Mike, Jean,

I won't be in Hawaii. I did register, but I've just been travelling too much and have more meetings coming up in late March and April. I've decided not to go to the AGU in Toronto, partly as I couldn't find a replacement for a keynote talk I've been down to give at a meeting in Finland on the same day. Apparently about 5 of the 30 AGU Fellows listed can't make it either.

As for the EGU, the session looks good. Pity you have got Friday - numbers will be quite low for the poster session in the late afternoon. The one thing to add in would be Chairpersons for the two oral sessions. I managed to get them in last year, but can't recall how. If I recall correctly Jean said he had an IPCC meeting, so maybe put Gene down as chairing the first morning slot. Nick would be another option. Assume you'll do the second morning slot.

Cheers

Phil

At 03:09 13/02/2009, Michael Mann wrote:

Hi Phil, Jean,

I've attached the final version of our session program. They allowed us a half day or oral sessions (12 15 minute talks, 4 were solicited), and the rest are in poster.

Please let me know if you see any problems. I think its still possible to make changes if absolutely necessary.

thanks,

mike

p.s. will I see either of you at the IPCC meeting in Hawaii in March?

On Feb 9, 2009, at 8:12 AM, Phil Jones wrote:

Jean,

I think he is as well.

Cheers

Phil

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Dear Michael

I think that you rae taking care Cheers Jean

MailScanner-NULl-Check: 1234782259.34667@KQFMks6eL6kkqBwrCA/5pQ

X-Ids: 166

To: [1]jean.jouzel@cea.fr

Subject: EGU2009 - Presentation Selection

Reply-to: [2]egu2009@copernicus.org

From: [3]egu2009@copernicus.org

X-Co-Tag: aa43ed727bfee453a8c3def9b6ff53b8

Date: Mon, 9 Feb 2009 12:04:08 +0100 (CET)

X-Greylist: IP, sender and recipient auto-whitelisted, not delayed
by milter-greylist-4.0.1 (shiva.jussieu.fr [134.157.0.166]); Mon,
09 Feb 2009 12:04:16 +0100 (CET)

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X-j-chkmail-Envelope: 49900DAF.00D/132.166.172.107/sainfoin-
out.extra.cea.fr/sainfoin-out.extra.cea.fr/<[4]egu2009@copernicus.org>

X-j-chkmail-Score: MSGID : 49900DAF.00D on jchkmail.jussieu.fr : j- chkmail score : . :
R=. U=. O=# B=0.086 -> S=0.108

X-j-chkmail-Status: Ham

X-IPSL-MailScanner: Found to be clean

X-IPSL-SpamCheck: not spam, SpamAssassin (not cached, score=-0.149,
required 5, BAYES_05 -1.11, NO_REAL_NAME 0.96)

X-IPSL-From: [5]egu2009@copernicus.org

Dear Mr Jouzel,

The Programme Group Chairs of the EGU2009 scheduled your following
Session:

CL10

Climate of the last millennium: reconstructions, analyses and
explanation of regional and seasonal changes

Now you are kindly asked to finalize the actual programme of your
Session from 10 Feb 2009 to 14 Feb 2009. Please enter the tool

SOIII - Presentation Selection at

[6]<http://meetingorganizer.copernicus.org/EGU2009/sessionmodification/218> by using your
Copernicus Office User ID 100391.

The following tasks should be taken into account:

- 1) subdivide your Abstracts into Oral and Poster presentations;
- 2) define the sequence and the length of the different Oral presentations;
- 3) define the sequence of the Poster presentations;
- 4) define chairpersons.

In addition, you are able to include subtitles. These may
structure your programme, or define events without a corresponding
contribution, e.g. 5 min. "Introduction" or "Discussion".

Your entries generate the draft programme which will be finally
approved by the Programme Group Chairs and published online
afterwards. The authors will then receive the Letter of Schedule,
informing them about the details of their presentation.

We thank you very much in advance for your cooperation, and please
do not hesitate to contact us in case that any questions may arise!

With kind regards,
Katja Gänger
Copernicus Meetings
[7]egu2009@copernicus.org
on behalf of the Programme Committee Chair

--

IPSL/ Laboratoire des Sciences du Climat et de l'Environnement (UMR
CEA-CNRS-UVSQ)
CE Saclay, Bt 701, Pièce 9a, Orme des Merisiers, 91191 Gif sur
Yvette, tél : 33 (0) 1 69 08 77 13,
Portable : 33 (0)6 84 75 96 82, fax : 33 (0) 1 69 08 77 16, e- mail :
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Michael E. Mann
Associate Professor
Director, Earth System Science Center (ESSC)
Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [10]mann@psu.edu
University Park, PA 16802-5013
website: [11]<http://www.meteo.psu.edu/~mann/Mann/index.html>
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[12]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
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j-chkmail dot ensmp dot fr](http://j-chkmail.dot.ensmp.dot.fr/))!

X-j-chkmail-Envelope:

49900DAF.00D/132.166.172.107/sainfoin-out.extra.cea.fr/sainfoin-out.extra.cea.fr/<[16]
egu2009@copernicus.org>

X-j-chkmail-Score: MSGID : 49900DAF.00D on jchkmail.jussieu.fr : j-chkmail score : . :
R=. U=. O=# B=0.086 -> S=0.108

X-j-chkmail-Status: Ham

X-IPSL-MailScanner: Found to be clean

X-IPSL-SpamCheck: not spam, SpamAssassin (not cached, score=-0.149,
required 5, BAYES_05 -1.11, NO_REAL_NAME 0.96)

X-IPSL-From: [17]egu2009@copernicus.org

Dear Mr Jouzel,

The Programme Group Chairs of the EGU2009 scheduled your following Session:

CL10

Climate of the last millennium: reconstructions, analyses and explanation of regional
and seasonal changes

Now you are kindly asked to finalize the actual programme of your Session from 10 Feb
2009 to 14 Feb 2009. Please enter the tool SOIII - Presentation Selection

at[18]<http://meetingorganizer.copernicus.org/EGU2009/sessionmodification/218> by using
your Copernicus Office User ID 100391.

The following tasks should be taken into account:

- 1) subdivide your Abstracts into Oral and Poster presentations;
- 2) define the sequence and the length of the different Oral presentations;
- 3) define the sequence of the Poster presentations;
- 4) define chairpersons.

In addition, you are able to include subtitles. These may structure your programme, or define events without a corresponding contribution, e.g. 5 min. "Introduction" or "Discussion".

Your entries generate the draft programme which will be finally approved by the Programme Group Chairs and published online afterwards. The authors will then receive the Letter of Schedule, informing them about the details of their presentation.

We thank you very much in advance for your cooperation, and please do not hesitate to contact us in case that any questions may arise!

With kind regards,

Katja Gänger

Copernicus Meetings

[19]egu2009@copernicus.org

on behalf of the Programme Committee Chair

--

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"Dire Predictions" book site:

[24]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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References

1. <mailto:jean.jouzel@cea.fr>
2. <mailto:egu2009@copernicus.org>
3. <mailto:egu2009@copernicus.org>
4. <mailto:egu2009@copernicus.org>
5. <mailto:egu2009@copernicus.org>
6. <http://meetingorganizer.copernicus.org/EGU2009/sessionmodification/218>
7. <mailto:egu2009@copernicus.org>
8. <mailto:jean.jouzel@lsce.ipsl.fr>
9. <mailto:p.jones@uea.ac.uk>
10. <mailto:mann@psu.edu>
11. <http://www.meteo.psu.edu/~mann/Mann/index.html>
12. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
13. <mailto:jean.jouzel@cea.fr>
14. <mailto:egu2009@copernicus.org>
15. <mailto:egu2009@copernicus.org>
16. <mailto:egu2009@copernicus.org>
17. <mailto:egu2009@copernicus.org>
18. <http://meetingorganizer.copernicus.org/EGU2009/sessionmodification/218>
19. <mailto:egu2009@copernicus.org>
20. <mailto:jean.jouzel@lsce.ipsl.fr>
21. <mailto:p.jones@uea.ac.uk>
22. <mailto:mann@psu.edu>
23. <http://www.meteo.psu.edu/~mann/Mann/index.html>
24. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
25. <mailto:p.jones@uea.ac.uk>
26. <mailto:jean.jouzel@lsce.ipsl.fr>
27. <mailto:mann@psu.edu>
28. <http://www.meteo.psu.edu/~mann/Mann/index.html>
29. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

From: Darrell Kaufman <Darrell.Kaufman@nau.edu>
To: "K.Briffa@uea.ac.uk" <K.Briffa@uea.ac.uk>
Subject: Re: 2k Arctic synthesis
Date: Fri, 6 Mar 2009 11:59:30 -0700

<x-flowed>

Great. I'll play with both the composite series and the three individuals. I was hoping to get some spatially distributed information, so might include all three. I will also subdivide by proxy time and use PCA to examine spatial patterns. I'll take a stab at revising the text to include a few sentences about how we chose the tree-ring series. Then maybe you can take a look on Monday. Have a good weekend. Darrell

On Mar 6, 2009, at 11:54 AM, K.Briffa@uea.ac.uk wrote:

> Darell
> the short answer is yes - you need to give the appropriate weight
> to the
> Eurasian aggregate series though ie this one series should count as
> 3 in
> an average of all high -latitude (e.g. compared to Rosanne D'Arrigo
> west
> N. American series) unless you use the 3 separate
> series(Fennoscandia,Yamal, Taimyr) individually. I would use my single
> average series as is though. While you are doing this work , I
> suggest you
> also produce separate proxy type series (ice, lakes, trees) - for
> explicit
> comparison and perhaps separate half-hemisphere (US side and Eurasian
> side) though not sure if Greenland ice should go in either. Cheers
> Keith
>
>
>
>
> directly> Keith:
>> Thanks for the update. I'd like to revise the composite proxy record
>> over the weekend (my only spare time). Can I assume that I need to
>> omit the three tree-ring series that I took from Mann et al. (2008)
>> because they were not processed to retain the low frequency signal,

>> and that I should replace the Euraisan series with the three from
>> your recent Phil Trans paper (using the data on your website)?

>>

>> If you agree, I can work on revising all of the calculations and
>> figures and we can modify the text early next week.

>>

>> Would that work?

>> Darrell

>>

>>

>> On Mar 6, 2009, at 9:52 AM, Keith Briffa wrote:

>>

>>> Darrell

>>> REALLY sorry - have not done this yet - had back

>>> to back meetings for 2 days and am due to leave

>>> now for the weekend - couple of days away from

>>> computer - my comments are nothing earth

>>> shattering or voluminous but I would still like

>>> to make them for your consideration. I will try

>>> to do this on Monday now - if too late - just ignore me . Sorry

>>> again

>>> Keith

>>>

>>> thanks for your consideration

>>> cheers

>>> Keith

>>>

>>> At 15:01 03/03/2009, you wrote:

>>>> Keith:

>>>> I appreciate your willingness to squeeze this in on such short

>>>> notice. If you could get your comments to me by the end of the

>>>> week,

>>>> that would be more than I had hoped for. Thank you. Darrell

>>>>

>>>>

>>>> On Mar 3, 2009, at 7:56 AM, Keith Briffa wrote:

>>>>

>>>>> Darrell

>>>>> I would like to make some comments but the

>>>>> earliest I can get to this is Thursday (we have

>>>>> visitors here all day tomorrow. In short I would

>>>>> like to be involved - but I would rather wait and

>>>>> see the basis of your reaction to my initial

>>>>> thoughts when I get a Tracked changes version
>>>>> back to you. You are correct that there are
>>>>> clear limitations in the preservation of trend
>>>>> over two millennia in SOME of the data Mann et al
>>>>> used - and in the current series you cite for
>>>>> Yamal (Hantemirov et al) . I do believe that the
>>>>> composite series in our Phil Trans paper is a
>>>>> convenient representation of the circum-western
>>>>> Eurasian Arctic tree-line data - though the Grudd
>>>>> and Nauzbaev papers are virtually similar to our
>>>>> data for their areas. However I have a few
>>>>> reservations/comments on other aspects of the
>>>>> manuscript that I believe any likely referee
>>>>> might pick up on . Is it ok to wait til Thursday
>>>>> or will this not be acceptable for getting
>>>>> comments back? I know how these time lines are crucial. Best
>>>>> wishes
>>>>> Keith

>>>>>

>>>>> At 14:15 02/03/2009, you wrote:

>>>>>> Hello Keith:
>>>>>> Following the recommendations of Malcolm and Phil (via Ray), it's
>>>>>> clear that I should have come to you sooner. I am now well along
>>>>>> on a
>>>>>> manuscript that summarizes 2000-year-long proxy temperature
>>>>>> records
>>>>>> from the Arctic (attached). The impetus for the paper is the new
>>>>>> compilation of high-resolution lake records that my group
>>>>>> recently
>>>>>> published in J Paleolimnology.

>>>>>

>>>>>> On the tree-ring side, it's clear to me now that I should not
>>>>>> have
>>>>>> used the series from the Mann et al. compilation, and I hadn't
>>>>>> see
>>>>>> your 2008 Phil Trans paper until just last week. As far as I can
>>>>>> tell, the only records that meet the criteria for this study are
>>>>>> your
>>>>>> three new RCS series from Eurasia and D'Arrigo's Gulf of Alaska
>>>>>> record. Apparently, none of the Malcolm's series in Mann et al.
>>>>>> were
>>>>>> processed in a way that would preserve the millennial trend, and
>>>>>> these should be omitted from the synthesis.

>>>>>

>>>>> I now need to substantially revamp the manuscript. Before I do, I
>>>>> want to be sure that I get it right this time and hope that you
>>>>> will
>>>>> be interested in joining as co-author to help guide the tree-ring
>>>>> component of the synthesis. I see that you have posted the Phil
>>>>> Trans
>>>>> data on your website, but would much prefer to have your
>>>>> involvement
>>>>> before using the data.

>>>>>

>>>>> Unfortunately, the timing for submission is an issue. I am
>>>>> leading a
>>>>> 12-PI proposal that is currently pending and would benefit
>>>>> greatly if
>>>>> this paper were accepted for publication.

>>>>>

>>>>> Please have a look at the manuscript, which I realize needs
>>>>> substantial revisions, and let me know if you have time and
>>>>> interest
>>>>> in getting involved.

>>>>>

>>>>> Thanks,
>>>>> Darrell
>>>>> ï¼¼

>>>>>

>>>>> Darrell S. Kaufman
>>>>> Professor of Geology and Environmental Sciences
>>>>> Northern Arizona University
>>>>> 928-523-7192
>>>>> <http://jan.ucc.nau.edu/~dsk5/>

>>>>>

>>>>>

>>>>>

>>>>>

>>>>>

>>>>>

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>>>>> The impetus for the paper is the new compilation

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>>>>> Darrell

>>>>>
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>>>>> <<http://jan.ucc.nau.edu/~dsk5/>><http://jan.ucc.nau.edu/~dsk5/>

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>>>>> **Climatic Research Unit**
>>>>> **University of East Anglia**
>>>>> **Norwich, NR4 7TJ, U.K.**
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>>>>> **Phone: +44-1603-593909**
>>>>> **Fax: +44-1603-507784**
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>>>>> <http://www.cru.uea.ac.uk/cru/people/briffa/>
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>>>>>
>>>>> <http://www.cru.uea.ac.uk/cru/people/briffa/>
>>>>>
>>
>>
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>

</x-flowed>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Tom Melvin <t.m.melvin@uea.ac.uk>
Subject: Fwd: NERC Consortium Proposal
Date: Fri Mar 13 11:28:10 2009

X-Authentication-Warning: ueamailgate01.uea.ac.uk: defang set sender to <turneychris@gmail.com> using -f

DKIM-Signature: v=1; a=rsa-sha256; c=relaxed/relaxed;
d=gmail.com; s=gamma;
h=domainkey-signature:received:received:message-id:from:to
:content-type:mime-version:subject:date:cc:x-mailer;
bh=vzM4qpeBuZ3NQSbfkIPACp4rqI5xIH9tfL6OUhWjxcE=;
b=EAAG1b17JLNg2YRgwSZWUqtdNH6FAbtHYku6HP2vIb37BakYy+nAI9oPe2vJmnlvkJ
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du0IYmPQvWXg+hHATrIfMR3WSPuzT+bsHby1M=

DomainKey-Signature: a=rsa-sha1; c=noFWS;
d=gmail.com; s=gamma;
h=message-id:from:to:content-type:mime-version:subject:date:cc
:x-mailer;
b=vshpN16BnkBITzIbqZGkiKhZRrLDTy4h9YDrCcr1arlUpXQoFm7wGfUrAY9IINDGiv
rTtJrNYHwK42PcQotJXHe7XlhWBVuII6hxTU5X811ycdc4IcIXNIyRWDYYJGZMFSHdyj
IJJD59a4V+W1eHp2Kkv9yiXdaWSBeshQE2gvQ=

From: Chris Turney <turneychris@gmail.com>
To: Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, t.osborn@uea.ac.uk
Subject: NERC Consortium Proposal
Date: Mon, 9 Mar 2009 12:42:53 +0100
Cc: Philip Brohan <philip.brohan@metoffice.gov.uk>, Rob Allan <rob.allan@metoffice.gov.uk>, Peter Cox <P.M.Cox@exeter.ac.uk>

X-Mailer: Apple Mail (2.930.3)

X-Canit-CHI2: 0.00

X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f023)

X-Spam-Score: 0.00 () [Tag at 5.00] HTML_MESSAGE,SPF(pass,0)

X-CanItPRO-Stream: UEA:f023 (inherits from UEA:10_Tag_Only,UEA:default,base:default)

X-Canit-Stats-ID: 18712069 - 127314cabecf (trained as not-spam)

X-Antispam-Training-Forget:

[1]https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=f

X-Antispam-Training-Nonspam:

[2]https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=n

X-Antispam-Training-Spam: [3]https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=s

X-Scanned-By: CanIt (www.roaringpenguin.com) on 139.222.131.184

Hi Keith, Phil and Tim,

Please find attached an outline bid for the NERC Consortium bid we discussed at the end of last year. I must apologise for the delay in getting back to you. Exeter has suddenly gone mad with appointments of staff and postgrads. It's all good fun but it's taken up a lot of my time over the past couple of months.

For a NERC Consortium we need to put in a 2 page document as an expression of interest. If approved we can then go forward for submission. The next deadline is 1 July.

Can you have a look at the attached and let me know what you think? Could you let me know what sort of support you'd need if we go forward. We have up to £3.5 million to spend over 5 years. Included in the document we have to include a summary of the funding we'd like to get from NERC, any other funds we have in support and other benefits e.g. training. For the latter we envisaged approximately 1 postdoc and 2 PhDs per UK institution.

I'm currently in Copenhagen for a meeting so my email contact will be a little erratic but am back Friday.

Hope things are going well.

All the best,

Chris

Professor Chris Turney FRSA FRGS

Author of Ice, Mud and Blood: Lessons from Climates Past

Popular science website:

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Journal of Quaternary Science Asian and Australasian Regional Editor

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Office Tel.: +44 (0)1392 263331

Fax.: +44 (0)1392 263342

Slartibartfast: Science has achieved some wonderful things of course, but I'd far rather be happy than right any day.

Arthur Dent: And are you?

Slartibartfast: No. Thats where it all falls down of course.

Arthur Dent: Pity. It sounded like quite a good lifestyle otherwise.

The Hitchhikers Guide to the Galaxy, Douglas Adams

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References

1. <https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=f>
2. <https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=n>
3. <https://canit.uea.ac.uk/b.php?i=18712069&m=127314cabecf&c=s>
4. <http://www.christurney.com/>
5. http://www.sogaer.ex.ac.uk/geography/people/staff/c_turney/main.shtml
6. <http://us.macmillan.com/icemudandblood>
7. <http://www.christurney.com/>
8. <http://www.interscience.wiley.com/journal/jqs>
9. <http://www.carbonscape.com/>
10. http://www.sogaer.ex.ac.uk/geography/people/staff/c_turney/main.shtml
11. <mailto:c.turney@exeter.ac.uk>
12. <http://www.cru.uea.ac.uk/cru/people/briffa/>

From: Ben Santer <santer1@llnl.gov>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Tom's Symposium
Date: Fri, 13 Mar 2009 12:35:18 -0700
Reply-to: santer1@llnl.gov
Cc: Phil Jones <p.jones@uea.ac.uk>, Sarah Raper <S.Raper@mmu.ac.uk>

<x-flowed>

Dear Keith,

I'm very sorry to hear that both you and Sarah have not been well. I hope that both of you are feeling better soon. While I understand your decision, it's very sad that you won't be there on June 19th. I was really looking forward to a reunion of the "CRU gang". Despite its relatively small size, CRU has had (and continues to have!) a rather remarkable "fingerprint" in the world of climate science. The times we spent together while Tom was Director of CRU were exciting and extraordinary. It would have been fun to get together and celebrate those times, and to celebrate CRU's achievements under Tom's leadership.

Once again, best wishes to you and Sarah. Get well soon, and please let me know if you reconsider.

With best regards,

Ben

Keith Briffa wrote:

> Ben and Phil
> Sorry but I am going to decline the invitation. You will know the respect I have for Tom and the high personal regard I have for him. I will send him a personal message explaining my decision. Sorry for the time it has taken to come to this decision but I had to think hard about it . At this moment I do not know whether Sarah will make it. She like me has not been well over the Christmas/New Year period but she has not yet managed a single day back at work yet. I will have to leave it to her to let you know her thoughts on this.

> Best wishes

> Keith

>
> At 17:58 30/01/2009, you wrote:

>> Dear Keith,

>>

>> Thanks for the update.

>>

>> Phil and I would like to send out a general announcement in the next
>> few weeks, so that folks can put the Symposium on their calendars. It
>> would be nice if we could send out a list of confirmed speakers
>> together with the general announcement. So I'd be very grateful if you
>> could get back to me in the next week or two.

>>

>> Once again, just let me say that it would be great to see you and
>> Sarah in Boulder...

>>

>> With best regards,

>>

>> Ben

>>

>> Keith Briffa wrote:

>>> Ben

>>> I can not confirm . Sorry. Everything you say is true. It didn't need
>>> saying, but things may not be straight forward. Will get back to you.
>>> I am not saying no for the present. I know you need to know one way
>>> or the other. Best wishes

>>> Keith

>>> At 22:30 29/01/2009, you wrote:

>>>> Dear Keith,

>>>>

>>>> I just wanted to check with you regarding your availability for
>>>> Tom's Symposium on June 19th. I'm really hoping that you'll be able
>>>> to attend. It would be great to see you in Boulder, and I know that
>>>> Tom would be delighted if both you and Sarah could make it.

>>>>

>>>> The way I see it, Tom had a big impact on the scientific careers of
>>>> many people, but particularly on the scientific lives of you, me,
>>>> Phil, and Sarah.

>>>>

>>>> Tom and I may not have seen eye-to-eye on everything - but Tom
>>>> taught me how to be a scientist, and the lessons I learned at CRU
>>>> have helped me through subsequent difficult times. I view the
>>>> Symposium as a means of saying "thanks". It would be nice to say
>>>> thanks in the company of Tom's friends and colleagues.

>>>>

>>>> It would be great to share a few beers in Boulder, and reminisce
>>>> about our infrequent "play 'til you drop" squash games at UEA...

>>>>

>>>> Hope you and Sarah and Amy and Kerstie are all well.

>>>>

>>>> With best regards,

>>>>

>>>> Ben

>>>> -----

>>>>

>>>> Benjamin D. Santer

>>>> Program for Climate Model Diagnosis and Intercomparison

>>>> Lawrence Livermore National Laboratory

>>>> P.O. Box 808, Mail Stop L-103

>>>> Livermore, CA 94550, U.S.A.

>>>> Tel: (925) 422-3840

>>>> FAX: (925) 422-7675

>>>> email: santer1@llnl.gov

>>>> -----

>>>>

>>> -- Professor Keith Briffa,

>>> Climatic Research Unit

>>> University of East Anglia

>>> Norwich, NR4 7TJ, U.K.

>>> Phone: +44-1603-593909

>>> Fax: +44-1603-507784

>>> [http:// www. cru.uea.ac.uk/cru/people/briffa/](http://www.cru.uea.ac.uk/cru/people/briffa/)

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</x-flowed>

From: Edward Cook <drdendro@ldeo.columbia.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Support letter request
Date: Tue, 17 Mar 2009 07:24:05 -1000
Cc: Edward Cook <drdendro@ldeo.columbia.edu>

Hi Phil, Thanks for this. Here is a support letter from Matt Collins that you can use as a guide on what to say. It was forwarded to me by Lowell. Cheers, Ed

===== Dr. Edward R. Cook Doherty Senior Scholar and Director,
Tree-Ring Laboratory Lamont-Doherty Earth Observatory Palisades, New York 10964 USA Email:
drdendro@ldeo.columbia.edu Phone: 845-365-8618 Fax: 845-365-8152

===== On Mar 17, 2009, at 3:13 AM, Phil Jones wrote: > > Ed, >
I can do this. Do you have any details of what you'd like me to > say? > Does Lowell have
any in yet? > Away all next week. > > Cheers > Phil > > > At 03:09 17/03/2009, you wrote:
>> Hi Phil, >> >> I wonder if you would be willing to write a letter of support for a >>
fairly massive NSF Science and Technology Center (STC) proposal that >> will be submitted
in mid-April. The STC would be the Center for >> Regional Decadal Climate Projections. This
is a 5-year, \$25 million >> dollar, effort spearheaded by Lowell Stott (Department of Earth
>> Science, University of Southern California). It is multi- >> institutional >> with both
climate modelers and palaeoclimatologists (including me) >> involved in an effort to
develop skillful climate prediction >> capability on decadal time scales. See the attached
project summary >> from the pre-proposal that was was accepted by NSF for a full >>
proposal >> to be submitted. If you are willing to write a letter of support, it >> is
probably best that it be written to Lowell: >> >> Dr. Lowell Stott >> Department of Earth
Science >> University of Southern California >> Los Angeles, CA 90089 >> >> However, you
should send the letter to me for forwarding on to >> Lowell. >> The letter emailed to me as
a pdf with electronic signature works >> fine. Thanks for any help you can give me. I am
happy to answer any >> questions you might have as well. >> >> Cheers, >> >> Ed >> >>

===== >> Dr. Edward R. Cook >> Doherty Senior Scholar and >>
Director, Tree-Ring Laboratory >> Lamont-Doherty Earth Observatory >> Palisades, New York
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Dr. Edward R. Cook >> Doherty Senior Scholar and >> Director, Tree-Ring Laboratory >> Lamont-Doherty Earth Observatory >> Palisades, New York 10964 USA >> Email: drdendro@ldeo.columbia.edu >> Phone: 845-365-8618 >> Fax: 845-365-8152 >>

===== > Prof. Phil Jones > Climatic Research Unit Telephone +44 (0) 1603 592090 > School of Environmental Sciences Fax +44 (0) 1603 507784 > University of East Anglia > Norwich Email p.jones@uea.ac.uk > NR4 7TJ > UK >

----- > Hi Phil,

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Cheers,

Ed

Attachment Converted: "c:\eudora\attach\Axel_support.doc"

=====

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 Palisades, New York 10964 USA
 Email: [1]drdendro@ldeo.columbia.edu
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Dr. Lowell Stott
Department of Earth Science
University of Southern California
Los Angeles, CA 90089

However, you should send the letter to me for forwarding on to Lowell.

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Ed

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Dr. Edward R. Cook
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Norwich Email [4]p.jones@uea.ac.uk
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References

1. <mailto:drdendro@ldeo.columbia.edu>
2. <mailto:drdendro@ldeo.columbia.edu>
3. <mailto:drdendro@ldeo.columbia.edu>
4. <mailto:p.jones@uea.ac.uk>

From: Phil Jones <p.jones@uea.ac.uk>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>, "Michael E. Mann" <mann@psu.edu>
Subject: FYI
Date: Thu Mar 19 10:52:54 2009

Gavin, Mike,

See the link below! Don't alert anyone up to this for a while. See if they figure it out for themselves.

I've sent this to the Chief Exec of the RMS, who said he was considering changing data policy with the RMS journals. He's away till next week. I just wanted him to see what a load of plonkers he's dealing with! I'm hoping someone will pick this up and put it somewhere more prominently.

The responses are even worse than you get on CA.

I've written up the London paper for the RMS journal Weather, but having trouble with their new editor. He's coming up with the same naive comments that these responders are. He can't understand that London has a UHI of X, but that X has got no bigger since 1900.

I'm away all next week.

Cheers

Phil

[1]<http://wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-by-a-major-climate-scientist/>

"Phil Jones, the director of the Hadley Climate Center in the UK."

--

Thomas C. Peterson, Ph.D.

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References

1. <http://wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-by-a-major-climate-scientist/>

From: Phil Jones <p.jones@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: FYI
Date: Thu Mar 19 12:39:26 2009
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Mike,

I want to get the more extensive London paper in first.
I hope my missive to the Chief Exec of the RMS does something next week.
By the way the HC doesn't have a Director.
John Mitchell is Head of Climate Science
Chris Gordon is Deputy Director of the HC.
It has never had a Director with that particular title.
It is impossible for anyone to find this on their web site. Only if you
were on the HC Scientific Review Group would you be aware.

Cheers

Phil

At 12:24 19/03/2009, Michael Mann wrote:

Hi Phil,

thanks, we've already seen numerous comments about this at RealClimate. Its a paper that
is easily misunderstood and/or intentionally misrepresented by contrarians (or both).
One possibility is that you might consider writing a guest article for RC placing this
in proper perspective. What do you think?

mike

On Mar 19, 2009, at 6:52 AM, Phil Jones wrote:

Gavin, Mike,

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out for themselves.

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ng-by-a-major-climate-scientist/](http://wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warmi-
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"Phil Jones, the director of the Hadley Climate Center in the UK."

--

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--
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website: [3]<http://www.meteo.psu.edu/~mann/Mann/index.html>
"Dire Predictions" book site:
[4]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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References

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2. <mailto:mann@psu.edu>
3. <http://www.meteo.psu.edu/~mann/Mann/index.html>
4. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

From: Phil Jones <p.jones@uea.ac.uk>
To: santer1@llnl.gov
Subject: Re: See the link below
Date: Thu Mar 19 17:02:53 2009

Ben,

I don't know whether they even had a meeting yet - but I did say I would send something to their Chief Exec.

In my 2 slides worth at Bethesda I will be showing London's UHI and the effect that it hasn't got any bigger since 1900. It's easy to do with 3 long time series. It is only one urban site (St James Park), but that is where the measurements are from. Heathrow has a bit of a UHI and it has go bigger.

I'm having a dispute with the new editor of Weather. I've complained about him to the RMS Chief Exec. If I don't get him to back down, I won't be sending any more papers to any RMS journals and I'll be resigning from the RMS.

The paper is about London and its UHI!

Cheers

Phil

At 16:48 19/03/2009, you wrote:

Thanks, Phil. The stuff on the website is awful. I'm really sorry you have to deal with that kind of crap.

If the RMS is going to require authors to make ALL data available - raw data PLUS results from all intermediate calculations - I will not submit any further papers to RMS journals.

Cheers,

Ben

Phil Jones wrote:

Paul,

I sent you this last night, but in another email. I should have sent you two emails - apologies. The issues were not linked. This email is to bring your attention to the link at the end.

The next few sentences repeat what I said last night. I had been meaning to email you about the RMS and IJC issue of data availability for numbers and data used in papers that appear in RMS journals. This results from the issue that arose with the paper by Ben Santer et al in IJC last year. Ben has made the data available that this complainant wanted. The issue is that this is intermediate data. The raw data that Ben had used to derive the intermediate data was all fully available. If you're going to consider asking authors to make some or all of the

data available, then they had done already. The complainant didn't want to have to go to the trouble of doing all the work that Ben had done.

I hope this is clear.

Another issue that should be considered as well is this.

With many papers, we're using Met Office observations. We've abstracted these from BADC to use them in the papers. We're not allowed to make these available to others. We'd need to get the Met Office's permission in all cases.

This email came overnight - from Tom Peterson, who works at NCDC in Asheville.

[1][http://](http://wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-by-a-major-climate-scientist/)

wattsupwiththat.com/2009/03/18/finally-an-honest-quantification-of-urban-warming-by-a-major-climate-scientist/

"Phil Jones, the director of the Hadley Climate Center in the UK."

We all know that this is not my job. The paper being referred to appeared in JGR last year. The paper is

Jones, P.D., Lister, D.H. and Li, Q., 2008: Urbanization effects in large-scale temperature records, with an emphasis on China. /J. Geophys. Res/. *113*, D16122, doi:10.1029/2008/JD009916.

The paper clearly states where I work - CRU at UEA. There is no mention of the Hadley Centre!

There is also no about face as stated on the web page.

Sending this as it gives a good example of the sort of people you are dealing with when you might be considering changes to data policies at the RMS.

Several years ago I decided there was no point in responding to issues raised on blog sites. Ben has made the same decision as well.

There are probably wider issues due to climate change becoming more main stream in the more popular media that the RMS might like to consider. I just think you should be aware of some of the background. CRU has had numerous FOI requests since the beginning of 2007. The Met Office, Reading, NCDC and GISS have had as well - many related to IPCC involvement. I know the world changes and the way we do things changes, but these requests and the sorts of simple mistakes, should not have an influence on the way things have been adequately dealt with for over a century.

Cheers

Phil

--

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References

1. <http://>

From: Darrell Kaufman <Darrell.Kaufman@nau.edu>
To: David Schneider <dschneid@ucar.edu>, Nick McKay
<nmckay@email.arizona.edu>, Caspar Ammann <ammann@ucar.edu>, Bradley Ray
<rbradley@geo.umass.edu>, Keith Briffa <k.briffa@uea.ac.uk>, Miller Giff
<gmiller@Colorado.EDU>, Otto-Bleisner Bette <ottobli@ucar.edu>, Overpeck
Jonathan <jto@u.arizona.edu>
Subject: Submitted!
Date: Mon, 23 Mar 2009 06:43:33 -0700

With thanks to all. I'll let you know when I hear anything. Darrell
Darrell S. Kaufman
Professor of Geology and Environmental Sciences Northern Arizona
University 928-523-7192
<http://jan.ucc.nau.edu/~dsk5/>

With thanks to all.

I'll let you know when I hear anything.

Darrell

Attachment Converted: "c:\eudora\attach\2k synthesis submitted.pdf"

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[1]<http://jan.ucc.nau.edu/~dsk5/>

References

1. <http://jan.ucc.nau.edu/~dsk5/>

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: Tim Osborn <t.osborn@uea.ac.uk>, Fortunat Joos
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zlding@mail.iggcas.ac.cn
Subject: Key new IPCC relevant paleo-science
Date: Sun, 12 Apr 2009 17:34:21 +0200
Cc: Laurent Labeyrie <Laurent.Labeyrie@lsce.ipsl.fr>, Gavin Schmidt
<gschmidt@giss.nasa.gov>

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Dear friends,

The scoping of IPCC AR5 will happen in July this year. In the community there have been opinions raised regarding paleo-science in the next report, e.g. whether to have paleo-science dispersed into various topical chapters, e.g. forcing, model-evaluation, sea level etc., or whether it might be best to do as in AR4 to have a separate Paleo-chapter.

There are good arguments for both options, and it is not the intent of this email to voice a specific opinion. Rather it is important to let the scoping process be aware of all the relevant new paleo-science which would be assessed in AR5, thereby leading to the need for a strong presence of paleoclimate scientists in the LA-team of AR5, particularly in WG1, but also in WG2.

In order to make the case that paleo-science continues to be highly relevant for IPCC, Peck and I have agreed to be the editors of a Slide-series (ppt style) which can be used to make the case in the scoping, and which of course could be a useful product for various outreach activities of PAGES and the paleoclimate community at large. The PAGES office will assist in producing the slides

We therefore send this email to you who worked as LAs in AR4 or who are on SSC or other relevant PAGES panels and ask for your input. What we hope you can help with is the following:

1. Provide your best examples of key new IPCC (Policy) relevant new

results post AR4, i.e. accepted after July 2006, that provide compelling arguments for paleoclimate science as a key contributor to IPCC. Please limit this to the results which are clearly IPCC-relevant

2. Ongoing projects or programmes that are likely to deliver such results in the next 2-3 years can also be included. The information must, however, be specific and compelling to a non-paleo audience.
3. Send PDF of the paper or other material (like ppt slide) to Peck (jto@u.arizona.edu), Myself and Thorsten Kiefer (thorsten.kiefer@pages.unibe.ch) at PAGES, preferably by May 2.

We think this might become a very useful service to our community and to the climate change communities at large, and will be very rewarding. Hoping to hear back from many of you.

Best wishes

Peck and Eystein

Eystein Jansen
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e-mail:eystein.jansen@bjerknes.uib.no
tel: 55-589803/55-583491
fax: 55-584330

</x-flowed>

From: David Rind <drind@giss.nasa.gov>
To: Eystein Jansen <Eystein.Jansen@geo.uib.no>
Subject: Key new IPCC relevant paleo-science
Date: Mon, 20 Apr 2009 15:03:17 -0400
Cc: Tim Osborn <t.osborn@uea.ac.uk>, Fortunat Joos
<joos@climate.unibe.ch>, Jonathan Overpeck <jto@u.arizona.edu>, David
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<Laurent.Labeyrie@lsce.ipsl.fr>, Gavin Schmidt <gschmidt@giss.nasa.gov>

Hi Eystein and Jonathan,

With respect to the question of a separate paleo-climate chapter: if
paleoclimate is an
adjunct to all of the other chapters, what would happen - would there
be a paleo-climate
person on each of those chapters, just for that component? If so, the
person would not
carry much influence - and if chapters had to be trimmed (which we
know always happens),
there's a chance that a lot of the paleoclimate aspect would be the
first to go. I'm afraid
that little in-depth discussion would survive.

On the other hand: now that there's been a paleoclimate chapter, a lot
of the
'introductory' material would not really be needed - just the
'updates', which make for
much fewer pages. Perhaps, then, paleoclimate observations could be
part of the climate
observation chapter; and paleoclimate modeling, part of the modeling
chapter. That way, at
least several people with paleoclimate heritage could be part of each
of these chapters,
and allow for a proper representation of the state of our
understanding in these areas. It
would also allow for better integration of paleoclimates with the
current climate. As in
the case of present climate, care would have to be taken to ensure
that the observations

and modeling chapters have strong linkages.

Concerning what new topic should be addressed: there should be a discussion about the use of paleoclimates as analogs for the future. Some scientists (including at least one at GISS) are certain of their utility in this regard. I think the topic should be addressed from all sides.

And as for 'new' paleoclimate work: we have an article about to come out in GRL on stratospheric ozone during the LGM; here's the link:

[1]<http://www.agu.org/journals/gl/papersinpress.shtml#id2009GL037617>

David

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References

1. <http://www.agu.org/journals/gl/papersinpress.shtml#id2009GL037617>

From: Pierre Francus <pfrancus@ete.inrs.ca>
To: Jonathan Overpeck <jto@email.arizona.edu>
Subject: Re: Key new IPCC relevant paleo-science
Date: Wed, 22 Apr 2009 07:03:50 -0400
Cc: Steve Colman <scolman@d.umn.edu>, Eystein Jansen
<Eystein.Jansen@geo.uib.no>, Jonathan Overpeck <jto@u.arizona.edu>, Tim
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Dear all,

I guess one point that can be outlined for the next IPCC report is
about the regional
differences in climate change and variability.

We can see that in the paleo record, and it is very clear from the
work of the PAGES "last
2k regional groups".

There is for instance a new Arctic 2k summary in Journal of
Paleolimnology (Kauffman et al
2009), and another paper in prep (I guess you are co-author Peck).

All the best

Pierre

Pierre Francus
Institut National de la Recherche Scientifique

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PASADO-CANADA: [4]<http://www2.ete.inrs.ca/pasado/PASADO-Canada/Welcome.html>
GEOTOP: [5]<http://www.geotop.uqam.ca/>
CEN: [6]<http://www.cen.ulaval.ca/>
PAGES: [7]<http://www.pages.unibe.ch/>
On 21-avr.-09, at 16:42, Jonathan Overpeck wrote:

Thanks Steve and friends I still need to read all the feedback, and appreciate it. I think you hit on a biggie that paleo provides critical evaluations of model realism. With regard to the others, the key for inclusion in an IPCC assessment, is to synthesize the published literature in a way that informs policy makers (the top audience) on what is happening in the climate system, and more important even what will happen in the climate system. Taking the terrific speleothem work for example, what are the key lessons that are NEW and important to highlight to policy makers? This is the kind of relevant science we need to compile/highlight. It's harder than at first glance, but that's ok quality of relevant issues is more important than quantity. Again, thanks all for taking this exercise seriously. Feel free to seek input from colleagues, although please help us by only sending what you think has a chance of fitting with the criteria above it is unclear if there will be time for anyone to read all the strong literature that has come out since mid-2006, so please summarize each key point with a couple sentence bullet, the complete reference (maybe even send the pdf), and if you think the key point isn't easy to understand to a non-paleo person T a little (e.g., para) supporting text, w/ a key figure if you think it really helps. Best, peck
On 4/16/09 1:48 PM, "Steve Colman" <[8]scolman@d.umn.edu> wrote:

Dear Peck and Eystein,
I tend to agree with Stefan that it would be conceptually nice to weave paleo-science into all the chapters of the next report, but that, as a practical matter, a tighter

focus on paleo results would have more impact. Most people seem to accept that past

history is the only way to assess what the climate system can actually do (e.g., how

fast it can change). However, I think that the fact that reconstructed history provides

the only calibration or test of models (beyond verification of modern simulations) is under-appreciated.

In terms of recent or near-future new results, I think that two areas of continental

paleoclimate research are exciting: (1) the new speleothem records, which are producing

extremely high-resolution, well-dated histories, especially in monsoon areas; and (2) the

network of long-term continental climate histories coming from drill cores in lakes

(Titicaca, Malawi, Bosumtwi, Peten Itza, Qinghai, El' gygytgyn) is reaching the point

where stimulating syntheses may be possible.

Best,

Steve Colman

Professor of Geological Sciences and Director,

Large Lakes Observatory, University of Minnesota Duluth

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[9]www.d.umn.edu/llo <[10]http://www.d.umn.edu/llo>

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References

1. <mailto:pfrancus@ete.inrs.ca>
2. <http://www.inrs-ete.quebec.ca/professeur.php?page=PierreFrancus>
3. <http://www.ete.inrs.ca/profs/pf/itrax/home.htm>
4. <http://www2.ete.inrs.ca/pasado/PASADO-Canada/Welcome.html>
5. <http://www.geotop.uqam.ca/>
6. <http://www.cen.ulaval.ca/>
7. <http://www.pages.unibe.ch/>
8. <file://localhost/tmp/scolman@d.umn.edu>
9. <http://www.d.umn.edu/llo>
10. <http://www.d.umn.edu/llo>

11. file:///localhost/tmp/jto@u.arizona.edu
12. file:///localhost/tmp/regalado@email.arizona.edu

From: Tom Wigley <wigley@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: [Fwd: CCNet Xtra: Climate Science Fraud at Albany University?]-
FROM TOM W
Date: Mon, 04 May 2009 01:37:07 -0600
Cc: Ben Santer <santer1@llnl.gov>

Content-Type: text/plain; charset=UTF-8; format=flowed
X-MIME-Autoconverted: from 8bit to quoted-printable by
ueacanitdb01.uea.ac.uk id n457EfQ5005459

<x-flowed>
Phil,

Do you know where this stands? The key things from the Peiser items are ...

"Wang had been claiming the existence of such exonerating documents for nearly a year, but he has not been able to produce them. Additionally, there was a report published in 1991 (with a second version in 1997) explicitly stating that no such documents exist. Moreover, the report was published as part of the Department of Energy Carbon Dioxide Research Program, and Wang was the Chief Scientist of that program."

and

"Wang had a co-worker in Britain. In Britain, the Freedom of Information Act requires that data from publicly-funded research be made available. I was able to get the data by requiring Wang's co-worker to release it, under British law. It was only then that I was able to confirm that Wang had committed fraud."

You are the co-worker, so you must have done something like provide Keenan with the DOE report that shows that there are no station records for 49 of the 84 stations. I presume Keenan therefore thinks that it was not possible to select stations on the basis of ...

"... station histories: selected stations have relatively few, if any, changes in instrumentation, location, or observation times"
[THIS IS ITEM "X"]

Of course, if the only stations used were ones from the 35 stations that *did* have station histories, then all could be OK. However, if some of the stations used were from the remaining 49, then the above selection method could not have been applied (but see below) -- unless there are other "hard copy" station history data not in the DOE report (but in China) that were used. From what Wang has said, if what he says is true, the second possibility appears to be the case.

What is the answer here?

The next puzzle is why Wei-Chyung didn't make the hard copy information available. Either it does not exist, or he thought it was too much trouble to access and copy. My guess is that it does not exist -- if it

did then why was it not in the DOE report? In support of this, it seems that there are other papers from 1991 and 1997 that show that the data do not exist. What are these papers? Do they really show this?

Now my views. (1) I have always thought W-C W was a rather sloppy scientist. I therefore would not be surprised if he screwed up here. But ITEM X is in both the W-C W and Jones et al. papers -- so where does it come from first? Were you taking W-C W on trust?

(2) It also seems to me that the University at Albany has screwed up. To accept a complaint from Keenan and not refer directly to the complaint and the complainant in its report really is asking for trouble.

(3) At the very start it seems this could have been easily dispatched. ITEM X really should have been ...

"Where possible, stations were chosen on the basis of station histories and/or local knowledge: selected stations have relatively few, if any, changes in instrumentation, location, or observation times"

Of course the real get out is the final "or". A station could be selected if either it had relatively few "changes in instrumentation" OR "changes in location" OR "changes in observation times". Not all three, simply any one of the three. One could argue about the science here -- it would be better to have all three -- but this is not what the statement says.

Why, why, why did you and W-C W not simply say this right at the start? Perhaps it's not too late?

I realise that Keenan is just a trouble maker and out to waste time, so I apologize for continuing to waste your time on this, Phil. However, I *am* concerned because all this happened under my watch as Director of CRU and, although this is unlikely, the buck eventually should stop with me.

Best wishes,
Tom

P.S. I am copying this to Ben. Seeing other peoples' troubles might make him happier about his own parallel experiences.

</x-flowed>

Return-Path: <b.j.peiser@ljmu.ac.uk>

X-Original-To: wigley@cgd.ucar.edu

Delivered-To: wigley@cgd.ucar.edu

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by post2.cgd.ucar.edu (Postfix) with ESMTTP id CB38C3803F;

Sun, 3 May 2009 08:57:40 -0600 (MDT)

Received: from localhost (localhost.localdomain [127.0.0.1])

by nscan3.ucar.edu (Postfix) with ESMTP id ABDD3230C024;
Sun, 3 May 2009 08:57:40 -0600 (MDT)
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by localhost (nscan3.ucar.edu [127.0.0.1]) (amavisd-new, port 10024)
with ESMTP id 12674-01; Sun, 3 May 2009 08:57:37 -0600 (MDT)
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X-SMTP-Auth: no
Received: from exch4.jmu.ac.uk (exch4.jmu.ac.uk [150.204.37.14])
by nscan3.ucar.edu (Postfix) with ESMTP id 9B970230C00B;
Sun, 3 May 2009 08:57:25 -0600 (MDT)
X-MimeOLE: Produced By Microsoft Exchange V6.5
Content-class: urn:content-classes:message
MIME-Version: 1.0
Content-Type: text/plain;
charset="utf-8"
Subject: CCNet Xtra: Climate Science Fraud at Albany University?
Date: Sun, 3 May 2009 15:57:08 +0100
Message-ID: <08927B60D87D374DB001D814D5D2250F01663F4F@exch4.jmu.ac.uk>
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
Thread-Topic: CCNet Xtra: Climate Science Fraud at Albany University?
Thread-Index: AcnIu0OvOgPY3fShTXip0PBdcf9mWwAAWuOQAGIoisAAbhWS4A==
From: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>
To: "cambridge-conference" <cambridge-conference@livjm.ac.uk>
X-Virus-Scanned: amavisd-new at ucar.edu

CCNet Xtra - 3 May 2009 -- Audiatur et altera pars

CLIMATE SCIENCE FRAUD AT ALBANY UNIVERSITY?

The University at Albany is in a difficult position. If the University received such records as part of the supposed misconduct investigation, then they could easily resolve the problem by making them available to the scientific community and to readers. If the University does not have such records then they have been complicit in misconduct and in coverup of misconduct. If the University at Albany does have such records, but such records are not in accordance with the stated methodology of the publications, then the University has more serious difficulties.

"Investigations" of scientific misconduct should themselves align with the usual principles of scientific discourse (open discussion, honesty, transparency of method, public disclosure of evidence, open public analysis and public discussion and reasoning underlying any conclusion). This was not the case at the University at Albany. When you see universities reluctant to investigate things properly, it provides reasonable evidence that they really don't want to investigate things properly.

-- Aubrey Blumsohn, Scientific Misconduct Blog, 2 May 2009

(1) ALLEGATIONS OF FRAUD AT ALBANY - THE WANG CASE

Aubrey Blumsohn, Scientific Misconduct Blog, 2 May 2009

(2) THE FRAUD ALLEGATION AGAINST SOME CLIMATIC RESEARCH OF WEI-CHYUNG WANG

Douglas J. Keenan, Informath, April 2009

(3) KAFKA AT ALBANY

Peter Risdon, Freeborn John, 15 March 2009

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(1) ALLEGATIONS OF FRAUD AT ALBANY - THE WANG CASE

Scientific Misconduct Blog, 2 May 2009

<http://scientific-misconduct.blogspot.com/2009/05/allegations-of-fraud-at-albany-wang.html>

Aubrey Blumsohn

Professor Wei-Chyung Wang is a star scientist in the Atmospheric Sciences Research Center at the University at Albany, New York. He is a key player in the climate change debate (see his self-description here). Wang has been accused of scientific fraud.

I have no inclination to "weigh in" on the topic of climate change. However the case involves issues of integrity that are at the very core of proper science. These issues are the same whether they are raised in a pharmaceutical clinical trial, in a basic science laboratory, by a climate change "denialist" or a "warmist". The case involves the hiding of data, access to data, and the proper description of "method" in science.

The case is also of interest because it provides yet another example of how *not* to create trust in a scientific misconduct investigation. It adds to the litany of cases suggesting that Universities cannot be allowed to investigate misconduct of their own star academics. The University response has so far been incoherent on its face.

Doug Keenan, the mathematician who raised the case of Wang is on the "denialist" side of the climate change debate. He maintains that "almost by itself, the withholding of their raw data by [climate] scientists tells us that they are not scientists".

Below is my own summary of the straightforward substance of this case. I wrote to Wei-Chyung Wang, to Lynn Videka (VP at Albany, responsible for the investigation), and to John H. Reilly (a lawyer at Albany) asking for any correction or comments on the details presented below. My request was acknowledged prior to publication, but no factual correction was suggested.

Case Summary

The allegations concern two publications. These are:

Jones P.D., Groisman P.Y., Coughlan M., Plummer N., Wang W.-C., Karl T.R. (1990), "Assessment of urbanization effects in time series of surface air temperature over land", *Nature*, 347: 169-172. (PDF here)

Wang W.-C., Zeng Z., Karl T.R. (1990), "Urban heat islands in China", *Geophysical Research Letters*, 17: 2377-2380. (PDF here)

The publications concern temperature at a variety of measuring stations over three decades (1954-1983). Stations are denoted by name or number. A potential confounder in such research is that measuring stations may be moved to different locations at different points in time. It is clearly important that readers of publications understand the methodology, and important confounders.

The publications make the following statements:

(Statement A) "The stations were selected on the basis of station history: we chose those with few, if any, changes in instrumentation, location or observation times." [Jones et al.]

(Statement B) "They were chosen based on station histories: selected stations have relatively few, if any, changes in instrumentation, location, or observation times." [Wang et al.]

The publications refer to a report produced jointly by the U.S. Department of Energy (DOE) and the Chinese Academy of Sciences (CAS) which details station moves, and the publications further suggest that stations with few if any moves or changes were selected on the basis of that report. However:

Of 84 stations that were selected, Keenan found that information about only 35 are available in the DOE/CAS report

Of those 35 stations at least half did have substantial moves (e.g. 25 km). One station had five different locations during 1954-1983 as far as 41 km apart.

It therefore appears that Statements A and B must be false. If false, readers would have been misled both in terms of the status of the stations and the manner in which they had been selected (or not selected).

Keenan then communicated with the author of one of the publications (Jones) to ask about the source of location information pertaining to the other 49 stations that had not been selected using the described methodology. Jones informed Keenan that his co-author Wang had selected those stations in urban and rural China based on his "extensive knowledge of those networks".

On 11 April 2007 Keenan E-mailed Wang, asking "How did you ensure the quality of the data?". Wang did not answer for several weeks, but on 30 April 2007 he replied as follows:

"The discussion with Ms. Zeng last week in Beijing have re-affirmed that she used the hard copies of station histories to make sure that the selected stations for the study of urban warming in China have relatively few, if any, changes in instrumentation, location, or observation times over the study period (1954-1983)"

Keenan points out that the "hard copies" to which Wang refers were not found by the authors of the DOE/CAS report, who had endeavored to be "comprehensive" (and that the DOE/CAS report was authored in part by Zeng, one of the co-authors on Wang). Keenan further notes that any form of comprehensive data covering these stations during the Cultural Revolution would be implausible.

In August 2007 Keenan submitted a report to the University at Albany, alleging fraud. Wang could at that stage have made the "hard copy" details of the stations selected available to the scientific community. However, he failed to do so.

In May 2008, the University at Albany wrote to Keenan that they had conducted an investigation and asked him to comment on it (see the rather odd letter). However they refused to show him the report of the investigation or any of the evidence to allow any comment (further odd letter).

In August 2008 the University sent Keenan an astonishing letter of "determination" stating that they did not find that Wang had fabricated data, but that they refused to provide any investigation report or any other information at all because "the Office of Research Integrity regulations preclude discussion of any information pertaining to this case with others who were not directly involved in the investigation".

Wang has still not made the station records available to the scientific community. If he provided such records to the University as part of a misconduct investigation, then the University has apparently concealed them.

Comments

In the absence of any explanation to the contrary, it seems that the methodology for station selection as described in these two publications was false and misleading.

Wang maintains that hard copy records do exist detailing the location of stations selected by himself outwith the published methodology. However the refusal to clarify "method" is inappropriate and a form of misconduct in and of itself. It does not lend credence to Wang's assertion that fraud did not take place. It would also be necessary to see records of stations that were not selected, in order to confirm that selection was indeed random, and only "on the basis of station history".

The University at Albany is in a difficult position.

If the University received such records as part of the supposed misconduct investigation, then they could easily resolve the problem by making them available to the scientific community and to readers.

If the University does not have such records then they have been complicit in misconduct and in coverup of misconduct.

If the University at Albany does have such records, but such records are not in accordance with the stated methodology of the publications, then the University has more serious difficulties.

"Investigations" of scientific misconduct should themselves align with the usual principles of scientific discourse (open discussion, honesty, transparency of method, public disclosure of evidence, open public analysis and public discussion and reasoning underlying any conclusion). This was not the case at the University at Albany. When you see universities reluctant to investigate things properly, it provides reasonable evidence that they really don't want to investigate things properly.

For further information on this case see here and here.

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(2) THE FRAUD ALLEGATION AGAINST SOME CLIMATIC RESEARCH OF WEI-CHYUNG WANG

Informath, April 2009
<http://www.informath.org/apprise/a5620.htm>

Douglas J. Keenan

Following are some remarks about my exposé, "The fraud allegation against some climatic research of Wei-Chyung Wang".

Wei-Chyung Wang is a professor at the University at Albany, State University of New York. He has been doing research on climate for over 30 years, and he has authored or co-authored more than 100 peer-reviewed scientific articles. He has also received an Appreciation Plaque from the Office of Science in the U.S.A., commending him, "For your insightful counsel and excellent science." The plaque resulted in particular from his research on global warming.

I have formally alleged that Wang committed fraud in important parts of his global-warming research. Below is a relevant timeline.

03 August 2007 My report, "Wei-Chyung Wang fabricated some scientific claims", is sent to the Vice President for Research at Wang's university.

31 August 2007 The university notifies me that it is initiating an inquiry into suspected research misconduct by Wang. (The notification includes a copy of the university's Policy and Procedures on Misconduct in Research and Scholarship.)

12 November 2007 My exposé on Wang's alleged fraud is published (reference below).

07 December 2007 Myself and the university's Inquiry Committee have a conference call.

20 February 2008 The university sends me the Report of the Inquiry Committee. The Committee unanimously concluded that "there was no data" (thus implicitly concluding Wang must have fabricated data) and that a full investigation should be undertaken.

23 May 2008 The university sends me a notice: the Investigation Committee has completed its work and found no evidence of fraud. The investigation was conducted without interviewing me, which is a violation of the university's policy. The university asks me to comment on the Committee's report; I am, however, not allowed to see the report.

04 June 2008 The university informs me that I am not allowed to see the report because they did not interview me when preparing it.

06 June 2008 I submit comments to the university, listing ways in which I believe the university has acted in breach of U.S. regulations and its own policy.

11 July 2008 I submit a complaint to the Public Integrity Bureau at the Office of the Attorney General of New York State, alleging criminal fraud.

12 August 2008 The university sends me the determination for its investigation, saying that there is "no evidence whatsoever [of] any research misconduct".

07 October 2008 I telephone the Public Integrity Bureau and am told that it might be some months before the Bureau begins to review the complaint.

17 March 2009 I telephone the Public Integrity Bureau and am told that the complaint is under review by an attorney.

18 March 2009 I file three requests under the Freedom of Information Law of New York State: for a copy of the full report by the Inquiry Committee; for a copy of the full report by the Investigation Committee; and, given that the relevant federal funding agencies are required to be notified when a misconduct investigation is initiated, for copies of all such notifications that were sent by the university and pertain to the investigation of Wang.

24 March 2009 Given that Wang received funding for the fraudulent research from the U.S. Department of Energy (DOE) and that the DOE has since supplied more funding to Wang, I report the fraud and the university's apparent cover up to the Office of Inspector General at the DOE.

This web page will be updated with news about the case, as the investigations progress.

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(3) KAFKA AT ALBANY

Freeborn John, 15 March 2009

<http://freebornjohn.blogspot.com/2009/03/kafka-at-albany.html>

Peter Risdon

Last June I reported on the allegations of academic fraud levelled by a British mathematician, Doug Keenan, against Professor Wei-Chyung Wang of New York State University at Albany.

Dr Keenan alleged that in work that has come to be widely cited in climate studies, work that included the collation of data from temperature measuring stations in China, Professor Wang made statements that "cannot be true and could not be in error by accident. The statements are fabricated."

In August 2007, Dr Keenan submitted a report (pdf) of his allegations to the Vice President for Research at Wang's university and an inquiry was initiated. In February 2008 this was escalated into a full investigation by the Inquiry Committee.

All this was summarised in my earlier post, together with quotations from Dr Keenan's allegation.

So far, things had run as might be expected. A fraud had been alleged, the University at Albany looked into it and decided to hold a formal investigation. Dr Keenan waited to be contacted by the investigation and asked to put his case, in line with the university's Policy and Procedures on Misconduct in Research and Scholarship (.doc). The relevant section of this document runs as follows (emphasis added):

III. A. Rights and Responsibilities of the Complainant

Rights: The Vice President for Research will make every effort to ensure the privacy and confidentiality of complainants. The University will protect, to the maximum extent possible, the position and the reputation of those who in good faith report alleged misconduct in research.

The Vice President for Research will work to ensure that complainants will not be retaliated against in the terms and conditions of their employment or other status at the University and will review instances of alleged retaliation for appropriate action. Any alleged or apparent retaliation should be reported immediately to the Vice President for Research.

The complainant will be provided a copy of the formal allegations when and if an inquiry is opened. The complainant will have the opportunity to review portions of the inquiry and investigation reports pertinent to the complainant's report or testimony, and will be informed in writing of the results of the inquiry and investigation, and of the final determination. After the final determination and upon request to the Vice

President for Research, the complainant shall be given access to the full documentation.

Responsibilities: The complainant is responsible for making allegations in good faith, maintaining confidentiality, and cooperating fully with an inquiry and/or investigation.

Dr Keenan lived up to the responsibility as stated in the final paragraph above so far as he could. He had made the allegation in good faith and given Professor Wang an opportunity to explain how he had reached his results, an opportunity the Professor had not taken. Keenan maintained confidentiality. In order to cooperate with the investigation, though he would first have to be contacted by it. Dr Keenan waited.

Late in May 2008 a communication arrived from Albany. It said:

After careful review of the evidence and thoughtful deliberation, the Investigation Committee finds no evidence of the alleged fabrication of results and nothing that rises to the level of research misconduct having been committed by DR. Wang.

As the institutional official responsible for this case, I have accepted the Committee's findings and the Report. You have fourteen (14) calendar days from the date of this letter to provide any comments to add to the report for the record.

Contrary to its own rules, the Committee had not given Keenan the opportunity to "review portions of the inquiry and investigation reports".

That's astonishing, but here's where it becomes Kafkaesque. Keenan was being asked, in this most recent communication, to comment on the report of the Committee. But he was not sent a copy of the report. When he challenged this, he received an email from Adrienne Bonilla explaining that:

[Keenan] did not receive a copy of the Investigation report because the report did not include portions addressing your role and opinions in the investigation phase.

Per the UAlbany Misconduct policy:

VI. E. Investigation Report and Recommendations of the Vice President for Research

"...The Vice President for Research will provide the respondent with a copy of the draft investigation report for comment and rebuttal and will provide the complainant with those portions of the draft report that address the complainant's role and opinions in the investigation. The respondent and complainant will be given 14 calendar days from the transmission of the report to provide their written comments. Any written responses to the report by either party will be made part of the report and record.

Keenan then wrote to the Vice President for Research at Albany, Lynn Videka, pointing out the various ways in which the University had breached its own policy, stating that its behaviour was consistent with a cover up, and pointing out that Professor Wang has received more than \$7 million in grants from a couple of US federal agencies.

In August 2008, Lynn Videka wrote to Keenan enclosing a final copy of a "determination" of the investigation. In her covering note, she stated:

I am notifying you of the case outcome because you were the complainant in this case. The University's misconduct policies and the Office of Research Integrity regulations preclude discussion of any information pertaining to this case with others who were not directly involved in the investigation.

To summarise, the university initiated an investigation, then broke its own rules by not involving Dr Keenan. It then produced a report that carefully avoided mentioning Dr Keenan, so it could claim he was not entitled to see a copy of this report. It then asked Keenan to comment on the report. It has completely disregarded its own policy that "After the final determination and upon request to the Vice President for Research, the complainant shall be given access to the full documentation."

But Doug Keenan is a tenacious man. In July 2008, after being refused sight of the report, he submitted a formal complaint (pdf) to the Public Integrity Bureau at the Office of the Attorney General of New York State, alleging criminal fraud. In this complaint, he said:

Wei-Chyung Wang is a professor at the University at Albany, State University of New York. He has been doing research for over 30 years. For this research, Wang has received at least \$7 million. The funds have come primarily from the Department of Energy, with additional funding from other federal agencies (DOD, FAA, NSF). I have formally alleged that Wang committed fraud in important parts of his research. My allegation was submitted to the University at Albany; a copy is enclosed.

The university conducted a preliminary inquiry; a copy of the report from the inquiry is enclosed (redacted, by the university). Briefly, Wang claimed that there were some documents that could exonerate him. The inquiry concluded that there should be a full investigation, which should be charged with obtaining and reviewing any such additional evidence ... so that a final resolution may be made regarding the allegation against Dr. Wang.

Wang had been claiming the existence of such exonerating documents for nearly a year, but he has not been able to produce them. Additionally, there was a report published in 1991 (with a second version in 1997) explicitly stating that no such documents exist. Moreover, the report was published as part of the Department of Energy Carbon Dioxide Research Program, and Wang was the Chief Scientist of that program.

The university conducted an investigation. The investigation concluded that Wang is innocent. I believe that the case against Wang is strong and clear, and that the university is trying to cover up the fraud so as to

protect its reputation. Wang is one of the university's star professors. The conduct of the investigation violated several of the university's own stated policies: details are given in an attached e-mail (dated 06 June 2008).

The e-mail was sent to Lynn Videka, Vice President for Research at the university: Videka was in charge of overseeing the investigation. Note, in particular, that the documents that Wang was relying on were never produced.

I have only examined a little of Wang's research; so I do not know the full extent of the fraud. It is difficult to examine more in part because Wang has not willingly made his data available: when asked for the data from the research that I later reported as fraudulent, Wang refused. For that research, though, Wang had a co-worker in Britain. In Britain, the Freedom of Information Act requires that data from publicly-funded research be made available. I was able to get the data by requiring Wang's co-worker to release it, under British law. It was only then that I was able to confirm that Wang had committed fraud. Details are given in my report to the university (page 4, last paragraph). I would be willing to help examine other research that Wang has done, if more data were made available.

There was another case of research fraud with a professor at the University of Vermont, in 2005. There, Prof. Eric Poehlman was convicted of making false statements on federal grant applications; he was sentenced to a year and a day in prison. Wang has done the same as Poehlman. The fraudulent work described in my report dates from 1990; Wang has been relying on that work in some of his grant applications since then. As I understand things, each of those applications is a violation of statute. (Additionally, Wang has been using the grants to go on frequent trips to China.)

In October 2008 Dr Keenan was told there could be a wait of several months while his complaint is investigated.

I'll let you know when there are any further developments.

UPDATE: I didn't mention this in the main piece above, but I did mail the relevant person at Albany myself, some time ago, asking for news of the investigation against Professor Wang. I received no reply.

However, within a couple of hours of this being posted, someone at Albany came to look at it, from the host `aspmi-cc326.cc.albany.edu` (169.226.172.35), having apparently been sent an email about it.

So even if they are not communicative about this case, it seems someone at Albany is keeping their eyes open for reports of it.

UPDATE: On reflection, the hit from Albany is also consistent with someone using Google Alerts to monitor coverage of this issue.

UPDATE: Doug Keenan has been told on the telephone that this case is now under review by an attorney at the OAG Public Integrity Bureau.

UPDATE: Also see new findings on the effect of urban warming.

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<http://www.staff.livjm.ac.uk/spsbpeis/>

From: "peter.thorne" <peter.thorne@metoffice.gov.uk>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: CRUTEM4
Date: Tue, 12 May 2009 08:54:44 +0100

Phil,

there may be some money this FY, substantial sums. Management here are casting around for ideas. As its to be spent this FY its largely going to be consultant work as we never have a cats chance in hell of recruiting on that timescale. What resource do you think we could contract from CRU (you, Harry, others?) for doing a CRUTEM4 which I would maintain had two aims ...

1. Rescue and incorporation of recent data (I'm pinging NCDC too to see what they could do vis-a-vis collating and sending the non-wmo US stations and other data you may not have ... their bi-lats may have sig. extra stations for Iran, Aus, Canada etc.)

2. A more robust error model that led to production of a set of equi-probable potential gridded products (HadSST3 will do simmilarly so we could combine to form HadCRUT4 equi-probable). This error model determination would ideally be modular so that we could assess how wrong our assumptions about the error would have to be to "matter" and what error sources are important for our ability to characterise the long-term trend (trivially these will be the red noise I know but then most people seem blind to the trivial sadly ...). The HadCRUT3 paper clearly started well down that path but a recent paper I had the displeasure of reviewing on my way back from WMO shows its poorly understood (deliberately so in this particular case ...).

We have a meeting Thursday. If it passes muster there we'll put it to DECC and see what happens. No promises.

This would mean we'd have HadCRUT4 which would be HadSST3 + CRUTEM4 each with more data and better error models well before AR5 which seems sensible ...

Mr. Fraudit never goes away does he? How often has he been told that we don't have permission? Ho hum. Oh, I heard that fraudit's Santer et al comment got rejected. That'll brighten your day at least a teensy bit?

Peter

--

Peter Thorne Climate Research Scientist
Met Office Hadley Centre, FitzRoy Road, Exeter, EX1 3PB
tel. +44 1392 886552 fax +44 1392 885681
www.metoffice.gov.uk/hadobs

From: "peter.thorne" <peter.thorne@metoffice.gov.uk>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: CRUTEM4
Date: Tue, 12 May 2009 09:53:11 +0100

Phil,

I can't believe that people think it remotely reasonable behaviour to send that sort of crud. They'd never say that to your face. I guess their home is just that much more cosy and impersonal.

Cash would need spending in FY09/10 as I understand it, but someone for six months (assuming they could start this Sept.) could be a route forwards. It would be a good paper for them career-wise.

HadSST3 is in first draft form. I'm not sure what papers you assume will arise. I think we were thinking of developing HadSST3 and CRUTEM4 seperately (but in a joined up way) and publishing as separate papers and then doing a paper that covers combination to HadCRUT4 and perhaps, for example, a d&a sensitivity to error model assumptions.

Peter

On Tue, 2009-05-12 at 09:43 +0100, Phil Jones wrote:

- > Peter,
- > Below is one of three emails I got last night following a new thread on CA.
- > I'll ignore them and wait for the FOI requests, which we have dealt
- > with before.
- > I did send an email to Thomas Stocker alerting him up to comment #17.
- > These are all about who changed what in various chapters of AR4. I
- > expect these
- > to get worse with AR5.
- >
- > Anyway back to the matter in hand.
- >
- > I'm planning to come down to see Ian Simpson (probably on June
- > 1). I'll get back
- > to David on this later today.
- > We've done some of what you aim for. We've sorted out the new Canadian
- > WMO numbers and have extra data for Australia and NZ in. Australia comes in
- > by email once a month. I'll have to find a new contact in NZ now
- > Jim Salinger has

> been sacked - but it's only a small country. Iran is pretty good.
> The US is the large bit of work. The US already has better
> station density than
> almost anywhere else, so the effort won't make much difference. But
> it is probably
> worth doing, as it would reduce errors - even if no-one understands
> them. Glad
> you got the poor paper to review!
> Soon we will be adding data for the Greater Alpine Region (32 sites) which
> go back to 1760. These data all have adjustments for screen issues prior to
> about 1880. This makes summers cooler by about 0.4 deg C and winters about
> the same. Similarly, we will also add a load of stations for Spain
> (again with Screen
> biases in). There is probably more we could add for European countries,
> but again it is likely to make little difference, except to lower errors.
> The real issue is South America and Africa. We have the whole
> Argentine network,
> but this is only digitized back to 1959 and the data we had wasn't
> that bad anyway.
> Problem in South America is Brazil. Africa is OK in a few
> countries, but poor in many.
> We could add loads in China.
> Issue with all this is that most of the additions wouldn't be
> available from whenever
> we stop. We can probably do the US in real time like Australia.
> We've also been trying to add in the precip for many of these
> extra stations (not
> the Alpine countries and Spain).
> There is a timing issue. As I understand HadSST3 won't be
> available to be merged
> with until it is successfully reviewed. So need to consider this as well.
>
> A final issue is people here. We're OK for most of 2010 for all.
> We have a good
> student finishing a PhD by Sept who wants to stay, so couldn't
> really do anything
> till then.
>
> Cheers
> Phil
>
>
> Dear Mr Jones

>
> As a UK tax payer from the productive economy, could you please
> explain why you restrict access to data sets that are gathered using
> tax payer funds e.g. CRUTEM3. Can you believe how embarassing this is
> to a UK TAX PAYER, putting up with your amateurish non disclosure of
> enviromental information.

>
> For reference <http://www.climateaudit.org/?p=5962> refers to your
> absymal attitude to public data, although this is just the latest in
> an embarassing set of reasonable requests from CRU, who the hell do
> you think you are? There will of course be an FOI on the back of this

> Regards

> Ian

>
>
> At 08:54 12/05/2009, peter.thorne wrote:

> >Phil,

> >
> >there may be some money this FY, substantial sums. Management here are
> >casting around for ideas. As its to be spent this FY its largely going
> >to be consultant work as we never have a cats chance in hell of
> >recruiting on that timescale. What resource do you think we could
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>>Peter
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From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: nomination: materials needed!
Date: Tue, 19 May 2009 12:12:55 -0400

thanks much Phil,

that sounds good. So why don't we wait until next round (June '10) on this then. That will give everyone an opportunity to get their ducks in a row. Plus I'll have one more Nature and one more Science paper on my resume by then (more about that soon!). I'll be sure to send you a reminder sometime next may or so!

Thanks for sending that paper. It takes some work to get a paper rejected by IJC. Want to take a bet that some version of this appears in "Energy and Environment"? Of course, any paper that appears there is not taken seriously anyway, its almost a joke.

The contrarians attacks certainly have not abated. The only hope is that they'll increasingly be ignored.

talk to you later,

mike

On May 19, 2009, at 9:03 AM, Phil Jones wrote:

Mike,

Have gotten replies - the're both happy to write supporting letters, but both are too busy to take it on this year. One suggested waiting till next year. Malcolm is supporting one other person this year. I'd be happy to do it next year, so I can pace it over a longer period. Malcom also said that Singer had an AGU Fellowship!!

Apart from my meetings I have skeptics on my back - still, can't seem to get rid of them. Also the new UK climate scenarios are giving govt ministers the jitters as they don't want to appear stupid when they introduce them (late June?).

Talking of skeptics - the attached was rejected by IJC. He put it up on something xarchiv. Easy to see why it was rejected. Parts appear quite well written, but they always go too far. Obviously have no idea how to write a paper.

Cheers

Phil

At 14:35 18/05/2009, you wrote:

thanks much Phil,
hopefully will see you before Vienna, but if not, I look forward to seeing you there
next year,
talk to you later,
mike

On May 18, 2009, at 9:28 AM, Phil Jones wrote:

Mike,

I'll email Ray and Malcolm. I'd be happy to contribute. Away all next week
and another couple of weeks in June.

EGU will be in Vienna again. It is set for May 2-7, 2010.

It will also be Vienna in 2011.

Cheers

Phil

At 22:31 16/05/2009, you wrote:

Hey Phil,

I hope all is well w/ you these days. Been a while since I've actually seen you. Perhaps
can convince you to make it to EGU next year? Looks like it will be in Vienna again. I
rather enjoyed this one, and I think I may go back next year.

On a completely unrelated note, I was wondering if you, perhaps in tandem w/ some of the
other usual suspects, might be interested in returning the favor this year ;)

I've looked over the current list of AGU fellows, and it seems to me that there are
quite a few who have gotten in (e.g. Kurt Cuffey, Amy Clement, and many others) who
aren't as far along as me in their careers, so I think I ought to be a strong candidate.

anyway, I don't want to pressure you in any way, but if you think you'd be willing to
help organize, I would naturally be much obliged. Perhaps you could convince Ray or
Malcolm to take the lead? The deadline looks as if it is again July 1 this year.

looking forward to catching up w/ you sometime soon, probably at some exotic location of
Henry's choosing ;)

mike

Prof. Phil Jones

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website: [3]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[4]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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"Dire Predictions" book site:

[8]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. <mailto:p.jones@uea.ac.uk>

2. <mailto:mann@psu.edu>

3. <http://www.meteo.psu.edu/~mann/Mann/index.html>

4. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

5. <mailto:p.jones@uea.ac.uk>

6. <mailto:mann@psu.edu>

7. <http://www.meteo.psu.edu/~mann/Mann/index.html>

8. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

9. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Gifford Miller <gmiller@colorado.edu>
To: Darrell Kaufman <Darrell.Kaufman@nau.edu>
Subject: Re: Fwd: Your Science manuscript 1173983 at revision
Date: Tue, 26 May 2009 16:23:05 -0400
Cc: David Schneider <dschneid@ucar.edu>, Nick McKay
<nmckay@email.arizona.edu>, Caspar Ammann <ammann@ucar.edu>,
Bradley Ray <rbradley@geo.umass.edu>, Keith Briffa
<k.briffa@uea.ac.uk>, Miller Giff <gmiller@colorado.edu>, Otto-Bleisner
Bette <ottobli@ucar.edu>, Overpeck Jonathan <jto@u.arizona.edu>

<x-flowed>

Darrell (from AGU Toronto):

Great news from Science!

A quick comment on Amplification and signal to noise issues (comment 1 below). It think you meant that the referee felt that Arctic amplification did not translate to a more robust signal because the noise would be equally amplified. I don't know that we can challenge the "climate noise" but we can make the case that the "proxy noise", that is, the uncertainty in proxy calibration, is, as far as I know, the same in the Arctic as in lower latitudes. Consequently, the larger temperature signal expected in the Arctic can be more reliably detected by our proxies because it is more likely to exceed the sensitivity limits of our proxies. If we assume the "climate noise" is more or less gaussian, then we should be better able to detect the relatively subtle temp changes of the Holocene in the Arctic than elsewhere.

Giff

>Co-authors:

>I just received the reviewers' comments and editor's decision on our
>SCIENCE manuscript (attached). The decision isn't final, but it
>looks like good news, with very reasonable revisions. Reviewer #1
>had nothing substantial to suggest. Reviewer #2 was rather thorough.
>I think I can address his/her suggestions but could use some help
>with three:

>

>(1) The reviewer challenged our assertion that, because climate
>change is amplified in the Arctic, the signal:noise ratio should be
>higher too. We don't have more than 1 sentence to expand on the
>assertion in the text. We could plead the case to editor and hope
>that it doesn't trip up the final acceptance, or we could omit it
>from the text. Suggestions?

>

>(2) The reviewer suggested that, if we are concerned about outliers
>influencing the mean values of the composite record, we should
>attempt a so-called "robust" regression procedure, such as median
>absolute deviation regression. Does anyone have experience with this?

>
>(3) The reviewer was concerned that we overestimated the strength of
>the relation between temperature and insolation in the long CCSM
>simulation. Namely s/he criticized the leveraging effect of the one
>outlier in the model-generated insolation vs temperature plot (Fig.
>4b), and suggested that we use 10-year means instead of 50 year.
>Dave: you up for this, please?
>
>Please forward any input to me and I'll compile them, and let you
>all have a look before I submit the final revisions. I'm hoping we
>can turn this around this week.
>
>Thanks.
>Darrell
>

--
Gifford H. Miller, Professor
INSTAAR and Geological Sciences
University of Colorado at Boulder
</x-flowed>

From: Eystein Jansen <Eystein.Jansen@geo.uib.no>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: AR5
Date: Wed, 27 May 2009 09:57:14 +0200
Cc: Jonathan Overpeck <jto@email.arizona.edu>

<x-flowed>

Hi Keith,

Nice to hear from you, and sorry to hear about your mother.

Contrary to what I heard a few days ago, I received yesterday the invitation to the Scoping meeting in July and look forward to be joining Peck in providing the paleo-input to the scoping of the report. On the issue of a separate chapter I agree that this option is most practical, yet I don't think there is solid support for that avenue, and fear that it will not be pursued. This means that there is a danger that single paleo-persons distributed into the chapters might become marginalised, and would need some x-chapter support at a time to deal with the issues, and also to provide enough breadth of knowledge about paleo-literature to be assessed. The Plan B option would require that there is a mechanism that pulls together the combined paleo-competence amongst the LAs during the writing of AR5. I think there will be a strong emphasis in AR5 on regional changes, and on climate predictions. Predictions need a strong basis in knowledge about natural modes of variability and the interplay between natural and man made changes on the regional scale, areas where the paleo perspectives are clearly needed and contribute. I think many new results are emerging, and we need to be on top of this to make the case in Venice.

It would be great if you could, within the next week preferably, send us a list of what new results you think will be good to use for the scoping.

Cheers
Eystein

Den 21. mai. 2009 kl. 18.36 skrev Jonathan Overpeck:

> Hi Keith - thanks. Sorry to hear about your Mother.

>
> I think the invites have gone out for Venice, and so far the only
> one from
> AR4 Chap 6 going is me - or rather, I haven't heard from anyone else.
> Eystein isn't going since Norway has a bunch from the other WGs. Seems
> "representation" isn't working in our favor. I would really like
> more there,
> and I fear that if it's just me, it's another sign that paleo won't
> be a
> chapter since I can't be CLA again of such a chapter (fortunately
> for my
> family!). Based on limited discussions w/ Thomas, I also get the
> sense of a
> paleo chapter might be an uphill battle, but on the other hand, a
> conservative approach would be to stick close to the AR4 outline.
> That said,
> it appears that the gov's are pushing even harder for more regional,
> so...
>
> Your list is a big help, and I wonder if you could arm us with some
> good
> graphics where you can on these issues, especially the latest on
>
> Paleo model evaluation - showing what the models can and can't do. Of
> course, the non-paleo folks like to argue that if their is mismatch,
> it's
> the paleo data, but with the right results and presentation, that
> can be
> overcome. Need some compelling graphics that are post AR4 - if there
> are
> papers or manuscripts that's even better, but even if not at that
> stage.
>
> I'm going to guess that Gabi will be there (do you know?) and will
> do the
> sensitivity part. But, if you know of new stuff, pls send also.
>
> Your regional idea is a good one - want to share some compelling
> examples of
> where paleo (more than one proxy always good) is informing the full
> range of
> variability in specific regions, and illustrating ca last 50 years
> vs the

> longer record. I can think of some good examples, but you might have
> some
> recent ones I haven't seen.
>
> Wegan followup - should I ask Caspar? I haven't heard anything, but
> it would
> be good...
>
> Hydrologic fits well with regional, so I think I'd emphasize it,
> although
> some temp would be good too. More on extremes? Anything out there
> that's new
> and compelling?
>
> This is just a scoping mtg, so only a small subset of those who will
> be
> involved. You need to get your gov to push you once the chapter
> outline is
> decided (i.e., you get nominated for specific roles in specific
> chapters -
> or at least that is how it worked before - suspect you know the
> drill).
>
> I'm guessing that if there is no paleo chapter, then the backup will
> be to
> have strong paleo (at least a person) in relevant chapters, with a
> cross-cutting paleo caucus or something so that the paleo Las across
> the AR5
> can work together to ensure there is consensus on things and that
> the parts
> make up a coherent and compelling whole. But, I'll be pushing for a
> chapter
> since that is clearly the best outcome. Need those compelling
> examples to
> make it work - need to show it's too much great stuff to be sprinkled
> throughout other chapters.
>
> Thanks again, Peck
>
>
> On 5/21/09 7:43 AM, "Keith Briffa" <k.briffa@uea.ac.uk> wrote:
>
>> Hi Peck and Eystein

>> sorry have not responded to recent emails re Palaeo stuff in next
>> IPCC assessment - have been away from the Unit and email because of
>> the death of my mother and ensuing issues. I simply would add that in
>> terms of pure pragmatism , efficiently stitching in Paleodata into
>> separate chapters is likely to be impractical - a self-standing
>> chapter - even of restricted length would be more feasibly achieved.
>> In terms of specific issues , top of my list would be model
>> validation progress , and a description of where we are in attempts
>> to constrain estimates of climate sensitivity with the use of
>> palaeodata - covered I know in Gab's chapter last time. Updating the
>> high-resolution work would have to be in there for continuity but
>> perhaps with an attempt to assess specific regional changes , and
>> between-proxy comparisons. If completed , "the big challenge" work
>> that arose from the Wengen meeting would be good. Then "new" data -
>> e.g. new proxies or areas not covered before - with much more on
>> hydrologic change. I agree about the inclusion of less-resolved
>> proxies. Finally, the "important issues we highlighted at the end of
>> the AR4 chapter should be reviewed and the issues updated.
>> Do you know whether the list for the scoping meeting in Venice has
>> been selected - if I have not been invited does this mean I will
>> not be?

>>

>> cheers

>> Keith

>>

>>

>>

>> --

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>>

>

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</x-flowed>

From: Jonathan Overpeck <jto@email.arizona.edu>
To: Darrell Kaufman <Darrell.Kaufman@nau.edu>, David Schneider
<dschneid@ucar.edu>, Nick McKay <nmckay@email.arizona.edu>, Caspar Ammann
<ammann@ucar.edu>, Bradley Ray <rbradley@geo.umass.edu>, Keith Briffa
<k.briffa@uea.ac.uk>, Miller Giff <gmiller@Colorado.EDU>, Otto-Bleisner
Bette <ottobli@ucar.edu>, Jonathan Overpeck <jto@u.arizona.edu>
Subject: Re: Your Science manuscript 1173983 at revision
Date: Thu, 28 May 2009 12:22:57 -0700

Hi Darrell et al - got a chance to read the paper and comments enroute
to Atlanta. Here's
some feedback..

General - comments are modest and should be easy to accommodate. That
said, I think we have
to take the comments of Rev 2 seriously. I'm guessing that its Francis
Zwiers and in any
case, he knows what he's talking about regarding stats.

Also - IMPORTANT - I'd make sure we check and recheck every single
calculation and dataset.

This paper is going to get the attention of the skeptics and they are
going to get all the

data and work hard to show were we messed up. We don't want this -
especially you, since it
could take way more of your time than you'd like, and it'll look bad.
VERY much worth the
effort in advance.

Ok Rev 1 - wow - never had it so good.

Rev 2

General comment - we should take this one seriously. Get Caspar and
Bette's help. The new

synthesis could be telling us (especially when the outlier in Fig 4B
is discounted - see

below) that the Arctic is, in reality, more sensitive to changes in
radiative forcing than

reflected in the model. Are there other experiments or reasons to
think this is true? If

so, let's make this point and back it up with these other pieces of
evidence. For example,

does the CCSM get Arctic warming from the earl/mid Holocene to present
correctly? Does the

model underestimate the Arctic change obs over the last 100 years.
Since the reviewer

raised this, you could add some refs and prose if needed to respond.
Not a lot, but some.

And, we need to respond one way or the other.

Specific comments

1. agree, in the abstract, I suggest changing the sentence to read
"This trend likely

reflects a steady orbitally-driven reduction in summer insolation,
as confirmed by a

1000-year transient climate simulation." Note that this removes
more than enough words

to meet the

eds requirement too.

2. for this one, I'd simply state that the forcing is stronger in the Arctic than at lower

lats (double check how much) and also add what Giff suggested.

3. agree, make the suggested clarification

4. important (!) and hopefully easy. I leave to whomever did the calculation to make sure

any serial correlation bias was taken into account. Make sure all p values are thus

corrected.

5. ditto, makes sense too

6. clarify

7. this reviewer knows what he/she is talking about - do what they suggest, and double check it's done well.

8. Don't delete the para. Instead point out that you've strengthened it and that it is

important to place the new synthesis in a longer term Holocene context. It also clarifies

to interdisciplinary readers why the Arctic is so sensitive (perhaps more sensitive than in

models? - see above). That said, I would cite Kerwin et al 99 - I've attached it. It

provides added detail and balance. Also, since you're responding to a reviewer comment and

strengthening the ms, you can add the ref w/o hassle (or so I'm guessing on recent experience).

9. yep, delete all "attribution"s in the ms. On p 6, lone 129, can say "...support the

connection between the Arctic summer cooling trend and a orbitally-driven reduction..."

10) reviewer is correct - see my response above for the general comment, and see if you can

work with his/her ideas to improve. The outlier has to be just that?! Need an explanation

before you can remove from any analysis, however.

11) makes sense - do it

12) yep - change text as suggested

13) agree, change p 7, line 153 to read "...1980s appears to have been the single..."

14) agree, change line 167 on p 8 to read "...trend. Our new synthesis suggests that the

most recent 10-year..."

Other suggested changes....

P. 3 line 69 - change region to read regional

P 6 line 128 - "(-2600 to -1600AD) isn't going to make sense to readers. Please provide

some context - SOM or ??

P 7 line 145 - insert "Arctic" before "summer"

P. 11 line 234 change to read "...century. Ten-year means (bold lines) were used..."

Because you don't really say what the bold and unbold lines are - this will help the reader

make sure they have it right.

Fig 4 and caption - need to explain why the isolation axes are labeled differently - the

numbers, and that both are still cover the same number of Wm-2.

Didn't look at SOM, but make sure it's all bomber too, since there is a good chance it will

get PICKED apart, and any errors thrown back in our face in a counter productive manner.

Thanks! Nice job. Best, Peck (probably w/o email for a while in the Amazon, although one never knows...)

On 5/26/09 1:08 PM, "Darrell Kaufman" <[1]Darrell.Kaufman@nau.edu> wrote:

Co-authors:

I just received the reviewers' comments and editor's decision on our SCIENCE manuscript

(attached). The decision isn't final, but it looks like good news, with very reasonable

revisions. Reviewer #1 had nothing substantial to suggest. Reviewer #2 was rather

thorough. I think I can address his/her suggestions but could use some help with three:

(1) The reviewer challenged our assertion that, because climate change is amplified in

the Arctic, the signal:noise ratio should be higher too. We don't have more than 1

sentence to expand on the assertion in the text. We could plead the case to editor and

hope that it doesn't trip up the final acceptance, or we could omit it from the text.

Suggestions?

(2) The reviewer suggested that, if we are concerned about outliers influencing the mean

values of the composite record, we should attempt a so-called "robust" regression

procedure, such as median absolute deviation regression. Does anyone have experience

with this?

(3) The reviewer was concerned that we overestimated the strength of the relation

between temperature and insolation in the long CCSM simulation.

Namely s/he criticized

the leveraging effect of the one outlier in the model-generated insolation vs

temperature plot (Fig. 4b), and suggested that we use 10-year means instead of 50 year.

Dave: you up for this, please?

Please forward any input to me and I'll compile them, and let you all have a look before

I submit the final revisions. I'm hoping we can turn this around this week.

Thanks.

Darrell

Begin forwarded message:

From: Lisa Johnson <[2]ljohnson@aaas.org>
Date: May 26, 2009 12:25:40 PM GMT-07:00
To: Darrell S Kaufman <[3]Darrell.Kaufman@nau.edu>
Subject: Your Science manuscript 1173983 at revision

26 May 2009

Dr. Darrell S Kaufman
Department of Geology
Frier Hall Knoles Dr
Northern Arizona University
Box 4099
Flagstaff, AZ 86011
UserID: 1173983
Password: 307923

Dear Dr. Kaufman:

Thank you for sending us your manuscript "Recent Warming Reverses Long-Term Arctic Cooling." We are interested in publishing the paper as a Report, but we cannot accept it in its present form. Please revise your manuscript in accord with the referees' comments (pasted below) and as indicated on the attached editorial checklist and marked manuscript. I have also made some suggestions regarding shortening and clarification directly on the manuscript. Because of the nature of the reviewers' comments and revisions required, we may send the revised manuscript back for further review.

Please return your revised manuscript with a cover letter describing your response to the referees' comments. We prefer to receive your revision electronically via our WWW site ([4]<http://www.submit2science.org/revisionupload/>) using the User information above. In your letter, please also include your travel schedule for the next several weeks so we can contact you if necessary. The revised manuscript must reach us within four weeks if we are to preserve your original submission date; if you cannot meet this deadline, please let us know as soon as possible when we can expect the revision.

The cost of color illustrations is \$650 for the first color figure and \$450 for each

additional color figure. In addition there is a comparable charge for use of color in reprints. We ask that you submit your payment with your reprint order, which you will receive with your galley proofs. We also now provide a free electronic reprint service; information will be sent by email immediately after your paper is published in Science Online.

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I look forward to receiving your revised manuscript. Please let me know if I can be of assistance.

Please let me know that you have received this email and can read the attached files.

Sincerely,

Jesse Smith, Ph.D.
Senior Editor

[cid:3326358178_1079548]

[cid:3326358178_1100494]

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Embedded Content: image.png: 00000001,3e910253,00000000,00000000

Embedded Content:

image1.png: 00000001,35902c45,00000000,00000000 Attachment Converted:
"c:\eudora\attach\kerwin_et_al&role&1999.pdf"

References

1. file:///localhost/tmp/Darrell.Kaufman@nau.edu
2. file:///localhost/tmp/ljohnson@aaaas.org
3. file:///localhost/tmp/Darrell.Kaufman@nau.edu
4. <http://www.submit2science.org/revisionupload/>
5. file:///localhost/tmp/jto@u.arizona.edu
6. file:///localhost/tmp/regalado@email.arizona.edu

From: David Schneider <dschneid@ucar.edu>
To: Darrell Kaufman <Darrell.Kaufman@nau.edu>
Subject: Re: spatial pattern
Date: Wed, 3 Jun 2009 18:23:38 -0600
Cc: Nick McKay <nmckay@email.arizona.edu>, Caspar Ammann
<ammann@ucar.edu>, Bradley Ray <rbradley@geo.umass.edu>, Keith Briffa
<k.briffa@uea.ac.uk>, Miller Giff <gmiller@colorado.edu>, Otto-Bleisner
Bette <ottobli@ucar.edu>, Overpeck Jonathan <jto@u.arizona.edu>, Bo
Vinther <bo@gfy.ku.dk>

I don't think we should go there. Any PC analysis on proxy data will be picked apart by the skeptics, even if it yields some useful insight, and I don't recall there being anything too exciting in the pattern given the limited amount of data.

Dave

On Wed, Jun 3, 2009 at 5:42 PM, Darrell Kaufman
<[1]Darrell.Kaufman@nau.edu> wrote:

Dave and Nick:

I've been thinking about the remaining holes in the manuscript. Spatial patterns are important. At one point we explored the spatial pattern of the PC scores. I think it would be good to bring this up in the SOM. I could make a dot map showing the site locations and their correlations with PC1. The upshot would be that the proxy types are not uniformly distributed, and there are too few records to discern any spatial patterns from any geographical or proxy-type bias (e.g., high-elevation ice cores).
Thoughts?
Darrell

References

1. <mailto:Darrell.Kaufman@nau.edu>

From: Phil Jones <p.jones@uea.ac.uk>
To: adrian.simmons@ecmwf.int, Dick Dee <Dick.De@ecmwf.int>
Subject: Re: [Fwd: 2009JD012442 (Editor - Steve Ghan): Decision Letter]
Date: Tue Jun 23 12:18:29 2009
Cc: "Willett, Kate" <kate.willett@metoffice.gov.uk>, Peter Thorne <peter.w.thorne@googlemail.com>

Adrian,

Emails to Kate yesterday were returned by the ECMWF server (for your email address) but not for Dick's?

I also found the two emails you sent last night in my spam list. No idea why this is happening. I found some other semi-important emails in my spam as well!

Anyway - hope you get this email!

All three reviewers are positive, which is good, but there is still a lot of to do as you say.

Here are some initial thoughts. Before I begin - it seems as though Rev 2 comments have ended abruptly during #13. I'd suggest you ask if there is any more?

Rev 1

I would have thought that the second point (larger trends in full ERA-INTERIM fields) was just an interesting aside, and not as important as the RH decline.

I'll need to go back to see if sections 5 and 6 can be reordered/restructured?

Both Reviewers 1 and 2 (they appear to be Kevin and Aiguo, but odd to have two people who only live a few rooms apart!) make quite a few statements about GPCC. We're doing updating work on the higher resolution CRU-TS (0.5 by 0.5 degree lat/long) datasets. We're doing comparisons with GPCC and for the Giorgi type regions (as in Fig 3.14 of Ch 3 of AR4) and the agreement is amazingly good. Maybe all you need to point to is this Figure and the previous one (Fig 3.12) to say that for land regions at the continental scale, it doesn't matter which datasets are used (for the period

from the 1970s). The key thing is that they just use gauges, with no satellites.

My view is that bringing in satellites as in CMAP and GPCP products can lead to problems, and some circularity with ERA results - as you'll be using some of the same satellite data products. The point to emphasize for precip is that GPCC is totally independent from any ERA (40 or Interim) input.

I've come across these issues about GPCC before. I've been haranguing Bruno Rudolf and now Tobias Fuchs of GPCC to write something up for a number of years within AOPC! I think their QC is likely the best of all the centres, but they will continue to get these

doubts if they don't write anything up. They should at least explain how they do their interpolation - it can certainly be done better.

GPCC is using so much more data that it has to be better than any other product.

They can't release the raw station data, and it seems they can't release the numbers in each grid box.

There will be an HC paper on the buoy/ship SST issue, but this isn't yet used operationally.

It will come, but not before your paper goes back.

I hope it is fairly straightforward to do RMSs as well as correlations. We had SDs in the 2004 paper. I don't think RMSs would show anything untoward, but would take up some

more space.

WRT Rev 2, I'm not that convinced by some of Aiguo's arguments. Between us, I'm not that convinced by some of his data analyses. The ones involving PDSI leave a lot to be desired (this is coming to light in other work we are doing).

Rev 2 #6 Obviously not read the paper(s). CRUTEM3 is a simple average of stations within a grid box. There is no interpolation! If there are no stations, then there is no value!

I think this is the same for HadCRUH as well.

Rev 2 #13 Comment seems to end abruptly. I'd like to know what I might have said! I don't think I've ever said I doubt GPCP!

I am around all the time except for the week of July 12-17, when I'll be at the IPCC Scoping meeting in Venice. Kevin will be there as well.

Aiguo will be in CRU the first few days of the week after (July 20/21)

Cheers

Phil

At 22:53 22/06/2009, Adrian Simmons wrote:

Dick

It's a bit irritating getting a review one wants to nail just before leaving for Brussels for three days of EC-related meetings.

I'm sure now that reviewer 2's comments on SYNOP numbers is easily answered. The number of GTS SYNOPs went up a lot, but that's not because there were a lot more stations installed - the existing one just started having their data transmitted more frequently than 6-hourly. But this should hardly have effected the RH2m analysis as it uses only the 0, 6, 12 and 18UTC obs that have been there pretty well all the time. It only uses off-time obs if the value for the main synoptic hour is missing. The 4D-Var does assimilate more data over time, but here we appeal to fig 8 and argue that the increment does not shift over time. We already argue in the Appendix that the extra obs over North America may well be part of the difficulty HadCRUHext has for that region.

Anyway I'd like to confirm that the number of used SYNOPs does not change much over time for the OI RH2m analysis. I know how to find the number in the job output, but I don't know how to retrieve the job output from the logfiles stored in ECFS. I would only look at a few samples. I'd be grateful if you'd let me know how to do this.

In any case even if there was a problem with the numbers increasing sharply around 2000, this would manifest itself in a sudden drop in the RH time series, not a steady decline over the last few years.

After a bit of thinking I can find several things wrong with reviewer 2's argument why q over land is insensitive to variations in q over sea (think coastal mountain ranges, deserts, drought regions - moisture does not simply build up everywhere over land via onshore winds from the boundary-layer until it rains), and the response can draw attention to other points made in the paper, such as the coherence of changes in the vertical, and the similarity (but lag) of the q series over land and sea. Hard to believe the latter is all coincidence.

Also, there is a relationship between q and precip, not generally strong, but there's a high correlation for Australia.

Better stop for now.

Adrian

----- Original Message -----

Subject: 2009JD012442 (Editor - Steve Ghan): Decision Letter

Date: Mon, 22 Jun 2009 16:42:51 UT

From: jgr-atmospheres@agu.org

Reply-To: jgr-atmospheres@agu.org

To: adrian.simmons@ecmwf.int

Manuscript Number: 2009JD012442

Manuscript Title: Low-frequency variations in surface atmospheric humidity, temperature and precipitation: Inferences from reanalyses and monthly gridded observational datasets

Dear Dr. Simmons:

Attached below please find 3 reviews on your above-referenced paper. One of the Reviewers has raised questions and made suggestions for important revisions, mostly involving organization and presentation. Please consider the Reviewer reports carefully, make the necessary changes in your manuscript and respond to me, explaining how you have addressed these comments. In your Response to Reviewer letter, please include a statement confirming that all authors listed on the manuscript concur with submission in its revised form.

The due date for your revised paper is July 20, 2009. If you will be unable to submit a revised manuscript by July 20, 2009, please notify my office and arrange for an extension (maximum two weeks). If we do not hear from you by the revision due date, your manuscript will be considered as withdrawn.

When you are ready to submit your revision, please use the link below.

*The link below will begin the resubmission of your manuscript, please Do Not click on the link until you are ready to upload your revised files. Any partial submission that sits for 3 days without files will be deleted.

<[1]<http://jgr-atmospheres-submit.agu.org/cgi-bin/main.plex?el=A5Bc4EasP6A2oLJ3I6A9jNWgLzbgfWly58nFGPxNeQZ>>

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**In order to save time upon acceptance, it would be helpful if files in the correct format are uploaded at revision. Article and table files may be in Word, WordPerfect or LaTeX and figure files should be separately uploaded as .eps, .tif or pdf files. If you have color figures, please go to the site below to select a color option. Please put your color option in the cover letter.

[2]http://www.agu.org/pubs/e_publishing/AGU-publication-fees.pdf

Sincerely,

Steve Ghan

Editor, Journal of Geophysical Research - Atmospheres

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Reviewer Comments

Reviewer #2 (Comments):

Review of JGR Manuscript entitled

Low-frequency variations in surface atmospheric humidity, temperature and precipitation: inferences from reanalyses and monthly gridded observational data sets

by A.J. Simmons, K.M. Willett, P.D. Jones, P.W. Thorne, and D. Dee

General comments:

This paper provides a nice and useful summary on how the ERA-40 and ERA-Interim surface analysis products of temperature and humidity were derived, and a fairly comprehensive evaluation/comparison with the HadCRU surface data sets derived purely from surface observations, as well as with three other precipitation products. They found that in general the ERA surface temperature and humidity data from 1973 onward are in close agreement with the HadCRU data sets and that ERA precipitation also follows closely with gauge-based products, although long-term changes differ. Furthermore, the study reports a significant and steady decline in surface relative humidity (RH) over land from ~1999-2008 and suggested that the recent steady SSTs might be responsible for this land RH decrease. The manuscript is well written, the analysis appears to be comprehensive, and the results are of interest to many readers in the climate community. I think the paper should be published after some relat

ively

minor revisions.

My main concern is the interpretation of the recent RH decline over land. To me, the RH decreases shown in Fig. 4 and Fig. 7 look a bit spurious (non-climatic, e.g., lack of variations in Fig. 4 and stepwise changes in Fig.7) rather than realistic changes. They are also inconsistent with the RH changes during recent decades (up to 2004) reported in Dai (2006, JC), and this is not pointed out in the paper. As shown in Dai (2006), there was a 3-fold increase around the late 1990s in the number of surface humidity reports (mostly in North America but also over some other regions) included in the WMO SYNOP GTS reports. Furthermore, I personally found that there were other (undocumented) changes in the SYNOP reports around that time that led to shifts in derived precipitation and cloud frequencies over Euroasia and other places. Thus, there are reasons to suspect some non-climatic changes in the SYNOP reports around the late 1990s that might alter the RH trend over land.

I also was not convinced by the physical explanation of the RH decline (p.23). Even if the surface q stayed the same over the oceans during the 1999-2008 period when land air temperature has been increasing, this can not explain the RH decrease over land. This is because as long as the marine air contains more water vapor than continental surface air (which is still true even if marine sfc. q did not increase), advection of marine air onto land should cause land q to accumulate and RH to increase until the land q and RH reach certain levels so that precipitation kicks in to remove the moisture over land.

Remember that the atmospheric moisture storage (PW) is very small compared with the annual P and E fluxes, thus any perturbation in RH is quickly (within days) restored through surface E , vertical mixing, or lateral advection/mixing. If the RH in the marine air had decreased, then land RH would likely to decrease too. Dai (2006) did not show RH decreases over oceans since the

1980s. I wish the authors of this paper would also show RH series over ocean, at least since the middle 1980s.

For the ERA humidity data, the large well-known inhomogeneities in radiosonde humidity records will certainly propagate into the ERA background forecast and its analysis fields, making them not really suitable for long-term trend analyses. For example, all U.S.-operated radiosonde records (including many in the Pacific) before about Oct. 1993 report a dew point depression (DPD) of 30deg.C or a RH of 20% for any cases where RH is below 20%, which resulted in an abnormally higher frequency of reports of DPD=30deg.C and few reports below and no reports above DPD=30deg.C. This practice is also found in some Mexican, Canadian, Australian, and few other places (but stopped at different times from the late 1980s to the 1990s). In general, the newer humidity sensors during the last 10-15 years report more low RH or large DPD cases, whereas earlier ones had no measurements or incorrect values for these cases. One can see this shift in the histograms of daily DPD made by different humidity sensors. Thus, one needs to be very cautious when radiosonde humidity data are used in assessing trends, even if they are used indirectly (as in the ERA surface humidity analysis).

Some other comments:

1. Abstract: it gives the impression that even the long-term mean values for surface T, q and RH are the same between ERA and HadCRU data sets, which appears to be not the case as the respective means are removed in all plots. Please mention that the climatological mean may differ (if this is the case) even though the anomaly variations are similar.

2. Abstract, at the end: Please note that the mean precipitation amount and its change rate are not controlled by atmospheric water vapor amount (q), although higher q is often associated with higher P (e.g., tropical vs. high latitudes). Locally, you can have moist air passing by without any rain. Globally, annual P is controlled by how much moisture gets evaporated from ocean and land surfaces (i.e., $P=E$), and this surface E is primarily controlled by surface energy terms. In essence, P and E are water fluxes, and PW (or q) is the water storage in the atmosphere. People often link P to q because of the associated mentioned above (through low-level moisture convergence in a storm, etc.), and think that P change rates somehow should follow that of q or PW. However, P (or E) and q are controlled by different processes and in general the flux terms are not coupled with the storage terms in a cycling system (e.g., no one would think P or E is controlled by water storage in the ocean).

3. p. 3, top: the net radiative effect of clouds is relatively small, when their effect on solar radiation is included. To include clouds in the natural greenhouse warmth is a bit misleading because the higher surface temperature is maintained primarily by the greenhouse effect of water vapor and CO₂.

4. p. 4, middle: Again, any sampling/reporting biases in WMO SYNOP reports could affect both ERA and HadCRUH humidity data. Thus caution is still needed.

5. pp.5-6, section 2a: So in essence, ERA-40 and ERA-Interim surface T, q, and RH are another analysis product based on surface observations, just like the HadCRU and other climate data sets. The only difference is in the analysis methodology (IO interpolation with the use of the ERA background forecast fields vs. other more conventional analysis methods). Like most users, I thought the ERA surface fields are more tightly coupled with the reanalysis model system. I think it would be helpful to point out the above at the beginning of this section or in the Introduction.

6. p. 7, top: Please briefly mention how the station anomalies were aggregated onto 5deg. grid in CRUTEMP3, e.g., by simply averaging station values within the grid box, or

making use of correlated, nearby station data outside the box when sampling inside the box is sparse? I think most people would use the later to increase the coverage in the gridded products.

7. p. 7, bottom: Have any adjustments/corrections done for the most recent decades (1999-2008) in HadCRUH+ext? This is the period when RH decreases. Are there any homogeneity issues in combining the extended records with the homogenized HadCRUH?

8. p. 9, top: How could the fit of the ERA background forecasts capture multiple shifts induced by instrumental changes or reporting practices, especially when the future changes are needed to determine the timing and the size of a shift. Many statistical methods specifically designed to do these two tasks by analyzing the whole historical series still have difficulties in reliably detecting the locations of shifts and can only make a best guess regarding the real shift size. I wonder how one can do this in a reanalysis system when future records are not used yet, or nearby station series are combined together to form a grid box series that contain shifts from multiple stations (i.e., the stepwise patterns become very complex and look more like real variations).

9. p. 9, middle: I can't believe the GPCC people are still gridding precipitation total, not anomalies. This makes their products useless for long-term change analyses. Another land precipitation product from 1948-present that is derived from gauge records and the OI method is the PRECL from the NCEP Climate Prediction Center (CPC, ref: Chen et al. 2002, J. Hydrometeorol.). I think that is a better products for assessing long-term changes in land precipitation, although the gauge coverage for recent years (after 1997) may be not as good as that of the GPCC.

10. p. 11, middle and bottom: need to point out in Abstract or Summary that differences in the mean exist between the ERA and HadCRU T and humidity data.

11. Fig. 1 and other Figures: I suspect that different mean values were removed in computing the difference series. If that's the case, then need to point out this (i.e., the difference is between the anomalies relative to their respective mean).

12. Fig. 4: also show RH over the oceans for the last 25 years?

13. Fig. 11: with the changing gauge coverage and gridding precipitation total, one can not trust the low-frequency variations in the GPCC products. Phil Jones and other have
Reviewer #3 (Comments):

Review of the paper entitled "Low-frequency variations in surface atmospheric humidity, temperature and precipitation: Inferences from reanalyses and monthly gridded observational dataset" by A.J. Simmons, K. M. Willett, P. D. Thorne and D. Dee.

Recommendation: Accept with minor changes.

Summary of the paper:

This is an elaborate study examining trends in temperature, humidity and precipitation from the latest ECMWF reanalysis, comparing with independent gridded analyses, which are also performed with utmost care. The paper revealed that the commonly accepted assumption that the relative humidity stays the same under global warming condition does not necessarily holds over land. This is an important finding and should be of interest to wide climate communities. There are several other important contributions, such as the sensitivity of observation coverage on long term trend, which can only be studied by the use of reanalysis that has full global coverage. This paper also presents that the ERA-40 and ERA-Interim are of very high quality and useable for low frequency climate studies.

Major comments:

1. I am particularly impressed with the way the work is performed. This is a very elaborate work using a variety of datasets to present that there is a strong long time trend in temperature and humidity. This thorough work made it possible to convince readers these observed facts. Although the finding of the decrease in relative humidity

over land is credible, it may be more meteorologically interesting and convincing if additional analysis is made to present the possible mechanisms of the absence of increase in specific humidity over land. If reanalysis is used, it is not impossible to estimate the change in the moisture transport into land areas (although this may involve considerable amount of work). It may also possible to examine the change in large scale mean land-ocean circulation that contributes to the transport of moisture. From heuristic point of view, stronger heating over land tends to strengthen upper level high and subsidence, which may prevent moisture to be transported inland, and such trend may be detectable from large scale reanalysis. In terms of the change in precipitation, moisture availability and relative humidity are important, but static stability and large scale convergence should also play an important role. If any of these additional analyses can be performed, or even discussed in qualitative manner, it will enhance the paper.

2. It is not very clear how the diurnal variations of temperature and humidity are handled in this study. It is helpful to state the time frequency of reanalysis output that is used to compute daily mean, and the way observed daily mean are obtained.

3. Are there any reason that the relative humidity or dew point depression is analyzed and not the specific humidity itself?

4. The paper is a little too long. One way to shorten it is to separating it into two parts by adding analysis suggested above, or separating the analysis of precipitation. This is just a suggestion and decision is up to the authors.

Minor comments:

1. Page 6 & 11. The authors claim that the use of anomaly will reduce the influence of surface elevation differences. Can this be true even the relation between elevation and relative humidity/specific humidity is very nonlinear?

2. It may be friendlier to the reader why relative humidity and specific humidity are both examined. Some introductory remarks on the different impact of relative and specific humidity will help.

3. Page 13. Lines 298-300. These lines just present why the ERA-40 and Interim are different but not the reason for the ERA-Interim worse than ERA-40 over Africa.

4. Page 14. Lines 316-328. Is it possible to separate the actual reduction in the number of observations and the reduction in data used by CRUTEM?

5. Page 15. Line 364. It seems that the difference in analysis between ERA-40 and ERA-Interim seems to be used as a measure of the reanalysis accuracy. Is this a good assumption?

6. Page 17. Lines 392-397. Can it be possible to mathematically estimate the relation between the correlation of specific humidity and relative humidity? Since relative humidity is a function of specific humidity, temperature and pressure, it seems natural that the correlation for relative humidity should be lower. However, this will depend on which parameters are analyzed in the first place.

--

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Prof. Phil Jones

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References

1. <http://jgr-atmospheres-submit.agu.org/cgi-bin/main.plex?el=A5Bc4EasP6A2oLJ3I6A9jNWgLzbgfWly58nFGPxNeQZ>
2. http://www.agu.org/pubs/e_publishing/AGU-publication-fees.pdf
3. <http://www.adobe.com/prodindex/acrobat/readstep.html>

From: "Nick Pepin" <nicholas.pepin@port.ac.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Re: Fwd: CRU surface temperature dataset
Date: Thu, 25 Jun 2009 10:59:26 +0100

Phil

Thanks for this great detail. I am thinking that probably a raw radiosonde dataset may be better (I tried this before using the LKS dataset but station density was an issue and only ended up with around 20 station pairs) - it sounds as though things have improved dramatically in that area and will look at the sources you suggest. My hope is that at least I can find hundreds/thousands of stations near to my high elevation surface ones for comparison. If not I could interpolate spatially maybe between radiosondes to my surface sites since free-air climate (not meteorology) should be relatively smooth in space. I cannot interpolate between surface stations.

I agree that reanalyses can be a can of worms (esp NCEP/NCAR)!

As for the surface I'll also look at the site you suggest and get back if I have any Q/problems. I appreciate the time you have taken to answer some of my Q!

Best wishes

Nick

>>> Phil Jones <p.jones@uea.ac.uk> 24/06/2009 13:09 >>>

Nick,

I don't want to put off, but there is an awful lot of things wrong with NCEP/NCAR.

They are probably OK for month-to-month variability, but if you look at some

of the figures in Simmons et al (2004) you'll see that for trends they are practically useless before 1979.

There is just so much wrong with the sondes which together with the introduction of satellite data in 1978/9 makes reanalyses awful.

The Simmons paper is about how much better ERA-40 is than NCEP/NCAR.

It is also telling you that you shouldn't be using NCEP/NCAR for trends - and ERA-40

is only OK in Europe and North America.

A group of us are hopeful of getting an EU project funded to go through the

Reanalysis input - surface and sonde. The aim is to put in all the homogenised

surface and sonde data, so giving reanalysis better data input - and putting back all the

data that missed the real-time cut. I'm not sure you're aware that no back data have

ever got into the reanalyses. If data doesn't make the cut in real time, it can never get

in later. The reanalysis source input doesn't collect back data!

You'd be better off getting one of the newer sonde datasets.
HadAT2 although developed
in 2005 is beyond it's sell-by date. Have a look at the attached
and this web site

<http://homepage.univie.ac.at/leopold.haimberger/leoweb/index.html>

Ra-ob core version 1.4 is the latest.

The drop off in surface data isn't the fault of GHCNv2. The
folks in Asheville are doing all
they can to get additional datasets. Currently about 2000 sites are
exchanged in real time.

If the sites you want are not exchanged by Met Services in real
time we can't get access
to them except by asking each Met Service and/or waiting till the
next volumes of the 10-year
books (for 2001-2010) get released.

CRUTEM3 has some additional station data going in for Australia
and Canada, but apart
from this we will have nothing more than GHCNv2. We could get a
load more from the US
quite easily, but coverage is reasonable there compared to the rest
of the world.

GHCNv2 and ourselves have lots of historic series, but these
aren't updatable in real
time, without continuous effort. Lots of projects were funded in
the US and Europe in the
1980s and 1990s to get loads of data digitized, homogenized and
accessible.

It is possible to do things with daily data (SYNOPS) but these are
only generally good enough for the good countries.

http://www.dwd.de/bvbw/appmanager/bvbw/dwdwwwDesktop/?_nfpb=true&switchLanguage=en&_pageLabel=_dwdwww_klima_umwelt_datenzentren_gsnmc

This site has what is available in real time - since 2001. This
site can be very annoying.
There is a link back to NCDC.

Cheers
Phil

Cheers
Phil

At 17:48 23/06/2009, you wrote:
>Phil
>

>Many thanks for your reply. This is very helpful, esp the Simmons paper.
>I am aware there are issues with reanalyses although I do want to
>try and use data representative of free air (and not contaminated
>with surface obs)- hence NCEP/NCAR rather than ERA-40 maybe, and use
>of pressure level data rather than 2 m or surface reanalysis temps
>(which I think the Simmons paper is about). I don't want the
>reanalysis to respond to surface issues and want it to be
>independent (purely based on radiosonde and satellite coupled with
modelling).

>Of course this doesn't make the points irrelevant and I am looking
>at these while deciding what to use.

>

>As regards surface data, I am interested in the Tmean you mention
>used for CRUTEM3. Is this available and for how many stations?
>GHCNV2 is not good after 1990 since many stations stop! It is
>particularly dire after 2005 as you may realise? Please let me know
>what you think?

>

>Best wishes and thanks for your help re this.

>

>Nick

>

> >>> Phil Jones <p.jones@uea.ac.uk> 22/06/2009 10:38 >>>

>

> Nick,

> I was away when your earlier message came in March, and I must have
> forgotten it when I got back to Norwich.

> We generally only put the gridded data on the web site. The
>station data that

> goes into CRUTEM3 is only monthly mean temperature. It is only
>since the mid-1990s

> that countries have routinely exchanged monthly mean Tx and Tn
>data. Many countries

> don't use these data to calculate mean T, instead using their
>historical methods based

> on fixed hours.

> We do have an archive of historic Tx and Tn (monthly) but this
>is almost entirely

> based on GHCNV2 sources. We use these data in products like this
paper

>

>

>Mitchell, T.D. and Jones, P.D., 2005: An improved method of
>constructing a database of monthly climate observations and
>associated high-resolution grids. Int. J. Climatol. 25, 693-712.

>

> When you compare with Reanalysis trends you want to consider
>looking at ERA-INTERIM

> available from 1989-2008. There are also longer reanalysis products
>developed by NOAA

> (Gil Compo) from surface station data only (i.e. no sondes and no
>satellites, so

> consistent through time).

>

> Are you aware of this paper? Basically reanalyses will be wrong
>before 1979 - except possibly
> in Europe and North America. This paper has the reasons why
>reanalyses will be wrong.
>
> Cheers
> Phil
>
>
>
>At 15:06 17/06/2009, you wrote:
> >Dear Prof. Jones
> >You maybe had forgotten that I e-mailed you a while ago (March)
> >asking about access to data for surface stations for work on
> >temperature trends in complex topography (original e-mail and
> details below).
> >Since then I have been awarded a Royal Society Travel Grant to do
> >some work on this in the U.S. and I will be examining the GHCNv2
> >dataset in detail (which I have). I would really like to be able to
> >include a CRU dataset as well, since I did this in my original
> >research and these datasets are highly regarded.
> >If you are not the correct person to ask, maybe you could guide me
> >to the right person!
> >Many thanks for your reply.
> >Best wishes
> >Nick Pepin
> >
> >
> >>> Nick Pepin 09/03/2009 16:43 >>>
> >Dear Prof. Jones
> >You may remember that a few years ago (2005) I published a paper
> >with Dian Seidel looking at temperature trends at high elevation
> >surface stations and comparing them with reanalysis trends. I wish
> >to update this work as part of another project, and was looking on
> >the UEA website to see if any of the original stations have been
> >updated. It is important that they are homogeneity adjusted as much
> >as possible.
> >
> >It appears that nearly all of the datasets available on the web are
> >gridded and therefore interpolated (which I don't want since
> >interpolation influences what I am examining). Are any of the 3000
> >approx original stations available (mean monthly maxima and minima
> >are good enough) which are used to create CRUTEM3 etc?
> >
> >In my original analysis I combined data from the CRU station dataset
> >and GHCN (some stations were in both) and I would like to do the
> >same again if possible. This is part of work looking at the effect
> >of topography on temperature trend patterns on a global scale (it
> >will be more detailed than preliminary work on this in the attached
> paper).
> >
> >Many thanks for your help
> >Best wishes
> >Nick Pepin

> >
> >
> >
>

>Prof. Phil Jones
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>

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From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Skeptics
Date: Thu, 25 Jun 2009 11:19:45 -0400
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Hi Phil,

well put, it is a parallel universe. irony is as you note, often the contrarian arguments are such a scientific straw man, that an effort to address them isn't even worthy of the peer-reviewed literature!

mike

On Jun 25, 2009, at 10:58 AM, Phil Jones wrote:

Mike,

Just spent 5 minutes looking at Watts up. Couldn't bear it any longer - had to stop!. Is there really such a parallel universe out there? I could understand all of the words some commenters wrote - but not in the context they used them.

It is a mixed blessing. I encouraged Tom Peterson to do the analysis with the limited number of USHCN stations. Still hoping they will write it up for a full journal article.

Problem might be though - they get a decent reviewer who will say there is nothing new in the paper, and they'd be right!

Cheers

Phil

At 15:53 24/06/2009, Michael Mann wrote:

Phil--thanks for the update on this. I think your read on this is absolutely correct. By the way, "Watts up" has mostly put "ClimateAudit" out of business. a mixed blessing I suppose.

talk to you later,

mike

On Jun 24, 2009, at 8:32 AM, Phil Jones wrote:

Gavin,

Good to see you, if briefly, at NCAR on Friday. The day went well, as did the dinner in the evening.

It must be my week on Climate Audit! Been looking a bit and Mc said he has no interest in developing an alternative global T series. He'd also said earlier

it would be easy to do. I'm 100% confident he knows how robust the land component is.

I also came across this on another thread. He obviously likes doing these sorts of things, as opposed to real science. They are going to have a real go at procedures when it comes to the AR5. They have lost on the science, now they are going for the process.

Cheers

Phil

Prof. Phil Jones

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Norwich Email [1]p.jones@uea.ac.uk

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<McIntyre_Submission_to_EPA.pdf>

--

Michael E. Mann

Professor

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503 Walker Building FAX: (814) 865-3663

The Pennsylvania State University email: [2]mann@psu.edu

University Park, PA 16802-5013

website: [3]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[4]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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website: [7]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[8]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. <mailto:p.jones@uea.ac.uk>
2. <mailto:mann@psu.edu>
3. <http://www.meteo.psu.edu/~mann/Mann/index.html>
4. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
5. <mailto:p.jones@uea.ac.uk>
6. <mailto:mann@psu.edu>
7. <http://www.meteo.psu.edu/~mann/Mann/index.html>
8. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

9. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Luterbacher Jürg <juerg.luterbacher@giub.unibe.ch>
Subject: Re: IPCC Fig. 6.10
Date: Wed Jul 1 10:31:36 2009

Hi Juerg,
At 21:56 16/06/2009, you wrote:

I hope you are very well. Douglas arrived savely here and hopefully he will be starting officially soon. I am looking very much forward having him here and of course working together with you on different topics!

Yes, that sounds great to me too.

I have a chinese paleo climatology researcher (Zhinxin Hao) with me for a couple of weeks.

She is working on the comparison with different chinese long temperature reconstructions and would like to present a similar figure as in the IPCC Fig 6.10.

Keith told me that he might not be able to work for the next time, so I thought I could address this issue to you as you were also much involved.

That's fine. Indeed I designed and drew the figure.

She asked me if I could ask you whether you could have a look at the attachment where she tried to explain how she calculated and plotted the curves for China. As she did not fully understand the way it was done in the IPCC report, would you mind having a look at the text and let me know if she applied it correctly?

It is a little hard to follow (some symbols got replaced by squares -- perhaps a PDF file would work better than a Word Doc?) but I think that the method looks approximately right but not quite right. Some things that look a bit different:

Se: it appears that the same value is used for all 4 reconstructions (in the example, Se=1.3165 is used). Why would the uncertainty on one reconstruction be the same as the uncertainty on all the others? Perhaps she has used the standard deviation of the instrumental temperature rather than the standard error of each reconstruction? Did the authors actually publish estimated uncertainties along with their best-estimate reconstruction series? You should also note that reconstruction errors/uncertainties may depend on time scale -- the IPCC fig 6.10 showed variations on timescales of 30-yrs and

longer, so I attempted to use uncertainties estimated for that timescale (or a similar multi-decadal timescale).

IPCC wanted to mostly standardise on the 90% range (5%-95%), so for my scoring I awarded $100\%/N$ to any temperature that falls within the ± 1 SE reconstruction range (the same as noted in her document) but awarded $0.5*100\%/N$ to any temperature that falls within ± 1.6448 SE reconstruction range (this differs from the ± 2 SE in her document). I originally used ± 2 SE, but (under assumption of normality), ± 1.6448 SE should encompass 5%-95% range, while ± 2 SE is of course approx 2.5%-97.5%. Either is of course equally defensible, but if you want to reproduce IPCC, then its ± 1.6448 SE for the half score ($0.5*100\%/N$).

This is of course repeated for all N reconstructions.

I was a little unsure about the actual plot produced too. When the Xu2003 curve is very low or very high, the brown shading extends in both directions (to very low *and* very high values at once). e.g. AD 650 (but there are others too). Also the range is very narrow at about AD 1050; although the 3 recons are quite similar here, it still looks too narrow, especially when you add on the reconstruction SE (and ± 1.6448 SE or ± 2 SE).

Hope this helps,

Tim

From: Tim Osborn <t.osborn@uea.ac.uk>
To: haozx@igsnr.ac.cn
Subject: Re: =?gb2312?B?Rnc6IFRpbXMgQW5zd2Vy?=
Date: Wed Jul 1 16:17:28 2009
Cc: Luterbacher Jürg <juerg.luterbacher@giub.unibe.ch>

Dear Zhixin,

At 15:14 01/07/2009, you wrote:

Do you mean Se should be the standard error from the individual reconstruction series
yes, that's what I mean.

(before I got your answer, I calculated the standard error for the 5 reconstruction data at one time point, e.g. 1470s, it is not from the original papers given by the authors)?

Ah. I understand what you've done now.

But my question is if the author did not publish the uncertainty, how can I deal with the value of Se?

Well, the original purpose of constructing IPCC Fig. 6.10c was to display the published uncertainty estimates of each study. If no uncertainties had been estimated by the original authors then we wouldn't have produced the figure in the first place!

So, do you really want to produce such a figure to show the uncertainty ranges when the uncertainty ranges haven't been calculated before?

If you do, then you'd need to somehow estimate the uncertainty. You could do this yourself, perhaps, e.g. from the differences between each reconstruction and the instrumental temperatures during some overlap (calibration, or independent verification) period? But this wouldn't measure any increase in uncertainty during periods when each reconstruction is perhaps based on less input proxy data.

Estimating the uncertainty from the spread of individual reconstruction values in a particular year, like you've done, is open to criticism. Do you really think that in a particular year when the three recons have very similar values that the uncertainty is much less than other nearby years? If you had a high number of

And now I understood the meaning of 5%-95% range, I will follow this, and replot my figures with $\pm 1.645SE$ for the half scores.

Thank you very much again, hopefully I can give the uncertainty of reconstruction results over China region soon. After finished, may I send the manuscript to you and give us comments and suggestions?

Best wishes,

Zhixin

----- Original Message -----

From: "Juerg Luterbacher"

To:

Subject: Tims Answer

Sent: Wed, 1 Jul 2009 12:27:44 +0200

here is the answer of Tim.

cheers maybe you can now email him directly to make things clear

cheers

Juerg

It is a little hard to follow (some symbols got replaced by squares -- perhaps a PDF file

would work better than a Word Doc?) but I think that the method looks approximately right

but not quite right. Some things that look a bit different:

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noted in her document) but awarded 0.5*100%/N to any temperature that falls within +- 1.6448 SE reconstruction range (this differs from the +-2 SE in her document). I originally used +- 2 SE, but (under assumption of normality), +- 1.6448 SE should encompass 5%-95% range, while +- 2 SE is of course approx 2.5%-97.5%. Either is of course equally defensible, but if you want to reproduce IPCC, then its +- 1.6448 SE for the half score (0.5*100%/N).

This is of course repeated for all N reconstructions.

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narrow, especially when you add on the reconstruction SE (and +- 1.6448 SE or +- 2 SE).

Hope this helps,
Tim

From: Tim Osborn <t.osborn@uea.ac.uk>
To: haozx@igsrr.ac.cn
Subject: Re: =?gb2312?B?Rnc6IFRpbXMgQW5zd2Vy?=
Date: Wed Jul 1 16:19:39 2009
Cc: Luterbacher Jürg <juerg.luterbacher@giub.unibe.ch>

Dear Zhixin (cc Juerg),
At 15:14 01/07/2009, you wrote:

Do you mean Se should be the standard error from the individual reconstruction series
yes, that's what I mean.

(before I got your answer, I calculated the standard error for the 5 reconstruction data at one time point, e.g. 1470s, it is not from the original papers given by the authors)?

Ah. I understand what you've done now.

But my question is if the author did not publish the uncertainty, how can I deal with the value of Se?

Well, the original purpose of constructing IPCC Fig. 6.10c was to display the published uncertainty estimates of each study. If no uncertainties had been estimated by the original authors then we wouldn't have produced the figure in the first place!

So, do you really want to produce such a figure to show the uncertainty ranges when the uncertainty ranges haven't been calculated before?

If you do, then you'd need to somehow estimate the uncertainty. You could do this yourself, perhaps, e.g. from the differences between each reconstruction and the instrumental temperatures during some overlap (calibration, or independent verification) period? But this wouldn't measure any increase in uncertainty during periods when each reconstruction is perhaps based on less input proxy data.

Estimating the uncertainty from the spread of individual reconstruction values in a particular year, like you've done, is open to criticism. Do you really think that in a particular year when the three recons have very similar values that the uncertainty is much less than other nearby years? If you had a high number of independent reconstructions then this might be ok, but with only 3 series before 1350 it is too susceptible to random sampling variability.

And now I understood the meaning of 5%-95% range, I will follow this, and replot my figures with $\pm 1.645SE$ for the half scores.

Thank you very much again, hopefully I can give the uncertainty of reconstruction

results over China region soon. After finished, may I send the manuscript to you and give us comments and suggestions?

Yes, that would be fine.

Tim

From: "Tim Osborn" <t.osborn@uea.ac.uk>
To: I.Harris@uea.ac.uk
Subject: cruts tmp to 2008
Date: Fri, 10 Jul 2009 00:19:58 +0100 (BST)
Reply-to: t.osborn@uea.ac.uk
Cc: "tim Osborn" <t.osborn@uea.ac.uk>

Hi Harry,

finally had time to take a look at the latest cruts3 run through to 2008 for tmp, picked up from /cru/cruts/

Two PDFs showing seasonal national means are attached.

Look at ...2008a_vs_2008b.pdf first. Black is your previous update to 2008, pink is the latest one. Many very similar, some small differences (presumably due to outlier 3/4 SD removal... note that as these are national/seasonal means, outliers might be quite large, yet only show up small in the means if many other stations contribute).

page 4. The hot spike in Guatemala SON has been removed in the new version. That looks much better.

page 6 & page 9: the hot spikes in France, Italy and Austria in JJA in 2003 have been reduce slightly too. Not sure if this is right or not, could ask Phil what he thinks. Could Jul & Aug 2003 have been so hot that some observations validly did exceed the +3SD outlier check? Or do you use a +4SD check for TMP? Anyway, this is one to ask Phil about.

There are various other erroneous hot spikes that have now been correctly removed, I won't list them all here.

However, there are some cold spikes in both previous and latest 2008 updates... see e.g. Mali SON on page 12. Have you turned on only outlier checking for +3SD, and not for -3SD? Some wrong-looking cold spikes are still present.

Now look at ...2005_vs_2008b.pdf. Black is last years CRUTS3 through to 2005 (I know the files went to mid 2006, but I stopped at last complete year). Note this isn't CRUTS2.1! :-). Pink is again the newest version of the update to 2008.

There are some early 20th century differences that I'm not too bothered about, though it would be nice to know why they arise. One concern is that the mean level is different between the versions... see e.g. JJA for various countries on pages 7 and 8. Seems to be a constant offset. It's too big to be a simple rounding error in my calculations (I may have changed from 1 dec. place to 2 dec. place, but some differences are about 0.5 deg C), and these are absolute values so there's no dependency on any anomalisation/reference period meaning as I'm not doing any.

Intriguing. Perhaps some normals have change in some regions/seasons?

So:

- (1) hot spikes have been corrected.
- (2) cold spikes still there.
- (3) some odd differences in mean level.

Progress!

Tim

--

Dr. Tim Osborn
RCUK Academic Fellow
Climatic Research Unit
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ, UK
www.cru.uea.ac.uk/~timo/

Attachment Converted: "c:\eudora\attach\idl_cruts3_2008a_vs_2008b.pdf"

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From: Kevin Trenberth <trenbert@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Tue, 28 Jul 2009 08:57:36 -0600
Cc: Michael Mann <mann@meteo.psu.edu>, Jim Salinger
<j.salinger@auckland.ac.nz>, j.renwick@niwa.co.nz, b.mullan@niwa.co.nz,
Gavin Schmidt <gschmidt@giss.nasa.gov>, James Annan
<jdannan@jamstec.go.jp>, Grant Foster <tamino_9@hotmail.com>

The leads and lags are analyzed in detail in this paper
Trenberth, K. E., J. M. Caron, D. P. Stepaniak, and S. Worley 2002:
[1]The evolution of
ENSO and global atmospheric surface temperatures J. Geophys. Res.,
107, D8,
10.1029/2000JD000298.
and we were not able to reproduce Tom Wigley's result (we tried). It
may depend in indices
used. In this paper we also document the extent to which ENSO
contributes to warming
overall.
Kevin
Phil Jones wrote:

Mike,
See below for instructions.
Also, just because IPCC (2007, Ch 3) didn't point out the 6/7-month
lag
between the SOI and global temperatures doesn't mean it hasn't been
known for years. IPCC is an assessment and not a review of
everything
done. If they had even read Wigley (2001) they would have seen this
lag pointed out. I wasn't the first to do this in 1989 either. I
don't
think Walker was either. I think the first was Hildebrandsson in
the
1890s. Why does it always go back to a Swede!
file is at [2]ftp.cru.uea.ac.uk
login anonymously with emails as pw
then go to people/philjones
and you should find santeretal2001.pdf
Cheers
Phil

At 14:08 28/07/2009, Michael Mann wrote:

thanks Phil,
this is very helpful and reaffirms what we've identified as some of
the main points that need to be covered in a formal response. I've
taken the liberty of copying in a couple other colleagues who have
been looking into this. Grant Foster was the first author on a
response to a similarly bad paper by Schwartz that was published
some
time ago, and has been doing a number of analyses aimed at
demonstrating the key problems in McClean et al.

is I've suggested that Grant sent out a draft of the response when it ready to the broader group of people who have been included in these exchanges for feedback and potential co-authorship,
mike

p.s. Santer et al paper still didn't come through in your followup message. Can you post in on ftp where it can be downloaded?

On Jul 28, 2009, at 5:15 AM, Phil Jones wrote:

Jim et al,

Having now read the paper in a moment of peace and quiet, there are a few things

to bear in mind. The authors of the original will have a right of reply, so need to

ensure that they don't have anything to come back on. From doing the attached a

year or so ago, there is a word limit and also it is important to concentrate only

on a few key points. As we all know there is so much wrong with the paper, it

won't be difficult to come up with a few, but it does need to be just two or three.

The three aspects I would emphasize are

1. The first difference type filtering. Para 14 implies that they smooth the series

with a 12 month running mean, then subtract the value in Jan 1980 from that in

Jan 1979, then Feb 1980 from Feb 1979 and so on. As we know this removes

any long-term trend.

The running mean also probably distorts the phase, so this is possibly why

they get different lags from others. Using running means also enhances the

explained variance. Perhaps we should repeat the exercise without the smoothing.

2. Figure 4 and Figure 1 show the unsmoothed GTTA series. These clearly have a

trend. Perhaps show the residual after extracting the ENSO part.

3. They do the same first difference on the smoothed SOI. The SOI doesn't explain

the climate jump in the 1976/77 period. Their arguments in para 30 are all wrong.

A few minor points

- there are some negative R*R values just after equation 3.

- I'm sure Tom Wigley wouldn't have proposed El Nino events occurring after volcanoes!

Attached this paper as well. From a quick read it doesn't say what is purported - in fact

it seems to show clearly how the analysis should have been done.

- there is a paper by Ben Santer (more recent) where he applies the same type

of extraction procedure to models. I'll send this separately as it

is large. In case it

is too large here is the reference.

Santer, B.D., Wigley, T.M.L., Doutriaux, C., Boyle, J.S., Hansen, J.E., Jones, P.D., Meehl, G.A., Roeckner, E., Sengupta, S. and Taylor K.E., 2001: Accounting for the effects of volcanoes and ENSO in comparisons of modeled and observed temperature trends. Journal of Geophysical Research 106, 2803328059.

Finally I've attached a paper I wrote in 1990, where I did something similar to

what they did. I looked at residuals from a Gaussian filter, and I added

the smoothed data back afterwards. I was working at the annual timescale

and I did have many more years.

Cheers

Phil

At 00:19 25/07/2009, Michael Mann wrote:

Hi Jim,

Grant Foster ('Tamino') did a nice job in a previous response (attached) we wrote to a similarly bad article by Schwartz which got a

lot of play in contrarian circles.

since he's already done some of the initial work in debunking this,

I

sent him an email asking hi if we was interested in spearheading a similar effort w/ this one.

let me get back to folks after I've heard back from him, and we can discuss possible strategy for moving this forward,

mike

On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:

Kia orana All from the Tropical South Pacific

Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin Tamino's bang up job is great, And good that you go up with stuff on Real Climate, Mike. As Kevin is preoccupied, for the scientific record we need a rebuttal somewhere pulled together. Who wants to join in on the multiauthored effort?? I am happy to coordinate it. Return to 'winter' this evening after enjoying a balmy south east trades and sunny dry 24 C in the Cook Islands.

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Who else wants to join in??

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Kevin

a formal comment to JGR seems like a worthwhile undertaking here. contrarians will continue to cite the paper regardless of whether or not its been rebutted, but for the purpose of future scientific assessments, its important that this be formally rebutted in the peer-reviewed literature.

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So the response to this procedure is to reduce periods of 10
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data and

uncorrected radiosonde temperature estimates. There were a
series

of

three key papers published in Science a few years ago, by
Mears

et al,

Santer et al, and Sherwood et al.

see Gavin's excellent RealClimate article on this:

[6]<http://www.realclimate.org/index.php/archives/2005/08/et-tu-lt/>

these papers collectively showed that both datasets were
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The Christy and Spencer MSU satellite-derived tropospheric temperature estimates contained two errors--a sign error and an algebraic error--that had the net effect of artificially removing the warming trend.

Christy and Spencer continue to produce revised versions of the MSU dataset, but they always seem to show less warming than every other independent assessment, and their estimates are largely disregarded by serious assessments such as that done by the NAS and the IPCC.

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other far more careful analyses (a paper by David Thompson of CSU, Phil Jones, and others published in Nature more than year ago) used proper, widely-accepted surface temperature data to estimate the influence of natural factors (El Nino and volcanos) on the surface temperature record. their analysis was so careful and clever that it detected a post-world war II error in sea surface temperature measurements (that yields artificial cooling during the mid 1940s) that had never before been discovered in the global surface temperature record. needless to say, they removed that error too.

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even
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It's Seth again. Attached is a paper in JGR today that
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Seth

Seth Borenstein
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How the h... did this get accepted!!
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--

Associate Professor Jim Salinger
School of Geography and Environmental Science
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Private Bag 92 019
Auckland, New Zealand
Tel: + 64 9 373 7599 ext 88473

This message was sent using IMP, the Internet Messaging
Program.

Kevin Trenberth
Climate Analysis Section, NCAR
PO Box 3000
Boulder CO 80307
ph 303 497 1318
[10]<http://www.cgd.ucar.edu/cas/trenbert.html>

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Michael E. Mann
Professor
Director, Earth System Science Center (ESSC)
Department of Meteorology Phone: (814)
863-4075
503 Walker Building FAX: (814)
865-3663
The Pennsylvania State University email: [11]mann@psu.edu
University Park, PA 16802-5013
website: [12]<http://www.meteo.psu.edu/~mann/Mann/index.html>
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"Dire Predictions" book site:

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NR4 7TJ
UK

<Parker-on-Pielke-2009.pdf><Jones_ENSO_1990.pdf><wigley2001.pdf>

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thanks Phil,
this is very helpful and reaffirms what we've identified as some of
the main points that
need to be covered in a formal response. I've taken the liberty of
copying in a couple
other colleagues who have been looking into this. Grant Foster was
the first author on a
response to a similarly bad paper by Schwartz that was published
some time ago, and has
been doing a number of analyses aimed at demonstrating the key
problems in McClean et
al.

I've suggested that Grant sent out a draft of the response when it
is ready to the
broader group of people who have been included in these exchanges
for feedback and
potential co-authorship,
mike

p.s. Santer et al paper still didn't come through in your followup
message. Can you post
in on ftp where it can be downloaded?

On Jul 28, 2009, at 5:15 AM, Phil Jones wrote:

Jim et al,
Having now read the paper in a moment of peace and quiet, there
are a few things

to bear in mind. The authors of the original will have a right of reply, so need to

ensure that they don't have anything to come back on. From doing the attached a

year or so ago, there is a word limit and also it is important to concentrate only

on a few key points. As we all know there is so much wrong with the paper, it

won't be difficult to come up with a few, but it does need to be just two or three.

The three aspects I would emphasize are

1. The first difference type filtering. Para 14 implies that they smooth the series

with a 12 month running mean, then subtract the value in Jan 1980 from that in

Jan 1979, then Feb 1980 from Feb 1979 and so on. As we know this removes

any long-term trend.

The running mean also probably distorts the phase, so this is possibly why

they get different lags from others. Using running means also enhances the

explained variance. Perhaps we should repeat the exercise without the smoothing.

2. Figure 4 and Figure 1 show the unsmoothed GTTA series. These clearly have a

trend. Perhaps show the residual after extracting the ENSO part.

3. They do the same first difference on the smoothed SOI. The SOI doesn't explain

the climate jump in the 1976/77 period. Their arguments in para 30 are all wrong.

A few minor points

- there are some negative R*R values just after equation 3.

- I'm sure Tom Wigley wouldn't have proposed El Nino events occurring after

volcanoes!

Attached this paper as well. From a quick read it doesn't say what is purported -

in fact

it seems to show clearly how the analysis should have been done.

- there is a paper by Ben Santer (more recent) where he applies the same type

of extraction procedure to models. I'll send this separately as it is large. In case it

is too large here is the reference.

Santer, B.D., Wigley, T.M.L., Doutriaux, C., Boyle, J.S., Hansen, J.E., Jones, P.D.,

Meehl, G.A., Roeckner, E., Sengupta, S. and Taylor K.E., 2001: Accounting for the

effects of volcanoes and ENSO in comparisons of modeled and observed temperature

trends. Journal of Geophysical Research 106, 28033-28059.

Finally I've attached a paper I wrote in 1990, where I did something similar to what they did. I looked at residuals from a Gaussian filter, and I added the smoothed data back afterwards. I was working at the annual timescale and I did have many more years.

Cheers
Phil

At 00:19 25/07/2009, Michael Mann wrote:

Hi Jim,
Grant Foster ('Tamino') did a nice job in a previous response (attached) we wrote to a similarly bad article by Schwartz which got

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lot of play in contrarian circles. since he's already done some of the initial work in debunking this,

I

sent him an email asking hi if we was interested in spearheading a similar effort w/ this one. let me get back to folks after I've heard back from him, and we can discuss possible strategy for moving this forward,
mike

On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:

Kia orana All from the Tropical South Pacific
Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin Tamino's bang up job is great, And good that you go up with stuff on Real Climate, Mike. As Kevin is preoccupied, for the scientific record we need a rebuttal somewhere pulled together. Who wants to join in on the multi-authored effort?? I am happy to coordinate it. Return to 'winter' this evening after enjoying a balmy south east trades and sunny dry 24 C in the Cook Islands.

Jim

Quoting Michael Mann <[49]mann@meteo.psu.edu>:

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On Jul 23, 2009, at 11:01 PM, Jim Salinger wrote:

I am tied up next week, but could frame something up the following week which , I hope would be multi-authored. It would be quite good to have a rebuttal from the same Department at Uni of Auckland (which Glenn McGregor of IJC is director of)! I haven't had the opportunity to download the text here in the Cook Islands, so this would give me the opportunity to do that. Who else wants to join in??

Jim

Quoting Kevin Trenberth <[50]trenbert@ucar.edu>:

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past 4 weeks (including AR5 IPCC scoping mtg); the NCAR summer Colloquium is coming up in a week and then I am off to Oz and NZ for 3 weeks (GEWEX/iLeaps, CEOP) and I have an oceanobs'09 plenary paper to do.
Kevin

a formal comment to JGR seems like a worthwhile undertaking here. contrarians will continue to cite the paper regardless of whether or not its been rebutted, but for the purpose of future scientific assessments, its important that this be formally rebutted in the peer-reviewed literature.

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On Jul 23, 2009, at 9:05 PM, Jim Salinger wrote:

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Quoting Michael Mann <[51]mann@meteo.psu.edu>:

2nd email

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minutes. took a cursory look at the paper, and it has all the
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first of all, the authors use two deeply flawed datasets that
understate the warming trends: the Christy and Spencer MSU
data and
uncorrected radiosonde temperature estimates. There were a
series
of
three key papers published in Science a few years ago, by
Mears
et al,
Santer et al, and Sherwood et al.
see Gavin's excellent RealClimate article on this:
[52]<http://www.realclimate.org/index.php/archives/2005/08/et-tu-1t/>
these papers collectively showed that both datasets were
deeply
flawed
and understate actual tropospheric temperature trends. I
find it
absolutely remarkable that this paper could get through a
serious
review w/out referencing any 3 of these critical papers-- papers
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The Christy and Spencer MSU satellite-derived tropospheric
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serious assessments such as that done by the NAS and the IPCC.

So these guys have taken biased estimates of tropospheric temperatures that have artificially too little warming trend, and then shown, quite unremarkably, that El Nino dominates much of what is left (the interannual variability). the paper has absolutely no implications that I can see at all for the role of natural variability on the observed warming trend of recent decades. other far more careful analyses (a paper by David Thompson of CSU, Phil Jones, and others published in Nature more than year ago) used proper, widely-accepted surface temperature data to estimate the influence of natural factors (El Nino and volcanos) on the surface temperature record. their analysis was so careful and clever that it detected a post-world war II error in sea surface temperature measurements (that yields artificial cooling during the mid 1940s) that had never before been discovered in the global surface temperature record. needless to say, they removed that error too. and the correct record, removing influences of ENSO, volcanoes, and even this newly detected error, reveal that a robust warming of global mean surface temperature over the past century of a little less than 1C which has nothing to do w/ volcanic influences or ENSO influences. the dominant source of the overall warming, as concluded in every legitimate major scientific assessment, is anthropogenic influences (human greenhouse gas concentrations w/ some offsetting cooling due to sulphate aerosols). this later paper provides absolutely nothing to cast that in doubt. it uses a flawed set of surface temperature measurements for which the trend has been artificially suppressed, to show that whats left over

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It's Seth again. Attached is a paper in JGR today that

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is hyping wildly. It's in a legit journal. Whatchya think?

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Bye the way June was the warmest month on record for the oceans

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Jim

Quoting Kevin Trenberth <[54]trenbert@ucar.edu>:

Exactly

They use 2 datasets that are deficient in the first place and then they use derivatives: differentiation is a high pass filter, and so they show what we have long known that ENSO accounts for a lot of high frequency variability. It should not have been published
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How the h... did this get accepted!!

Jim

Dominion today {24/7/09}

Nature blamed over warming - describing recently published paper

in

JGR by Chris de Freitas, Bob Carter and J McLean, and including

comment by J Salinger "little new"

McLean J. D., C. R. de Freitas, R. M. Carter (2009), Influence

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ph 303 497 1318

[56]<http://www.cgd.ucar.edu/cas/trenbert.html>

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of
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et al,
Santer et al, and Sherwood et al.
see Gavin's excellent RealClimate article on this:
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these papers collectively showed that both datasets were deeply
flawed
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absolutely remarkable that this paper could get through a serious
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From: Phil Jones <p.jones@uea.ac.uk>

To: Michael Mann <mann@meteo.psu.edu>, Jim Salinger <j.salinger@auckland.ac.nz>

Subject: Re: ENSO blamed over warming - paper in JGR

Date: Tue Jul 28 10:15:45 2009

Cc: trenbert@ucar.edu, j.renwick@niwa.co.nz, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>

Jim et al,

Having now read the paper in a moment of peace and quiet, there are a few things to bear in mind. The authors of the original will have a right of reply, so need to ensure that they don't have anything to come back on. From doing the attached a year or so ago, there is a word limit and also it is important to concentrate only on a few key points. As we all know there is so much wrong with the paper, it won't be difficult to come up with a few, but it does need to be just two or three.

The three aspects I would emphasize are

1. The first difference type filtering. Para 14 implies that they smooth the series with a 12 month running mean, then subtract the value in Jan 1980 from that in Jan 1979, then Feb 1980 from Feb 1979 and so on. As we know this removes any long-term trend.

The running mean also probably distorts the phase, so this is possibly why they get different lags from others. Using running means also enhances the explained variance. Perhaps we should repeat the exercise without the smoothing.

2. Figure 4 and Figure 1 show the unsmoothed GTTA series. These clearly have a trend. Perhaps show the residual after extracting the ENSO part.

3. They do the same first difference on the smoothed SOI. The SOI doesn't explain the climate jump in the 1976/77 period. Their arguments in para 30 are all wrong.

A few minor points

- there are some negative R^2 values just after equation 3.

- I'm sure Tom Wigley wouldn't have proposed El Nino events occurring after volcanoes!

Attached this paper as well. From a quick read it doesn't say what is purported - in fact

it seems to show clearly how the analysis should have been done.

- there is a paper by Ben Santer (more recent) where he applies the same type of extraction procedure to models. I'll send this separately as it is large. In case it is too large here is the reference.

Santer, B.D., Wigley, T.M.L., Doutriaux, C., Boyle, J.S., Hansen, J.E., Jones, P.D., Meehl, G.A., Roeckner, E., Sengupta, S. and Taylor K.E., 2001: Accounting for the effects of volcanoes and ENSO in comparisons of modeled and observed temperature trends. *Journal of Geophysical Research* 106, 2803328059.

Finally I've attached a paper I wrote in 1990, where I did something similar to what they did. I looked at residuals from a Gaussian filter, and I added the smoothed data back afterwards. I was working at the annual timescale and I did have many more years.

Cheers

Phil

At 00:19 25/07/2009, Michael Mann wrote:

Hi Jim,

Grant Foster ('Tamino') did a nice job in a previous response (attached) we wrote to a similarly bad article by Schwartz which got a lot of play in contrarian circles.

since he's already done some of the initial work in debunking this, I sent him an email asking hi if we was interested in spearheading a similar effort w/ this one.

let me get back to folks after I've heard back from him, and we can discuss possible strategy for moving this forward,
mike

On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:

Kia orana All from the Tropical South Pacific

Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin Tamino's bang up job is great, And good that you go up with stuff on Real Climate, Mike. As Kevin is preoccupied, for the scientific record we need a rebuttal somewhere pulled together. Who wants to join in on the multiauthored effort?? I am happy to coordinate it. Return to 'winter' this evening after enjoying a balmy south east trades and sunny dry 24 C in the Cook Islands.

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Quoting Michael Mann <mann@meteo.psu.edu>:

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I am tied up next week, but could frame something up the following week which , I hope would be multi-authored. It would be quite good to have a rebuttal from the same Department at Uni of Auckland (which Glenn McGregor of IJC is director of)!

I haven't had the opportunity to download the text here in the Cook Islands, so this would give me the opportunity to do that.

Who else wants to join in??

Jim

Quoting Kevin Trenberth <trenbert@ucar.edu>:

I am on vacation today and don't have the time. I have been on travel the past 4 weeks (including AR5 IPCC scoping mtg); the NCAR summer Colloquium is coming up in a week and then I am off to Oz and NZ for 3 weeks (GEWEX/iLeaps, CEOP) and I have an oceanobs'09 plenary paper to do.

Kevin

a formal comment to JGR seems like a worthwhile undertaking here. contrarians will continue to cite the paper regardless of whether or not its been rebutted, but for the purpose of future scientific assessments, its important that this be formally rebutted in the peer-reviewed literature.

mike

On Jul 23, 2009, at 9:05 PM, Jim Salinger wrote:

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Thanks for the pro-activeness. Is there an opportunity to write a letter to JGR pointing out the junk science in this?....if it is not rebutted, then all sceptics will use this to justify their position.

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Quoting Michael Mann <mann@meteo.psu.edu>:

2nd email

Thanks Kevin, hadn't even noticed that in my terse initial skim of

it. yes--that makes things even worse than my initial impression.

this is a truly horrible paper. one wonders who the editor was, and what he/she was thinking (or drinking),

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L is the period.

So the response to this procedure is to reduce periods of 10 years by a factor of 5 compared with periods of 2 years, or 20 and 50

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reduced by factors of 10 and 25 relative to two year periods.
i.e. Their
procedure is designed to only analyse the interannual
variability
not the
trends.
Kevin

hi Seth, you always seem to catch me at airports. only got a
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minutes. took a cursory look at the paper, and it has all the
worry
signs of extremely bad science and scholarship. JGR is a
legitimate
journal, but some extremely bad papers have slipped through
the
cracks
in recent years, and this is another one of them.
first of all, the authors use two deeply flawed datasets that
understate the warming trends: the Christy and Spencer MSU
data and
uncorrected radiosonde temperature estimates. There were a
series
of
three key papers published in Science a few years ago, by
Mears
et al,
Santer et al, and Sherwood et al.
see Gavin's excellent RealClimate article on this:
[1]<http://www.realclimate.org/index.php/archives/2005/08/et-tu- It/>
these papers collectively showed that both datasets were
deeply
flawed
and understate actual tropospheric temperature trends. I
find it
absolutely remarkable that this paper could get through a
serious
review w/out referencing any 3 of these critical papers-- papers
whose
findings render that conclusions of the current article
completely
invalid!
The Christy and Spencer MSU satellite-derived tropospheric
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and the correct record, removing influences of ENSO, volcanoes, and even this newly detected error, reveal that a robust warming of global mean surface temperature over the past century of a little less than 1C

which has nothing to do w/ volcanic influences or ENSO influences. the dominant source of the overall warming, as concluded in every legitimate major scientific assessment, is anthropogenic influences (human greenhouse gas concentrations w/ some offsetting cooling due to sulphate aerosols). this later paper provides absolutely nothing to cast that in doubt. it uses a flawed set of surface temperature measurements for which the trend has been artificially suppressed, to show that whats left over (interannual variability) is due to natural influences. duh! its a joke! and the aptly named Mark "Morano" has fallen for it!

m
On Jul 23, 2009, at 11:54 AM, Borenstein, Seth wrote:

Kevin, Gavin, Mike,
It's Seth again. Attached is a paper in JGR today that Marc Morano is hyping wildly. It's in a legit journal. Whatchya think?

Seth
Seth Borenstein
Associated Press Science Writer
sborenstein@ap.org
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20005-4076
202-641-9454

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Precisely.

Mike Mann: You better rush something up on RealClimate. Jim,
Brett, myself and maybe others will have to deal with the
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Bye the way June was the warmest month on record for the oceans
according to NOAA

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Quoting Kevin Trenberth <trenbert@ucar.edu>:

Exactly

They use 2 datasets that are deficient in the first place and
then they
use derivatives: differentiation is a high pass filter, and so
they show
what we have long known that ENSO accounts for a lot of high
frequency
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Kevin

kia orana from Rarotonga

How the h... did this get accepted!!

Jim

Dominion today {24/7/09}

Nature blamed over warming - describing recently published
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McLean J. D., C. R. de Freitas, R. M. Carter (2009),

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--

Associate Professor Jim Salinger
School of Geography and Environmental Science
University of Auckland
Private Bag 92 019
Auckland, New Zealand
Tel: + 64 9 373 7599 ext 88473

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Kevin Trenberth
Climate Analysis Section, NCAR
PO Box 3000
Boulder CO 80307
ph 303 497 1318
[3]<http://www.cgd.ucar.edu/cas/trenbert.html>

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The Pennsylvania State University email: mann@psu.edu
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website: [4]<http://www.meteo.psu.edu/~mann/Mann/index.html>
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first of all, the authors use two deeply flawed datasets that understate the warming trends: the Christy and Spencer MSU data and uncorrected radiosonde temperature estimates. There were a series of three key papers published in Science a few years ago, by Mears et al,

Santer et al, and Sherwood et al.

see Gavin's excellent RealClimate article on this:

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these papers collectively showed that both datasets were deeply flawed

and understate actual tropospheric temperature trends. I find it absolutely remarkable that this paper could get through a serious review w/out referencing any 3 of these critical papers--papers whose

findings render that conclusions of the current article completely invalid!

The Christy and Spencer MSU satellite-derived tropospheric temperature

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Nature blamed over warming - describing recently published paper
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JGR by Chris de Freitas, Bob Carter and J McLean, and including
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McLean J. D., C. R. de Freitas, R. M. Carter (2009), Influence
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Southern Oscillation on tropospheric temperature, J. Geophys.
Res.,

114, D14104, doi:10.1029/2008JD011637.

paper at

[19]<http://www.agu.org/pubs/crossref/2009/2008JD011637.shtml>

--

Associate Professor Jim Salinger

School of Geography and Environmental Science

University of Auckland

Private Bag 92 019
Auckland, New Zealand
Tel: + 64 9 373 7599 ext 88473

This message was sent using IMP, the Internet Messaging Program.

Kevin Trenberth
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PO Box 3000
Boulder CO 80307
ph 303 497 1318
[20]<http://www.cgd.ucar.edu/cas/trenbert.html>

This message was sent using IMP, the Internet Messaging Program.

--
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503 Walker Building FAX: (814)
865-3663
The Pennsylvania State University email: [21]mann@psu.edu
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website: [22]<http://www.meteo.psu.edu/~mann/Mann/index.html>
"Dire Predictions" book site:
[23]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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Prof. Phil Jones
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School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

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2. <http://www.agu.org/pubs/crossref/2009/2008JD011637.shtml>
3. <http://www.cgd.ucar.edu/cas/trenbert.html>
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16. <http://www.realclimate.org/index.php/archives/2005/08/et-tu-lt/>
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32. <http://www.meteo.psu.edu/~mann/Mann/index.html>
33. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

From: Jim Salinger <j.salinger@auckland.ac.nz>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Wed, 29 Jul 2009 06:22:53 +1200
Cc: Phil Jones <p.jones@uea.ac.uk>, Michael Mann <mann@meteo.psu.edu>, j.renwick@niwa.co.nz, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Annan <jdannan@jamstec.go.jp>, Grant Foster <tamino_9@hotmail.com>

<x-flowed>

Good morning all from tomorrowland (Wednesday!)

Gosh, you have all been very busy overnight here. Thank you, and Mike & I will start wordsmithing our section. We now have (in IPCC terms) a nice bunch of LA's and CAs for this commentary!

'Talk' to you later!

Jim

Quoting Kevin Trenberth <trenbert@ucar.edu>:

> Phil
> see also this:
> Trenberth, K. E., and L. Smith, 2009: Variations in the three
> dimensional structure of the atmospheric circulation with different
> flavors of El Niño. /J. Climate/, *12*, No. 11, 2978-2991, doi:
> 10.1175/2008JCLI2691.1. [PDF]
>
> <<http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/TrenberthSmithVTempJC109.pdf>>
> It has tables with relationships with Nino 3.4 and SOI and you can
> see the differences in lead lag e.g. Table 1. SOI leads Nino 3.4 by
> 1 or 2 months typically but as in the 2002 paper, the leads and
> lags vary with Nino index, see also
> Trenberth, K. E., and D. P. Stepaniak, 2001: Indices of El Niño
> evolution. /J. Climate./, *14*, 1697-1701. [Paper(.pdf)]
> <<http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/tniJC.pdf>>
> ^*
> <<http://www.cgd.ucar.edu/cas/Trenberth/trenberth-publish.html#amscr>>
>
> Kevin
>
> Phil Jones wrote:
>>
>> Kevin, Mike et al,
>> Figure 3 in what Kevin just sent is the sort of thing we need to
>> show.
>> On the lags, I think the reason the lag with what Tom did was
>> different is
>> that you used Nino3.4 SST and Tom used SOI. I know people think
>> they are the same thing, but I think SOI lags a little behind 3.4 SST.
>> It would be a useful bit of new science to look at the links
between SOI

>> and 3.4 SST, but it shouldn't be part of a comment on what's wrong with the
>> awful paper For that you're going to have to use the Bureau Of Meteorology
>> version of the SOI. These are on this web site
>> <http://www.bom.gov.au/climate/current/soihtml.shtml>
>> I did check a few years ago and these numbers look pretty much the same
>> as the CRU ones (allowing for the BoM multiplier of 10).
>> When you calculate the SOI you normalize the Darwin
>> and Tahiti series. BoM change the base period with each new year, so
>> don't expect to get exactly the same results as McLean.
>> You have to smooth the SOI series in some way as it is noisy.
>> Their running mean
>> is a lousy filter. I'd recommend using the one we did in Ch 3 of
>> IPCC. It is on
>> p336. The second filter will work fine, with all the months in
>> sequence. It will
>> approximate a 10-12 month filter and won't do anything to the phase.
Maybe
>> doing this with SOI and Nino3.4 will show a slight lag between the
>> two - 3 months
>> maybe!
>>
>> Cheers
>> Phil
>>
>>
>> At 15:57 28/07/2009, Kevin Trenberth wrote:
>>> The leads and lags are analyzed in detail in this paper
>>> Trenberth, K. E., J. M. Caron, D. P. Stepaniak, and S. Worley
>>> 2002: The evolution of ENSO and global atmospheric surface
>>> temperatures <<http://www.cgd.ucar.edu/cas/papers/2000JD000298.pdf>>
>>> /J. Geophys. Res./, *107*, D8, 10.1029/2000JD000298.
>>> and we were not able to reproduce Tom Wigley's result (we tried).
>>> It may depend in indices used. In this paper we also document the
>>> extent to which ENSO contributes to warming overall.
>>> Kevin
>>>
>>> Phil Jones wrote:
>>>> Mike,
>>>> See below for instructions.
>>>>
>>>> Also, just because IPCC (2007, Ch 3) didn't point out the 6/7-month lag
>>>> between the SOI and global temperatures doesn't mean it hasn't been
>>>> known for years. IPCC is an assessment and not a review of
everything
>>>> done. If they had even read Wigley (2001) they would have seen this
>>>> lag pointed out. I wasn't the first to do this in 1989 either. I
don't
>>>> think Walker was either. I think the first was Hildebrandsson in the
>>>> 1890s. Why does it always go back to a Swede!
>>>>

>>>> file is at ftp.cru.uea.ac.uk <ftp://ftp.cru.uea.ac.uk/>
>>>>
>>>> login anonymously with emails as pw
>>>>
>>>> then go to people/philjones
>>>>
>>>> and you should find santeretal2001.pdf
>>>>
>>>> Cheers
>>>> Phil
>>>>
>>>>
>>>> At 14:08 28/07/2009, Michael Mann wrote:
>>>>> thanks Phil,
>>>>>
>>>>> this is very helpful and reaffirms what we've identified as some
>>>>> of the main points that need to be covered in a formal response.
>>>>> I've taken the liberty of copying in a couple other colleagues
>>>>> who have been looking into this. Grant Foster was the first
>>>>> author on a response to a similarly bad paper by Schwartz that
>>>>> was published some time ago, and has been doing a number of
>>>>> analyses aimed at demonstrating the key problems in McClean et al.
>>>>>
>>>>> I've suggested that Grant sent out a draft of the response when
>>>>> it is ready to the broader group of people who have been
>>>>> included in these exchanges for feedback and potential
>>>>> co-authorship,
>>>>>
>>>>> mike
>>>>>
>>>>> p.s. Santer et al paper still didn't come through in your
>>>>> followup message. Can you post in on ftp where it can be
>>>>> downloaded?
>>>>> On Jul 28, 2009, at 5:15 AM, Phil Jones wrote:
>>>>>
>>>>>>
>>>>>> Jim et al,
>>>>>> Having now read the paper in a moment of peace and quiet,
>>>>>> there are a few things
>>>>>> to bear in mind. The authors of the original will have a right
>>>>>> of reply, so need to
>>>>>> ensure that they don't have anything to come back on. From
>>>>>> doing the attached a
>>>>>> year or so ago, there is a word limit and also it is important
>>>>>> to concentrate only
>>>>>> on a few key points. As we all know there is so much wrong with
>>>>>> the paper, it
>>>>>> won't be difficult to come up with a few, but it does need to
>>>>>> be just two or three.
>>>>>>
>>>>>> The three aspects I would emphasize are
>>>>>>
>>>>>> 1. The first difference type filtering. Para 14 implies that
>>>>>> they smooth the series

>>>>> with a 12 month running mean, then subtract the value in Jan
>>>>> 1980 from that in
>>>>> Jan 1979, then Feb 1980 from Feb 1979 and so on. As we know
>>>>> this removes
>>>>> any long-term trend.
>>>>>
>>>>> The running mean also probably distorts the phase, so this is
>>>>> possibly why
>>>>> they get different lags from others. Using running means also
>>>>> enhances the
>>>>> explained variance. Perhaps we should repeat the exercise
>>>>> without the smoothing.
>>>>>
>>>>> 2. Figure 4 and Figure 1 show the unsmoothed GTTA series. These
>>>>> clearly have a
>>>>> trend. Perhaps show the residual after extracting the ENSO part.
>>>>>
>>>>> 3. They do the same first difference on the smoothed SOI. The
>>>>> SOI doesn't explain
>>>>> the climate jump in the 1976/77 period. Their arguments in para
>>>>> 30 are all wrong.
>>>>>
>>>>> A few minor points
>>>>>
>>>>> - there are some negative R*R values just after equation 3.
>>>>> - I'm sure Tom Wigley wouldn't have proposed El Nino events
>>>>> occurring after volcanoes!
>>>>> Attached this paper as well. From a quick read it doesn't
>>>>> say what is purported - in fact
>>>>> it seems to show clearly how the analysis should have been
done.
>>>>> - there is a paper by Ben Santer (more recent) where he
>>>>> applies the same type
>>>>> of extraction procedure to models. I'll send this separately as
>>>>> it is large. In case it
>>>>> is too large here is the reference.
>>>>>
>>>>> Santer, B.D., Wigley, T.M.L., Doutriaux, C., Boyle, J.S.,
>>>>> Hansen, J.E., Jones, P.D., Meehl, G.A., Roeckner, E., Sengupta,
>>>>> S. and Taylor K.E., 2001: Accounting for the effects of
>>>>> volcanoes and ENSO in comparisons of modeled and observed
>>>>> temperature trends. Journal of Geophysical Research 106,
>>>>> 28033;28059.
>>>>>
>>>>>
>>>>> Finally I've attached a paper I wrote in 1990, where I did
>>>>> something similar to
>>>>> what they did. I looked at residuals from a Gaussian filter, and I
added
>>>>> the smoothed data back afterwards. I was working at the annual
timescale
>>>>> and I did have many more years.
>>>>>
>>>>> Cheers

>>>>> Phil
>>>>>
>>>>>
>>>>> At 00:19 25/07/2009, Michael Mann wrote:
>>>>> Hi Jim,
>>>>>
>>>>> Grant Foster ('Tamino') did a nice job in a previous response
>>>>> (attached) we wrote to a similarly bad article by Schwartz which
got a
>>>>> lot of play in contrarian circles.
>>>>>
>>>>> since he's already done some of the initial work in debunking
this, I
>>>>> sent him an email asking hi if we was interested in spearheading
a
>>>>> similar effort w/ this one.
>>>>>
>>>>> let me get back to folks after I've heard back from him, and we
can
>>>>> discuss possible strategy for moving this forward,
>>>>>
>>>>> mike
>>>>>
>>>>> On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:
>>>>>
>>>>>> Kia orana All from the Tropical South Pacific
>>>>>>
>>>>>> Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin
>>>>>> Tamino's bang up job is great, And good that you go up with
stuff on
>>>>>> Real Climate, Mike. As Kevin is preoccupied, for the scientific
>>>>>> record we need a rebuttal somewhere pulled together. Who wants
to
>>>>>> join in on the multiauthored effort?? I am happy to coordinate
it.
>>>>>>
>>>>>> Return to 'winter' this evening after enjoying a balmy south
east
>>>>>> trades and sunny dry 24 C in the Cook Islands.
>>>>>>
>>>>>> Jim
>>>>>>
>>>>>> Quoting Michael Mann <mann@meteo.psu.edu>
<mailto:mann@meteo.psu.edu>:
>>>>>>
>>>>>>> folks, we're going to go up w/ something brief on RealClimate
>>>>>>> later today, mostly just linking to other useful
deconstructions
>>>>>>> of the paper already up on other sites,
>>>>>>>
>>>>>>> mike
>>>>>>>
>>>>>>> On Jul 23, 2009, at 11:01 PM, Jim Salinger wrote:
>>>>>>>

>>>>>>>>> I am tied up next week, but could frame something up the
>>>>>>>>> following week which , I hope would be multi-authored. It
would
>>>>>>>>> be quite good to have a rebuttal from the same Department at
Uni
>>>>>>>>> of Auckland (which Glenn McGregor of IJC is director of)!

>>>>>>>>>
>>>>>>>>> I haven't had the opportunity to download the text here in the
>>>>>>>>> Cook Islands, so this would give me the opportunity to do
that.
>>>>>>>>> Who else wants to join in??
>>>>>>>>>
>>>>>>>>> Jim
>>>>>>>>>
>>>>>>>>>
>>>>>>>>>
>>>>>>>>>
>>>>>>>>> Quoting Kevin Trenberth <trenbert@ucar.edu>
>>>>>>>>> <mailto:trenbert@ucar.edu>:
>>>>>>>>>
>>>>>>>>>> I am on vacation today and don't have the time. I have been
on
>>>>>>>>>> travel the
>>>>>>>>>> past 4 weeks (including AR5 IPCC scoping mtg); the NCAR
summer
>>>>>>>>>> Colloquium
>>>>>>>>>> is coming up in a week and then I am off to Oz and NZ for 3
weeks
>>>>>>>>>> (GEWEX/iLeaps, CEOP) and I have an oceanobs'09 plenary paper
to do.
>>>>>>>>>> Kevin
>>>>>>>>>>
>>>>>>>>>>> a formal comment to JGR seems like a worthwhile undertaking
here.
>>>>>>>>>>>> contrarians will continue to cite the paper regardless of
>>>>>>>>>>>> whether or
>>>>>>>>>>>> not its been rebutted, but for the purpose of future
scientific
>>>>>>>>>>>> assessments, its important that this be formally rebutted in
>>>>>>>>>>>> the peer-
>>>>>>>>>>>> reviewed literature.
>>>>>>>>>>>>
>>>>>>>>>>>> mike
>>>>>>>>>>>>
>>>>>>>>>>>>> On Jul 23, 2009, at 9:05 PM, Jim Salinger wrote:
>>>>>>>>>>>>>
>>>>>>>>>>>>>> Hi All
>>>>>>>>>>>>>>
>>>>>>>>>>>>>>> Thanks for the pro-activeness. Is there an opportunity to
write a
>>>>>>>>>>>>>>>>> letter to JGR pointing out the junk science in this??....if
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>>>>>>>>>>>>>>>>>> not rebutted, then all sceptics will use this to justify
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>>>>>>>>>> position.
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>>>>>>>>>> Jim
>>>>>>>>>>
>>>>>>>>>>
>>>>>>>>>> Quoting Michael Mann <mann@meteo.psu.edu>
>>>>>>>>>> <mailto:mann@meteo.psu.edu>:
>>>>>>>>>>
>>>>>>>>>> 2nd email
>>>>>>>>>>
>>>>>>>>>> _____
>>>>>>>>>>
>>>>>>>>>> Thanks Kevin, hadn't even noticed that in my terse
>>>>>>>>>> initial skim of
>>>>>>>>>> it. yes--that makes things even worse than my initial
>>>>>>>>>> impression.
>>>>>>>>>>
>>>>>>>>>> this is a truly horrible paper. one wonders who the editor
was,
>>>>>>>>>> and what he/she was thinking (or drinking),
>>>>>>>>>>
>>>>>>>>>> m
>>>>>>>>>>
>>>>>>>>>> On Jul 23, 2009, at 3:51 PM, Kevin Trenberth wrote:
>>>>>>>>>>
>>>>>>>>>> I just looked briefly at the paper. Their relationships
use
>>>>>>>>>> derivatives
>>>>>>>>>> of the series. Well derivatives are equivalent to a high
pass
>>>>>>>>>> filter,
>>>>>>>>>> that is to say it filters out all the low frequency
>>>>>>>>>> variability and
>>>>>>>>>> trends.
>>>>>>>>>>
>>>>>>>>>> If one takes $y = A \sin wt$
>>>>>>>>>> and does a differentiation one gets
>>>>>>>>>> $dy = Aw \cos wt$.
>>>>>>>>>>

>>>>>>>> -----

--

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>>>>>>>>

>>>>>>>> --

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>>>>>>>> Professor

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>>>>>>>>

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>>>> analyses aimed at demonstrating the key problems in McClean et al.
>>>>
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>>>> it is ready to the broader group of people who have been
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>>>> p.s. Santer et al paper still didn't come through in your
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>>>>>
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>>>>>>
>>>>>> since he's already done some of the initial work in debunking
>>>>>> this, I sent him an email asking hi if we was interested in
>>>>>> spearheading a similar effort w/ this one.
>>>>>>
>>>>>> let me get back to folks after I've heard back from him, and
>>>>>> we can discuss possible strategy for moving this forward,
>>>>>>
>>>>>> mike
>>>>>>
>>>>>> On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:
>>>>>>
>>>>>> Kia orana All from the Tropical South Pacific
>>>>>>
>>>>>> Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin
>>>>>> Tamino's bang up job is great, And good that you go up with
>>>>>> stuff on Real Climate, Mike. As Kevin is preoccupied, for the
>>>>>> scientific record we need a rebuttal somewhere pulled
>>>>>> together. Who wants to join in on the multiauthored effort??
>>>>>> I am happy to coordinate it.
>>>>>>
>>>>>> Return to 'winter' this evening after enjoying a balmy south
>>>>>> east trades and sunny dry 24 C in the Cook Islands.
>>>>>>
>>>>>> Jim
>>>>>>
>>>>>> Quoting Michael Mann <mann@meteo.psu.edu
<mailto:mann@meteo.psu.edu>>:
>>>>>>
>>>>>>> folks, we're going to go up w/ something brief on
>>>>>>> RealClimate later today, mostly just linking to other
>>>>>>> useful deconstructions of the paper already up on other
>>>>>>> sites,
>>>>>>>
>>>>>>> mike
>>>>>>>
>>>>>>> On Jul 23, 2009, at 11:01 PM, Jim Salinger wrote:
>>>>>>>
>>>>>>>> I am tied up next week, but could frame something up the
>>>>>>>> following week which , I hope would be multi-authored. It
>>>>>>>> would be quite good to have a rebuttal from the same
>>>>>>>> Department at Uni of Auckland (which Glenn McGregor of IJC
>>>>>>>> is director of)!
>>>>>>>>
>>>>>>>> I haven't had tne oportunity to download the text here in
>>>>>>>> the Cook Islands, so this would give me the opportunity to
>>>>>>>> do that. Who else wants to join in??
>>>>>>>>
>>>>>>>> Jim
>>>>>>>>

>>>>>>
>>>>>> mike
>>>>>>
>>>>>> On Jul 24, 2009, at 6:11 PM, Jim Salinger wrote:
>>>>>>
>>>>>> Kia orana All from the Tropical South Pacific
>>>>>>
>>>>>> Yes, Phil, a bit like 'A midsummer night's dream!'. and Gavin
>>>>>> Tamino's bang up job is great, And good that you go up with
>>>>>> stuff on Real Climate, Mike. As Kevin is preoccupied, for the
>>>>>> scientific record we need a rebuttal somewhere pulled
>>>>>> together. Who wants to join in on the multiauthored effort??
>>>>>> I am happy to coordinate it.
>>>>>>
>>>>>> Return to 'winter' this evening after enjoying a balmy south
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>>>>>>>>
>>>>>>>> Jim
>>>>>>>>
>>>>>>>>
>>>>>>>>
>>>>>>>>
>>>>>>>> Quoting Kevin Trenberth <trenbert@ucar.edu
>>>>>>>> <mailto:trenbert@ucar.edu>>:
>>>>>>>>
>>>>>>>>> I am on vacation today and don't have the time. I have
>>>>>>>>> been on travel the
>>>>>>>>> past 4 weeks (including AR5 IPCC scoping mtg); the NCAR
>>>>>>>>> summer Colloquium

>>>>>>>>>> is coming up in a week and then I am off to Oz and NZ for 3 weeks
>>>>>>>>>> (GEWEX/iLeaps, CEOP) and I have an oceanobs'09 plenary paper to do.
>>>>>>>>>> Kevin
>>>>>>>>>>
>>>>>>>>>> a formal comment to JGR seems like a worthwhile undertaking here.
>>>>>>>>>> contrarians will continue to cite the paper regardless of
>>>>>>>>>> whether or
>>>>>>>>>> not its been rebutted, but for the purpose of future scientific
>>>>>>>>>> assessments, its important that this be formally rebutted
>>>>>>>>>> in the peer-
>>>>>>>>>> reviewed literature.
>>>>>>>>>>
>>>>>>>>>> mike
>>>>>>>>>>
>>>>>>>>>> On Jul 23, 2009, at 9:05 PM, Jim Salinger wrote:
>>>>>>>>>>
>>>>>>>>>> Hi All
>>>>>>>>>>
>>>>>>>>>> Thanks for the pro-activeness. Is there an opportunity to write a
>>>>>>>>>> letter to JGR pointing out the junk science in this??....if it is
>>>>>>>>>> not rebutted, then all sceptics will use this to justify their
>>>>>>>>>> position.
>>>>>>>>>>
>>>>>>>>>> Jim
>>>>>>>>>>
>>>>>>>>>>
>>>>>>>>>> Quoting Michael Mann <mann@meteo.psu.edu
>>>>>>>>>> <mailto:mann@meteo.psu.edu>:
>>>>>>>>>>
>>>>>>>>>> 2nd email
>>>>>>>>>>
>>>>>>>>>> _____
>>>>>>>>>>
>>>>>>>>>> Thanks Kevin, hadn't even noticed that in my terse
>>>>>>>>>> initial skim of
>>>>>>>>>> it. yes--that makes things even worse than my initial
>>>>>>>>>> impression.
>>>>>>>>>>
>>>>>>>>>> this is a truly horrible paper. one wonders who the editor was,
>>>>>>>>>> and what he/she was thinking (or drinking),
>>>>>>>>>>
>>>>>>>>>> m
>>>>>>>>>>
>>>>>>>>>> On Jul 23, 2009, at 3:51 PM, Kevin Trenberth wrote:
>>>>>>>>>>>>

>> -----

>
> --
> *****
> Kevin E. Trenberth e-mail: trenbert@ucar.edu
> Climate Analysis Section, www.cgd.ucar.edu/cas/trenbert.html
> NCAR
> P. O. Box 3000, (303) 497 1318
> Boulder, CO 80307 (303) 497 1333 (fax)
>
> Street address: 1850 Table Mesa Drive, Boulder, CO 80305
>
>

This message was sent using IMP, the Internet Messaging Program.
</x-flowed>

From: Kevin Trenberth <trenbert@ucar.edu>
To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Wed, 29 Jul 2009 10:23:09 -0600
Cc: Grant Foster <tamino_9@hotmail.com>, p.jones@uea.ac.uk, "J. Salinger"
<j.salinger@auckland.ac.nz>, j.renwick@niwa.co.nz, b.mullan@niwa.co.nz,
Gavin Schmidt <gschmidt@giss.nasa.gov>, James Annan
<jdannan@jamstec.go.jp>

Hi all

Wow this is a nice analysis by Grant et al. What we should do is turn this into a learning

experience for everyone: there is often misuse of filtering.

Obviously the editor and

reviewers need to to also be taken to task here. I agree with Mike Mann that a couple of

other key points deserve to be made wrt this paper. Making sure that the important

relationships and role of ENSO on interannual variability of global temperatures should

also be pointed out with some select references (as in recent emails and the refs

therein). In terms of the paper, I recommend consolidating the figures to keep them fewer

in number if this is a comment: combine Figs 3 with 4 , and 6 with 7.

Make sure the plots

of spectra have period prominently displayed as well as frequency and maybe even highlight

with stipple some bands like >10 years. Glad to sign on: I would need an acknowledgment

that NCAR is sponsored by NSF.

Regards

Kevin

Michael Mann wrote:

thanks Grant, the paper is starting to shape up well now. Jim and I (well, mostly Jim,

w/ some input from me) are iterating on a blurb about past studies on ENSO/temperature

relationships and should have something for you soon on that,

As James has pointed out, its important to stick to the key points and not get sidetracked

with nonsense. I would avoid any commentary on their ignorant ramblings about the Hadley

Cell, etc. We want to cut straight to the deep flaws in their analysis which are, in order

of importance in my view,

1. indefensible use of a differencing filter, which has the effect of selectively damping

low-frequency variability and renders any conclusions about factors underlying long-term

trends completely spurious.

2. ignoring the fact that the influence of ENSO on global temperature has been known for decades, and much better quantified in past studies than in the current deeply flawed analysis.

3. the selective use of a flawed temperature data and curious splicing in of inappropriate recent data (UAH TMT) to further suppress trends. A bit of overkill given that they already eliminated the trends anyway. Guess they wanted to play it extra cautious just in case some bit of warming trend tried to sneak in.

The other stuff is just a distraction.

mike

On Jul 29, 2009, at 10:51 AM, Grant Foster wrote:

Gentlemen,

Attached is a zip file with LaTeX and pdf for a first draft. I've included everybody's name (in alphabetical order after mine), but of course it should only include in submission those who give explicit consent.

There are a few other issues. One is that MFC have recently removed the pdf version of their paper from the "New Zealand Climate Coalition" website. They've replaced it with this:

[1]http://nzclimatescience.net/index.php?option=com_content&task=view&id=502&Itemid=1

which refers to a graph showing only part of figure 7, and suggests that there's not trend

in GTTA so "nothing to worry about." Yet the plotted GTTA is from UAH TMT (*not* TLT) so

of course it shows no trend, and the MT channel is contaminated by stratospheric cooling.

In figure 7 of the paper itself they compare the 50-year record of SOI and GTTA, but their

graph of GTTA is made of RATPAC-A data until 1980 grafted onto UAH TMT data afterward --

hence the lack of an obvious trend. I think this too should be mentioned, especially as

the entire RATPAC-A record shows a very pronounced trend.

One last thing: there's a lot of stuff in the paper about Hadley cells and heat transport

and so forth. I suspect this is really a bunch of gobbledygook -- but I don't know. But

I'll bet you guys do. Comments?

Sincerely,

Grant

Windows Live(TM) Hotmail«: Celebrate the moment with your favorite sports pics. [2]Check it out. <comment.zip>

--

Michael E. Mann
Professor
Director, Earth System Science Center (ESSC)
Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [3]mann@psu.edu
University Park, PA 16802-5013
website: [4]<http://www.meteo.psu.edu/~mann/Mann/index.html>
"Dire Predictions" book site:
[5]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

--

Kevin E. Trenberth e-mail: [6]trenbert@ucar.edu
Climate Analysis Section, [7]www.cgd.ucar.edu/cas/trenbert.html
NCAR
P. O. Box 3000, (303) 497 1318
Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

References

1.
http://nzclimatescience.net/index.php?option=com_content&task=view&id=502&Itemid=1
2.
http://www.windowslive.com/Online/Hotmail/Campaign/QuickAdd?ocid=TXT_TAGL_M_WL_QA_HM_sports_photos_072009&cat=sports
3. <mailto:mann@psu.edu>
4. <http://www.meteo.psu.edu/%7Emann/Mann/index.html>
5. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
6. <mailto:trenbert@ucar.edu>
7. <http://www.cgd.ucar.edu/cas/trenbert.html>

From: Phil Jones <p.jones@uea.ac.uk>
To: Thomas.C.Peterson@noaa.gov
Subject: Re: This and that
Date: Wed Jul 29 17:19:53 2009

Tom,

Good idea with that BAMS paper. There is also the KNMI web site, which tells that they have restricted data from Europe - on the ECA part. Both despite WMO-Res40!

On IPCC, I suggested Thomas to not get too many hangers on amongst the LAs. Chs 2 and 14 are prime candidates for upping the geographic spread. We had about half of ours not doing that much last time.

Isn't Tom Karl on the US nominating committee?

Away all day tomorrow - CRU barbecue - so will pour down.

Cheers

Phil

At 17:07 29/07/2009, you wrote:

Hi, Phil,

Yes, Friday-Saturday I noticed that ClimateFraudit had renewed their interest in you. I was thinking about sending an email of sympathy, but I was busy preparing for a quick trip to Hawaii - I left Monday morning and flew out Tuesday evening and am now in the Houston airport on my way home.

Data that we can't release is a tricky thing here at NCDC. Periodically, Tom Karl will twist my arm to release data that would violate agreements and therefore hurt us in the long run, so I would prefer that you don't specifically cite me or NCDC in this.

But I can give you a good alternative. You can point to the Peterson-Manton article on regional climate change workshops. All those workshops resulted in data being provided to the author of the peer-reviewed paper with a strict promise that none of the data would be released. So far as far as I know, we have all lived up to that agreement - myself with the Caribbean data (so that is one example of data I have that are not released by NCDC), Lucie and Malcolm for South America, Enric for Central America, Xuebin for Middle Eastern data, Albert for south/central Asian data, John Ceasar for SE Asia, Enric again for central Africa, etc. The point being that such agreements are common and are the only way that we have access to quantitative insights into climate change in many parts of the world. Many countries don't mind the release of derived products such as your gridded field or Xuebin's ETCCDI indices, but very much object to the release of actual

data (which they might sell to potential users). Does that help?

Regarding AR4, I would like to be part of it. I have no idea what role would be deemed appropriate. One thing I noticed with the CLAs in my old chapter is that if one isn't up to doing his part (too busy, or a different concept of timeliness, or ...) it can make for a difficult job. You and I have worked well together before (e.g., GSN) so I'd be delighted to work with you on it and I know you'd hold up your side of the tasks. We touched on this briefly at the AOPC meeting. If I get an opportunity, I would say yes.

But I also don't know what the U.S. IPCC nominating approach would be or even who decides that. There is an upcoming IPCC report on extremes and impacts of extremes and I wasn't privy to any insights into the U.S. nominations other than when it was over it was announced in NCDC staff notes that the nominations had been made. However, Kumar had earlier asked if he could nominate me, so he did (I provided him with the details).

Regards,

Tom

Tom,

If you look on Climate Audit you will see that I'm all over it! Our ftp site is regularly trawled as I guess yours is. It seems that a Canadian along with two Americans copied some files we put there for MOHC in early 2003. So saying they have the CRU data is not quite correct. What they have is our raw data for CRUTEM2 which went into Jones and Moberg (2003) - data through end of 2002.

Anyway enough of my problems - I have a question for you. I'm going to write a small document for our web site to satisfy (probably the wrong word) the 50 or so FOI/EIR requests we've had over the weekend. I will put up the various agreements we have with Met Services.

The question - I think you told me one time that you had a file containing all the data you couldn't release (i.e. it's not in GHCN). Presumably this is not in your gridded datasets? Do you know off hand how much data is in this category? Would NCDC mind if I mentioned that you have such data - not the amount/locations/anything, just that there is some?

On something positive - attached is the outlines for the proposed Chs in AR5/WG1. Ch1 is something Thomas thinks he can write himself - well with Qin Dahe, so only 13 chapters. There are a lot of issues with overlaps between some of the data chapters 2 with 3, 2 with 5 and 2 with 14.

I'm still thinking about whether to get involved. It would be 2 if I decide. At the moment I'd say yes, but I might change my mind tomorrow! Nominations are from Nov09 thru Jan10 with the selection made in April 10. Are you considering getting involved?

I have got the IPCC Secretariat and Thomas to raise the FOI issues with the full IPCC Plenary, which meets in Bali in September or October. Thomas is fully aware of all the issues we've had here wrt Ch 6 last time, and others in the US have had.

Cheers

Phil

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich Email p.jones@uea.ac.uk

NR4 7TJ

UK

Prof. Phil Jones

Climatic Research Unit Telephone +44 (0) 1603 592090

School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich Email p.jones@uea.ac.uk

NR4 7TJ

UK

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: "Michael Mann" <mann@meteo.psu.edu>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Wed, 29 Jul 2009 21:15:39 -0600 (MDT)
Reply-to: trenbert@ucar.edu
Cc: "Jim Salinger" <j.salinger@auckland.ac.nz>, "James Renwick"
<j.renwick@niwa.co.nz>, gschmidt@giss.nasa.gov, tamino_9@hotmail.com,
jdannan@jamstec.go.jp, "Brett Mullan" <b.mullan@niwa.co.nz>,
p.jones@uea.ac.uk

Content-Type: text/plain; charset="iso-8859-1"
X-MIME-Autoconverted: from 8bit to quoted-printable by
ueamailgate02.uea.ac.uk id n6U3Feqd018708

See some suggested mods

BTW the T et al 2002 paper was one that got horribly caught up in the JGR transition to electronic publication and the doi etc was not properly set.

It was not published on time but delayed by some 6 months when about 10 issues came out all at once, and no one read it!

Kevin

> dear all,
>
> here's a revised intro based on a few iterations between Jim and me.
> Grant--please incorporate this into your next revision of the m.s.,
>
> others feel free to suggest changes/additions/etc.
>
> thanks,
>
> mike
>
> On Jul 29, 2009, at 4:26 PM, Jim Salinger wrote:
>
>> Kia ora all and Austral Jim
>>
>> Don't get sacked now (lol)....well you must be famous if he is
>> making a complaint...I guess he can't get at me here. Mike and I are
>> just putting some wee finishing touches to the intro bit then Mike
>> will circulate it more widely later.
>>
>> It seems that Hildebrandsson was the real originator of atmospheric
>> centres of action (see attached), and that Walker was just using his
>> ideas...interesting stuff - and perhaps it is time for a review by
>> someone....Kevin???
>>
>> I concur with Phil and Mike in that we don't critique their rather
>> bad knowledge of Hadley Cell and stuff and just cut to the chase.
>> Interesting that they are EVEN cherry picking their own paper. They
>> have whipped up a storm through farmers in NZ who are using this to
>> vehemently deny climate change, and therefore not address on farm
>> emissions from CH4 and N2O and leave it to all the rest of us (when
>> 60-70% of our electricity is renewable!) so I guess we all will be

>> walking and cycling very quickly as farmers keep their animals
>> burping out methane...that's my little sermon for this morning!
>>
>> Adios for now
>>
>> Not quite so Austral Jim
>>
>>
>> James Renwick wrote:
>>> Dear all:
>>> Great stuff, while I've sat back and watched... For info, I've just
>>> heard that Bob Carter has sent a formal complaint to NIWA, about
>>> comments I made, to a local reporter, on the paper. I'll be talking
>>> to
>>> our comms people tomorrow about a response (and I haven't actually
>>> seen
>>> the complaint yet).
>>> Regards,
>>> Jim R
>>> -----
>>> Dr James Renwick
>>> Principal Scientist, Climate Variability & Change
>>> NIWA
>>> Private Bag 14901, Wellington
>>> +64-4-386-0343 +64-21-178-5550
>>>> Jim Salinger <j.salinger@auckland.ac.nz> 07/30/09 6:22 AM >>>
>>> Kia ora All from the Land of the Long White Cloud and Thursday
>>> Thanks all...Phil I found reference to the Hildrebrandsson stuff
>>> ibn 'Recent Researches on Climate by N N Dickson in The
>>> geographical Journal 10 (3) 1897 303-306. Good fun! Mike and I
>>> will finish iterating our bit this morning and then it can be
>>> added in to Grant's fine work!
>>> Talk to you later
>>> Jim
>>> Quoting Kevin Trenberth <trenbert@ucar.edu>:
>>>> Hi all
>>>> Wow this is a nice analysis by Grant et al. What we should do is
>>>> turn this into a learning experience for everyone: there is often
>>>> misuse of filtering. Obviously the editor and reviewers need to
>>>> to also be taken to task here. I agree with Mike Mann that a
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>>>> consolidating the figures to keep them fewer in number if this is
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>>>> plots of spectra have period prominently displayed as well as
>>>> frequency and maybe even highlight with stipple some bands like
>>>> >10 years. Glad to sign on: I would need an acknowledgment that
>>>> NCAR is sponsored by NSF.
>>>> Regards
>>>> Kevin
>>>>

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>>>>
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>>>> analysis which are, in order of importance in my view,
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>>>> 2. ignoring the fact that the influence of ENSO on global
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>>>> already eliminated the trends anyway. Guess they wanted to play
>>>> it extra cautious just in case some bit of warming trend tried
>>>> to sneak in.
>>>>
>>>> The other stuff is just a distraction.
>>>>
>>>> mike
>>>>
>>>> On Jul 29, 2009, at 10:51 AM, Grant Foster wrote:
>>>>
>>>>> Gentlemen,
>>>>>
>>>>> Attached is a zip file with LaTeX and pdf for a first draft.
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>>>>>
>>>>> There are a few other issues. One is that MFC have recently
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>>>>> Climate Coalition" website. They've replaced it with this:
>>>>>
>>>>>
>>>>
http://nzclimatescience.net/index.php?option=com_content&task=view&id=502&Itemid=1
>>>>
>>>>> which refers to a graph showing only part of figure 7, and
>>>>> suggests that there's not trend in GTTA so "nothing to worry
>>>>> about." Yet the plotted GTTA is from UAH TMT (*not* TLT) so of
>>>>> course it shows no trend, and the MT channel is contaminated by
>>>>> stratospheric cooling.
>>>>>
>>>>> In figure 7 of the paper itself they compare the 50-year record

>>>>> of SOI and GTTA, but their graph of GTTA is made of RATPAC-A
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>>>>> the lack of an obvious trend. I think this too should be
>>>>> mentioned, especially as the entire RATPAC-A record shows a
>>>>> very pronounced trend.

>>>>>

>>>>> One last thing: there's a lot of stuff in the paper about
>>>>> Hadley cells and heat transport and so forth. I suspect this
>>>>> is really a bunch of gobbledygook -- but I don't know. But
>>>>> I'll bet you guys do. Comments?

>>>>>

>>>>> Sincerely,

>>>>> Grant

>>>>>

>>>>>

>>>>>

>>>>> -----

>>>>> Windows Live® Hotmail®: Celebrate the moment with your favorite
>>>>> sports pics. Check it out.

>>>>>

<[http://www.windowslive.com/Online/Hotmail/Campaign/QuickAdd?ocid=TXT_TAG
LM_WL_QA_HM_sports_photos_072009&cat=sports](http://www.windowslive.com/Online/Hotmail/Campaign/QuickAdd?ocid=TXT_TAG_LM_WL_QA_HM_sports_photos_072009&cat=sports)

>>>>> >

>>>>>

>>>>> <comment.zip>

>>>>> --

>>>>> Michael E. Mann

>>>>> Professor

>>>>> Director, Earth System Science Center (ESSC)

>>>>>

>>>>> Department of Meteorology Phone: (814) 863-4075

>>>>> 503 Walker Building FAX: (814)

>>>>> 865-3663

>>>>> The Pennsylvania State University email: mann@psu.edu

>>>>> <<mailto:mann@psu.edu>

>>>>> >

>>>>> University Park, PA 16802-5013

>>>>>

>>>>> website: <http://www.meteo.psu.edu/~mann/Mann/index.html>

>>>>> <<http://www.meteo.psu.edu/%7Emann/Mann/index.html>

>>>>> >

>>>>> "Dire Predictions" book site:

>>>>> http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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From: Grant Foster <tamino_9@hotmail.com>
To: Mike Mann <mann@meteo.psu.edu>, <p.jones@uea.ac.uk>
Subject: RE: ENSO blamed over warming - paper in JGR
Date: Thu, 30 Jul 2009 14:53:11 +0000
Cc: James Annan <jdannan@jamstec.go.jp>, <trenbert@ucar.edu>, "J. Salinger" <j.salinger@auckland.ac.nz>, <j.renwick@niwa.co.nz>, Gavin Schmidt <gschmidt@giss.nasa.gov>, <b.mullan@niwa.co.nz>

Gentlemen,
I've combined everything (I hope!) into the latest revision. I've probably made some glaring mistake somewhere, so read it critically. It's also necessary to ensure that it all fits together coherently, and that anything we claim we'll do is actually done. I want this to be airtight, let's not leave them any "wiggle room."
Referring to the inappropriate application of filters, I have a feeling that saying "perhaps not an uncommon error" is too easy on them. I have no motivation to go easy on them. Perhaps I'm being too aggressive; I defer to the majority opinion.
On a few technical details, I need altaffils and authoraddresses for everybody. And make sure I've got your name right!
Sincerely,
Grant

Bing brings you maps, menus, and reviews organized in one place.
[1]Try it now. Attachment
Converted: "c:\eudora\attach\comment.zip"

References

1.
<http://www.bing.com/search?q=restaurants&form=MLOGEN&publ=WLHMTAG&crea=TX>
T_MLOGEN_Local_Local_Restaurants_1x1

From: Ben Santer <santer1@llnl.gov>
To: "Thomas R. Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: [Fwd: Re: [Fwd: concerns about the Southeast chapter]]
Date: Thu, 30 Jul 2009 18:41:44 -0700
Reply-to: santer1@llnl.gov
Cc: Virginia Burkett <virginia_burkett@usgs.gov>, Thomas C Peterson <Thomas.C.Peterson@noaa.gov>, Michael Wehner <mfwehner@lbl.gov>, Karl Taylor <taylor13@llnl.gov>, peter gleckler <gleckler1@llnl.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, carl mears <mears@remss.com>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>

<x-flowed>
Dear Tom,

Thanks for forwarding the message from John Christy. Excuse me for being so blunt, but John's message is just a load of utter garbage.

I got a laugh out of John's claim that Santer et al. (2008) was "poorly done". This was kind of ironic coming from a co-author of the Douglass et al. (2007) paper, which used a fundamentally flawed statistical test to compare modeled and observed tropospheric temperature trends. To my knowledge, John has NEVER acknowledged that Douglass et al. used a flawed statistical test to reach incorrect conclusions - despite unequivocal evidence from the "synthetic data" experiments in Santer et al. (2008) that the Douglass et al. "robust consistency" test was simply wrong. Unbelievably, Christy continues to assert that the results of Douglass et al. (2007) "still stand". I can only shake my head in amazement at such intellectual dishonesty. I guess the best form of defense is a "robust" attack.

So how does John support his contention that Santer et al. (2008) was "poorly done"? He begins by stating that:

"Santer et al. 2008 used ERSST data which I understand has now been changed in a way that discredits the conclusion there".

Maybe you or Tom Peterson or Dick Reynolds can enlighten me on this one. How exactly have NOAA ERSST surface data changed? Recall that Santer et al. (2008) actually used two different versions of the ERSST data (version 2 and version 3). We also used HadISST sea-surface temperature data, and combined SSTs and land 2m temperature data from HadCRUT3v. In other words, we used four different observational estimates of surface temperature changes. Our bottom-line conclusion (no significant discrepancy between modeled and observed lower-tropospheric lapse-rate trends) was not sensitive to our choice of observed surface temperature dataset.

John next asserts that:

"Haimberger's v1.2-1.4 (of the radiosonde data) are clearly spurious due

to the error in ECMWF as published many places".

I'll let Leo Haimberger respond to that one. And if v1.2 of Leo's data is "clearly spurious", why did John Christy agree to be a co-author on the Douglass et al. paper which uses upper-air data from v1.2?

Santer et al. (2008) comprehensively examined structural uncertainties in the observed upper-air datasets. They looked at two different satellite and seven different radiosonde-based estimates of tropospheric temperature change. As in the case of the surface temperature data, getting the statistical test right was much more important (in terms of the bottom-line conclusions) than the choice of observational upper-air dataset.

Christy's next criticism of our IJoC paper is even more absurd. He states that:

"Santer et al. 2008 asked a very different question...than we did. Our question was "Does the IPCC BEST ESTIMATE agree with the Best Data (including RSS)?" Answer - No. Santer et al. asked, "Does ANY IPCC model agree with ANY data set?" ... I think you can see the difference.

Actually, we asked and answered BOTH of these questions. "Tests with individual model realizations" are described in Section 4.1 of Santer et al. (2008), while Section 4.2 covers "Tests with multi-model ensemble-mean trend". As should be obvious - even to John Christy - we did NOT just compare observations with results from individual models.

For both types of test ("individual model" and "multi-model average"), we found that, if one applied appropriate statistical tests (which Douglass et al. failed to do), there was no longer a serious discrepancy between modeled and observed trends in tropical lapse rates or in tropical tropospheric temperatures.

Again, I find myself shaking my head in amazement. How can John make such patently false claims about our paper? The kindest interpretation is that he is a complete idiot, and has not even bothered to read Santer et al. (2008) before making erroneous criticisms of it. The less kind interpretation is that he is deliberately lying.

A good scientist is willing to acknowledge the errors he or she commits (such as applying an inappropriate statistical test). John Christy is not a good scientist. I'm not a religious man, but I'm sure willing to thank some higher authority that Dr. John Christy is not the "gatekeeper" of what constitutes sound science.

I hope you don't mind, Tom, but I'm copying this email to some of the other co-authors of the Santer et al. (2008) IJoC paper. They deserve to know about the kind of disinformation Christy is spreading.

With best regards,

Ben

Thomas R. Karl wrote:

> FYI

>

> ----- Original Message -----

> Subject: Re: [Fwd: concerns about the Southeast chapter]

> Date: Mon, 27 Jul 2009 09:54:22 -0500

> From: John Christy <john.christy@nsstc.uah.edu>

> To: Thomas C Peterson <Thomas.C.Peterson@noaa.gov>

> CC: Thomas R Karl <Thomas.R.Karl@noaa.gov>

> References: <4A534CF9.9080700@noaa.gov>

>

>

>

> Tom:

>

> I've been on a heavy travel schedule and just now getting to emails
I've

> delayed. I was in Asheville briefly Thursday for a taping for the CDMP
> project at the Biltmore estates (don't know why that was the backdrop)
> while traveling between meetings in Chapel Hill, Atlanta and here.

>

> We disagree on the use of available climate information regarding the
> many things related to climate/climate change as I see by your
responses

> below - that is not unexpected as climate is an ugly, ambiguous, and
> complex system studied by a bunch of prima donnas (me included) and
> which defies authoritative declarations. I base my views on hard-core,
> published literature (some of it mine, but most of it not), so saying
> otherwise is not helpful or true. The simple fact is that the opinions
> expressed in the CCSP report do not represent the real range of
> scientific literature (the IPCC fell into the same trap - so running to
> the IPCC's corner doesn't move things forward).

>

> I think I can boil my objections to the CCSP Impacts report to this one
> idea for the SE (and US): The changes in weather variables (measured in
> a systematic settings) of the past 30 years are within the range of
> natural variability. That's the statement that should have been front
> and center of this whole document because it is
> mathematically/scientifically defensible. And, it carries more weight
> with planners so you can say to them, "If it happened before, it will
> happen again - so get ready now." By the way, my State Climatologist
> response to the CCSP was well-received by legislators and stakeholders
> (including many in the federal government) and still gets hits at
> http://*vortex.nsstc.uah.edu/aosc/.

>

> There also was a page or so on the tropical troposphere-surface issue
> that I didn't talk about on my response. It was wrong because it did
> not include all the latest research (i.e. since 2006) on the continuing
> and significant difference between the two trends. Someone was acting
> as a fierce gatekeeper on that one - citing only things that agreed
with

> the opinion shown even if poorly done (e.g. Santer et al. 2008 used
> ERSST data which I understand has now been changed in a way that
> discredits the conclusion there, and Haimberger's v1.2-1.4 are clearly

> spurious due to the error in ECMWF as published many places, but
> analyzed in detail in Sakamoto and Christy 2009). The results of
> Douglass et al. 2007 (not cited by CCSP) still stand since Santer et
al.
> 2008 asked a very different question (and used bad data to boot) than
we
> did. Our question was "Does the IPCC BEST ESTIMATE agree with the Best
> Data (including RSS)?" Answer - No. Santer et al. asked, "Does ANY
IPCC
> model agree with ANY data set?" ... I think you can see the difference.
> The fact my 2007 tropical paper (the follow-on papers in 2009 were
> probably too late, but they substantiate the 2007 paper) was not cited
> indicates how biased this section was. Christy et al. 2007 assessed
the
> accuracy of the datasets (Santer et al. did not - they assumed all
> datasets were equal without looking at the published problems) and we
> came up with a result that defied the "consensus" of the CCSP report -
> so, it was doomed to not be mentioned since it would disrupt the
> storyline. (And, as soon as RSS fixes their spurious jump in 1992, our
> MSU datasets will be almost indistinguishable.)
>
> This gets to the issue that the "consensus" reports now are just the
> consensus of those who agree with the consensus. The
> government-selected authors have become gatekeepers rather than honest
> brokers of information. That is a real tragedy, because when someone
> becomes a gatekeeper, they don't know they've become a gatekeeper - and
> begin to (sincerely) think the non-consensus scientists are just nuts
> (... it's more comfortable that way rather than giving them credit for
> being skeptical in the face of a paradigm).
>
> Take care.
>
> John C.
>
> p.s. a few quick notes are interspersed below.
>
>
> Thomas C Peterson wrote:
>> Hi, John,
>> I didn't want this to catch you by surprise.
>> Tom
>>
>> ----- Original Message -----
>> Subject: concerns about the Southeast chapter
>> Date: Tue, 07 Jul 2009 09:25:45 -0400
>> From: Thomas C Peterson <thomas.c.peterson@noaa.gov>
>> To: jim.obrien@coaps.fsu.edu
>> CC: Tom Karl <Thomas.R.Karl@noaa.gov>
>>
>>
>>
>> Dear Jim,
>>
>>

>> First off and most importantly, congratulations on your recent
>> marriage. Anthony said it was the most touching wedding he has ever
>> been to. I wish you and your bride all the best.
>>
>> Thank you for your comments and for passing on John Christy's detailed
>> concerns about the Southeast chapter of our report, /Global Climate
>> Change Impacts in the United States/. Please let me respond to the key
>> points he raised.
>>
>> In Dr. John Christy's June 23, 2009 document "Alabama climatologist
>> responds to U.S. government report on regional impacts of global
>> climate change", he primarily focused on 4 prime concerns:
>>
>> 1. Assessing changes since 1970.
>>
>> 2. Statements on hurricanes.
>>
>> 3. Electrical grid disturbances (from the Energy section).
>>
>> 4. Using models to assess the future.
>>
>>
>>
>> /1. Assessing changes since 1970./
>>
>> The Southeast section has 5 figures and one table. One figure is on
>> changes in precipitation patterns from 1901-2007. The next figure is
>> on patterns of days per year over 90F with two maps, one 1961-1979,
>> the other 2080-2099. One figure is on the change in freezing days per
>> year, 1976-2007. The next figure is on changes to a barrier island
>> land from 2002 to 2005. And the last figure was on Sea Surface
>> Temperature from 1900 to the present. The table indicates trends in
>> temperature and precipitation over two periods, 1901-2008 and
>> 1970-2008. As Dr. Christy indicates in his paper, the full period and
>> the period since 1970 are behaving differently. To help explain this,
>> the table shows them both. Of the 5 figures, only one shows the
>> changes over this shorter period.
>>
>> Since, as the IPCC has indicated, the human impact on climate isn't
>> distinguishable from natural variability until about 1950, describing
>> the changes experienced in the majority of the time since 1950 would
>> be a more logical link to future anthropogenic climate change. In
>> most of the report, maps have shown the changes over the last 50
>> years. Because of the distinct behavior of time series of
>> precipitation and temperature in the Southeast, discussing the period
>> since 1970 seemed more appropriate. Though as the figures and table
>> indicate, this shorter period is not the sole or even major focus.
>
> See crux of the matter in email above - looking at the whole time
series
> is demanded by science. Any 30 or 50-year period will give changes -
> blaming the most recent on humans ignores the similar (or even more
> rapid) changes that occurred before industrialization (e.g. western
> drought in 12th century). The period since 1970 WAS the major focus in

> the SE section (mentioned 6 times in two pages). And, OF COURSE any
> 30-year sub-period will have different characteristics than the 100-
year
> population from which it is extracted ... that doesn't prove anything.

>>

>>

>>

>> /2. Statements on hurricanes./

>>

>> Dr. Christy takes issue with the report's statements about hurricanes
>> and quotes a line from the report and quotes an individual hurricane
>> expert who says that he disagrees with the conclusions. The line in
>> the report that Dr. Christy quotes comes almost word for word out of
>> CCSP SAP 3.3. While individual scientists may disagree with the
>> report's conclusions, this conclusion came directly out of the
>> peer-reviewed literature and assessments. Dr. Christy also complains
>> that "the report did not include a plot of the actual hurricane
>> landfalls". However, the section in the Southeast chapter discussing
>> landfalling hurricanes states "see /National Climate Change/ section
>> for a discussion of past trends and future projections" and sure
>> enough on page 35 there is a figure showing land falling hurricanes
>> along with a more in depth discussion of hurricanes.

>>

> You didn't read my State Climatologist response carefully - I mentioned
> page 35 and noted again it talked about the most recent decades (and
> even then, the graph still didn't go back to 1850). This hurricane
> storyline was hit hard by many scientists - hence is further evidence
> the report was generated by a gatekeeper mentality.

>>

>>

>> /3. Electrical grid disturbances (from the Energy section)./

>>

>> Moving out of the Southeast, Dr. Christy complains about one figure in
>> the Energy Chapter. Citing a climate skeptic's blog which cites an
>> individual described as the keeper of the data for the Energy
>> Information Administration (EIA), John writes that the rise in weather
>> related outages is largely a function of better reporting. Yet the
>> insert of weather versus non-weather-related outages shows a much
>> greater increase in weather-related outages than non-weather-related
>> outages. If all the increases were solely due to better reporting,
>> the differences between weather- and non-weather-related outages would
>> indicate a dramatic decrease over this time period in non-weather
>> related problems such as transmission equipment failures, earthquakes,
>> faults in line, faults at substations, relaying malfunctions, and
>> vandalism.

>>

>> Thanks to the efforts of EIA, after they took over the responsibility
>> of running the Department of Energy (DOE) data-collection process
>> around 1997, data collection became more effective. Efforts were made
>> in subsequent years to increase the response rate and upgrade the
>> reporting form. It was not until EIA's improvement of the data
>> collection that the important decoupling of weather- and
>> non-weather-related events (and a corresponding increase in the
>> proportion of all events due to weather extremes) became visible.

>>
>> To adjust for potential response-rate biases, we have separated
>> weather- and non-weather-related trends into indices and found an
>> upward trend only in the weather-related time series.
>>
>> As confirmed by EIA, *if there were a systematic bias one would expect
>> it to be reflected in both data series (especially since any given
>> reporting site would report both types of events).*>>
>> As an additional precaution, we focused on trends in the number of
>> events (rather than customers affected) to avoid fortuitous
>> differences caused by the population density where events occur. This,
>> however, has the effect of understating the weather impacts because of
>> EIA definitions (see survey methodology notes below).
>>
>> More details are available at:
>> http://*eetd.lbl.gov/emills/pubs/grid-disruptions.html
>
> The data were not systematically taken and should not have been shown
> .. basic rule of climate.
>>
>>
>>
>> /4. Using models to assess the future./
>>
>> Can anyone say anything about the future of the Southeast's climate?
>> Evidently according to John Christy, the answer is no. The basic
>> physics of the greenhouse effect and why increasing greenhouse gases
>> are warming and should be expected to continue to warm the planet are
>> well known and explained in the /Global Climate Change/ section of the
>> report. Climate models are used around the world to both diagnose the
>> observed changes in climate and to provide projections for the
>> future. There is a huge body of peer-reviewed literature, including a
>> large number of peer-reviewed climate change assessments, supporting
>> this use. But in Dr. Christy's "view," models should not be used for
>> projections of the future, especially for the Southeast. The report
>> based, and indeed must base, its results on the huge body of
>> peer-reviewed scientific literature rather than the view of one
>> individual scientist.
>
> No one has proven models are capable of long-range forecasting.
> Modelers write and review their own literature - there are millions of
> dollars going into these enterprises, so what would you expect?
> Publication volume shouldn't impress anyone. The simple fact is we
> demonstrated in a straightforward and reproducible way that the actual
> trends over the past 30, 20, and 10 years are outside of the envelop of
> model predictions ... no one has disputed that finding with an
> alternative analysis - even when presented before congressional
hearings
> where the opportunity for disagreement was openly available.
>>
>> I hope this helps relieve some of your concerns.
>>
>> Regards,

>>
>> Tom Peterson
>>
>>
>>
>
>
> --
> *****
> John R. Christy
> Director, Earth System Science Center voice: 256-961-7763
> Professor, Atmospheric Science fax: 256-961-7751
> Alabama State Climatologist
> University of Alabama in Huntsville
> http://*www.*nsstc.uah.edu/atmos/christy.html
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> Express: Cramer Hall/ESSC, 320 Sparkman Dr., Huntsville AL 35805
>
>
>
> --
>
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>
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>
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>
>
>

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Benjamin D. Santer
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P.O. Box 808, Mail Stop L-103
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Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: "Kevin Trenberth" <trenbert@ucar.edu>
To: "Grant Foster" <tamino_9@hotmail.com>
Subject: RE: ENSO blamed over warming - paper in JGR
Date: Thu, 30 Jul 2009 20:01:06 -0600 (MDT)
Reply-to: trenbert@ucar.edu
Cc: "J. Salinger" <j.salinger@auckland.ac.nz>, "Mike Mann" <mann@meteo.psu.edu>, p.jones@uea.ac.uk, "James Annan" <jdannan@jamstec.go.jp>, j.renwick@niwa.co.nz, "Gavin Schmidt" <gschmidt@giss.nasa.gov>, b.mullan@niwa.co.nz

You have a go from me. By all means clean up. I think you should argue that it should be expedited for the reasons of interest by the press. Key question is who was the editor who handled the original, because this is an implicit criticism of that person. May need to point this out and ensure that someone else handles it.

Thanks
Kevin

>
> Gentlemen,
>
> I've added additional suggestions received today, and made a few minor
> changes myself. Here's the latest version. Enjoy!

>
> Sincerely,
> Grant

>
>
>
> _____
> Windows Live™ SkyDrive™: Store, access, and share your photos. See how.
> http://windowslive.com/Online/SkyDrive?ocid=TXT_TAGLM_WL_CS_SD_photos_072009

Kevin Trenberth
Climate Analysis Section, NCAR
PO Box 3000
Boulder CO 80307
ph 303 497 1318
<http://www.cgd.ucar.edu/cas/trenbert.html>

From: Michael Mann <mann@meteo.psu.edu>

To: trenbert@ucar.edu

Subject: Re: ENSO blamed over warming - paper in JGR

Date: Thu, 30 Jul 2009 22:26:32 -0400

Cc: "Grant Foster" <tamino_9@hotmail.com>, "J. Salinger" <j.salinger@auckland.ac.nz>, p.jones@uea.ac.uk, "James Annan" <jdannan@jamstec.go.jp>, j.renwick@niwa.co.nz, "Gavin Schmidt" <gschmidt@giss.nasa.gov>, b.mullan@niwa.co.nz

folks, I was thinking exactly the same thing. the problems are so unusually fundamental and obvious, as we lay them out, that it does immediately call into suspicion the integrity of the review process.

We probably need to take this directly to the chief editor at JGR, asking that this not be handled by the editor who presided over the original paper, as this would represent a conflict of interest. if we are told that is not possible, then we would at least want the chief editor himself to closely monitor the handling of the paper.

I too am happy to sign of at this point,

mike

On Jul 30, 2009, at 10:01 PM, Kevin Trenberth wrote:

You have a go from me. By all means clean up. I think you should argue that it should be expedited for the reasons of interest by the press. Key question is who was the editor who handled the original, because this is an implicit criticism of that person. May need to point this out and ensure that someone else handles it.

Thanks

Kevin

Gentlemen,

I've added additional suggestions received today, and made a few minor

changes myself. Here's the latest version. Enjoy!

Sincerely,

Grant

Windows Live SkyDrive: Store, access, and share your photos. See how.

[1]http://windowslive.com/Online/SkyDrive?ocid=TXT_TAGLM_WL_CS_SD_photos_072009

Kevin Trenberth
Climate Analysis Section, NCAR
PO Box 3000
Boulder CO 80307
ph 303 497 1318
[2]<http://www.cgd.ucar.edu/cas/trenbert.html>

--

Michael E. Mann

Professor

Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075

503 Walker Building FAX: (814) 865-3663

The Pennsylvania State University email: [3]mann@psu.edu

University Park, PA 16802-5013

website: [4]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[5]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. http://windowslive.com/Online/SkyDrive?ocid=TXT_TAGLM_WL_CS_SD_photos_072009
2. <http://www.cgd.ucar.edu/cas/trenbert.html>
3. <mailto:mann@psu.edu>
4. <http://www.meteo.psu.edu/~mann/Mann/index.html>
5. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

6. <http://www.met.psu.edu/dept/faculty/mann.htm>

My colleague Emma and I are submitting everything this morning. I'm doing the email version, Emma the 4 hard copies to the office in York before 2pm.

Peter provided a very useful edit yesterday which has got the proposal down under 4,000 words.

Please find attached:

1. Proposal registration form (I have just put in CAG details as main proposer but flagged up its a partnership bid)
2. Summary (just under 600 words as required)
3. Proposal
4. Budget form (their's and an extra one they agreed I could do to show who does what days - don't worry about days shown - its provisional - we can revise and rearrange it if we get the job!)
5. Staff Costs forms (attached to the budget form but not filled in as they agreed we didn't have to submit these - they don't work with day rates)
6. Full CVs for all Proposers (Emma is adding in some final material she has but couldnt access yesterday - we will send round the very final version for your records once done this morning)
7. Three appendices as one Word document (to go with the proposal but separately so as not to increase the word count of the proposal)
8. A rather long covering letter to go with email and hard copy versions.

If you notice I've missed something please email me!
Thanks to everyone for the their work on this. Very much appreciated.
I will let you know as soon as I hear anything.

best wishes

Susan

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x-mac-creator=4D535744; name=Application summary CAG and partners.doc
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mac-creator=4D535744;
name=CAG and Partners Application Final.doc Content-Disposition:
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and Partners Application Final.doc" Attachment Converted:
"c:\eudora\attach\CAG and
Partners Application Final.doc" Content-Type: application/octet-
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name=CAG and Partners Budget
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mac-creator=4D535744;
name=CAG and Partners Additional Budget Form and Explanatory Notes.doc
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Explanatory Notes.do";
filename*1=c Attachment Converted: "c:\eudora\attach\CAG.doc" Content-
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mac-creator=4D535744;
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filename="CAG and Partners
CVs.doc" Attachment Converted: "c:\eudora\attach\CAG and Partners
CVs.doc" Content-Type:
application/octet-stream; x-mac-type=5738424E; x-unix-mode=0644; x-
mac-creator=4D535744;
name=CAG and Partners Application Appendices.doc Content-Disposition:
attachment;
filename="CAG and Partners Application Appendices.doc" Attachment
Converted:
"c:\eudora\attach\CAG and Partners Application Appendices.doc"
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application/octet-stream; x-mac-type=5738424E; x-unix-mode=0644; x-
mac-creator=4D535744;
name=CAG and Partners covering letter final.doc Content-Disposition:
attachment;
filename="CAG and Partners covering letter final.doc" Attachment
Converted:
"c:\eudora\attach\CAG and Partners covering letter final.doc"

Dr Susan Parham
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London, N5 2UH

HQ: Gordon House, 6 Lissenden Gardens, London, NW5 1LX

References

1. <mailto:sp@cagconsult.co.uk>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Thorne, Peter (Climate Research)" <peter.thorne@metoffice.gov.uk>
Subject: See below
Date: Fri Jul 31 08:59:22 2009

Peter,

Don't know if you got this. There is a link below to something Tom P said.

Keith is fine - seems as though there nothing malignant or cancerous in the post op tests. Just needs to ensure the scar heals OK, then he can come back to the madhouse.

Cheers

Phil

X-Failed-Recipients: peter.thorne@metoffice.gov.uk
Auto-Submitted: auto-replied
From: Mail Delivery System <Mailer-Daemon@uea.ac.uk>
To: p.jones@uea.ac.uk
Subject: Mail delivery failed: returning message to sender
Date: Fri, 31 Jul 2009 08:31:08 +0100

This message was created automatically by mail delivery software.
A message that you sent could not be delivered to one or more of its recipients. This is a permanent error. The following address(es) failed:

peter.thorne@metoffice.gov.uk

SMTP error from remote mail server after end of data:

host ueamailgate01.uea.ac.uk [139.222.131.184]:

554 5.7.1 Message rejected because of unacceptable content. For help, please quote incident ID 3442835.

----- This is a copy of the message, including all the headers. -----

Return-path: <p.jones@uea.ac.uk>

Received: from [139.222.104.75] (helo=crupdj2.uea.ac.uk)
by ueams02.uea.ac.uk with esmtps (TLSv1:AES256-SHA:256)
(Exim 4.69)

(envelope-from <p.jones@uea.ac.uk>)

id 1MWma3-0007wd-KH

for peter.thorne@metoffice.gov.uk; Fri, 31 Jul 2009 08:31:07 +0100

X-Mailer: QUALCOMM Windows Eudora Version 7.1.0.9

Date: Fri, 31 Jul 2009 08:31:19 +0100

To: "Thorne, Peter (Climate Research)" <peter.thorne@metoffice.gov.uk>

From: Phil Jones <p.jones@uea.ac.uk>

Subject: Fwd: did you get a chance to see

Mime-Version: 1.0

Content-Type: multipart/alternative;

boundary="===== _1878687==.ALT"

--===== _1878687==.ALT

Content-Type: text/plain; charset="us-ascii"; format=flowed

>Date: Wed, 29 Jul 2009 13:50:57 -0400

>From: Thomas.C.Peterson@noaa.gov

>Subject: did you get a chance to see

>To: Thomas.C.Peterson@noaa.gov

>Cc: Phil Jones <p.jones@uea.ac.uk>

>X-Mailer: iPlanet Messenger Express 5.2 HotFix 2.01 (built Aug 26 2004)

>X-Accept-Language: en

>Priority: normal

>X-Canit-CHI2: 0.00

>X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)

>X-Spam-Score: 1.00 (*) [Hold at 5.00]

>APOSTROPHE_OBFUSCATION,HTML_MESSAGE,SPF(none,0)

>X-CanItPRO-Stream: UEA:f028 (inherits from UEA:default,base:default)
>X-Canit-Stats-ID: 26983044 - 2dc0798c114f
>X-Antispam-Training-Forget:
>[1]<https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=f>
>X-Antispam-Training-Nonspam:
>[2]<https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=n>
>X-Antispam-Training-Spam:
>[3]<https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=s>
>X-Scanned-By: CanIt (www . roaringpenguin . com) on 139.222.131.185
>
>[4]<http://climateprogress.org/2009/07/29/the-video-that-anthony-watts-does-not-want-you-to-see-the-sinclair-climate-denial-crock-of-the-week/>

>----- Original Message -----

>From: <Thomas.C.Peterson@noaa.gov>
>Date: Wednesday, July 29, 2009 12:07 pm
>Subject: Re: This and that

>> Hi, Phil,

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>> Yes, Friday-Saturday I noticed that ClimateFraudit had renewed their
>> interest in you. I was thinking about sending an email of
>> sympathy, but
>> I was busy preparing for a quick trip to Hawaii - I left Monday
>> morning and flew out Tuesday evening and am now in the Houston
>> airport on my way
>> home.

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>> Data that we can't release is a tricky thing here at NCDC.
>> Periodically, Tom Karl will twist my arm to release data that would
>> violate agreements
>> and therefore hurt us in the long run, so I would prefer that you
>> don't specifically cite me or NCDC in this.

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>> But I can give you a good alternative. You can point to the
>> Peterson-Manton article on regional climate change workshops. All
>> those workshops resulted in data being provided to the author of the
>> peer-reviewed paper with a strict promise that none of the data
>> would be
>> released. So far as far as I know, we have all lived up to that
>> agreement - myself with the Caribbean data (so that is one example of
>> data I have that are not released by NCDC), Lucie and Malcolm for
>> South America, Enric for Central America, Xuebin for Middle Eastern
>> data, Albert for south/central Asian data, John Ceasar for SE Asia,
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>> countries don't
>> mind the release of derived products such as your gridded field or
>> Xuebin's ETCCDI indices, but very much object to the release of actual
>> data (which they might sell to potential users). Does that help?

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>> Regarding AR4, I would like to be part of it. I have no idea what
>> role would be deemed appropriate. One thing I noticed with the CLAs
>> in my
>> old chapter is that if one isn't up to doing his part (too busy, or a
>> different concept of timeliness, or ...) it can make for a difficult
>> job. You and I have worked well together before (e.g., GSN) so I'd be
>> delighted to work with you on it and I know you'd hold up your side of

>> the tasks. We touched on this briefly at the AOPC meeting. If I
>> get an
>> opportunity, I would say yes.
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>> But I also don't know what the U.S. IPCC nominating approach would
>> be or
>> even who decides that. There is an upcoming IPCC report on
>> extremes and
>> impacts of extremes and I wasn't privy to any insights into the U.S.
>> nominations other than when it was over it was announced in NCDC staff
>> notes that the nominations had been made. However, Kumar had earlier
>> asked if he could nominate me, so he did (I provided him with the
>> details).
>> Regards,
>> Tom
>>
>>
>> Tom,
> If you look on Climate Audit you will see that I'm all over it!
> Our ftp site is regularly trawled as I guess yours is. It seems that
> a Canadian along with two Americans copied some files we put there
> for MOHC in early 2003. So saying they have the CRU data is not
> quite correct. What they have is our raw data for CRUTEM2 which
> went into Jones and Moberg (2003) - data through end of 2002.
> Anyway enough of my problems - I have a question for you. I'm
> going to write a small document for our web site to satisfy (probably the
> wrong word) the 50 or so FOI/EIR requests we've had over the weekend.
> I will put up the various agreements we have with Met Services.
> The question - I think you told me one time that you had a file
> containing all the data you couldn't release (i.e. it's not in
> GHCN). Presumably
> this is not in your gridded datasets? Do you know off hand how much
> data is in this category? Would NCDC mind if I mentioned that you
> have such data - not the amount/locations/anything, just that there is some?
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> On something positive - attached is the outlines for the
> proposed Chs in AR5/WG1.
> Ch1 is something Thomas thinks he can write himself - well with Qin Dahe, so
> only 13 chapters. There are a lot of issues with overlaps between
> some of the
> data chapters 2 with 3, 2 with 5 and 2 with 14.
> I'm still thinking about whether to get involved. It would be 2
> if I decide. At the
> moment I'd say yes, but I might change my mind tomorrow! Nominations are
> from Nov09 thru Jan10 with the selection made in April 10. Are you
> considering
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> I have got the IPCC Secretariat and Thomas to raise the FOI issues with
> the full IPCC Plenary, which meets in Bali in September or October. Thomas
> is fully aware of all the issues we've had here wrt Ch 6 last
> time, and others in
> the US have had.
>
> Cheers
> Phil
>
>
>
>Prof. Phil Jones

>Climatic Research Unit Telephone +44 (0) 1603 592090
>School of Environmental Sciences Fax +44 (0) 1603 507784
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>Norwich Email p.jones@uea.ac.uk
>NR4 7TJ
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>-----
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--===== _1878687==.ALT

Content-Type: text/html; charset="iso-8859-1"

Content-Transfer-Encoding: quoted-printable

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From: Thomas.C.Peterson@noaa.gov
Subject: did you get a chance to see
To: Thomas.C.Peterson@noaa.gov
Cc: Phil Jones <p.jones@uea.ac.uk>
X-Mailer: iPlanet Messenger Express 5.2 HotFix 2.01 (built Aug 26 2004)
X-Accept-Language: en
Priority: normal
X-Canit-CHI2: 0.00
X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)
X-Spam-Score: 1.00 (*) [Hold at 5.00]
APOSTROPHE_OBFUSCATION,HTML_MESSAGE,SPF(none,0)
X-CanItPRO-Stream: UEA:f028 (inherits from UEA:default,base:default)
X-Canit-Stats-ID: 26983044 - 2dc0798c114f
X-Antispam-Training-Forget:
[5]<https://canit.uea.ac.uk/b.php?i=3D26983044&m=3D2dc0798c114f&c=3Df>
X-Antispam-Training-Nospam:
[6]<https://canit.uea.ac.uk/b.php?i=3D26983044&m=3D2dc0798c114f&c=3Dn>
X-Antispam-Training-Spam:
[7]<https://canit.uea.ac.uk/b.php?i=3D26983044&m=3D2dc0798c114f&c=3Ds>
X-Scanned-By: CanIt (www.roaringpenguin.com) on 139.222.131.185
[8]<http://climateprogress.org/2009/07/29/the-video-that-anthony-watts-does-not-want-you-to-see-the-sinclair-climate-denial-crock-of-the-week/>

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> Tom
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Cheers

Phil

Prof. Phil Jones

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Norwich &nb=
sp; &=
nbsp; Email p.jones@uea.ac.uk
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References

1. <https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=f>
2. <https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=n>
3. <https://canit.uea.ac.uk/b.php?i=26983044&m=2dc0798c114f&c=s>
4. <http://climateprogress.org/2009/07/29/the-video-that-anthony-watts-does-not-want-you-to-see-the-sinclair-climate-denial-crock-of-the-week/>
5. <file://localhost/tmp/3D.htm>
6. <file://localhost/tmp/3D.htm>
7. <file://localhost/tmp/3D.htm>
8. <file://localhost/tmp/3D.htm>

From: Grant Foster <tamino_9@hotmail.com>

To: <p.jones@uea.ac.uk>, "J. Salinger" <j.salinger@auckland.ac.nz>

Subject: RE: ENSO blamed over warming - paper in JGR

Date: Fri, 31 Jul 2009 10:54:57 +0000

Cc: <j.renwick@niwa.co.nz>, Mike Mann <mann@meteo.psu.edu>, <trenbert@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Annan <jdannan@jamstec.go.jp>, <b.mullan@niwa.co.nz>

Gentlemen,

We're very close to being ready for submission; here's the latest version. I suggest a close reading, and don't forget to point out all the typos you notice.

James, since you can cover the page charges I suggest you handle the actual submission (when the time comes). Would you be willing to write the cover letter? Any other volunteers?

So far I've produced versions in 2-column format with graphs inline (so we can all see what it'll look like), but when we're ready I'll create a draft version with all the figures at the end (or if you really want to James, you can do this as well). The 2-column version takes jpg files as input, but I've already created eps files for all the figures.

I *think* I've got everybody's suggestions in here, but if I've missed anything or you have further suggestions send 'em along. We're still waiting for explicit consent (and affiliation info) from B. Mullan and G. Schmidt! If either of you fellas would rather opt out that's OK -- as far as I'm concerned you're completely welcome to join or to decline. If we're as close as I think, we may be ready by Monday.

Thanks, Phil, for the link to the video; a good laugh! Maybe the most amusing blog post I've seen about MFC09 is this one:

<http://deepclimate.org/2009/07/30/is-enso-responsible-for-recent-global-warming-no/>

What amuses me most is that "in its original news item on the paper, the International Climate Science Coalition had actually substituted the title of the first press release for for the actual title in its link to the paper ... That's right according to the ICSC, the papers title was Nature, not Man, is responsible for global warming. Stop the presses!

<http://deepclimate.files.wordpress.com/2009/07/icsc-july-26-short-2.jpg>

Sincerely,

Grant

Bing brings you maps, menus, and reviews organized in one place. [1]Try it now. Attachment

Converted: "c:\eudora\attach\comment2.zip"

References

1. http://www.bing.com/search?q=restaurants&form=MLOGEN&publ=WLHMTAG&crea=TXT_MLOGEN_Local_Local_Restaurants_1x1

From: Jim Salinger <j.salinger@auckland.ac.nz>
To: Grant Foster <tamino_9@hotmail.com>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Fri, 31 Jul 2009 11:07:28 +1200
Cc: trenbert@ucar.edu, Mike Mann <mann@meteo.psu.edu>, p.jones@uea.ac.uk, James Annan <jdannan@jamstec.go.jp>, j.renwick@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, b.mullan@niwa.co.nz

<x-flowed>
Grant et al

All good to me apart from adding in the IPCC 2007 WG1 Chap 3 reference.

I checked with IJC chief editor here (Glenn McGregor) and editors usually like to publish comments asap, and send them only to the original authors to respond to as soon as possible.

So once the USA contingent has signed it off 'today' (Friday) and submitted it, I will send a copy to our Australian colleagues for information.

All good stuff

Best

Auckland Jim

Grant Foster wrote:

> Gentlemen,
>
> I've added additional suggestions received today, and made a few minor
> changes myself. Here's the latest version. Enjoy!
>
> Sincerely,
> Grant
>
>
> -----
> Windows Live™ SkyDrive™: Store, access, and share your photos. See how.
> <http://windowslive.com/Online/SkyDrive?ocid=TXT_TAGLM_WL_CS_SD_photos_072009>

</x-flowed>

From: Kevin Trenberth <trenbert@ucar.edu>
To: Grant Foster <tamino_9@hotmail.com>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Mon, 03 Aug 2009 11:34:59 -0600

Cc: p.jones@uea.ac.uk, "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, j.renwick@niwa.co.nz, Mike Mann <mann@meteo.psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, b.mullan@niwa.co.nz

Hi Grant,

I have been tied up with other things. In looking at the paper some questions.

1) In Fig 1, why is the scale zero to 2? Normally a filter would be scaled to have a response function zero to 1.

2) In Fig 2 and 3 what are the units of "power"? It is not in the caption. Are these normalized spectra so that the area under the curve is unity? My guess is that this is the case and hence the amplification at ENSO bands. But it is important to say this and perhaps point out. Maybe the captions are sufficient? Add something like: The spectra have been normalized to have unit variance, which relatively inflates the values in the 0.2 to 0.5 frequency band. In a couple of places in text add "normalized" before "power spectrum" such as 2 lines above Fig 3 in the JGR set version.

3) A minor point: in the $x = \sin(2\pi vt)$ I would be inclined to add an amplitude which would then be included also in eq (1) on RHS emphasizing how the amplitude is changed. [My own preference would be to call the amplitude A and the A you have R (for response function)]. However it is fine as is.

Thanks

Kevin

Grant Foster wrote:

Gentlemen,

Well, I got some free time and it didn't take as long as I expected. Attached are:

comment.zip Comment in preprint form

draft.zip Comment in draft form (for submission)

freeform.zip Comment NOT as preprint or draft, with larger font and double-wide graphs

I suggest we don't circulate it until folks have had one further day to check. And double check and triple-check. If we don't hear an objection by tomorrow morning, I suggest we submit it to JGR and feel free to circulate it.

So -- this is your last chance to suggest changes before submission, or to suggest restraint in circulation.

Sincerely,

Grant

Windows Live(TM): Keep your life in sync. [1]Check it out.

--

Kevin E. Trenberth e-mail: [2]trenbert@ucar.edu
Climate Analysis Section, [3]www.cgd.ucar.edu/cas/trenbert.html
NCAR
P. O. Box 3000, (303) 497 1318
Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

References

1. http://windowslive.com/explore?ocid=PID23384::T:WLMTAGL:ON:WL:en-US:NF_BR_sync:082009
2. <mailto:trenbert@ucar.edu>
3. <http://www.cgd.ucar.edu/cas/trenbert.html>

From: Jim Salinger <j.salinger@auckland.ac.nz>
To: James Annan <jdannan@jamstec.go.jp>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Mon, 03 Aug 2009 15:08:02 +1200
Cc: Grant Foster <tamino_9@hotmail.com>, p.jones@uea.ac.uk, "J. Salinger" <j.salinger@auckland.ac.nz>, j.renwick@niwa.co.nz, Mike Mann <mann@meteo.psu.edu>, trenbert@ucar.edu, Gavin Schmidt <gschmidt@giss.nasa.gov>, b.mullan@niwa.co.nz

<x-flowed>

Dear James

From the Land of the Long White Cloud to the Land of the Rising Sun....

Should we not also inquire about their time line for publishing the comment, and on the basis that is so serious, and the implications of their flawed findings ask it to be expedited.

Perhaps

We also note that the paper is now being used as the basis of campaigns against climate change policy and, should you decide to go ahead and publish our comment, expedite its acceptance.

Best

Auckland James

James Annan wrote:

> Grant Foster wrote:

>> James, since you can cover the page charges I suggest you handle the

>> actual submission (when the time comes). Would you be willing to

>> write the cover letter? Any other volunteers?

>

> Sure, I propose something like the below. I don't think there is

> anything to be gained by being overly combative wrt JGR.

>

> I look forward to the next final version of the paper :-)

>

>

> Covering Letter:

>

>

> Dear Sir/Madam,

>

> Please consider the attached manuscript for publication in the Journal

> of Geophysical Research (Atmospheres). We consider that the errors in

> the analysis of McLean et al are so serious that the publication of a

> Comment to correct the public record is amply justified. In view of the

> high profile of the issue, we would prefer if one of the senior editors

> could take charge of the editorial process.

>

> Yours sincerely..

>

>

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>, Grant Foster <tamino_9@hotmail.com>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Wed Aug 5 16:14:34 2009
Cc: "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, Mike Mann <mann@meteo.psu.edu>, j.renwick@niwa.co.nz

Hi all,

Agree with Kevin that Tom Karl has too much to do. Tom Wigley is semi retired and like Mike Wallace may not be responsive to requests from JGR.

We have Ben Santer in common ! Dave Thompson is a good suggestion. I'd go for one of Tom Peterson or Dave Easterling.

To get a spread, I'd go with 3 US, One Australian and one in Europe. So Neville Nicholls and David Parker.

All of them know the sorts of things to say - about our comment and the awful original, without any prompting.

Cheers

Phil

At 15:50 05/08/2009, Kevin Trenberth wrote:

Hi all

I went to JGR site to look for index codes, and I see that the offending article has been downloaded 128 times in past week (second). All the more reason to get on with it.

see below

Kevin

Grant Foster wrote:

Gentlemen,

I've completed most of the submission to JGR, but there are three required entries I hope you can help me with.

1) Keyword

Please provide 1 unique keyword

global temperatures, statistical methods, El Nino-Southern Oscillation, global warming

2) Index Terms

Please provide 3 unique index terms

1600 GLOBAL CHANGE

1616 Climate variability

3309 Climatology

1694 Instruments and techniques

3) Suggested Reviewers to Include

Please list the names of 5 experts who are knowledgeable in your area and could give an unbiased review of your work. Please do not list colleagues who are close associates, collaborators, or family members. (this requires name, email, and institution).

Tom Wigley [1]wigley@ucar.edu NCAR

Ben Santer [2]<santer1@llnl.gov> Lawrence Livermore

Mike Wallace [3]<wallace@atmos.washington.edu> U Washington [May not be most

responsive]

Dave Thompson [4]<davet@atmos.colostate.edu> Col State Univ

Dave Easterling [5]<David.Easterling@noaa.gov> NCDC

Sincerely,

Grant

Windows Live: Keep your life in sync. [6]Check it out.

--

Kevin E. Trenberth e-mail: [7]trenbert@ucar.edu
Climate Analysis Section, [8]www.cgd.ucar.edu/cas/trenbert.html
NCAR
P. O. Box 3000, (303) 497 1318
Boulder, CO 80307 (303) 497 1333 (fax)

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References

1. <mailto:wigley@ucar.edu>
2. <mailto:santer1@llnl.gov>
3. <mailto:wallace@atmos.washington.edu>
4. <mailto:davet@atmos.colostate.edu>
5. <mailto:David.Easterling@noaa.gov>
6. http://windowslive.com/explore?ocid=PID23384::T:WLMTAGL:ON:WL:en-US:NF_BR_sync:082009
7. <mailto:trenbert@ucar.edu>
8. <http://www.cgd.ucar.edu/cas/trenbert.html>

From: Ben Santer <santer1@llnl.gov>
To: "Thomas R. Karl" <Thomas.R.Karl@noaa.gov>
Subject: Re: [Fwd: Re: [Fwd: concerns about the Southeast chapter]]
Date: Fri, 07 Aug 2009 09:34:10 -0700
Reply-to: santer1@llnl.gov
Cc: Virginia Burkett <virginia_burkett@usgs.gov>, Thomas C Peterson <Thomas.C.Peterson@noaa.gov>, Michael Wehner <mfwehner@lbl.gov>, Karl Taylor <taylor13@llnl.gov>, peter gleckler <gleckler1@llnl.gov>, "Thorne, Peter" <peter.thorne@metoffice.gov.uk>, Leopold Haimberger <leopold.haimberger@univie.ac.at>, Tom Wigley <wigley@cgd.ucar.edu>, John Lanzante <John.Lanzante@noaa.gov>, Susan Solomon <ssolomon@frii.com>, "'Philip D. Jones'" <p.jones@uea.ac.uk>, carl mears <mears@remss.com>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Steven Sherwood <Steven.Sherwood@yale.edu>, Frank Wentz <frank.wentz@remss.com>

<x-flowed>
Dear Tom,

I'm inclined to agree with Mike. Some people are accessible to rational scientific debate. They are good Bayesians - when confronted with new scientific information, they are capable of modifying previously-held views. John Christy is not accessible to rational scientific debate. New evidence does not cause him to change his views. He simply claims that the new evidence is wrong. From John's perspective, any datasets in disagreement with UAH-based estimates of tropospheric temperature change constitute "bad data".

John is incapable of recognizing and admitting that Douglass et al. used a flawed statistical test to reach incorrect conclusions. He continues to misrepresent the analyses we performed in our response to Douglass et al. I don't see what useful purpose can be served by trying to engage him in reasonable scientific debate.

At the Hawaii IPCC meeting in March, John stood up in front of an audience of IPCC Working Group I Lead Authors and attempted to portray himself as a victim of scientific discrimination. He claimed that his "alternative" views on the nature and causes of climate change were being ignored by the mainstream scientific community. This claim is bogus. The "mainstream" scientific community has not ignored the "alternative" views of folks like John Christy. The sad reality is that we've wasted an inordinate amount of time responding to the flawed science and incorrect claims of John and his colleagues.

I'm hopeful that I won't have to waste much more time on the "great satellite debate". In my personal opinion, we're already well past the point of diminishing returns on this debate. The point of diminishing returns was reached three years ago, when you overcame great obstacles to lead a fractious bunch of scientists to the successful completion of the first CCSP Report.

With best regards,

Ben
Thomas R. Karl wrote:

> Ben,
>
> Just got to this. I wonder if it would be useful to directly respond
to
> John, or would this be a time sink? Maybe a cleaned up version of this
> is a single reponse? Just thinking out loud.
>
> Thanks Ben
>
> P.S. I have no idea what he is talking about regarding ERST.
>
>
> Ben Santer said the following on 7/30/2009 9:41 PM:
>> Dear Tom,
>>
>> Thanks for forwarding the message from John Christy. Excuse me for
>> being so blunt, but John's message is just a load of utter garbage.
>>
>> I got a laugh out of John's claim that Santer et al. (2008) was
>> "poorly done". This was kind of ironic coming from a co-author of the
>> Douglass et al. (2007) paper, which used a fundamentally flawed
>> statistical test to compare modeled and observed tropospheric
>> temperature trends. To my knowledge, John has NEVER acknowledged that
>> Douglass et al. used a flawed statistical test to reach incorrect
>> conclusions - despite unequivocal evidence from the "synthetic data"
>> experiments in Santer et al. (2008) that the Douglass et al. "robust
>> consistency" test was simply wrong. Unbelievably, Christy continues to
>> assert that the results of Douglass et al. (2007) "still stand". I can
>> only shake my head in amazement at such intellectual dishonesty. I
>> guess the best form of defense is a "robust" attack.
>>
>> So how does John support his contention that Santer et al. (2008) was
>> "poorly done"? He begins by stating that:
>>
>> "Santer et al. 2008 used ERSST data which I understand has now been
>> changed in a way that discredits the conclusion there".
>>
>> Maybe you or Tom Peterson or Dick Reynolds can enlighten me on this
>> one. How exactly have NOAA ERSST surface data changed? Recall that
>> Santer et al. (2008) actually used two different versions of the ERSST
>> data (version 2 and version 3). We also used HadISST sea-surface
>> temperature data, and combined SSTs and land 2m temperature data from
>> HadCRUT3v. In other words, we used four different observational
>> estimates of surface temperature changes. Our bottom-line conclusion
>> (no significant discrepancy between modeled and observed
>> lower-tropospheric lapse-rate trends) was not sensitive to our choice
>> of observed surface temperature dataset.
>>
>> John next asserts that:
>>
>> "Haimberger's v1.2-1.4 (of the radiosonde data) are clearly spurious
>> due to the error in ECMWF as published many places".
>>
>> I'll let Leo Haimberger respond to that one. And if v1.2 of Leo's data

>> is "clearly spurious", why did John Christy agree to be a co-author on
>> the Douglass et al. paper which uses upper-air data from v1.2?
>>
>> Santer et al. (2008) comprehensively examined structural uncertainties
>> in the observed upper-air datasets. They looked at two different
>> satellite and seven different radiosonde-based estimates of
>> tropospheric temperature change. As in the case of the surface
>> temperature data, getting the statistical test right was much more
>> important (in terms of the bottom-line conclusions) than the choice of
>> observational upper-air dataset.
>>
>> Christy's next criticism of our IJoC paper is even more absurd. He
>> states that:
>>
>> "Santer et al. 2008 asked a very different question...than we did. Our
>> question was "Does the IPCC BEST ESTIMATE agree with the Best Data
>> (including RSS)?" Answer - No. Santer et al. asked, "Does ANY IPCC
>> model agree with ANY data set?" ... I think you can see the
difference.
>>
>> Actually, we asked and answered BOTH of these questions. "Tests with
>> individual model realizations" are described in Section 4.1 of Santer
>> et al. (2008), while Section 4.2 covers "Tests with multi-model
>> ensemble-mean trend". As should be obvious - even to John Christy - we
>> did NOT just compare observations with results from individual models.
>>
>> For both types of test ("individual model" and "multi-model average"),
>> we found that, if one applied appropriate statistical tests (which
>> Douglass et al. failed to do), there was no longer a serious
>> discrepancy between modeled and observed trends in tropical lapse
>> rates or in tropical tropospheric temperatures.
>>
>> Again, I find myself shaking my head in amazement. How can John make
>> such patently false claims about our paper? The kindest interpretation
>> is that he is a complete idiot, and has not even bothered to read
>> Santer et al. (2008) before making erroneous criticisms of it. The
>> less kind interpretation is that he is deliberately lying.
>>
>> A good scientist is willing to acknowledge the errors he or she
>> commits (such as applying an inappropriate statistical test). John
>> Christy is not a good scientist. I'm not a religious man, but I'm sure
>> willing to thank some higher authority that Dr. John Christy is not
>> the "gatekeeper" of what constitutes sound science.
>>
>> I hope you don't mind, Tom, but I'm copying this email to some of the
>> other co-authors of the Santer et al. (2008) IJoC paper. They deserve
>> to know about the kind of disinformation Christy is spreading.
>>
>> With best regards,
>>
>> Ben
>>
>> Thomas R. Karl wrote:
>>> FYI

>>>
>>> ----- Original Message -----
>>> Subject: Re: [Fwd: concerns about the Southeast chapter]
>>> Date: Mon, 27 Jul 2009 09:54:22 -0500
>>> From: John Christy <john.christy@nsstc.uah.edu>
>>> To: Thomas C Peterson <Thomas.C.Peterson@noaa.gov>
>>> CC: Thomas R Karl <Thomas.R.Karl@noaa.gov>
>>> References: <4A534CF9.9080700@noaa.gov>
>>>
>>>
>>>
>>> Tom:
>>>
>>> I've been on a heavy travel schedule and just now getting to emails
>>> I've delayed. I was in Asheville briefly Thursday for a taping for
>>> the CDMP project at the Biltmore estates (don't know why that was the
>>> backdrop) while traveling between meetings in Chapel Hill, Atlanta
>>> and here.
>>>
>>> We disagree on the use of available climate information regarding the
>>> many things related to climate/climate change as I see by your
>>> responses below - that is not unexpected as climate is an ugly,
>>> ambiguous, and complex system studied by a bunch of prima donnas (me
>>> included) and which defies authoritative declarations. I base my
>>> views on hard-core, published literature (some of it mine, but most
>>> of it not), so saying otherwise is not helpful or true. The simple
>>> fact is that the opinions expressed in the CCSP report do not
>>> represent the real range of scientific literature (the IPCC fell into
>>> the same trap - so running to the IPCC's corner doesn't move things
>>> forward).
>>>
>>> I think I can boil my objections to the CCSP Impacts report to this
>>> one idea for the SE (and US): The changes in weather variables
>>> (measured in a systematic settings) of the past 30 years are within
>>> the range of natural variability. That's the statement that should
>>> have been front and center of this whole document because it is
>>> mathematically/scientifically defensible. And, it carries more
>>> weight with planners so you can say to them, "If it happened before,
>>> it will happen again - so get ready now." By the way, my State
>>> Climatologist response to the CCSP was well-received by legislators
>>> and stakeholders (including many in the federal government) and still
>>> gets hits at http://**vortex.nsstc.uah.edu/aosc/.
>>>
>>> There also was a page or so on the tropical troposphere-surface issue
>>> that I didn't talk about on my response. It was wrong because it did
>>> not include all the latest research (i.e. since 2006) on the
>>> continuing and significant difference between the two trends.
>>> Someone was acting as a fierce gatekeeper on that one - citing only
>>> things that agreed with the opinion shown even if poorly done (e.g.
>>> Santer et al. 2008 used ERSST data which I understand has now been
>>> changed in a way that discredits the conclusion there, and
>>> Haimberger's vl.2-1.4 are clearly spurious due to the error in ECMWF
>>> as published many places, but analyzed in detail in Sakamoto and
>>> Christy 2009). The results of Douglass et al. 2007 (not cited by

>>> CCSP) still stand since Santer et al. 2008 asked a very different
>>> question (and used bad data to boot) than we did. Our question was
>>> "Does the IPCC BEST ESTIMATE agree with the Best Data (including
>>> RSS)?" Answer - No. Santer et al. asked, "Does ANY IPCC model agree
>>> with ANY data set?" ... I think you can see the difference. The fact
>>> my 2007 tropical paper (the follow-on papers in 2009 were probably
>>> too late, but they substantiate the 2007 paper) was not cited
>>> indicates how biased this section was. Christy et al. 2007 assessed
>>> the accuracy of the datasets (Santer et al. did not - they assumed
>>> all datasets were equal without looking at the published problems)
>>> and we came up with a result that defied the "consensus" of the CCSP
>>> report - so, it was doomed to not be mentioned since it would disrupt
>>> the storyline. (And, as soon as RSS fixes their spurious jump in
>>> 1992, our MSU datasets will be almost indistinguishable.)
>>>

>>> This gets to the issue that the "consensus" reports now are just the
>>> consensus of those who agree with the consensus. The
>>> government-selected authors have become gatekeepers rather than
>>> honest brokers of information. That is a real tragedy, because when
>>> someone becomes a gatekeeper, they don't know they've become a
>>> gatekeeper - and begin to (sincerely) think the non-consensus
>>> scientists are just nuts (... it's more comfortable that way rather
>>> than giving them credit for being skeptical in the face of a
paradigm).

>>>

>>> Take care.

>>>

>>> John C.

>>>

>>> p.s. a few quick notes are interspersed below.

>>>

>>>

>>> Thomas C Peterson wrote:

>>>> Hi, John,

>>>> I didn't want this to catch you by surprise.

>>>>

Tom

>>>>

>>>> ----- Original Message -----

>>>> Subject: concerns about the Southeast chapter

>>>> Date: Tue, 07 Jul 2009 09:25:45 -0400

>>>> From: Thomas C Peterson <thomas.c.peterson@noaa.gov>

>>>> To: jim.obrien@coaps.fsu.edu

>>>> CC: Tom Karl <Thomas.R.Karl@noaa.gov>

>>>>

>>>>

>>>>

>>>> Dear Jim,

>>>>

>>>>

>>>> First off and most importantly, congratulations on your recent
>>>> marriage. Anthony said it was the most touching wedding he has ever
>>>> been to. I wish you and your bride all the best.

>>>>

>>>> Thank you for your comments and for passing on John Christy's

>>>> detailed concerns about the Southeast chapter of our report, /Global
>>>> Climate Change Impacts in the United States/. Please let me respond
>>>> to the key points he raised.

>>>>

>>>> In Dr. John Christy's June 23, 2009 document "Alabama climatologist
>>>> responds to U.S. government report on regional impacts of global
>>>> climate change", he primarily focused on 4 prime concerns:

>>>>

>>>> 1. Assessing changes since 1970.

>>>>

>>>> 2. Statements on hurricanes.

>>>>

>>>> 3. Electrical grid disturbances (from the Energy section).

>>>>

>>>> 4. Using models to assess the future.

>>>>

>>>>

>>>>

>>>> /1. Assessing changes since 1970./

>>>>

>>>> The Southeast section has 5 figures and one table. One figure is on
>>>> changes in precipitation patterns from 1901-2007. The next figure is
>>>> on patterns of days per year over 90F with two maps, one 1961-1979,
>>>> the other 2080-2099. One figure is on the change in freezing days
>>>> per year, 1976-2007. The next figure is on changes to a barrier
>>>> island land from 2002 to 2005. And the last figure was on Sea
>>>> Surface Temperature from 1900 to the present. The table indicates
>>>> trends in temperature and precipitation over two periods, 1901-2008
>>>> and 1970-2008. As Dr. Christy indicates in his paper, the full
>>>> period and the period since 1970 are behaving differently. To help
>>>> explain this, the table shows them both. Of the 5 figures, only one
>>>> shows the changes over this shorter period.

>>>>

>>>> Since, as the IPCC has indicated, the human impact on climate isn't
>>>> distinguishable from natural variability until about 1950,
>>>> describing the changes experienced in the majority of the time since
>>>> 1950 would be a more logical link to future anthropogenic climate
>>>> change. In most of the report, maps have shown the changes over the
>>>> last 50 years. Because of the distinct behavior of time series of
>>>> precipitation and temperature in the Southeast, discussing the
>>>> period since 1970 seemed more appropriate. Though as the figures and
>>>> table indicate, this shorter period is not the sole or even major
>>>> focus.

>>>>

>>>> See crux of the matter in email above - looking at the whole time
>>>> series is demanded by science. Any 30 or 50-year period will give
>>>> changes - blaming the most recent on humans ignores the similar (or
>>>> even more rapid) changes that occurred before industrialization (e.g.
>>>> western drought in 12th century). The period since 1970 WAS the
>>>> major focus in the SE section (mentioned 6 times in two pages). And,
>>>> OF COURSE any 30-year sub-period will have different characteristics
>>>> than the 100-year population from which it is extracted ... that
>>>> doesn't prove anything.

>>>>

>>>>
>>>>
>>>> /2. Statements on hurricanes./
>>>>
>>>> Dr. Christy takes issue with the report's statements about
>>>> hurricanes and quotes a line from the report and quotes an
>>>> individual hurricane expert who says that he disagrees with the
>>>> conclusions. The line in the report that Dr. Christy quotes comes
>>>> almost word for word out of CCSP SAP 3.3. While individual
>>>> scientists may disagree with the report's conclusions, this
>>>> conclusion came directly out of the peer-reviewed literature and
>>>> assessments. Dr. Christy also complains that "the report did not
>>>> include a plot of the actual hurricane landfalls". However, the
>>>> section in the Southeast chapter discussing landfalling hurricanes
>>>> states "see /National Climate Change/ section for a discussion of
>>>> past trends and future projections" and sure enough on page 35 there
>>>> is a figure showing land falling hurricanes along with a more in
>>>> depth discussion of hurricanes.
>>>>
>>> You didn't read my State Climatologist response carefully - I
>>> mentioned page 35 and noted again it talked about the most recent
>>> decades (and even then, the graph still didn't go back to 1850).
>>> This hurricane storyline was hit hard by many scientists - hence is
>>> further evidence the report was generated by a gatekeeper mentality.
>>>>
>>>>
>>>> /3. Electrical grid disturbances (from the Energy section)./
>>>>
>>>> Moving out of the Southeast, Dr. Christy complains about one figure
>>>> in the Energy Chapter. Citing a climate skeptic's blog which cites
>>>> an individual described as the keeper of the data for the Energy
>>>> Information Administration (EIA), John writes that the rise in
>>>> weather related outages is largely a function of better reporting.
>>>> Yet the insert of weather versus non-weather-related outages shows a
>>>> much greater increase in weather-related outages than
>>>> non-weather-related outages. If all the increases were solely due
>>>> to better reporting, the differences between weather- and
>>>> non-weather-related outages would indicate a dramatic decrease over
>>>> this time period in non-weather related problems such as
>>>> transmission equipment failures, earthquakes, faults in line, faults
>>>> at substations, relaying malfunctions, and vandalism.
>>>>
>>>> Thanks to the efforts of EIA, after they took over the
>>>> responsibility of running the Department of Energy (DOE)
>>>> data-collection process around 1997, data collection became more
>>>> effective. Efforts were made in subsequent years to increase the
>>>> response rate and upgrade the reporting form. It was not until EIA's
>>>> improvement of the data collection that the important decoupling of
>>>> weather- and non-weather-related events (and a corresponding
>>>> increase in the proportion of all events due to weather extremes)
>>>> became visible.
>>>>
>>>> To adjust for potential response-rate biases, we have separated
>>>> weather- and non-weather-related trends into indices and found an

>>>> upward trend only in the weather-related time series.
>>>>
>>>> As confirmed by EIA, *if there were a systematic bias one would
>>>> expect it to be reflected in both data series (especially since any
>>>> given reporting site would report both types of events).*
>>>>
>>>> As an additional precaution, we focused on trends in the number of
>>>> events (rather than customers affected) to avoid fortuitous
>>>> differences caused by the population density where events occur.
>>>> This, however, has the effect of understating the weather impacts
>>>> because of EIA definitions (see survey methodology notes below).
>>>>
>>>> More details are available at:
>>>> http://**eetd.lbl.gov/emills/pubs/grid-disruptions.html
>>>>
>>> The data were not systematically taken and should not have been shown
>>> .. basic rule of climate.
>>>>
>>>>
>>>>
>>>> /4. Using models to assess the future./
>>>>
>>>> Can anyone say anything about the future of the Southeast's climate?
>>>> Evidently according to John Christy, the answer is no. The basic
>>>> physics of the greenhouse effect and why increasing greenhouse gases
>>>> are warming and should be expected to continue to warm the planet
>>>> are well known and explained in the /Global Climate Change/ section
>>>> of the report. Climate models are used around the world to both
>>>> diagnose the observed changes in climate and to provide projections
>>>> for the future. There is a huge body of peer-reviewed literature,
>>>> including a large number of peer-reviewed climate change
>>>> assessments, supporting this use. But in Dr. Christy's "view,"
>>>> models should not be used for projections of the future, especially
>>>> for the Southeast. The report based, and indeed must base, its
>>>> results on the huge body of peer-reviewed scientific literature
>>>> rather than the view of one individual scientist.
>>>>
>>>> No one has proven models are capable of long-range forecasting.
>>>> Modelers write and review their own literature - there are millions
>>>> of dollars going into these enterprises, so what would you expect?
>>>> Publication volume shouldn't impress anyone. The simple fact is we
>>>> demonstrated in a straightforward and reproducible way that the
>>>> actual trends over the past 30, 20, and 10 years are outside of the
>>>> envelop of model predictions ... no one has disputed that finding
>>>> with an alternative analysis - even when presented before
>>>> congressional hearings where the opportunity for disagreement was
>>>> openly available.
>>>>
>>>> I hope this helps relieve some of your concerns.
>>>>
>>>> Regards,
>>>>
>>>> Tom Peterson
>>>>

>>>>
>>>>
>>>
>>>
>>> --
>>> *****
>>> John R. Christy
>>> Director, Earth System Science Center voice: 256-961-7763
>>> Professor, Atmospheric Science fax: 256-961-7751
>>> Alabama State Climatologist
>>> University of Alabama in Huntsville
>>> http://**www.**nsstc.uah.edu/atmos/christy.html
>>>
>>> Mail: ESSC-Cramer Hall/University of Alabama in Huntsville,
>>> Huntsville AL 35899
>>> Express: Cramer Hall/ESSC, 320 Sparkman Dr., Huntsville AL 35805
>>>
>>>
>>> --
>>>
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</x-flowed>

From: Michael Mann <mann@meteo.psu.edu>
To: Grant Foster <tamino_9@hotmail.com>
Subject: Re: ENSO blamed over warming - paper in JGR
Date: Fri, 7 Aug 2009 10:28:31 -0400
Cc: <trenbert@ucar.edu>, <p.jones@uea.ac.uk>, "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, <b.mullan@niwa.co.nz>, Gavin Schmidt <gschmidt@giss.nasa.gov>, <j.renwick@niwa.co.nz>

good news Grant, we can trust him to be professional.

on a related note, a few folks have expressed concern that the galley-formatting of the article w/out any label such as "submitted to JGR" is a bit misleading. some people think the paper has already gone to press!

we should add a clear label such as "sub judice" or "submitted" to any posted and/or circulating version of this,

mike

p.s. I've already had to correct both Andy Revkin and Joe Romm on this!

On Aug 6, 2009, at 7:19 PM, Grant Foster wrote:

Greetings,
I thought I'd let you all know that Steve Gahn has been assigned as editor for the submission.
Sincerely,
Grant

Windows Live: Keep your life in sync. [1]Check it out.

--

Michael E. Mann
Professor
Director, Earth System Science Center (ESSC)
Department of Meteorology Phone: (814) 863-4075
503 Walker Building FAX: (814) 865-3663
The Pennsylvania State University email: [2]mann@psu.edu
University Park, PA 16802-5013
website: [3]<http://www.meteo.psu.edu/~mann/Mann/index.html>
"Dire Predictions" book site:
[4]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. http://windowslive.com/explore?ocid=PID23384::T:WLMTAGL:ON:WL:en-US:NF_BR_sync:082009
2. <mailto:mann@psu.edu>
3. <http://www.meteo.psu.edu/~mann/Mann/index.html>
4. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

5. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Phil Jones <p.jones@uea.ac.uk>

To: Michael Mann <mann@meteo.psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>

Subject: Nature Aug 12

Date: Thu Aug 13 09:13:53 2009

Mike, Gavin,

See the attached - odd quote by McIntyre in the middle of this
.. he is not interested in challenging the science of climate change or in nit-picking,
but is simply asking that the data be made available. "The only policy I want people to
change is their data-access policy"

I must have been in a parallel universe for the past 7-8 years!

The CRU web page referred to in the article is this one.

[1]<http://www.cru.uea.ac.uk/cru/data/availability/>

I'm off at noon today - back in on Aug 20. I'll be checking email once a day,
but will not be looking at blog sites.

Olive Heffernan at Nature expects the Nature blog site to be hijacked by the deniers.
She also said she would put up an expanded article, but I can't see this.

Cheers

Phil

Prof. Phil Jones

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References

1. <http://www.cru.uea.ac.uk/cru/data/availability/>

From: Phil Jones <p.jones@uea.ac.uk>
To: "Niklaus Zimmermann" <niklaus.zimmermann@wsl.ch>
Subject: ECOCHANGE budget available to UEA
Date: Thu Aug 13 10:46:04 2009
Cc: k.briffa@uea.ac.uk

Nick,

Apologies if I've asked you this before, but I'm being asked about the ECOCHANGE budget that appears to be available to UEA.

With the UEA budget there is money in categories that UEA has not had money in before (in other EU projects). Do you know what this money is supposed to be for? We understand the budget for personnel and also travel, but it is the other categories - which seem to relate to more travel and costs for capital equipment.

Keith is still off work, but is recovering well from his operation. I'm off in the next few hours for 2 weeks away.

Cheers

Phil

Prof. Phil Jones
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Norwich Email p.jones@uea.ac.uk
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UK

From: P.Jones@uea.ac.uk

To: "Niklaus E. Zimmermann" <niklaus.zimmermann@wsl.ch>

Subject: Re: ECOCHANGE budget available to UEA - update

Date: Thu, 27 Aug 2009 10:55:06 +0100 (BST)

Cc: "Phil Jones" <p.jones@uea.ac.uk>, "Emmanuel Muhr" <emuhr@vitamib.com>, k.briffa@uea.ac.uk

Nick,

Thanks. Perhaps I'll need to contact Keith
as to why some of the items are in the budget.

I understand about the salary money.

Cheers

Phil

> Dear Phil, Emmanuel,

>

> sorry for late reply, I undergo
> evaluation these days. I add Emmanuel, so that he
> can correct if my answers are wrong!!!

>

> - In general, you decide how much you spend where as long
> as you have open tasks you are expected to contribute
> (which is the case for UEA, you are still involved in A5).

>

> - This means that you spend the money by declaration on
> the project netboard, and not by the original budget.

>

> - You cannot spend more salary, should there be no open
> task left for you.

>

> - You can spend more salary months than expected from the
> budget for a specific position, but you cannot spend
> more total money than the budget is.

>

> - One major constraint is teaching activity, which can
> only be spent in ECOCHANGE teaching activities (summer
> school), but you did not list any here.

>

> best,

> Nick

>

> PS: Dear Keith, I wish you all the best for

> recovery! Hope to see you soon again.

>
> At 17:34 26.08.2009, Phil Jones wrote:

>
>> Nick,
>> I've now found out some more information.

>>
>> In the Consumables category, we had £5070 and
>> have left £4543. There is little, we are
>> generally able to buy in this category.

>>
>> In a new category to us (called Recurrent
>> costs) there is £7013, with nothing spent.

>>
>> In another new category to us (called
>> Equipment under £5000) there is £5766, again with nothing spent.

>>
>> In another new category to us (called
>> Exceptional Non Payments) there is £3844, again with nothing spent.

>>
>> Finally in travel there was £22923 of which
>> we've spent (for meetings so far) £3445 so far, leaving £19477.

>>
>> These numbers were in Euros, but our accounts have them in UK pounds.
>> They have been converted using the official EU
>> rates euros/pounds. This should be about
>> 1.2 Euros equals one UK pound.

>>
>> We are talking about 36 thousand pounds! We
>> are almost spent up on salaries.

>>
>> Cheers
>> Phil

>>
>> Nick,
>> Apologies if I've asked you this before, but I'm being asked about
>> the ECOCHANGE budget that appears to be available to UEA.

>>
>> With the UEA budget there is money in categories that UEA has not
>> had money in before (in other EU projects). Do you know what this money
>> is supposed to be for? We understand the budget for personnel and also
>> travel, but it is the other categories - which seem to relate to more
>> travel

>> and costs for capital equipment.

>>

>> Keith is still off work, but is recovering well from his operation.

>>

>> I'm off in the next few hours for 2 weeks away.

>>

>> Cheers

>> Phil

>>

>>

>>

>>

>>Prof. Phil Jones

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>>School of Environmental Sciences Fax +44 (0) 1603 507784

>>University of East Anglia

>>Norwich Email p.jones@uea.ac.uk

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>>

>

>

>-----

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> Research Unit Head

> Land Use Dynamics

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> Zuercherstrasse 111, CH-8903 Birmensdorf, Switzerland

>

> phone: +41 (0)44-739-2337, fax: +41 (0)44-739-2215

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>

> email: niklaus.zimmermann@wsl.ch

> URL: <http://www.wsl.ch/staff/niklaus.zimmermann/>

>-----

>

>

From: Ian Harris <i.harris@uea.ac.uk>
To: t.osborn@uea.ac.uk
Subject: Re: Hopefully fixed TMP
Date: Fri, 4 Sep 2009 14:50:20 +0100

<x-flowed>

Hi Tim

I've re-run with the same database used for the previous 2006 run (tmp.0705101334.dtb).

/cru/cruts/version_3_0/update_top/gridded_finals/data/data.0909041051/
tmp/cru_ts_3_00.1901.2008.tmp.dat.nc.gz

Is that any better? If not please can you send the traditional multi-page country plots for me to pore over?

Cheers

Harry

On 3 Sep 2009, at 17:04, Tim Osborn wrote:

> Hi Harry and Phil,
>
> the mean level of the "updated-to-2008" CRU TS 3.0 now looks good,
> matching closely with the 1961-1990 means of the earlier CRU TS 3.0
> and
> CRU TS 2.1.
>
> Please see the attached PDF of country mean time series, comparing
> last-year's CRU TS 3.0 (black, up to 2005) with the most-recent CRU
> TS 3.0
> (pink, up to 2008).
>
> Latest version matches last-year's version well for the most part, and
> where differences do occur I can't say that the new version is any
> worse
> than last-year's version (some may be better).
>
> One exception is the hot JJA in Europe in 2003. This is less

> extreme in
> the latest version. See attached PNG for a blow-up of France in JJA.
>
> I'm sure some people will use CRU TS 3.0 to look at 2003 in Europe,
> so we
> need to be happy with the version we release.
>
> Perhaps some hot stations have been dropped as outliers (more than 3
> standard deviations from the mean)?
>
> But I'm not sure if that is the reason, since outlier checking was
> already
> used in last-year's version, wasn't it?
>
> Does the outlier checking always check ± 3 SD from 61-90 mean (or
> normal),
> or does it check ± 3 SD from the local mean (30-years centred on the
> value) which would allow for a gradual warming in both mean and
> outlier
> threshold?
>
> Cheers
>
> Tim
>
> On Wed, September 2, 2009 6:08 pm, Ian Harris wrote:
>> Tim
>>
>> When you have the time and/or the inclination, please can you run the
>> new TMP output through your IDL thingummajig?
>>
>> /cru/cruts/version_3_0/update_top/gridded_finals/data/data.
>> 0909021348/
>> tmp/cru_ts_3_00.1901.2008.tmp.dat.nc.gz
>>
>> Please let me know if you can't access it. I do appreciate your help!
>>
>> Cheers
>>
>> Harry
>
> --
> Dr. Tim Osborn

- > RCUK Academic Fellow
- > Climatic Research Unit
- > School of Environmental Sciences
- > University of East Anglia
- > Norwich NR4 7TJ, UK
- > www.cru.uea.ac.uk/~timo/

Ian "Harry" Harris
Climatic Research Unit
School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

</x-flowed>

From: Darrell Kaufman <Darrell.Kaufman@nau.edu>
To: Nick McKay <nmckay@email.arizona.edu>, Caspar Ammann
<ammann@ucar.edu>, David Schneider <dschneid@ucar.edu>, Jonathan Overpeck
<jto@email.arizona.edu>, "Bette L. Otto-Bliesner" <ottobli@ucar.edu>,
Raymond Bradley <rbradley@geo.umass.edu>, Miller Giff
<gmiller@colorado.edu>, Bo Vinther <bo@gfy.ku.dk>, Keith Briffa
<k.briffa@uea.ac.uk>
Subject: Arctic2k update?
Date: Sat, 5 Sep 2009 08:44:19 -0700
Cc: <mann@psu.edu>

All:

I received my first hate mail this AM, which helped me to realize that I shouldn't be wasting time reading the blogs.

Regarding the "upside down man", as Nick's plot shows, when flipped, the Korttajarvi series has little impact on the overall reconstructions. Also, the series was not included in the calibration. Nonetheless, it's unfortunate that I flipped the Korttajarvi data. We used the density data as the temperature proxy, as recommended to me by Antti Ojala (co-author of the original work). It's weakly inversely related to organic matter content. I should have used the inverse of density as the temperature proxy. I probably got confused by the fact that the 20th century shows very high density values and I inadvertently equated that directly with temperature.

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(4) We selected records that showed 20th century warming. The only records that I know of that go back 1000 years that we left out were from the Gulf of Alaska that are known to be related strongly to precipitation, not temperature, and we stated this upfront. Do we want to clarify that it would be inappropriate to use a record of precip to reconstruct temperature? Or do we want to assume that precip should increase with temperature and add those records in and show that the primary signals remain?

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Please let me -- better yet, the entire group -- know whether you think we should post a revision on RealScience, and whether we should include a reply to other criticism (1 through 5 above). I'm also thinking that I should write to Ojala and Tiljander directly to apologize for inadvertently reversing their data.

Other thoughts or advise?

Darrell

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<[1]nmckay@email.arizona.edu> wrote:

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[cid:2D818DEB-2A02-494E-B050-C1C5BACE9984@domain.actdslttmp] Embedded Content: Effect of flipping Korttajarvi.jpg: 00000001,0da94ca9,00000000,00000000

References

1. <mailto:nmckay@email.arizona.edu>

From: Jonathan Overpeck <jto@email.arizona.edu>
To: Darrell Kaufman <Darrell.Kaufman@nau.edu>, Nick McKay
<nmckay@email.arizona.edu>, Caspar Ammann <ammann@ucar.edu>, David
Schneider <dschneid@ucar.edu>, "Bette L. Otto-Bliesner"
<ottobli@ucar.edu>, Raymond Bradley <rbradley@geo.umass.edu>, Miller Giff
<gmiller@colorado.edu>, Bo Vinther <bo@gfy.ku.dk>, Keith Briffa
<k.briffa@uea.ac.uk>
Subject: Re: Arctic2k update?
Date: Sat, 05 Sep 2009 11:25:02 -0700
Cc: <mann@psu.edu>

D et al - Please write all emails as though they will be made public.
I would not rush and I would not respond to any of them until the best
strategy is
developed - don't want to waste anyone's time, including yours or
Mc's. Since the recon in
Science has an error, I think you do need to publish a correction in
Science. In that, you
can very briefly not it didn't affect the calibration, nor the final
result. I don't think
you have a choice here. And I don't think RealClimate alone is the
place for this, although
RC could be good for the bigger list of issues. Don't do it on Mc's
blog. But, it would be
good to hear from Ray and Mike, since they have the most experience in
getting it right.
Here are some other QUICK thoughts - don't count on me for the next
week. Proposal hell and
traveling.
Make sure you have Keith's feedback before saying anything about the
dendro aspects.
Don't know about Dye3 issue
Error analysis should be done and be the topic of another paper - it
wasn't included in
this paper, so it's something that should be done outside the peer-
review process. There is
lots of new research to be done, and someone should do it as time
allows. Don't get pushed
into something too rushed or preliminary, and your defense is that you
wrote a paper that
reviewed well and was published. The goal wasn't to do everything in
this paper.
#4 - your are absolutely right and that could be in a blog someplace,
or just let them go
ahead and do a stupid thing. If this was a climate field recon it
would be different, no?
#5 is tricky. Giving him the data would be good, but only if it is
yours to give. You can't
give him data that you got from others and are not allowed to share.
But, it would be nice
if he could have access to all the data that we used - that's the way
science is supposed
to work. See what Mike and Ray say...

Be careful, very careful. But now you know why I advocated redoing all the analyses a few months ago - to make sure we got it all right. We knew we'd get this scrutiny.

This paper has had great impact so far, so that's something to remember - its good work.

Thanks, peck

On 9/5/09 8:44 AM, "Darrell Kaufman" <[1]Darrell.Kaufman@nau.edu> wrote:

All:

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wasting time reading the blogs.

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[cid:3334994702_4110695]

Jonathan T. Overpeck

Co-Director, Institute of the Environment

Professor, Department of Geosciences

Professor, Department of Atmospheric Sciences

Mail and Fedex Address:

Institute of the Environment

715 N. Park Ave. 2nd Floor

University of Arizona

Tucson, AZ 85721

direct tel: +1 520 622-9065

Email: [3]jto@u.arizona.edu

PA Lou Regalado +1 520 792-8712

[4]regalado@email.arizona.edu

Embedded Content: image7.jpg: 00000001,780e1428,00000000,00000000

References

1. file://localhost/tmp/Darrell.Kaufman@nau.edu
2. file://localhost/tmp/nmckay@email.arizona.edu
3. file://localhost/tmp/jto@u.arizona.edu
4. file://localhost/tmp/regalado@email.arizona.edu

From: Darrell Kaufman <Darrell.Kaufman@nau.edu>
To: Bo Vinther <bo@gfy.ku.dk>
Subject: Re: Arctic2k update?
Date: Sun, 6 Sep 2009 06:31:35 -0700
Cc: Nick McKay <nmckay@email.arizona.edu>, Caspar Ammann
<ammann@ucar.edu>, David Schneider <dschneid@ucar.edu>, Jonathan Overpeck
<jto@email.arizona.edu>, "Bette L. Otto-Bliesner" <ottobli@ucar.edu>,
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<gmiller@colorado.edu>, "Keith Briffa" <k.briffa@uea.ac.uk>,
"mann@psu.edu" <mann@psu.edu>

Bo and others:

Regarding the annual data: You're correct that we only use 10-year means throughout our calculations (Fig 2 shows annual values, but are not used in any calculation/conclusion).

In his e-mail to me, McIntyre requested the annual data that we say are not publicly available as a footnote to Table S1.

Unless anyone has another suggestion, I will reply and send him the 10-year data (which is already posted at NOAA-Paleoclimate) and explain that they were the basis for all of the calculations. He might want the annual data that the mean values were based on. I suppose we'll cross that bridge when we get to it.

Darrell

On Sep 6, 2009, at 5:42 AM, Bo Vinther wrote:

Hi Darrell

Sorry to hear that you are getting trouble for doing such a nice paper....I by the way

agree completely with Peck that we should not be rushed and that a correction probably should go into Science.

Anyway, let me answer the two questions you had for me:

2) Correcting ice core data for upstream effects should not be controversial (while not correcting in areas of flow should be highly controversial indeed!).

Upstream correction of delta-180 was in fact already done 30 years ago for the Milcent ice

core - a quick quote from Hammer et al. 1978, page 14:

"The delta values are corrected for decreasing deltas up-slope at the site of formation of the individual layers"

Hammer, C. U., H. B. Clausen, W. Dansgaard, N. Gundestrup, S. J. Johnsen and N. Reeh, Dating of Greenland ice cores by flow models, isotopes, volcanic debris, and continental dust, J. Glaciol., 20, 326, 1978.

So upstream correction of delta data from ice cores 8using ice flow models9 has in fact

been performed since the year I was born.....

5) I will suggest that we release the 1860-2000 section of the annually resolved ice core

data, as these are the data that go into figure 2 in the paper.

Such a limited release I can permit immediately.

Releasing everything is something different and I can't see the need - as far as I rememver

we are not presenting/using the 1-1859 part of the series in annual resolution anywhere in

the paper - or am I wrong?

Cheers.....Bo

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References

1. <mailto:nmckay@email.arizona.edu>

From: Ben Santer <santer1@llnl.gov>
To: claudia tebaldi <ctebaldi@climatecentral.org>
Subject: Re: Important: Input for Funding
Date: Fri, 11 Sep 2009 08:30:19 -0700
Reply-to: santer1@llnl.gov
Cc: Myles Allen <allen@atm.ox.ac.uk>, Knutti Reto
<reto.knutti@env.ethz.ch>, "Stott, Peter" <peter.stott@metoffice.gov.uk>, Gabi Hegerl <gabi.hegerl@ed.ac.uk>, "Zwiers, Francis [Ontario]" <francis.zwiers@ec.gc.ca>, Tim Barnett <tbarnett-ul@ucsd.edu>, Hans von Storch <hvonstorch@web.de>, Phil Jones <p.jones@uea.ac.uk>, David Karoly <dkaroly@unimelb.edu.au>, Toru Nozawa <nozawa@nies.go.jp>, Daithi Stone <stoned@atm.ox.ac.uk>, Richard Smith <rls@email.unc.edu>, Nathan Gillett <n.gillett@uea.ac.uk>, Michael Wehner <MFWehner@lbl.gov>, Doug Nychka <nychka@ucar.edu>, Xuebin Zhang <Xuebin.Zhang@ec.gc.ca>, Tom Knutson <Tom.Knutson@noaa.gov>, Tim Delsole <delsole@cola.iges.org>, "Jones, Gareth S" <gareth.s.jones@metoffice.gov.uk>, Stephen Leroy <leroy@huarp.harvard.edu>, seung-ki.min@ec.gc.ca, dpierce@ucsd.edu

<x-flowed>

Dear Claudia,

The 13th session of the Working Group on Climate Modelling (WGCM) is going to be taking place in San Francisco at the end of this month. PCMDI is hosting this event. I just received an invitation to talk about IDAG at this meeting. I'd be very happy to do this, but would appreciate some guidance from you and others regarding what aspects of IDAG you'd like me to discuss.

With best regards,

Ben

claudia tebaldi wrote:

> Hi again

>

> I'm attaching the current version after some remessaging, especially of
> the task list.

> There is a need for a reference that I would like to get from David
> Karoly, and a general request for input having to do with the synthesis
> products that originally were described as instrumental to AR5 but Gabi
> thinks they would not be prepared in time for that. So I'm wondering if
> people have specific ideas for the next round of review papers that we
> could describe at the end of Section 3 of the document.

>

> MOST IMPORTANTLY:

> I need some very specific input from *all of you* (only exception,
> Francis's group).

>

> After asking Anjuli I can confirm that government employees cannot
> receive funding besides travel reimbursement. So for those of you that
> are GOVERNMENT EMPLOYEES, the only thing that remains to do is to go
> through the document once again, make sure your work (past and future)
> is not misrepresented, and then send me a note with an "OK" or your new
> comments, specifying that you are a government employee (please don't
> let me guess it).

>
> For those of you that are ACADEMICS WITH 12 MONTHS SALARY all that we
> can budget is a small amount of consulting fees, up to 2 weeks' worth.
> If you belong to this category please respond saying that you are or
you
> are not interested. If you are, then include in the document at the end
> in the place already arranged for it a statement of work referring to
> specific tasks as they stand in Section 3 of the narrative, and a
> bio-sketch (see end of this email for specific instructions).
>
> For THOSE OF YOU THAT CAN GET FULL SUPPORT, please say if you want it
or
> not, and if you do, then do as I requested above: include in the
> document at the end in the place already arranged for it a statement of
> work referring to specific tasks as they stand in Section 3 of the
> narrative, and a bio-sketch (see end of this email for specific
> instructions).
>
> Please shoot me an email and say something, esp. those of you abroad
for
> whom I'm not familiar with affiliations/months of salary. Needless to
> say, if you don't send the bio and don't put yourself down in the
> Statements of Work session you won't be budgeted but for travel
> reimbursement.
>
> Can I ask you to do this at your earliest convenience, but at the
latest
> before mid-week next week?
>
> Thanks
>
> c
>
> PS I received only 2 figures in response to my earlier request. If you
> take the time to read the narrative and have a good figure for it, send
> it along!
>
> #####
> Biographical Sketches: Instructions
> #####
>
> The biographical sketch is limited to a maximum of two pages. It must
> contain name and position title, organization, degree, years and field
> of study for each academic degree; a listing of research and
> professional positions, awards, and honors; and references to all
> publications for the past three years along with any earlier
> publications pertinent to this application. If this list causes the
> biographical sketch to exceed two pages, select the most pertinent
> publications to stay within the page limit.
>
>
> Current and Pending Support
>
> The PI/PD(s) are requested to list all their current and pending

> non-Federal and Federal support.
>
> Identification of Potential Conflicts of Interest/Bias in Selection of
> Reviewers
>
> Provide the following information:
>
> Collaborators and Co-editors: List in alphabetical order all
> persons, including their current organizational affiliation, who are,
or
> who have been, collaborators or co-authors with you on a research
> project, book or book article, report, abstract, or paper during the 48
> months preceding the submission of this application. Also, list any
> individuals who are currently, or have been, co-editors with you on a
> special issue of a journal, compendium, or conference proceedings
during
> the 24 months preceding the submission of this application. If there
are
> no collaborators or co-editors to report, state 'none'.
>
> Graduate and Postdoctoral Advisors and Advisees: List the names
and
> current organizational affiliations of your graduate advisor(s) and
> principal postdoctoral sponsor(s) during the last 5 years. Also, list
> the names and current organizational affiliations of your graduate
> students and postdoctoral associates during the past 5 years.
>
> --
> Claudia Tebaldi
> Research Scientist, Climate Central
> http://*www.*climatecentral.org
> & Adjunct Professor
> Department of Statistics - UBC Vancouver
> office 604 822 3595 (Canadian area code)
> cell 303 775 5365 (US area code)
>

--

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Lawrence Livermore National Laboratory
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Livermore, CA 94550, U.S.A.
Tel: (925) 422-3840
FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: Malcolm Hughes <mhughes@lrr.arizona.edu>
To: Tom Melvin <t.m.melvin@uea.ac.uk>
Subject: Re: recent paper
Date: Mon, 21 Sep 2009 15:23:49 -0700
Cc: Keith <K.Briffa@uea.ac.uk>

<x-flowed>

Hi Tom - please find the Esper article in question attached. The so-called Indigirka River data set is not yet available because it has not been published. I am currently working on that with Russian colleagues, and was indeed in Switzerland the week before last to work with one of them on specifically this. All being well, there will be an accepted manuscript before next summer, and at that point I will make the data freely available. Once we get to that point, I'll let you know, of course. Cheers, Malcolm

Tom Melvin wrote:

> Malcolm,

>

> 1. There was a recent Esper Siberian paper I recall reading but I
> cannot find it at the moment (my comment was on the Divergence
> pitfalls paper). I will find the paper and see if there is an
> explanation.

>

> 2. For trend distortion to produce a "divergence" effect there needs
> to be a distinct increase (or decrease) over the last few decades of
> growth, e.g. at TTHH and curve fitting methods should be used. In the
> attached figure the Scandinavian site groups (red) have an increase at
> 1920 and are likely to show divergence using curve fitting methods.
> Some of the eastern most chronologies might also show divergence if
> 250+ year old trees were used.

>

> 3. RCS should not produce "divergence" over decades as an artifact if
> sub-fossil trees are used. RCS on modern chronologies has all sorts
> of bias. We have lots of ideas to test in the divergence project and
> lots of data to test them on.

>

> 4. Keith has been complained at by Climate Audit for cherry picking
> and not using your long Indigirka River data set. Not used because we
> did not have the data. Please, could we have the data? We will make
> proper acknowledgement/coauthorship if we use the data.

>

> Tom

>

>

>

> At 16:35 21/09/2009, you wrote:

>> Tom, I don't disagree with your take on the lack of originality of
>> much of what is in the paper. The question is: why is there apparently
>> divergence in ring width in some of this region in Briffa et al 98 but
>> not in this paper? Isn't espers failure to see divergence
>> counterintuitive when using RCS in his way?

>> Cheers, Malcolm

>>

>>

>> On Sep 21, 2009, at 2:11 AM, Tom Melvin <t.m.melvin@uea.ac.uk> wrote:

>>

>>> Malcolm,

>>>

>>> The Esper "Divergence pitfalls .." paper does not appear to add
>>> anything of significance. None of the figures show any form of the
>>> divergence discussed in papers e.g. a recent (last few decades)
>>> change in the slope of tree-ring growth indices compared to climate.
>>> Differences in overall slope, generally weak relationships,
>>> differences in variance, and the effects of using selected
>>> calibration periods are all problems to be addressed in
>>> reconstructions but are not divergence.

>>>

>>> I cannot foresee needing to reference this paper in discussions of
>>> divergence as all the suggestions have more detailed, earlier
>>> references.

>>>

>>> Tom

>>>

>>>

>>> At 22:33 18/09/2009, you wrote:

>>>> Hi Tom - I had a good talk with Keith on the phone the other day,
>>>> mainly to wish him well. He did suggest I ask you for your take on
>>>> the recent Esper et al paper on divergence (or rather the lack of
>>>> it) in Siberia. Looks like the problem disappears. WHat do you
>>>> think? Cheers, Malcolm

>>>>

>>>> Dr. Tom Melvin

>>>> Climatic Research Unit

>>>> University of East Anglia

>>> Norwich, NR4 7TJ, U.K.

>>>

>>> Phone: +44-1603-593161

>>> Fax: +44-1603-507784

> Dr. Tom Melvin

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>

> Phone: +44-1603-593161

> Fax: +44-1603-507784

</x-flowed>

Attachment Converted: "c:\eudora\attach\Eesper-2009-GCB.pdf"

From: Michael Mann <mann@meteo.psu.edu>

To: Tom Wigley <wigley@ucar.edu>

Subject: Re: help

Date: Tue, 22 Sep 2009 11:00:28 -0400

Cc: Phil Jones <p.jones@uea.ac.uk>, Caspar Ammann <ammann@ucar.edu>

Hey Tom, thanks for checking w/ me on this. Re, Moberg: Yes, in fact we (me, Phil, Tim, Keith, Caspar, etc.) submitted a comment to Nature about the problem w/ the variance scaling used by Moberg. It can easily be shown to inflate the low- frequency variance in synthetic experiments. I've attached both the original comment (which they judged to be too technical to merit publication) and also a J. Climate paper where we discussed the same result (see Figure 5 and associated discussion). Re, Von Storch et al. Yes, the paper you have in mind is Osborn et al Climate Dynamics '06. I only seem to have the preprint though (attached), please let me know if I can be of any further help w/ an of this, mike p.s. you can delete the U.Va email address--haven't been there for 4 years! On Sep 22, 2009, at 10:31 AM, Tom Wigley wrote: > Dear all, >> (Apologies Mike for email address confusion -- one of them will > get you I hope.) >> I need some help to finish a report I've had to write for EPRI -- > which is due in a few days. Hence the questions below ... >> (1) The Moberg paper (2005 Nature) is used by the skeptics as evidence > that most of recent warming could still be natural. Has anyone > published a critique/criticism of this? It seems to me take this > work is fundamentally flawed. First, variance scaling is crap > statistics as it produces results with far less explained variance > than normal least-squares regression. Second, the paper seems to > have no independent validation. Third, what happens if one just takes > his low-frequency (numbered in his Fig. 1) points and calculates > the area average? Surely this will have much greater variability > than the full global mean? (If no-one has done this please let me > know -- I can do it very easily myself.) But perhaps his scaling > method circumvents this "problem"? >> (2) What is the paper of Caspar's (with Doug Nychka) that shows > that McIntyre is wrong? Are there other papers I should see/cite > in this regard? >> (3) What are the papers that explain what is wrong with the von > Storch ECHO simulation? I think Tim Osborn did something on this. >> Many thanks for your help, > Tom. > -- Michael E. Mann Professor Director, Earth System Science Center (ESSC) Department of Meteorology Phone: (814) 863-4075 503 Walker Building FAX: (814) 865-3663 The Pennsylvania State University email: mann@psu.edu University Park, PA 16802-5013 website: <http://www.meteo.psu.edu/~mann/Mann/index.html> "Dire Predictions" book site: http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html Hey Tom,

thanks for checking w/ me on this.

Re, Moberg: Yes, in fact we (me, Phil, Tim, Keith, Caspar, etc.) submitted a comment to Nature about the problem w/ the variance scaling used by Moberg. It can easily be shown to inflate the low-frequency variance in synthetic experiments.

I've attached both the original comment (which they judged to be too technical to merit publication) and also a J. Climate paper where we discussed the same result (see Figure 5 and associated discussion).

Re, Von Storch et al. Yes, the paper you have in mind is Osborn et al Climate Dynamics '06. I only seem to have the preprint though (attached),

please let me know if I can be of any further help w/ an of this,

mike

p.s. you can delete the U.Va email address--haven't been there for 4 years!

On Sep 22, 2009, at 10:31 AM, Tom Wigley wrote:

Dear all,

(Apologies Mike for email address confusion -- one of them will get you I hope.)

I need some help to finish a report I've had to write for EPRI -- which is due in a few days. Hence the questions below ...

(1) The Moberg paper (2005 Nature) is used by the skeptics as evidence that most of recent warming could still be natural. Has anyone published a critique/criticism of this? It seems to me take this work is fundamentally flawed. First, variance scaling is crap statistics as it produces results with far less explained variance than normal least-squares regression. Second, the paper seems to have no independent validation. Third, what happens if one just takes his low-frequency (numbered in his Fig. 1) points and calculates the area average? Surely this will have much greater variability than the full global mean? (If no-one has done this please let me know -- I can do it very easily myself.) But perhaps his scaling method circumvents this "problem"?

(2) What is the paper of Caspar's (with Doug Nychka) that shows that McIntyre is wrong? Are there other papers I should see/cite in this regard?

(3) What are the papers that explain what is wrong with the von Storch ECHO simulation? I think Tim Osborn did something on this.

Many thanks for your help,

Tom.

--

Michael E. Mann

Professor

Director, Earth System Science Center (ESSC)

Department of Meteorology Phone: (814) 863-4075

503 Walker Building FAX: (814) 865-3663

The Pennsylvania State University email: [1]mann@psu.edu

University Park, PA 16802-5013

website: [2]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[3]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Attachment Converted: "c:\eudora\attach\MRWA-JClimate05.pdf" Attachment Converted:

"c:\eudora\attach\62811_0_merged_1109271201.pdf" Attachment Converted:

"c:\eudora\attach\osbornetalClimDynInPress06.pdf"

References

Visible links

1. <mailto:mann@psu.edu>

2. <http://www.meteo.psu.edu/~mann/Mann/index.html>

3. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

4. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Tom Wigley <wigley@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: 1940s
Date: Sun, 27 Sep 2009 23:25:38 -0600
Cc: Ben Santer <santer1@llnl.gov>

<x-flowed>

Phil,

Here are some speculations on correcting SSTs to partly explain the 1940s warming blip.

If you look at the attached plot you will see that the land also shows the 1940s blip (as I'm sure you know).

So, if we could reduce the ocean blip by, say, 0.15 degC, then this would be significant for the global mean -- but we'd still have to explain the land blip.

I've chosen 0.15 here deliberately. This still leaves an ocean blip, and i think one needs to have some form of ocean blip to explain the land blip (via either some common forcing, or ocean forcing land, or vice versa, or all of these). When you look at other blips, the land blips are 1.5 to 2 times (roughly) the ocean blips -- higher sensitivity plus thermal inertia effects. My 0.15 adjustment leaves things consistent with this, so you can see where I am coming from.

Removing ENSO does not affect this.

It would be good to remove at least part of the 1940s blip, but we are still left with "why the blip".

Let me go further. If you look at NH vs SH and the aerosol effect (qualitatively or with MAGICC) then with a reduced ocean blip we get continuous warming in the SH, and a cooling in the NH -- just as one would expect with mainly NH aerosols.

The other interesting thing is (as Foukal et al. note -- from MAGICC) that the 1910-40 warming cannot be solar. The Sun can get at most 10% of this with Wang et al solar, less with Foukal solar. So this may well be NADW, as Sarah and I noted in 1987

(and also Schlesinger later). A reduced SST blip in the 1940s makes the 1910-40 warming larger than the SH (which it currently is not) -- but not really enough.

So ... why was the SH so cold around 1910? Another SST problem? (SH/NH data also attached.)

This stuff is in a report I am writing for EPRI, so I'd appreciate any comments you (and Ben) might have.

Tom.

</x-flowed>

Attachment Converted: "c:\eudora\attach\TTHEMIS.xls"

Attachment Converted: "c:\eudora\attach\TTLVSO.XLS"

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: 1940s
Date: Mon Sep 28 10:20:14 2009
Cc: Ben Santer <santer1@llnl.gov>

Tom,

A few thoughts

[1]http://ams.allenpress.com/archive/1520-0442/preprint/2009/pdf/10.1175_2009JCLI3089.1.pdf

This is a link to the longer Thompson et al paper. It isn't yet out in final form - Nov09 maybe?

[2]<http://wattsupwiththat.com/2009/09/24/a-look-at-the-thompson-et-al-paper-hi-tech-wiggle-matching-and-removal-of-natural-variables/>

is a link to wattsupwiththat - not looked through this apart from a quick scan. Dave Thompson just emailed me this over the weekend and said someone had been busy! They seemed to have not fully understood what was done.

Have looked at the plots. I'm told that the HadSST3 paper is fairly near to being submitted, but I've still yet to see a copy. More SST data have been added for the WW2 and WW1 periods, but according to John Kennedy they have not made much difference to these periods.

Here's the two ppts I think I showed in Boulder in June. These were from April 09, so don't know what these would look like now. SH is on the left and adjustment there seems larger, for some reason - probably just British ships there?

Maybe I'm misinterpreting what you're saying, but the adjustments won't reduce the 1940s blip but enhance it. It won't change the 1940-44 period, just raise the 10 years after Aug 45.

I expect MOHC are looking at the NH minus SH series re the aerosols. My view is that a cooler temps later in the 1950s and 1960s it is easier to explain.

Land warming in the 1940s and late 1930s is mainly high latitude in NH.

One other thing - MOHC are also revising the 1961-90 normals. This will likely have more effect in the SH.

With the SH around 1910s there is the issue of exposure problems in Australia - see Neville's paper.

This shouldn't be an issue in NZ - except maybe before 1880, but could be in southern South America. New work in Spain suggest screens got renewed about 1900, so maybe this happened in Chile and Argentina, but Mossmann was head of the Argentine NMS so he may have got them to use Stevenson screens early.

Neville has never been successful getting any OZ funding to sort out pre-1910 temps everywhere except Qld.

Here's a paper in CC on European exposure problems. There is also one on Spanish series.

Cheers

Phil

At 06:25 28/09/2009, Tom Wigley wrote:

Phil,

Here are some speculations on correcting SSTs to partly explain the 1940s warming blip.

If you look at the attached plot you will see that the land also shows the 1940s blip (as I'm sure you know). So, if we could reduce the ocean blip by, say, 0.15 degC, then this would be significant for the global mean -- but we'd still have to explain the land blip.

I've chosen 0.15 here deliberately. This still leaves an ocean blip, and i think one needs to have some form of ocean blip to explain the land blip (via either some common

forcing, or ocean forcing land, or vice versa, or all of these). When you look at other blips, the land blips are 1.5 to 2 times (roughly) the ocean blips -- higher sensitivity plus thermal inertia effects. My 0.15 adjustment leaves things consistent with this, so you can see where I am coming from. Removing ENSO does not affect this.

It would be good to remove at least part of the 1940s blip, but we are still left with "why the blip".

Let me go further. If you look at NH vs SH and the aerosol effect (qualitatively or with MAGICC) then with a reduced ocean blip we get continuous warming in the SH, and a cooling in the NH -- just as one would expect with mainly NH aerosols. The other interesting thing is (as Foukal et al. note -- from MAGICC) that the 1910-40 warming cannot be solar. The Sun can get at most 10% of this with Wang et al solar, less with Foukal solar. So this may well be NADW, as Sarah and I noted in 1987 (and also Schlesinger later). A reduced SST blip in the 1940s makes the 1910-40 warming larger than the SH (which it currently is not) -- but not really enough.

So ... why was the SH so cold around 1910? Another SST problem? (SH/NH data also attached.)

This stuff is in a report I am writing for EPRI, so I'd appreciate any comments you (and Ben) might have.
Tom.

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. http://ams.allenpress.com/archive/1520-0442/preprint/2009/pdf/10.1175_2009JCLI3089.1.pdf
2. <http://wattsupwiththat.com/2009/09/24/a-look-at-the-thompson-et-al-paper-hi-tech-wiggle-matching-and-removal-of-natural-variables/>

From: Susan Parham <sp@cagconsult.co.uk>
To: Peter Kenway <peter.kenway@npi.org.uk>, "Adger Neil Prof (ENV)" <N.Adger@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Mick Denness <m.denness@bctv.org.uk>, Andrew Gouldson <a.gouldson@leeds.ac.uk>, c.l.busfield@see.leeds.ac.uk, Tom MacInnes <tom.macinnes@npi.org.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Niamh Carey <ncarey@wwf.org.uk>, amanda@cdx.org.uk
Subject: I am afraid we didn't get the JRF climate change research
Date: Mon, 28 Sep 2009 14:41:41 +0100
Cc: Denny Gray <dg@cagconsult.co.uk>, Emma Cranidge <ec@cagconsult.co.uk>, Tim Maiden <tm@cagconsult.co.uk>, Mary Anderson <ma@cagconsult.co.uk>, Helen Chalmers <hc@cagconsult.co.uk>, Niall Machin <nm@cagconsult.co.uk>, Gerard Couper <gc@cagconsult.co.uk>

Dear All

Im afraid its bad news on the JRF bid. We were not selected.

The gist of the letter I have now received says the problem was that it went over the £100,000 mark for a single bid and was therefore out of contention on those grounds - they accepted I'd rung to check about this and so said they reviewed the proposal given 'the potential confusion' about this.

They also said "it was unfortunate that the proposal did not more clearly demonstrate how it could build on the findings from the existing review of social impacts CAG has conducted". (No mention of the 3,000 word word limit for the whole proposal).

I just want to say thanks again for all you great work on this. I do think JRF wrote a confusing and difficult brief and we did a good job despite their strange requirements.

I hope this won't put people bidding again should other suitable work come up.

all the best

Susan

ps I will be scanning their letter and will send round tomorrow.
Dr Susan Parham
Director - CAG Consultants
Tel: 020 7704 0018 Mob: 07967 816 295
[1]sp@cagconsult.co.uk
www.cagconsult.co.uk
Office: 30 Aberdeen Road,
London, N5 2UH
HQ: Gordon House, 6 Lissenden Gardens, London, NW5 1LX

References

1. <mailto:sp@cagconsult.co.uk>

From: Kevin Trenberth <trenbert@ucar.edu>
To: Grant Foster <tamino_9@hotmail.com>
Subject: Re: FW: 2009JD012960 (Editor - Steve Ghan):Decision Letter
Date: Mon, 28 Sep 2009 14:45:18 -0600
Cc: Mike Mann <mann@meteo.psu.edu>, p.jones@uea.ac.uk, "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, j.renwick@niwa.co.nz

Hi all

About time. Incidentally i gave a copy to Mike McPhaden and discussed it with him last week when we were together at the OceanObs'09 conference. Mike is President of AGU. Basically this is an acceptance with a couple of suggestions for extras, and some suggestions for toning down the rhetoric. I had already tried that a bit. My reaction is that the main thing is to expedite this. That means no extras unless it really makes sense. And removal of a few unnecessary words like "absolutely".

In the abstract, we have a number of such adjectives that could be removed: I agree with Rev 3 in this.

"greatly overstates" could be just "overstates" as it is reinforced better later.

"severely overestimates" could be just "overestimates"

"faulty analysis" maybe "flawed analysis"?

"extremely high" maybe "very high" or "unduly high"

I would leave last sentence alone though as the main comment.

A few more comments embedded below.

Grant Foster wrote:

> From: [1]jgr-atmospheres@agu.org
> Date: Mon, 28 Sep 2009 15:54:05 +0000
> To: [2]tamino_9@hotmail.com
> Subject: 2009JD012960 (Editor - Steve Ghan):Decision Letter
> CC: [3]twistor9@gmail.com
>
> Manuscript Number: 2009JD012960
> Manuscript Title: Comment on "Influence of the Southern Oscillation on tropospheric temperature" by J. D. McLean, C. R. de Freitas, and R. M. Carter
>
> -----
>
> Reviewer Comments
>
> Reviewer #1 (Comments):
>
> This paper does an excellent job of showing the errors in the analytical methods used by McLean et al. and why their conclusions
> about the influence of ENSO on global air temperature is incorrect.
> I have only a couple of suggestions to help clarify their analysis of the methods. First, a little more explanation of the comment about the time derivative reduced to an additive constant would help. Second, in the analysis of the artificial time series I think it would be interesting to show the results of both steps of filtering (running mean and derivative) as separate time series. This would help the reader understand why the filtering creates false correlations. The only other suggestion is to find a better adjective than "faulty" in the abstract to characterize the analysis.
>

It is not so easy to see the result from the derivative owing to the phase shift. The spectrum actually does a better job. I would address this comment in this way and change "faulty".

>
> Reviewer #2 (Comments):
>
> I think this comment on McLean et al can be published more or less as is.

>

> I have two comments

>

> First, in the abstract (page 3, line 15), I'm not sure that "inflating" is quite the right verb - the paper itself does not make the point that the filter constructed by McLean et al inflates power in the 2-6 year window. Perhaps "isolating" would be a better verb.

Yes it should not be in abstract if not in text. Need to point out that the response function in Fig. 1 is greater than unity and does "inflate". So adjust the text.

>

> Secondly, I think the points that are being made with Figures 4 and 5 could be strengthened by adding to the right of each plot of a pair of time series, a scatter plot of the pairs of values available at each time. Such a scatter plot would help to clearly illustrate the absence (upper panels) or presence (lower panels) of correlation between red and black values.

I don't think this helps. There is nothing to be gained from a scatter plot that a correlation or regression value does not summarize.

>

>

> Reviewer #3 (Comments):

>

> Accept pending major changes (mainly in style not scientific comment)

>

> The real mystery here, of course, is how the McLean et al. paper ever made it into JGR. How that happened, I have no idea. I can't see it ever getting published through J Climate. The analyses in McLean et al. are among the worst I have seen in the climate literature. The paper is also a poorly guised attack on the integrity of the climate community, and I guess that is why Foster et al. have taken the energy to contradict its findings.

>

> So the current paper (Foster et al.) should certainly be accepted. Someone needs to address the science in the McLean et al paper in the peer-reviewed literature. But the current paper could be - and should be - done better. That's why I am suggesting major changes before the paper is accepted. All of my suggestions have to do more with the tone and framing of the current paper, rather than its content.

>

> 1. As noted above, I agree McLean et al is problematic. But as it is written, the current paper almost stoops to the level of "blog diatribe". The current paper does not read like a peer-reviewed journal article. The tone is sometimes dramatic and sometimes accusatory. It is inconsistent with the language one normally encounters in the objectively-based, peer-reviewed literature. For examples...

> - In the abstract: Do you really need all of these adjectives?... 'greatly overstates'; 'severely overestimates'; 'faulty analysis'; 'extremely high'.

Agree, see above

> - In the introduction... 'Unfortunately, their conclusions are seriously in error...' strikes me as overly subjective. Better to say: 'We will demonstrate that their conclusions are strongly dependent on' or something like that...

Don't go that far. Could drop "seriously" but they are "in error"

> - Page X-6: 'tell us absolutely nothing'. Surely it's enough to state 'tell us nothing'.

agree

> - Page X-9: 'it is misleading...' That's a strong word. It may be true. But I think we should rise above such accusations.

misleading is OK. I did a search (not sure I have latest) and found "grossly misleading" and the "grossly" could be removed.

>
> Anyway, I'm sure the lead author gets my point. I think the current paper will have a much greater impact (and can claim the high road) if it is rewritten in a more objective manner.
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> 2. Similarly, instead of framing the paper as "Taking down McLean et al.", why not focus more on interesting aspects of the science, such as the frequency dependence between ENSO and global-mean temperature (perhaps cross-correlation analysis would be useful); the importance of not extrapolating results from one timescale to another timescale; or the lack of trends in ENSO. That way, the current paper contributes to the peer-reviewed literature while also doing a service by highlighting the problems with McLean et al.

I think I tried to emphasize that this should be a teaching moment. Even more important given the time lapse.

>
> 3. In general, the current paper is sloppy and needs tightening. I don't think the lead author needs 10 pages of text to make the main points.
>
>

So over to you to generate the next draft.

Thanks
Kevin

Kevin E. Trenberth e-mail: [4]trenbert@ucar.edu
Climate Analysis Section, [5]www.cgd.ucar.edu/cas/trenbert.html
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Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

References

1. <mailto:jgr-atmospheres@agu.org>
2. mailto:tamino_9@hotmail.com
3. <mailto:twistor9@gmail.com>
4. <mailto:trenbert@ucar.edu>
5. <http://www.cgd.ucar.edu/cas/trenbert.html>

From: Gavin Schmidt <gschmidt@giss.nasa.gov>

To: Tim Osborn <t.osborn@uea.ac.uk>

Subject: latest

Date: 28 Sep 2009 17:59:04 -0400

Hi Tim, I know Keith is out of commission for a while (give him my regards when you see him), but someone needs to at least give some context to the latest McIntyre meme.

<http://planetgore.nationalreview.com/post/?q=Y2Q5ZGExZTc3ZTlmMTA5OTdhOGRjNzdINmU4N2M4ZTg=>

None of us at RC have any real idea what was done or why and so we are singularly unable to sensibly counter the flood of nonsense. Of course, most of the reaction is hugely overblown and mixed up but it would be helpful to have some kind of counterpoint to the main thrust. If you can point to someone else that could be helpful, please do!

Thanks

Gavin

From: Grant Foster <tamino_9@hotmail.com>
To: <trenbert@ucar.edu>, Mike Mann <mann@meteo.psu.edu>, <p.jones@uea.ac.uk>, "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, <b.mullan@niwa.co.nz>, Gavin Schmidt <gschmidt@giss.nasa.gov>, <j.renwick@niwa.co.nz>
Subject: FW: 2009JD012960 (Editor - Steve Ghan):Decision Letter
Date: Mon, 28 Sep 2009 19:08:21 +0000

> From: jgr-atmospheres@agu.org
> Date: Mon, 28 Sep 2009 15:54:05 +0000
> To: tamino_9@hotmail.com
> Subject: 2009JD012960 (Editor - Steve Ghan):Decision Letter
> CC: twistor9@gmail.com
>
> Manuscript Number: 2009JD012960
> Manuscript Title: Comment on "Influence of the Southern Oscillation on tropospheric
temperature" by J. D. McLean, C. R. de Freitas, and R. M. Carter

> Dear Dr. Foster:

> 3 reviews of your above-referenced manuscript are attached below. Reviewer 3 is concerned
with the tone on the writing; while I appreciate the value of "taking the high road", I do
not object to emphatic statements that conclusions are incorrect. Strong language is needed
sometimes when errors must be corrected. Please carefully consider the Reviewers'
recommendations for revisions, make the necessary changes, and respond to me with a
point-by-point response of how you have addressed each concern. In your cover letter,
please include a statement confirming that all authors listed on the manuscript concur with
submission in its revised form.

> The due date for your revised paper is October 28, 2009. If you will be unable to submit
a revised manuscript by this time, please notify my office and arrange for an extension
(maximum two weeks). If we do not hear from you by the revision due date, your manuscript
will be considered as withdrawn.

> When you are ready to submit your revision, please use the link below.

> *The link below will begin the resubmission of your manuscript, please Do Not click on
the link until you are ready to upload your revised files. Any partial submission that sits
for 3 days without files will be deleted.

> <<http://jgr-atmospheres-submit.agu.org/cgi-bin/main.plex?el=A7Bc6EiyL2A2FTof1I3A9OLsgIoKEcG4DW4K5nQ0wZ>>

> (NOTE: The link above automatically submits your login name and password. If you wish to
share this link with co-authors or colleagues, please be aware that they will have access
to your entire account for this journal.)

> **In order to save time upon acceptance, it would be helpful if files in the correct
format are uploaded at revision. Article and table files may be in Word, WordPerfect or
LaTeX and figure files should be separately uploaded as .eps, .tif or pdf files. If you
have color figures, please go to the site below to select a color option. Please put your
color option in the cover letter.

> http://www.agu.org/pubs/e_publishing/AGU-publication-fees.pdf

> Please see the AGU web site for more information about preparing text and art files
(<http://www.agu.org/pubs/inf4aus.shtml>). If you have any questions, please contact the
editor–s assistant.

> Sincerely,

> Steve Ghan
> Editor, Journal of Geophysical Research - Atmospheres

> -----Important JGR-Atmospheres Information-----

>
> The real mystery here, of course, is how the McLean et al. paper ever made it into JGR. How that happened, I have no idea. I can't see it ever getting published through J Climate. The analyses in McLean et al. are among the worst I have seen in the climate literature. The paper is also a poorly disguised attack on the integrity of the climate community, and I guess that is why Foster et al. have taken the energy to contradict its findings.
>
> So the current paper (Foster et al.) should certainly be accepted. Someone needs to address the science in the McLean et al paper in the peer-reviewed literature. But the current paper could be - and should be - done better. That's why I am suggesting major changes before the paper is accepted. All of my suggestions have to do more with the tone and framing of the current paper, rather than its content.
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> - In the abstract: Do you really need all of these adjectives?... 'greatly overstates'; 'severely overestimates'; 'faulty analysis'; 'extremely high'.
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Hotmail® has ever-growing storage! Dont worry about storage limits. [1]Check it out.

References

1. http://windowslive.com/Tutorial/Hotmail/Storage?ocid=TXT_TAGLM_WL_HM_Tutorial_Storage_062009

From: Phil Jones <p.jones@uea.ac.uk>

To: Tim Osborn <t.osborn@uea.ac.uk>, Michael Mann <mann@meteo.psu.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>

Subject: Re: attacks against Keith

Date: Tue Sep 29 09:17:12 2009

Mike, Gavin,

As Tim has said Keith is making a good recovery and hopes to be back in soon, gradually during October and hopefully full time from November.

I talked to him by phone yesterday and sent him and Tom Melvin the threads on CA. As you're fully aware, trying to figure out what McIntyre has done is going to be difficult. It would be so much easier if they followed normal procedure and wrote up a comment and submitted it to a journal. I looked through the threads yesterday trying to make sense of what he's done. My suspicion is that he's brought in other tree ring series from more distant sites, some of which may not even be larch. There are two chronologies that have been used - one called the Polar Urals and one called Yamal. PU is a Schweingruber site with density as well as ring width. The PU reconstruction is therefore not a chronology, but a regression based reconstruction from both MXD and TRW. Yamal is just a ring width series (with lots of sub-fossil material, so much older) from an area some distance (at least 500km) north of PU. It was developed by Hantemirov and Shiyatov and was poorly standardized - corridor method. I also don't think McIntyre understands the RCS method even though he claims to have a program. The ends and the age structure of the samples are crucial in all this, but I think he just throws series in.

I totally agree that these attacks (for want of a better word) are getting worse. Comments on the thread are snide in the extreme, with many saying they see no need to submit the results to a journal. They have proved Keith has manipulated the data, so job done.

Hadn't thought of Senate debates. I'd put this down to the build up to Copenhagen, which is sort of the same.

[1]<http://wattsupwiththat.com/2009/09/24/a-look-at-the-thompson-et-al-paper-hi-tech-wiggle-matching-and-removal-of-natural-variables/>

is a complete reworking of Dave Thompson's paper which is in press in J. Climate (online). Looked at this, but they have made some wrong assumptions, but someone has put a lot of work into it.

[2]<http://wattsupwiththat.com/2009/09/24/ooops-dutch-meteorological-institute-caught-in-weather-station-siting-failure-moved-station-and-told-nobody/>

This one is a complete red herring - nothing wrong with De Bilt measurements. This is what it is about according to someone at KNMI

The issue you refer to is causing a lot of noise in the Netherlands (even MP's asking questions to the minister). It seems this is not at all about the observational series (nothing strange is going on), but more related to the "Law on KNMI" and the division of tasks between commercial providers and KNMI to be discussed by parliament soon.

Cheers

Phil

At 08:46 29/09/2009, Tim Osborn wrote:

Hi Mike and Gavin,

thanks for your emails re McIntyre, Yamal and Keith.

I'll pass on your best wishes for his recovery when I next speak to Keith. He's been off almost 4 months now and won't be back for at least another month (barring a couple of lectures that he's keen to do in October as part of a gradual return). Hopefully he'll be properly back in November.

Regarding Yamal, I'm afraid I know very little about the whole thing -- other than that I am 100% confident that "The tree ring data was hand-picked to get the desired result" is complete crap. Having one's integrity questioned like this must make your blood boil (as I'm sure you know, with both of you having been the target of numerous such attacks). Though it would be nice to shield Keith from this during his recovery, I think Keith will already have heard about this because he had recently been asked to look at CA in relation to the Kaufman threads (Keith was a co-author on that and Darrell had asked Keith to help with a response to the criticisms).

Apart from Keith, I think Tom Melvin here is the only person who could shed light on the McIntyre criticisms of Yamal. But he can be a rather loose cannon and shouldn't be directly contacted about this (also he wasn't involved in the Yamal chronology being discussed, though he has been involved in a regional reconstruction that we've recently been working towards that uses these -- and more -- data).

Perhaps Phil and I should talk with Tom and also see if Keith is already considering a response.

Off to lecture for a couple of hours now...

Cheers

Tim

Dr Timothy J Osborn, Academic Fellow

Climatic Research Unit

School of Environmental Sciences

University of East Anglia

Norwich NR4 7TJ, UK

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phone: +44 1603 592089

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web: [3]<http://www.cru.uea.ac.uk/~timo/>
sunclock: [4]<http://www.cru.uea.ac.uk/~timo/sunclock.htm>

Prof. Phil Jones

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School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. <http://wattsupwiththat.com/2009/09/24/a-look-at-the-thompson-et-al-paper-hi-tech-wiggle-matching-and-removal-of-natural-variables/>
2. <http://wattsupwiththat.com/2009/09/24/ooops-dutch-meteorological-institute-caught-in-weather-station-siting-failure-moved-station-and-told-nobody/>
3. <http://www.cru.uea.ac.uk/~timo/>
4. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@ucar.edu>, Grant Foster <tamino_9@hotmail.com>
Subject: Re: FW: 2009JD012960 (Editor - Steve Ghan):Decision Letter
Date: Tue Sep 29 10:00:55 2009
Cc: Mike Mann <mann@meteo.psu.edu>, "J. Salinger" <j.salinger@auckland.ac.nz>, James Annan <jdannan@jamstec.go.jp>, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, j.renwick@niwa.co.nz

Grant, Kevin,

Agree on the responses. It does just seem a case of removing a number of the adjectives. It is important to keep the moral high ground in this, if just to show how a comment on a paper should be written and submitted to the same journal that had the poor paper in the first instance. Might be worth reiterating this if any of us get called when the comment does come out. There does seem a trend these days to slam a paper on blogs with no attempt to submit a comment to a journal.

Agree on the running mean/derivative issue - the spectral diagram is better.

Scatter plots aren't that useful unless. They's might help with the (a) parts, but it's obvious from the time series plots and the r-squareds are so different!

Finally - there was this comment via Jim S from Neville Nicholls. I vaguely recall Angell and Korshover papers

from that time. The attached refers to some of them - also found Newell and Weare. This isn't the first, but it might be worth adding. Attached this one from Science as well.

Neville Nicholls wrote:

Hi Jim.

I hop things are going well with you.

Thanks for being part of this robust response to the latest silliness. You have certainly gathered an illustrious group of co-authors.

I am disappointed that you didnt cite the very early (1970s) work by Newell and Weare, and by Angell and Korshover. I think you should squeeze these in, to demonstrate that the climate community did not have to wait for McLean et al to understand the influence of ENSO on global temperatures. In fact, our colleagues in the 1970s understood this, and demonstrated it much more scientifically than does the McLean et al paper.

Cheers,

Neville

Cheers

Phil

At 21:45 28/09/2009, Kevin Trenberth wrote:

Hi all

About time. Incidentally i gave a copy to Mike McPhaden and discussed it with him last week when we were together at the OceanObs'09 conference. Mike is President of AGU.

Basically this is an acceptance with a couple of suggestions for extras, and some suggestions for toning down the rhetoric. I had already tried that a bit. My reaction is that the main thing is to expedite this. That means no extras unless it really makes sense. And removal of a few unnecessary words like "absolutely".

In the abstract, we have a number of such adjectives that could be removed: I agree with Rev 3 in this.

"greatly overstates" could be just "overstates" as it is reinforced better later.

"severely overestimates" could be just "overestimates"

"faulty analysis" maybe "flawed analysis"?

"extremely high" maybe "very high" or "unduly high"

I would leave last sentence alone though as the main comment.

A few more comments embedded below.

Grant Foster wrote:

> From: [1]jgr-atmospheres@agu.org
> Date: Mon, 28 Sep 2009 15:54:05 +0000
> To: [2]tamino_9@hotmail.com
> Subject: 2009JD012960 (Editor - Steve Ghan):Decision Letter
> CC: [3]twistor9@gmail.com
>
> Manuscript Number: 2009JD012960
> Manuscript Title: Comment on "Influence of the Southern Oscillation on tropospheric temperature" by J. D. McLean, C. R. de Freitas, and R. M. Carter

>
> -----
>

> Reviewer Comments

>
> Reviewer #1 (Comments):

>
> This paper does an excellent job of showing the errors in the analytical methods used by McLean et al. and why their conclusions about the influence of ENSO on global air temperature is incorrect. I have only a couple of suggestions to help clarify their analysis of the methods. First, a little more explanation of the comment about the time derivative reduced to an additive constant would help. Second, in the analysis of the artificial time series I think it would be interesting to show the results of both steps of filtering (running mean and derivative) as separate time series. This would help the reader understand why the filtering creates false correlations. The only other suggestion is to find a better adjective than "faulty" in the abstract to characterize the analysis.

>

It is not so easy to see the result from the derivative owing to the phase shift. The spectrum actually does a better job. I would address this comment in this way and change "faulty".

>
> Reviewer #2 (Comments):

>
> I think this comment on McLean et al can be published more or less as is.

>
> I have two comments

>
> First, in the abstract (page 3, line 15), I'm not sure that "inflating" is quite the right verb - the paper itself does not make the point that the filter constructed by McLean et al inflates power in the 2-6 year window. Perhaps "isolating" would be a better verb.

Yes it should not be in abstract if not in text. Need to point out that the response function in Fig. 1 is greater than unity and does "inflate". So adjust the text.

>
> Secondly, I think the points that are being made with Figures 4 and 5 could be strengthened by adding to the right of each plot of a pair of time series, a scatter plot of the pairs of values available at each time. Such a scatter plot would help to clearly illustrate the absence (upper panels) or presence (lower panels) of correlation between red and black values.

I don't think this helps. There is nothing to be gained from a scatter plot that a correlation or regression value does not summarize.

- >
- >
- > Reviewer #3 (Comments):
- >
- > Accept pending major changes (mainly in style not scientific comment)
- >
- > The real mystery here, of course, is how the McLean et al. paper ever made it into JGR. How that happened, I have no idea. I can't see it ever getting published through J Climate. The analyses in McLean et al. are among the worst I have seen in the climate literature. The paper is also a poorly guised attack on the integrity of the climate community, and I guess that is why Foster et al. have taken the energy to contradict its findings.
- >
- > So the current paper (Foster et al.) should certainly be accepted. Someone needs to address the science in the McLean et al paper in the peer-reviewed literature. But the current paper could be - and should be - done better. That's why I am suggesting major changes before the paper is accepted. All of my suggestions have to do more with the tone and framing of the current paper, rather than its content.
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- > - In the abstract: Do you really need all of these adjectives?... 'greatly overstates'; 'severely overestimates'; 'faulty analysis'; 'extremely high'.

Agree, see above

- > - In the introduction... 'Unfortunately, their conclusions are seriously in error...' strikes me as overly subjective. Better to say: 'We will demonstrate that their conclusions are strongly dependent on' or something like that...

Don't go that far. Could drop "seriously" but they are "in error"

- > - Page X-6: 'tell us absolutely nothing'. Surely it's enough to state 'tell us nothing'.

agree

- > - Page X-9: 'it is misleading...' That's a strong word. It may be true. But I think we should rise above such accusations.

misleading is OK. I did a search (not sure I have latest) and found "grossly misleading" and the "grossly" could be removed.

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- > Anyway, I'm sure the lead author gets my point. I think the current paper will have a much greater impact (and can claim the high road) if it is rewritten in a more objective manner.
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I think I tried to emphasize that this should be a teaching moment. Even more important given the time lapse.

>
> 3. In general, the current paper is sloppy and needs tightening. I don't think the lead author needs 10 pages of text to make the main points.

>
>

So over to you to generate the next draft.

Thanks
Kevin

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References

1. <mailto:jgr-atmospheres@agu.org>
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3. <mailto:twistor9@gmail.com>
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5. <http://www.cgd.ucar.edu/cas/trenbert.html>

From: Michael Mann <mann@meteo.psu.edu>

To: Tim Osborn <t.osborn@uea.ac.uk>

Subject: Re: attacks against Keith

Date: Tue, 29 Sep 2009 10:45:16 -0400

Cc: "gschmidt@giss.nasa.gov" <gschmidt@giss.nasa.gov>, Phil Jones <p.jones@uea.ac.uk>

<x-flowed>

Thanks for the clarification Tim, doesn't change the fact the the attack was inappropriate and unfair of course, but perhaps not as despicable as at first might appear,

M

--

Michael E. Mann

Professor

Director, Earth System Science Center (ESSC)

<http://www.meteo.psu.edu/~mann/Mann/index.html>

http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

On Sep 29, 2009, at 9:50 AM, Tim Osborn <t.osborn@uea.ac.uk> wrote:

> At 14:30 29/09/2009, Gavin Schmidt wrote:

>> The fact is that they launched an assault on Keith knowing full

>> well he isn't in a position to respond. This is despicable.

>

> Gavin,

>

> be careful here, I think it more likely that McIntye only learned of

> Keith's absence after he started posting about Yamal and the real

> reason for the timing of all this is that we made the Yamal tree-

> core measurements available about 2-3 weeks ago (in fact Keith had

> thought they had been made available before he fell ill, and only

> realised in early September that they weren't -- and asked for that

> to be rectified).

>

> Cheers

>

> Tim

>

>

>

>

>
> Dr Timothy J Osborn, Academic Fellow
> Climatic Research Unit
> School of Environmental Sciences
> University of East Anglia
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>
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> web: <http://www.cru.uea.ac.uk/~timo/>
> sunclock: <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
>
>
</x-flowed>

From: Michael Mann <mann@meteo.psu.edu>
To: Andrew Revkin <anrevk@nytimes.com>
Subject: Re: mcintyre's latest....
Date: Tue, 29 Sep 2009 17:11:03 -0400

p.s. Tim Osborn ([1]t.osborn@uea.ac.uk) is probably the best person to contact for further details, in Keith's absence,

mike

On Sep 29, 2009, at 5:08 PM, Michael Mann wrote:

Hi Andy,

I'm fairly certain Keith is out of contact right now recovering from an operation, and is not in a position to respond to these attacks. However, the preliminary information I have from others familiar with these data is that the attacks are bogus.

It is unclear that this particular series was used in any of our reconstructions (some of the underlying chronologies may be the same, but I'm fairly certain the versions of these data we have used are based on a different composite and standardization method), let alone any of the dozen other reconstructions of Northern Hemisphere mean temperature shown in the most recent IPCC report, which come to the conclusion that recent warming is anomalous in a long-term context.

So, even if there were a problem w/ these data, it wouldn't matter as far as the key conclusions regarding past warmth are concerned. But I don't think there is any problem with these data, rather it appears that McIntyre has greatly distorted the actual information content of these data. It will take folks a few days to get to the bottom of this, in Keith's absence.

if McIntyre had a legitimate point, he would submit a comment to the journal in question. of course, the last time he tried that (w/ our '98 article in Nature), his comment was rejected. For all of the noise and bluster about the Steig et al Antarctic warming, its now nearing a year and nothing has been submitted. So more likely he won't submit for peer-reviewed scrutiny, or if it does get his criticism "published" it will be in the discredited contrarian home journal "Energy and Environment". I'm sure you are aware that McIntyre and his ilk realize they no longer need to get their crap published in legitimate journals. All they have to do is put it up on their blog, and the contrarian noise machine kicks into gear, pretty soon Druge, Rush Limbaugh, Glenn Beck and their ilk (in this case, The Telegraph were already on it this morning) are parroting the claims. And based on what? some guy w/ no credentials, dubious connections with the energy industry, and who hasn't submitted his claims to the scrutiny of peer review.

Fortunately, the prestige press doesn't fall for this sort of stuff, right?

mike

I'm sure you're aware that you will dozens of bogus, manufactured distortions of the science in the weeks leading up to the vote on cap & trade in the U.S. senate. This is no

On Sep 29, 2009, at 4:30 PM, Andrew Revkin wrote:

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Has he communicated directly to you on this and/or is there any indication he's seeking journal publication for his deconstruct?

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620 Eighth Ave., NY, NY 10018
Tel: 212-556-7326 Mob: 914-441-5556
Fax: 509-357-0965
[2]<http://www.nytimes.com/revkin>

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From: Michael Mann <mann@meteo.psu.edu>
To: Andrew Revkin <anrevk@nytimes.com>
Subject: Re: mcintyre's latest....
Date: Tue, 29 Sep 2009 17:27:25 -0400
Cc: t.osborn@uea.ac.uk

Hi Andy,

Yep, what was written below is all me, but it was purely on background, please don't quote anything I said or attribute to me w/out checking specifically--thanks.

Re, your point at the end--you've taken the words out of my mouth. Skepticism is essential for the functioning of science. It yields an erratic path towards eventual truth. But legitimate scientific skepticism is exercised through formal scientific circles, in particular the peer review process. A necessary though not in general sufficient condition for taking a scientific criticism seriously is that it has passed through the legitimate scientific peer review process. those such as McIntyre who operate almost entirely outside of this system are not to be trusted.

mike

On Sep 29, 2009, at 5:19 PM, Andrew Revkin wrote:

thanks heaps.

tom crowley has sent me a direct challenge to mcintyre to start contributing to the reviewed lit or shut up. i'm going to post that soon.

just want to be sure that what is spliced below is from YOU ... a little unclear . ?

I'm copying this to Tim, in hopes that he can shed light on the specific data assertions made over at climateaudit.org.....

I'm going to blog on this as it relates to the value of the peer review process and not on the merits of the mcintyre et al attacks.

peer review, for all its imperfections, is where the herky-jerky process of knowledge building happens, would you agree?

p.s. Tim Osborn ([1]t.osborn@uea.ac.uk) is probably the best person to contact for further details, in Keith's absence,

mike

On Sep 29, 2009, at 5:08 PM, Michael Mann wrote:

Hi Andy,

I'm fairly certain Keith is out of contact right now recovering from an operation, and is not in a position to respond to these attacks. However, the preliminary information I have from others familiar with these data is that the attacks are bogus.

It is unclear that this particular series was used in any of our reconstructions (some of the underlying chronologies may be the same, but I'm fairly certain the versions of these data we have used are based on a different composite and standardization method), let alone any of the dozen other reconstructions of Northern Hemisphere mean temperature shown in the most recent IPCC report, which come to the conclusion that recent warming is anomalous in a long-term context.

So, even if there were a problem w/ these data, it wouldn't matter as far as the key conclusions regarding past warmth are concerned. But I don't think there is any problem with these data, rather it appears that McIntyre has greatly distorted the actual information content of these data. It will take folks a few days to get to the bottom of this, in Keith's absence.

if McIntyre had a legitimate point, he would submit a comment to the journal in question. of course, the last time he tried that (w/ our '98 article in Nature), his comment was rejected. For all of the noise and bluster about the Steig et al Antarctic warming, its now nearing a year and nothing has been submitted. So more likely he won't submit for peer-reviewed scrutiny, or if it does get his criticism "published" it will be in the discredited contrarian home journal "Energy and Environment". I'm sure you are aware that McIntyre and his ilk realize they no longer need to get their crap published in legitimate journals. All they have to do is put it up on their blog, and the contrarian noise machine kicks into gear, pretty soon Druge, Rush Limbaugh, Glenn Beck and their ilk (in this case, The Telegraph were already on it this morning) are parroting the claims. And based on what? some guy w/ no credentials, dubious connections with the energy industry, and who hasn't submitted his claims to the scrutiny of peer review.

Fortunately, the prestige press doesn't fall for this sort of stuff, right?

mike

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From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: attacks against Keith
Date: Wed, 30 Sep 2009 11:06:20 -0400
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>, Tim Osborn <t.osborn@uea.ac.uk>

Hi Phil,

lets not get into the topic of hate mail. I promise you I could fill your inbox w/ a very long list of vitriolic attacks, diatribes, and threats I've received.

Its part of the attack of the corporate-funded attack machine, i.e. its a direct and highly intended outcome of a highly orchestrated, heavily-funded corporate attack campaign. We saw it over the summer w/ the health insurance industry trying to defeat Obama's health plan, we'll see it now as the U.S. Senate moves on to focus on the cap & trade bill that passed congress this summer. It isn't coincidental that the original McIntyre and McKitrick E&E paper w/ press release came out the day before the U.S. senate was considering the McCain Lieberman climate bill in '05.

we're doing the best we can to expose this. I hope our Realclimate post goes some ways to exposing the campaign and pre-emptively deal w/ the continued onslaught we can expect over the next month.

thanks for alerting us to that detail of Kaufman et al which I'd overlooked. We'd already asked Darrell if he could compute a Yamal-less version of his series, but as you point out he's really already done this! And Osborn and Briffa '06 is also immune to this issue, as it eliminated any combination of up to 3 of the proxies and showed the result was essentially the same (fair to say this Tim?).

Also, is it fair to say that this particular version of Keith's Yamal series was not what we used in Mann and Jones '03 (we reference Briffa et al '01)?

thanks for the help! We're hoping to have something up tomorrow at the latest, and any updates at your end will be extremely helpful to the case,

mike

On Sep 30, 2009, at 10:30 AM, Phil Jones wrote:

Mike, Gavin,

The short note may not say much. As you're aware Kaufman et al have a plot without trees - their plots shows trees, lakes and ice separately.

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We're getting a handful of nasty emails coming and requests for comments on other blog sites. One email has gone to the University Registrar because of the language used. Keith had one that said he was responsible for millions of deaths! Even one reading far too much into his off ill message.

Even though I've had loads of FOIs and nasty emails, a few in the last 2 days have been the worst yet. I'm realizing more what those working on animal experiments must have gone through.

Cheers

Phil

At 14:56 30/09/2009, Michael Mann wrote:

great--thanks Tim, sounds like we have a plan. in our post, which we'll target for tomorrow as well, we'll simply link to whatever CRU puts up and re-iterate the sentiment of the temporary short response (i.e. that there was no cherry-picking, a careful and defensible selection procedure was used) and we'll mostly focus on the broader issues, i.e. that any impact of this one series in the vast array of paleoclimate reconstructions (and the importance of the paleoclimate reconstructions themselves) has been over-stated, why these sorts of attacks are not legitimate science, etc.

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On Sep 30, 2009, at 9:51 AM, Gavin Schmidt wrote:

of course. we're preparing a 'bigger picture' response and will link directly to CRU and maybe quote from it directly.

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Keith may post them on the CRU website, but presumably they could be linked to from a RealClimate page or, if Keith agrees, be reproduced on RealClimate?

Cheers

Tim

At 14:16 30/09/2009, Michael Mann wrote:

Hi Tim,

Just checking if there are any further developments here, i.e. some more info from either Tom or Keith.

Gavin and I feel we need to do something on RealClimate on this quickly, probably by later today.

thanks in advance for any help you can offer,

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On Sep 29, 2009, at 3:46 AM, Tim Osborn wrote:

Hi Mike and Gavin,

thanks for your emails re McIntyre, Yamal and Keith.

I'll pass on your best wishes for his recovery when I next speak to Keith. He's been off almost 4 months now and won't be back for at least another month (barring a couple of lectures that he's keen to do in October as part of a gradual return). Hopefully he'll be properly back in November.

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suncllock: <[7] <http://www.cru.uea.ac.uk/~timo/suncllock.htm> >[8]

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Prof. Phil Jones

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To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: attacks against Keith
Date: Wed Sep 30 17:12:54 2009
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>, Tim Osborn <t.osborn@uea.ac.uk>

Mike,
I realized you'd have many more bad emails!
As for MJ2003 what we used was an average of Fennoscan, Yamal and Taymir (as one of the series).
Briffa et al (2001) was just referred to in that as a ref to RCS. The paper also talks about N Eurasia, so the sites get a mention.

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--

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"Dire Predictions" book site:
[20]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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[24]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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References

Visible links

1. <mailto:gschmidt@giss.nasa.gov>
2. <http://www.giss.nasa.gov/~gavin>
3. <mailto:t.osborn@uea.ac.uk>
4. <mailto:t.osborn@uea.ac.uk>
5. <http://www.cru.uea.ac.uk/~timo/>
6. <http://www.cru.uea.ac.uk/~timo/>
7. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
8. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
9. <mailto:mann@psu.edu>

10. <mailto:mann@psu.edu>
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15. <mailto:t.osborn@uea.ac.uk>
16. <http://www.cru.uea.ac.uk/~timo/>
17. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
18. <mailto:mann@psu.edu>
19. <http://www.meteo.psu.edu/~mann/Mann/index.html>
20. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
21. <mailto:p.jones@uea.ac.uk>
22. <mailto:mann@psu.edu>
23. <http://www.meteo.psu.edu/~mann/Mann/index.html>
24. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

25. <http://www.cru.uea.ac.uk/~timo/>
26. <http://www.cru.uea.ac.uk/~timo/sunclock.htm>
27. <http://www.meteo.psu.edu/~mann/Mann/index.html>
28. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>, Phil Jones <p.jones@uea.ac.uk>
Subject: Re: attacks against Keith
Date: Wed Sep 30 17:15:29 2009
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

At 16:06 30/09/2009, Michael Mann wrote:

And Osborn and Briffa '06 is also immune to this issue, as it eliminated any combination of up to 3 of the proxies and showed the result was essentially the same (fair to say this Tim?).

Mike,

yes, you're right: figs S4-S6 in our supplementary information do indeed show results leaving out individual, groups of two, and groups of three proxies, respectively. It's attached.

I wouldn't say we were immune to the issue -- results are similar for these leave 1, 2 or 3 out cases, but they certainly are not as strong as the case with all 14 proxies. Certainly in figure S6, there are some cases with 3 omitted (i.e. some sets of 11) where modern results are comparable with intermittent periods between 800 and 1100.

Plus there is the additional uncertainty, discussed on the final page of the supplementary information, associated with linking the proxy records to real temperatures (remember we have no formal calibration, we're just counting proxies -- I'm still amazed that Science agreed to publish something where the main analysis only involves counting from 1 to 14! :-)).

But this is fine, since the IPCC AR4 and other assessments are not saying the evidence is 100% conclusive (or even 90% conclusive) but just "likely" that modern is warmer than MWP. So, yes, it should be possible to find some subsets of data where MWP and Modern are comparable and similarly for some seasons and regions. And as you've pointed out before, if any season/region is comparable (or even has MWP>Modern) then it will probably be the northern high latitudes in summer time (I think you published on this, suggesting that combination of orbital forcing, land-use change and sulphate aerosols could cause this for that season/region, is that right?).

So, this Yamal thing doesn't damage Osborn & Briffa (2006), but important to note that O&B (2006) and others support the "likely" statement rather than being conclusive.

Cheers

Tim

From: Michael Mann <mann@meteo.psu.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Malcom Hughes <mhughes@ltrr.arizona.edu>
Subject: draft of Yamal RealClimate post
Date: Wed, 30 Sep 2009 22:42:39 -0400
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Dear Tim, Phil, Malcolm, I've enclosed a draft of our article, which we'd like to go online w/ tomorrow (attached as a word file--unfortunately this distorts the post relative to the way it will actually look on the website, but it was the easiest way to send w/ hyperlinks and figures intact). Please let us know if there is anything that you think is either erroneous, unclear, etc. in the piece. we'll link to whatever CRU puts up tomorrow as soon as a link is available. thanks in advance for your help, mike -- Michael E. Mann Professor Director, Earth System Science Center (ESSC) Department of Meteorology Phone: (814) 863-4075 503 Walker Building FAX: (814) 865-3663 The Pennsylvania State University email: mann@psu.edu University Park, PA 16802-5013 website: <http://www.meteo.psu.edu/~mann/Mann/index.html> "Dire Predictions" book site: http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html Dear Tim, Phil, Malcolm,

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thanks in advance for your help,

mike

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Attachment Converted: "c:\eudora\attach\HeyYa.doc"

References

Visible links

1. <mailto:mann@psu.edu>
2. <http://www.meteo.psu.edu/~mann/Mann/index.html>
3. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

4. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Phil Jones <p.jones@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>, Tim Osborn <t.osborn@uea.ac.uk>, Malcom Hughes <mhughes@ltrr.arizona.edu>
Subject: Re: draft of Yamal RealClimate post
Date: Thu Oct 1 10:56:44 2009
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Mike, Gavin,

Here are a few important mods to your piece. Don't mention Keith has been off ill. Remove the bit about provenance and about access to more data. We'll go into the latter in the longer bit next week.

We'll send the piece we're putting up later - or give you the link.

Rest of your piece is great - especially the bit on how science should be done. Keith has also picked up in the bit we'll post that McIntyre has put in the caveats but lets others say the outrageous things in comments or on other blogs.

Cheers

Phil

At 03:42 01/10/2009, Michael Mann wrote:

Dear Tim, Phil, Malcolm,

I've enclosed a draft of our article, which we'd like to go online w/ tomorrow (attached as a word file--unfortunately this distorts the post relative to the way it will actually look on the website, but it was the easiest way to send w/ hyperlinks and figures intact).

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mike

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The Pennsylvania State University email: mann@psu.edu

University Park, PA 16802-5013

website: [1]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[2]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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thanks in advance for your help,

mike

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Michael E. Mann

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References

1. <http://www.meteo.psu.edu/~mann/Mann/index.html>
2. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
3. <mailto:mann@psu.edu>
4. <http://www.meteo.psu.edu/~mann/Mann/index.html>
5. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

From: Keith Briffa <k.briffa@uea.ac.uk>
To: t.m.melvin@uea.ac.uk
Subject: Fwd: Re: URGENT
Date: Fri, 02 Oct 2009 12:43:21 +0100

<x-flowed>

>Date: Fri, 02 Oct 2009 13:43:50 +0200
>From: Anders Moberg <anders.moberg@natgeo.su.se>
>User-Agent: Thunderbird 2.0.0.16 (X11/20080720)
>To: Keith Briffa <k.briffa@uea.ac.uk>
>Subject: Re: URGENT
>X-Virus-Scanned: by amavisd-new at smtp.su.se
>X-Spam-Status: No, hits=-2.202 tagged_above=-99 required=7 tests=[AWL=0.110,
> BAYES_00=-2.312]
>X-Spam-Level:
>X-Canit-CHI2: 0.00
>X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f023)
>X-Spam-Score: 0.00 () [Tag at 5.00] SPF(none,0)
>X-CanItPRO-Stream: UEA:f023 (inherits from
>UEA:10_Tag_Only,UEA:default,base:default)
>X-Canit-Stats-ID: 32039918 - 2186b9c79b71
>X-Antispam-Training-Forget:
><https://canit.uea.ac.uk/b.php?i=32039918&m=2186b9c79b71&c=f>
>X-Antispam-Training-Nonspam:
><https://canit.uea.ac.uk/b.php?i=32039918&m=2186b9c79b71&c=n>
>X-Antispam-Training-Spam:
><https://canit.uea.ac.uk/b.php?i=32039918&m=2186b9c79b71&c=s>
>X-Scanned-By: CanIt (www . roaringpenguin . com) on 139.222.131.184

>
>Yes, of course! It is attached here. As you might perhaps imagine,
>the little corrigendum in Nature 2006 which led me to produce this
>data file was a consequence of requests from McIntyre to get the data.

>
>Actually, Phil has already got the data from me (but he might have
>forgotten it). I don't have any raw data, just the data sent here.

>
>cheers,
>Anders

>
>
>

>Keith Briffa skrev:

>>Anders

>>now I must ask a favour - could you send me the data for the long

>>Russian chronology that was produced by Sidorova et al.

>>At the very least I need the numbers representing their final

>>chronology straight away - I need to include them in a reworking of

>>a recent science paper (rather than trying to digitise them from a

>>scan). I would also like the raw data but understand if you are not able

>>to release these .

>>thanks

>>Keith

>>

>>14:56 01/10/2009, you wrote:

>>>Dear Keith,

>>>

>>>Thanks for the support letter. It is perfect for our case!

>>>

>>>Anders

>>

>>--

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</x-flowed>

Attachment Converted: "c:\eudora\attach\indigirka_moberg05.dat"

From: Malcolm Hughes <mhughes@lrr.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: IN STRICTEST CONFIDENCE
Date: Fri, 02 Oct 2009 13:46:11 -0700
Cc: t.m.melvin@uea.ac.uk

<x-flowed>

Keith - is there a time in the next few days when you could stand talking briefly about this on the phone? I think the fog about the status of the Indigirka/Yakutua data could be cleared really quickly that way. Once again, I'm really sorry it has been necessary to bother you with this. Cheers, Malcolm

Keith Briffa wrote:

> Malcolm
> honestly just a cross thread between Tom and I. I had been asked by
> Darrell whether we should use the Sidorova chronology - because of
> hassle by you know who - so asked Tom a while ago to ask you. I did
> not see your answer - sorry if you cc'd me in as I have not been
> checking emails. I fully accept and would NEVER go behind your back to
> ask for the data. I understood that the chronology was published and
> so thought to compare our RCS version with it if we could produce it
> in time . We are being accused of not using that chronology in the
> Science paper- so then asked Anders for it. I am happy to send Darrell
> the single chronology if that is what Anders has sent. I am having to
> start thinking about the Yamal crap and then this Darrell stuff
> suddenly arises. I just wanted him to consider including the Polar
> Urals reconstruction and the Sidorova series in his analysis before
> publishing a correction in Science- apparently the selection criterion
> for inclusion of series was anything published north of 60 degrees and
> longer than 1000 years. I could do without all this now - don't really
> understand what Climate Audit are getting so hysterical about but feel
> that I can not ignore it this time - but don't feel up to getting
> involved. I fully admit to being out of the loop as regards all this
> and having trouble getting back to it.
>
> To restate - this was a confusion. I fully accept your point (as you
> know I would). Sorry if you thought I was doing anything without your
> knowledge - TO BE HONEST ALSO - I actually was not really aware that
> the data you were producing and that used by Sidorova were one and the
> same. Best wishes hopefully all ok
> I assume that we are allowed to use the chronolgy as published - are

> we? I have not contacted Sidorova. Can you cc answer to Tom as I have
> no email at present. (this coming from someone elses computer)

> Keith

>
>
> At 16:50 02/10/2009, you wrote:

>> Dear Keith - I do hope your recovery continues apace, in spite of the
>> recent nonsense. I really have had no intention to bother you with
>> work stuff, and had strongly encouraged Mike and Gavin to contact
>> Tim and/or Tom putting a response on RICI. So, I'm really reticent to
>> raise something else, but must.
>> What's going on? 21st September I got an email from Tom M that
>> contained the following para, among other more general discussion:
>> "Keith has been complained at by Climate Audit for cherry picking and
>> not using your long Indigirka River data set. Not used because we did
>> not have the data. Please, could we have the data? We will make
>> proper acknowledgement/coauthorship if we use the data."
>> I replied pretty much straight away thus: "Hi Tom - please find the
>> Esper article in question attached. The so-called Indigirka River
>> data set is not yet available because it has not been published. I
>> am currently working on that with Russian colleagues, and was indeed
>> in Switzerland the week before last to work with one of them on
>> specifically this. All being well, there will be an accepted
>> manuscript before next summer, and at that point I will make the data
>> freely available. Once we get to that point, I'll let you know, of
>> course. Cheers, Malcolm" .
>> So far, no direct response to this email from Tom.
>> This morning I get an email from Anders Moberg, telling me that you
>> had asked him for the "Indigirka data". I've waited a couple of hours
>> before writing this email so as to try to be constructive. To be sure
>> that you understand what that dataset is and is not, please read the
>> attached 2006 Moberg corrigendum.
>> Once again, the actual data are unpublished, in spite of having been
>> discussed in the Russian literature by Siderova et al. A large
>> proportion of the raw data are not yet in the public domain, and so
>> you would not be able to critically evaluate the chronology as a
>> possible climate proxy. Why can that not be said - adequate metadata
>> not available, please see Moberg corrigendum? By the way, a 600-year
>> reconstruction is available (Hughes et al 1999, also attached), and
>> all those raw data are at the ITRDB.
>> As you know, it is my intention to friendly, cooperative and open,
>> but I'm determined to get some scientific value from all the years of
>> work I've invested in the Yakutia work, and in cooperation with

>> Russia in general. Releasing these data now would be too much.

>> Cheers, Malcolm

>>

>>

>> --

>> Malcolm K Hughes

>> Regents' Professor

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>> The University of Arizona

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>> <http://www.ltrr.arizona.edu/people/8>

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</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: Malcolm Hughes <mhughes@ltrr.arizona.edu>
Subject: Re: [Fwd: IN STRICTEST CONFIDENCE]
Date: Fri Oct 2 17:06:06 2009

Malcolm,

Keith should be reading emails. Probably been a misunderstanding. I've only glanced at the nonsense but didn't see anything related to Indigirka. I see they are now getting at the Taimyr site, so Keith/Tom having to look at that one too.

They have some extra data from Vlad which CA won't have, so whatever they say there will get more emails about keeping hold of more data. All the issues seem to relate to canopy closed sites like Fritz would have likely sampled and more open sites. They are trying to contact the Russians to get site pictures or anything else.

Keith is on +44 1953 851013 if you fancy calling at the weekend.

They get at us for keeping hold of data, but they have no intention of publishing in the peer-review literature!

Cheers

Phil

At 16:56 02/10/2009, you wrote:

Phil - just in case Keith is not opening email and Tom is helping him out by taking initiative, here's an email I just sent Keith. Unfortunately, I really had to respond to this. I hope all is going well for you. Cheers, Malcolm

--

Malcolm K Hughes
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Tucson, AZ 85721
USA
tel: +1-520-621-6470
fax: +1-520-621-8229
mhughes@ltrr.arizona.edu
[1]<http://www.ltrr.arizona.edu/people/8>
Message-ID: <4AC6212D.7070401@ltrr.arizona.edu>
Date: Fri, 02 Oct 2009 08:50:05 -0700
From: Malcolm Hughes <mhughes@ltrr.arizona.edu>
User-Agent: Thunderbird 2.0.0.23 (Windows/20090812)
MIME-Version: 1.0
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: IN STRICTEST CONFIDENCE

Content-Type: multipart/mixed;
boundary="-----090305040400060007010009"

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"Keith has been complained at by Climate Audit for cherry picking and not using your long Indigirka River data set. Not used because we did not have the data. Please, could we have the data? We will make proper acknowledgement/coauthorship if we use the data." I replied pretty much straight away thus: "Hi Tom - please find the Esper article in question attached. The so-called Indigirka River data set is not yet available because it has not been published. I am currently working on that with Russian colleagues, and was indeed in Switzerland the week before last to work with one of them on specifically this. All being well, there will be an accepted manuscript before next summer, and at that point I will make the data freely available. Once we get to that point, I'll let you know, of course. Cheers, Malcolm" .

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References

1. <http://www.ltrr.arizona.edu/people/8>
2. <http://www.ltrr.arizona.edu/people/8>

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Malcolm Hughes <mhughes@ltrr.arizona.edu>
Subject: Re: IN STRICTEST CONFIDENCE
Date: Fri, 02 Oct 2009 17:28:22 +0100
Cc: t.m.melvin@uea.ac.uk

<x-flowed>

Malcolm

honestly just a cross thread between Tom and I. I had been asked by Darrell whether we should use the Sidorova chronology - because of hassle by you know who - so asked Tom a while ago to ask you. I did not see your answer - sorry if you cc'd me in as I have not been checking emails. I fully accept and would NEVER go behind your back to ask for the data. I understood that the chronology was published and so thought to compare our RCS version with it if we could produce it in time . We are being accused of not using that chronology in the Science paper- so then asked Anders for it. I am happy to send Darrell the single chronology if that is what Anders has sent. I am having to start thinking about the Yamal crap and then this Darrell stuff suddenly arises. I just wanted him to consider including the Polar Urals reconstruction and the Sidorova series in his analysis before publishing a correction in Science- apparently the selection criterion for inclusion of series was anything published north of 60 degrees and longer than 1000 years. I could do without all this now - don't really understand what Climate Audit are getting so hysterical about but feel that I can not ignore it this time - but don't feel up to getting involved. I fully admit to being out of the loop as regards all this and having trouble getting back to it.

To restate - this was a confusion. I fully accept your point (as you know I would). Sorry if you thought I was doing anything without your knowledge - TO BE HONEST ALSO - I actually was not really aware that the data you were producing and that used by Sidorova were one and the same. Best wishes hopefully all ok

I assume that we are allowed to use the chronolgy as published - are we? I have not contacted Sidorova. Can you cc answer to Tom as I have no email at present. (this coming from someone elses computer)

Keith

At 16:50 02/10/2009, you wrote:

>Dear Keith - I do hope your recovery continues apace, in spite of

>the recent nonsense. I really have had no intention to bother you
>with work stuff, and had strongly encouraged Mike and Gavin to
>contact Tim and/or Tom putting a response on R1Cl. So, I'm really
>reticent to raise something else, but must.
>What's going on? 21st September I got an email from Tom M that
>contained the following para, among other more general discussion:
>"Keith has been complained at by Climate Audit for cherry picking
>and not using your long Indigirka River data set. Not used because
>we did not have the data. Please, could we have the data? We will
>make proper acknowledgement/coauthorship if we use the data."
>I replied pretty much straight away thus: "Hi Tom - please find the
>Esper article in question attached. The so-called Indigirka River
>data set is not yet available because it has not been published. I
>am currently working on that with Russian colleagues, and was indeed
>in Switzerland the week before last to work with one of them on
>specifically this. All being well, there will be an accepted
>manuscript before next summer, and at that point I will make the
>data freely available. Once we get to that point, I'll let you know,
>of course. Cheers, Malcolm" .
>So far, no direct response to this email from Tom.
>This morning I get an email from Anders Moberg, telling me that you
>had asked him for the "Indigirka data". I've waited a couple of
>hours before writing this email so as to try to be constructive. To
>be sure that you understand what that dataset is and is not,
>please read the attached 2006 Moberg corrigendum.
>Once again, the actual data are unpublished, in spite of having been
>discussed in the Russian literature by Siderova et al. A large
>proportion of the raw data are not yet in the public domain, and so
>you would not be able to critically evaluate the chronology as a
>possible climate proxy. Why can that not be said - adequate metadata
>not available, please see Moberg corrigendum? By the way, a 600-year
>reconstruction is available (Hughes et al 1999, also attached), and
>all those raw data are at the ITRDB.
>As you know, it is my intention to friendly, cooperative and open,
>but I'm determined to get some scientific value from all the years
>of work I've invested in the Yakutia work, and in cooperation with
>Russia in general. Releasing these data now would be too much.
>Cheers, Malcolm
>
>
>--
>Malcolm K Hughes
>Regents' Professor

>Laboratory of Tree-Ring Research
>The University of Arizona
>105 W Stadium
>Tucson, AZ 85721
>USA
>
>tel: +1-520-621-6470
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>
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>
><http://www.ltrr.arizona.edu/people/8>
>
>
>
>
>

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Professor Keith Briffa,
Climatic Research Unit
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Phone: +44-1603-593909
Fax: +44-1603-507784

<http://www.cru.uea.ac.uk/cru/people/briffa/>

</x-flowed>

From: Michael Mann <mann@meteo.psu.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: thanks and one question
Date: Mon, 5 Oct 2009 08:46:42 -0400
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

thanks Phil,

I wondered where this completely false claim was coming with. Are these people really so clueless that they don't even understand that I have nothing to do with this whatsoever. Pretty much tells you everything you need to do.

I never acknowledge emails from people I don't know, about topics that are in any way sensitive. this is a perfect example of something that goes right to the trash bin,

mike

On Oct 5, 2009, at 5:55 AM, Phil Jones wrote:

Gavin, Mike,

Thanks for this!

I assume you are both aware of this prat - Neil Craig, see below. Keith won't be responding.

Checking facts doesn't seem important these days. As CA threads aren't publications this is difficult for non scientists.

I am going further over one email I got at the weekend - see also below. Typical of Sonia - although she now seems to only be an emeritus reader!

Cheers

Phil

Return-path: <[1]CrgN143@aol.com>

From: [2]CrgN143@aol.com

Full-name: CrgN143

Message-ID: <[3]d03.64b01875.37f87aa4@aol.com>

Date: Sat, 3 Oct 2009 06:00:04 EDT

Subject: Tree rings - accusation that you were solely responsible.

To: [4]k.briffa@uea.ac.uk

MIME-Version: 1.0

Content-Type: multipart/alternative; boundary="-----1254564004"

X-Mailer: 9.0 SE for Windows sub 5045

Professor Briffa,

I have written a couple of blogs on the current report by Steve McIntyre that the data used by Mann to "prove" the hockey Stick was fabricated. This & the

following day's

[5]<http://a-place-to-stand.blogspot.com/2009/10/global-warming-proven-deliberate-fraud.html>

As a result I have received this email from somebody I am not acquainted with throwing the entire blame on you. This seems improbable to me & possibly an alarmist damage limitation exercise. If you wish to comment I would be happy for you to do so.

"Please note: Steve McIntyre's post concerns work by climate scientist Keith Briffa and not Michael Mann. You will probably wish to correct your post.

Cheers

Avisame"

I have posted this as an update with my reply:

"My understanding is that while Briffa did the tree ring measurement, Mann, in his paper, chose to choose 12 atypical tree rings out of at least 34 to fabricate the global warming trend. My assumption is that Mann is responsible for fabrications in his own paper & that this is a damage limitation exercise. I am open to correction on this & indeed have emailed Mr Briffa to see. "

Neil Craig

You may be interested in my political blog

[6]<http://a-place-to-stand.blogspot.com/>

We received this through our enquiries desk. I assume that you are aware of this person, including those copied on the message.

If we are to respond, it would be to indicate that there are multiple sources of supporting evidence and that we continue to place our confidence in the international scientific assessment process. This confidence has proven to be well placed.

Roger

From: Sonja A Boehmer-Christiansen <[7]Sonja.B-C@hull.ac.uk>
Date: 2 October 2009 18:09:39 GMT+01:00
To: Stephanie Ferguson <[8]stephanie.ferguson@ukcip.org.uk>
Cc: "Peiser, Benny" <[9]B.J.Peiser@ljmu.ac.uk>, Patrick David Henderson <[10]pdhenderson18@googlemail.com>, Christopher Monckton <[11]monckton@mail.com>
Subject: RE: Please take note of potetially serious allegations of scientific 'fraud' by CRU and Met Office

Dear Stephanie

I expect that a great deal of UKCIP work is based on the data provided by CRU (as does the work of the IPCC and of course UK climate policy). Some of this, very fundamentally, would now seem to be open to scientific challenge, and may even face future legal enquiries. It may be in the interest of UKCIP to inform itself in good time and become a little more 'uncertain' about its policy advice.

Perhaps you can comment on the following and pass the allegations made on to the relevant people.

It is beyond my expertise to assess the claims made, but they would fit into my perception of the whole 'man-made global warming' cum energy policy debate. I know several of the people involved personally and have no reason to doubt their sincerity and honour as scientists, though I am also aware of their highly critical (of IPCC science) policy positions.

I could also let you have statements by Steve McIntyre and Ross McKittrick. Ross McKittrick currently teaches at Westminster Business School and who is fully informed about the relevant issues. He recently addressed a meeting of about 50 people in London.

Best wishes

Sonja B-C

Dr.Sonja A.Boehmer-Christiansen
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Fax: (0044) 1482 466340

TWO copied pieces follow, both relate to CRU and UK climate policy

a. THE MET OFFICE AND CRU'S YAMAL SCANDAL: EXPLAIN OR RESIGN

" Jennifer Marohasy <[13]jennifermarohasy@jennifermarohasy.com>

Leading UK Climate Scientists Must Explain or Resign, Jennifer Marohasy
< <[14] <http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists> ->
[15][http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists -
must-explain-or-resign/](http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists-must-explain-or-resign/)>

Prof. Phil Jones

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University of East Anglia

Norwich Email [16]p.jones@uea.ac.uk

NR4 7TJ

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Michael E. Mann

Professor

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Department of Meteorology Phone: (814) 863-4075

503 Walker Building FAX: (814) 865-3663

The Pennsylvania State University email: [17]mann@psu.edu

University Park, PA 16802-5013

website: [18]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[19]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

References

Visible links

1. <mailto:CrgN143@aol.com>
2. <mailto:CrgN143@aol.com>
3. <mailto:d03.64b01875.37f87aa4@aol.com>
4. <mailto:k.briffa@uea.ac.uk>
5. <http://a-place-to-stand.blogspot.com/2009/10/global-warming-proven-deliberate-fraud.html>
6. <http://a-place-to-stand.blogspot.com/>
7. <mailto:Sonja.B-C@hull.ac.uk>
8. <mailto:stephanie.ferguson@ukcip.org.uk>
9. <mailto:B.J.Peiser@ljmu.ac.uk>
10. <mailto:pdhenderson18@gmail.com>
11. <mailto:monckton@mail.com>
12. <http://www.multi-science.co.uk/>
13. <mailto:jennifermarohasy@jennifermarohasy.com>
14. <http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists>
15. <http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists>
16. <mailto:p.jones@uea.ac.uk>
17. <mailto:mann@psu.edu>
18. <http://www.meteo.psu.edu/~mann/Mann/index.html>

19. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

20. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: [geo] Re: CCNet: A Scientific Scandal Unfolds
Date: Mon Oct 5 10:03:02 2009

Tom,

Thanks for trying to clear the air with a few people. Keith is still working on a response. Having to contact the Russians to get some more site details takes time.

Several things in all this are ludicrous as you point out. Yamal is one site and isn't in most of the millennial reconstructions. It isn't in MBH, Crowley, Moberg etc. Also picking trees for a temperature response is not done either.

The other odd thing is that they seem to think that you can reconstruct the last millennium from a few proxies, yet you can't do this from a few instrumental series for the last 150 years! Instrumental data are perfect proxies, after all.

[1]http://www.americanthinker.com/2009/10/un_climate_reports_they_lie.html

This one is wrong as well. IPCC (1995) didn't use that silly curve that Chris Folland or Geoff Jenkins put together.

Cheers

Phil

At 02:59 05/10/2009, you wrote:

David,

This is entirely off the record, and I do not want this shared with anyone. I hope you will respect this. This issue is not my problem, and I await further developments.

However, Keith Briffa is in the Climatic Research Unit (CRU), and I was Director of CRU for many years so I am quite familiar with Keith and with his work. I have also done a lots of hands on tree ring work, both in the field and in developing and applying computer programs for climate reconstruction from tree rings. On the other hand, I have not been involved in any of this work since I left CRU in 1993 to move to NCAR. But I do think I can speak with some modicum of authority.

You say, re dendroclimatologists, "they rely on recent temperature data by which to *select* recent tree data" (my emphasis). I don't know where you get this idea, but I can assure you that it is entirely wrong.

Further, I do not know the basis for your claim that "Dendrochonology is a bankrupt approach". It is one of the few proxy data areas where rigorous multivariate statistical tools are used and where reconstructions are carefully tested on independent data.

Finally, the fact that scientists (in any field) do not willingly share their hard-earned primary data implies that they have something to hide has no logical basis.

Tom.

+++++

David Schnare wrote:

Tom:

Briffa has already made a preliminary response and he failed to explain his selection procedure. Further, he refused to give up the data for several years, and was forced to do so only when he submitted to a journal that demanded data archiving and actually enforced the practice.

More significantly, Briffa's analysis is irrelevant. Dendrochronology is a bankrupt approach. They admit that they cannot distinguish causal elements contributing to tree ring size. Further, they rely on recent temperature data by which to select recent tree data (excluding other data) and then turn around and claim that the tree ring data explains the recent temperature data. If you can give a principled and reasoned defense of Briffa (see the discussion on Watt's website) then go for it. I'd be fascinated, as would a rather large number of others.

None of this, of course, detracts for the need to do research on geoengineering. David Schnare

On Sun, Oct 4, 2009 at 8:50 PM, Tom Wigley <wigley@ucar.edu <mailto:wigley@ucar.edu>> wrote:

Dear all,

I think it would be wise to let Briffa respond to these accusations before compounding them with unwarranted extrapolations.

With regard to the Hockey Stick, it is highly unlikely that a single site can be very important. M&M have made similar accusations in the past and they have been shown, in the peer-reviewed literature, to be ill-founded.

Two recent papers you should read are those in the attached Word document (first pages only).

Tom.

+++++

Eugene I. Gordon wrote:

David:

I concede all of your points but add one other thought. It is my grandchildren I worry about and I suspect their grand children will find it exceedingly warm because sunspots will return and carbon abatement is only a game; It wont happen significantly in their lifetime AND IT WONT BE ENOUGH IN ANY CASE. HENCE _WE WILL NEED A GEOENGINEERING SOLUTION_ COME WHAT MAY!
-gene

/Eugene I. Gordon/

/(908) 233 4677/

/euggordon@comcast.net/ <[2]http://euggordon@comcast.net/>

/[3]www.germgardlighting.com/ <[4]http://www.germgardlighting.com/>

From: geoengineering@googlegroups.com

<[5]mailto:geoengineering@googlegroups.com>

[mailto:geoengineering@googlegroups.com

<[6]mailto:geoengineering@googlegroups.com>] *On Behalf Of *David

Schnare

Sent: Sunday, October 04, 2009 10:49 AM

Cc: Alan White; geoengineering@googlegroups.com

<[7]mailto:geoengineering@googlegroups.com>

Subject: [geo] Re: CCNet: A Scientific Scandal Unfolds

Gene:

I've been following this issue closely and this is what I take away from it:

1) Tree ring-based temperature reconstructions are fraught with so much uncertainty, they have no value whatever. It is impossible to tease out the relative contributions of rainfall, nutrients, temperature and access to sunlight. Indeed a single tree can, and apparently has, skewed the entire 20th century temperature reconstruction.

2) The IPCC peer review process is fundamentally flawed if a lead author is able to both disregard and ignore criticisms of his own work, where that work is the critical core of the chapter. It not only destroys the credibility of the core assumptions and data, it destroys the credibility of the larger work - in this case, the IPCC summary report and the underlying technical reports. It also destroys the utility and credibility of the modeling efforts that use assumptions on the relationship of CO2 to temperature that are based on Britta's work, which is, of course, the majority of such analyses.

As Corcoran points out, "the IPCC has depended on 1) computer models, 2) data collection, 3) long-range temperature forecasting and 4) communication. None of these efforts are sitting on firm ground."

Nonetheless, and even if the UNEP thinks it appropriate to rely on Wikipedia as their scientific source of choice, greenhouse gases may (at an ever diminishing probability) cause a significant increase in global temperature. Thus, research, including field trials, on the leading geoengineering techniques are appropriate as a backstop in case our children find out that the current alarmism is justified.

David Schnare

On Sun, Oct 4, 2009 at 8:35 AM, Eugene I. Gordon

<eggordon@comcast.net <[8]mailto:eggordon@comcast.net>

<[9]mailto:eggordon@comcast.net <mailto:eggordon@comcast.net>>>

wrote:

Alan:

Thanks for the extensive and detailed e-mail. This is terrible but not surprising. Obviously I do not know what gives with these guys. However, I have my own suspicions and hypothesis. I dont think they are scientifically inadequate or stupid. I think they are dishonest and members of a club that has much to gain by practicing and perpetuating global warming scare tactics. That is not to say that global warming is not occurring to some extent since it would be even without CO2 emissions. The CO2 emissions only accelerate the warming and there are other factors controlling climate. As a result, the entire process may

be going slower than the powers that be would like. Hence, (I postulate) the global warming contingent has substantial motivation to be dishonest or seriously biased, and to be loyal to their equally dishonest club members. Among the motivations are increased and continued grant funding, university advancement, job advancement, profits and payoffs from carbon control advocates such as Gore, being in the limelight, and other motivating factors I am too inexperienced to identify.

Alan, this is nothing new. You and I experienced similar behavior from some of our colleagues down the hall, the Bell Labs research people, in the good old days. Humans are hardly perfect creations. I am never surprised at what they can do. I am perpetually grateful for those who are honest and fair and thankfully there is a goodly share of those.

-gene

From: Alan White [mailto:adwhite99@comcast.net
<[10]mailto:adwhite99@comcast.net> <[11]mailto:adwhite99@comcast.net>
<[12]mailto:adwhite99@comcast.net>>]
Sent: Saturday, October 03, 2009 8:28 PM
To: Gene Gordon
Subject: Fw: CCNet: A Scientific Scandal Unfolds

more of the same. what gives with these guys?

----- Original Message -----

From: Peiser, Benny <[13]mailto:B.J.Peiser@ljmu.ac.uk>
<[14]mailto:B.J.Peiser@ljmu.ac.uk>>
To: CCNetMedia <[15]mailto:CCNetMedia@livjm.ac.uk>
<[16]mailto:CCNetMedia@livjm.ac.uk>>
Sent: Friday, October 02, 2009 6:36 AM
Subject: CCNet: A Scientific Scandal Unfolds

CCNet 153/2009 - 2 October 2009 -- Audiatur et altera pars
CRU'S HIDDEN DATA AND THE IPCC: A SCIENTIFIC SCANDAL UNFOLDS

A scientific scandal is casting a shadow over a number of recent peer-reviewed climate papers. The scandal has serious implications for public trust in science. The IPCC's mission is to reflect the science, not create it. As the IPCC states, its duty is "assessing the scientific, technical and socioeconomic information relevant for the understanding of the risk of human-induced climate change. It does not carry out new research nor does it monitor climate-related data." But as IPCC lead author, Briffa was a key contributor in shaping the assessment. When the IPCC was alerted to peer-reviewed research that refuted the idea, it declined to include it. This leads to the more

general, and more serious issue: what happens when peer-review fails -

as it did here?

--Andrew Orłowski, The Register, 29 September 2009

Over the next nine years, at least one paper per year appeared in prominent journals using Briffa's Yamal composite to support a hockey

stick-like result. The IPCC relied on these studies to defend the Hockey

Stick view, and since it had appointed Briffa himself to be the IPCC

Lead Author for this topic, there was no chance it would

question the

Yamal data. Despite the fact that these papers appeared in top journals

like Nature and Science, none of the journal reviewers or editors ever

required Briffa to release his Yamal data. Steve McIntyre's repeated requests for them to uphold their own data disclosure rules were ignored.

--Ross McKittrick, Financial Post, 1 October 2009

The official United Nation's global warming agency, the Intergovernmental Panel on Climate Change, is a four-legged stool that

is fast losing its legs. To carry the message of man-made global warming theory to the world, the IPCC has depended on 1) computer models, 2) data collection, 3) long-range temperature forecasting and 4)

communication. None of these efforts are sitting on firm ground.

--Terence Corcoran, National Post, 1 October 2009

Media reaction to the Yamal story has been rather limited so far. I'm

not sure whether this is because people are trying to digest what it means or whether it's "too hot to handle". None of the global warming

supporters in the mainstream media have gone near it. The reaction of

the Guardian - to delete any mention of the affair from their comment

threads - has been extraordinary.

--Bishop Hill, 1 October 2009

Britain will have to stop building airports, switch to electric cars and

shut down coal-fired power stations as part of a 'planned recession' to

avoid dangerous climate change. A new report from the Tyndall Centre for

Climate Change Research says the only way to avoid going beyond the dangerous tipping point is to double the target to 70 per cent by 2020.

This would mean reducing the size of the economy through a "planned recession".

--Louise Gray, The Daily Telegraph, 30 September 2009

Tokyo governor Shintaro Ishihara warned on Wednesday the 2016

Olympics

could be the last Games, with global warming an immediate threat to mankind. "It could be that the 2016 Games are the last Olympics in the history of mankind," Ishihara told reporters at a Tokyo 2016 press event ahead of the vote. "Global warming is getting worse. We have to come up with measures without which Olympic Games could not last long. "Scientists have said we have passed the point of no return," said Ishihara.

--Karolos Grohmann, Reuters, 30 September 2009

(1) TREEMOMETERS: A NEW SCIENTIFIC SCANDAL

Andrew Orlowski, The Register, 29 September 2009

(2) ANALYSIS: DEFECTS IN KEY CLIMATE DATA ARE UNCOVERED

Ross McKittrick, Financial Post, 1 October 2009

(3) OPINION: CLIMATE DATA BUSTER

Terence Corcoran, National Post, 1 October 2009

(4) OPINION: COOLING DOWN THE CASSANDRAS

George F. Will, The Washington Post, 1 October 2009

(5) U.S. THROWS SPANNER INTO CLIMATE TALKS

Times of India, 2 October 2009

(6) CAP AND TRADE MAY SINK OPPOSITION LEADER DOWN UNDER

Lenore Taylor, The Australian, 2 October 2009

(7) THE MET OFFICE AND CRU'S YAMAL SCANDAL: EXPLAIN OR RESIGN

Jennifer Marohasy <jennifermarohasy@jennifermarohasy.com

<[17]mailto:jennifermarohasy@jennifermarohasy.com>

<[18]mailto:jennifermarohasy@jennifermarohasy.com>

<[19]mailto:jennifermarohasy@jennifermarohasy.com>>>

(8) COOLING?

Rodney Chilton <maberrd@hotmail.com

<[20]mailto:maberrd@hotmail.com> <[21]mailto:maberrd@hotmail.com>

<[22]mailto:maberrd@hotmail.com>>>

(9) RESOURCES DEPLETION WORRIES

Steven Zoraster <szoraster@szoraster.com

<[23]mailto:szoraster@szoraster.com> <[24]mailto:szoraster@szoraster.com>

<[25]mailto:szoraster@szoraster.com>>>

(10) COPENHAGEN SUMMIT: DO SCIENCE AND ECONOMICS SUPPORT GOVERNMENT ACTION ON GLOBAL WARMING?

Peter Kidson <peterdkidson@googlemail.com

<[26]mailto:peterdkidson@googlemail.com>

<[27]mailto:peterdkidson@googlemail.com>

<[28]mailto:peterdkidson@googlemail.com>>]

(11) A DEATH SPIRAL FOR CLIMATE ALARMISM?

Robert Bradley <rbradley@iertx.org

<[29]mailto:rbradley@iertx.org> <[30]mailto:rbradley@iertx.org>

<[31]mailto:rbradley@iertx.org>>>

(12) AND FINALLY: 'PLANNED RECESSION' COULD AVOID CATASTROPHIC CLIMATE CHANGE

Louise Gray, The Daily Telegraph, 30 September 2009

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(1) TREEMOMETERS: A NEW SCIENTIFIC SCANDAL

The Register, 29 September 2009

<[32]http://www.theregister.co.uk/2009/09/29/yamal_scandal/>

By Andrew Orlowski

A scientific scandal is casting a shadow over a number of recent peer-reviewed climate papers.

At least eight papers purporting to reconstruct the historical temperature record times may need to be revisited, with significant implications for contemporary climate studies, the basis of the IPCC's

assessments. A number of these involve senior climatologists at the British climate research centre CRU at the University East Anglia. In

every case, peer review failed to pick up the errors.

At issue is the use of tree rings as a temperature proxy, or dendrochronology. Using statistical techniques, researchers take the ring data to create a "reconstruction" of historical temperature anomalies. But trees are a highly controversial indicator of temperature, since the rings principally record Co2, and also record humidity, rainfall, nutrient intake and other local factors.

Picking a temperature signal out of all this noise is problematic, and a

dendrochronology can differ significantly from instrumented data. In dendro jargon, this disparity is called "divergence". The process of creating a raw data set also involves a selective use of samples - a choice open to a scientist's biases.

Yet none of this has stopped paleoclimatologists from making bold claims using tree ring data.

In particular, since 2000, a large number of peer-reviewed climate papers have incorporated data from trees at the Yamal Peninsula in Siberia. This dataset gained favour, curiously superseding a newer and

larger data set from nearby. The older Yamal trees indicated pronounced

and dramatic uptick in temperatures.

How could this be? Scientists have ensured much of the measurement data

used in the reconstructions remains a secret - failing to fulfill procedures to archive the raw data. Without the raw data, other scientists could not reproduce the results. The most prestigious peer

reviewed journals, including Nature and Science, were reluctant to demand the data from contributors. Until now, that is.

At the insistence of editors of the Royal Society's Philosophical Transactions B the data has leaked into the open - and Yamal's mystery

is no more.

>From this we know that the Yamal data set uses just 12 trees from a

larger set to produce its dramatic recent trend. Yet many more were cored, and a larger data set (of 34) from the vicinity shows no dramatic

recent warming, and warmer temperatures in the middle ages.

In all there are 252 cores in the CRU Yamal data set, of which

ten were
alive 1990. All 12 cores selected show strong growth since the
mid-19th
century. The implication is clear: the dozen were cherry-picked.
Controversy has been raging since 1995, when an explosive paper
by Keith
Briffa at the Climate Research Unit at the University of East Anglia
asserted that that the medieval warm period was actually really
cold,
and recent warming is unusually warm. Both archaeology and the
historical accounts, Briffa was declaring, were bunk. Briffa
relied on
just three cores from Siberia to demonstrate this.
Three years later Nature published a paper by Mann, Bradley and
Hughes
based on temperature reconstructions which showed something similar:
warmer now, cooler then. With Briffa and Mann as chapter editors
of the
UN's Intergovernmental Panel on Climate Change (IPCC), this
distinctive
pattern became emblematic - the "Logo of Global Warming".
IPCC's Assessment Report from 2001 - with the error bars in grey
emphasised
Hokey hockey sticks
Mann too used dendrochronology to chill temperatures, and rebuffed
attempts to publish his measurement data. Initially he said he had
forgotten where he put it, then declined to disclosed it. (Some of
Mann's data was eventually discovered, by accident, on his ftp
server in
a directory entitled 'BACKTO_1400-CENSORED'.)
Tree data was secondary in importance to Mann's statistical
technique,
which would produce a dramatic modern upturn in temperatures - which
became nicknamed the "Hockey Stick" - even using red noise.
Similarly, all the papers that used the Yamal data have the same
point
to make. All suggest recent dramatic warming. Having scored a
global hit
with a combination of flawed statistics and dubious
dendrochronology,
the acts repeated the formula.
"Late 20th century warmth is unprecedented for at least roughly
the past
two millennia for the Northern Hemisphere," wrote the two authors of
Global Surface Temperatures over the Past Two Millennia published in
Geophysical Research Letters in 2003 - Mann, and Phil Jones of CRU.
For example, Briffa's 2008 paper concludes that: "The extent of
recent
widespread warming across northwest Eurasia, with respect to 100- to
200-year trends, is unprecedented in the last 2000 years."
The same authors in 2004:
It continues to this day. A study purporting to show the Arctic was
warmer now than for 2,000 years received front-page attention last

month. Led by Northern Arizona University professor Darrell S Kaufman,

and including dendro veteran Mann, this too relied heavily on Yamal, and produced the signature shape.

Now here's Yamal.

And when Yamal is plotted against the wider range of cores, the implications of the choice is striking:

A comparison of Yamal RCS chronologies. red - as archived with 12 picked

cores; black - including Schweingruber's Khadyta River, Yamal (russ035w)

archive and excluding 12 picked cores. Both smoothed with 21-year gaussian smooth. y-axis is in dimensionless chronology units centered on

1 (as are subsequent graphs (but represent age-adjusted ring width).

"The majority of these trees (like the Graybill bristlecones) have a prolonged growth pulse (for whatever reason) starting in the 19th century," wrote Canadian mathematician Steve McIntyre on his blog on Sunday. "When a one-size fits all age profile is applied to these particular tries, the relatively vigorous growth becomes monster growth

- 8 sigma anomalies in some of them."

McIntyre's determination to reproduce the reconstructions has resulted

in the Yamal data finally coming to light.

All the papers come from a small but closely knit of scientists who mutually support each other's work. All use Yamal data.

What went wrong?

The scandal has serious implications for public trust in science. The

IPCC's mission is to reflect the science, not create it.

As the panel states, its duty is "assessing the scientific, technical

and socioeconomic information relevant for the understanding of the risk

of human-induced climate change. It does not carry out new research nor

does it monitor climate-related data." But as lead author,

Briffa was a

key contributor in shaping (no pun intended) the assessment.

When the IPCC was alerted to peer-reviewed research that refuted the idea, it declined to include it. This leads to the more general, and more serious issue: what happens when peer-review fails - as it did here?

The scandal has only come to light because of the dogged persistence of

a Canadian mathematician who attempted to reproduce the results.

Steve

McIntyre has written dozens of letters requesting the data and methodology, and over 7,000 blog posts. Yet Yamal has remained elusive

for almost a decade. (r)

Bootnote

The Royal Society's motto from the enlightenment era is Nullius in verba. "On nobody's authority" or colloquially, "take nobody's word for it". In 2007, the Society's then president suggested this be changed to

"respect the facts".

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(2) ANALYSIS: DEFECTS IN KEY CLIMATE DATA ARE UNCOVERED

Financial Post, 1 October 2009

<[33]http://network.nationalpost.com/np/blogs/fpcomment/archive/2009/10/01/ross-mckitrick-defects-in-key-climate-data-are-uncovered.aspx>

By Ross McKittrick

Beginning in 2003, I worked with Stephen McIntyre to replicate a famous

result in paleoclimatology known as the Hockey Stick graph.

Developed by

a U.S. climatologist named Michael Mann, it was a statistical compilation of tree ring data supposedly proving that air temperatures

had been stable for 900 years, then soared off the charts in the 20th

century. Prior to the publication of the Hockey Stick, scientists had

held that the medieval-era was warmer than the present, making the scale

of 20th century global warming seem relatively unimportant. The dramatic

revision to this view occasioned by the Hockey Stick's publication made

it the poster child of the global warming movement. It was featured prominently in a 2001 report of the U.N. Intergovernmental Panel on Climate Change (IPCC), as well as government websites and countless review reports.

Steve and I showed that the mathematics behind the Mann Hockey Stick were badly flawed, such that its shape was determined by suspect bristlecone tree ring data. Controversies quickly piled up: Two expert

panels involving the U.S. National Academy of Sciences were asked to investigate, the U.S. Congress held a hearing, and the media followed

the story around the world.

The expert reports upheld all of our criticisms of the Mann Hockey Stick, both of the mathematics and of its reliance on flawed bristlecone

pine data. One of the panels, however, argued that while the Mann Hockey

Stick itself was flawed, a series of other studies published since 1998

had similar shapes, thus providing support for the view that the late

20th century is unusually warm. The IPCC also made this argument in its 2007 report. But the second expert panel, led by statistician Edward Wegman, pointed out that the other studies are not independent. They are written by the same small circle of authors, only the names are in different orders, and they reuse the same few data climate proxy series over and over. Most of the proxy data does not show anything unusual about the 20th century. But two data series have reappeared over and over that do have a hockey stick shape. One was the flawed bristlecone data that the National Academy of Sciences panel said should not be used, so the studies using it can be set aside. The second was a tree ring curve from the Yamal Peninsula in Siberia, compiled by UK scientist Keith Briffa. Briffa had published a paper in 1995 claiming that the medieval period actually contained the coldest year of the millennium. But this claim depended on just three tree ring records (called cores) from the Polar Urals. Later, a colleague of his named F. H. Schweingruber produced a much larger sample from the Polar Urals, but it told a very different story: The medieval era was actually quite warm and the late 20th century was unexceptional. Briffa and Schweingruber never published those data, instead they dropped the Polar Urals altogether from their climate reconstruction papers. In its place they used a new series that Briffa had calculated from tree ring data from the nearby Yamal Peninsula that had a pronounced Hockey Stick shape: relatively flat for 900 years then sharply rising in the 20th century. This Yamal series was a composite of an undisclosed number of individual tree cores. In order to check the steps involved in producing the composite, it would be necessary to have the individual tree ring measurements themselves. But Briffa didn't release his raw data. Over the next nine years, at least one paper per year appeared in prominent journals using Briffa's Yamal composite to support a hockey stick-like result. The IPCC relied on these studies to defend the Hockey Stick view, and since it had appointed Briffa himself to be the IPCC Lead Author for this topic, there was no chance it would

question the

Yamal data.

Despite the fact that these papers appeared in top journals like

Nature

and Science, none of the journal reviewers or editors ever required

Briffa to release his Yamal data. Steve McIntyre's repeated

requests for

them to uphold their own data disclosure rules were ignored.

Then in 2008 Briffa, Schweingruber and some colleagues published

a paper

using the Yamal series (again) in a journal called the Philosophical

Transactions of the Royal Society, which has very strict

data-sharing

rules. Steve sent in his customary request for the data, and

this time

an editor stepped up to the plate, ordering the authors to

release their

data. A short while ago the data appeared on the Internet. Steve

could

finally begin to unpack the Yamal composite.

It turns out that many of the samples were taken from dead

(partially

fossilized) trees and they have no particular trend. The sharp

uptrend

in the late 20th century came from cores of 10 living trees

alive as of

1990, and five living trees alive as of 1995. Based on scientific

standards, this is too small a sample on which to produce a

publication-grade proxy composite. The 18th and 19th century

portion of

the sample, for instance, contains at least 30 trees per year.

But that

portion doesn't show a warming spike. The only segment that does

is the

late 20th century, where the sample size collapses. Once again a

dramatic hockey stick shape turns out to depend on the least

reliable

portion of a dataset.

But an even more disquieting discovery soon came to light. Steve

searched a paleoclimate data archive to see if there were other tree

ring cores from at or near the Yamal site that could have been

used to

increase the sample size. He quickly found a large set of 34

up-to-date

core samples, taken from living trees in Yamal by none other than

Schweingruber himself! Had these been added to Briffa's small

group the

20th century would simply be flat. It would appear completely

unexceptional compared to the rest of the millennium.

Combining data from different samples would not have been an unusual

step. Briffa added data from another Schweingruber site to a

different

composite, from the Taimyr Peninsula. The additional data were

gathered

more than 400 km away from the primary site. And in that case the primary site had three or four times as many cores to begin with as the

Yamal site. Why did he not fill out the Yamal data with the readily-available data from his own coauthor? Why did Briffa seek out

additional data for the already well-represented Taimyr site and not for the inadequate Yamal site?

Thus the key ingredient in most of the studies that have been invoked to

support the Hockey Stick, namely the Briffa Yamal series, depends on the

influence of a woefully thin subsample of trees and the exclusion of readily-available data for the same area. Whatever is going on here, it is not science.

I have been probing the arguments for global warming for well over a decade. In collaboration with a lot of excellent coauthors I have consistently found that when the layers get peeled back, what lies at

the core is either flawed, misleading or simply non-existent. The surface temperature data is a contaminated mess with a significant warm

bias, and as I have detailed elsewhere the IPCC fabricated evidence in

its 2007 report to cover up the problem. Climate models are in gross disagreement with observations, and the discrepancy is growing with each

passing year. The often-hyped claim that the modern climate has departed

from natural variability depended on flawed statistical methods and low-quality data. The IPCC review process, of which I was a member last

time, is nothing at all like what the public has been told:

Conflicts of

interest are endemic, critical evidence is systematically ignored and

there are no effective checks and balances against bias or distortion.

I get exasperated with fellow academics, and others who ought to know

better, who pile on to the supposed global warming consensus without bothering to investigate any of the glaring scientific discrepancies and

procedural flaws. Over the coming few years, as the costs of global warming policies mount and the evidence of a crisis continues to collapse, perhaps it will become socially permissible for people to start thinking for themselves again. In the meantime I am grateful for

those few independent thinkers, like Steve McIntyre, who continue to ask

the right questions and insist on scientific standards of
openness and
transparency.

Ross McKittrick is a professor of environmental economics at the
University of Guelph, and coauthor of Taken By Storm: The Troubled
Science, Policy and Politics of Global Warming.

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EDITOR'S NOTE: More on the CRU's Yamal scandal and its impact, see:
<[34]<http://www.climateaudit.org/>>

<[35]<http://wattsupwiththat.com/2009/10/01/response-from-briffa-on-the-yamal-tree-ring-affair-plus-rebuttal/>>

<[36]<http://bishophill.squarespace.com/blog/2009/9/29/the-yamal-implosion.html>>

<[37]<http://bishophill.squarespace.com/blog/2009/10/1/yamal-the-debate-continues.html>>

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(3) OPINION: CLIMATE DATA BUSTER
National Post, 1 October 2009

<[38]<http://network.nationalpost.com/np/blogs/fullcomment/archive/2009/10/01/terence-corcoran-climate-data-buster.aspx>>

By Terence Corcoran

The official United Nation's global warming agency, the
Intergovernmental Panel on Climate Change, is a four-legged
stool that

is fast losing its legs. To carry the message of man-made global
warming theory to the world, the IPCC has depended on 1) computer
models, 2) data collection, 3) long-range temperature
forecasting and 4)

communication. None of these efforts are sitting on firm ground.

Over the past month, one of the IPCC's top climate scientists, Mojib
Latif, attempted to explain that even if global temperatures were to
cool over the next 10 to 20 years, that would not mean that man-made
global warming is no longer catastrophic. It was a tough case to
make,

and it is not clear Mr. Latif succeeded. In a presentation to a
world

climate conference in early September, Mr. Latif rambled
somewhat and

veered off into inscrutable language that is now embedded in a
million

blog posts attempting to prove one thing or another.

A sample: "It may well happen that you enter a decade, or maybe even
two, you know, when the temperature cools, all right, relative
to the

present level...And then, you know, I know what's going to
happen. You

know, I will get, you know, millions of phone calls, you know
-'What's

going on?' 'So is global warming disappearing, you know?' 'Have

you lied
on us, you know?' So, and, therefore, this is the reason why we
need to
address this decadal prediction issue."
The decadal prediction issue appears to be a combination of computer
model problems, the unpredictability of natural climate
variation, and
assorted uncertainties. Making all this clear to the average global
citizen will not be easy and climate scientists need to be able
to make
it clear, said Mr. Latif. "We have to ask the nasty questions
ourselves,
all right, or some other people will do it."
All this is still swirling around the global climate issue
today. But
now along comes another problem. Canadian data buster Steve
McIntyre has
spend most of the last three years deconstructing the IPCC's famous
claim that the last couple of decades of the 20th century were the
hottest in a thousand years. Using what was called The Hockey Stick
graph, the IPCC claimed to have the smoking gun that showed a
sharp run
up in global temperatures through to 1997. The validity of the
IPCC data
began to crumble when Mr. McIntyre and Ross McKittrick of Guelph
University found serious data problems that raised doubts about the
graph and the claims of record high temperatures.
As Ross McKittrick explains in his op-ed, Steve McIntyre has
uncovered
another data distortion that further undermines the original graphic
claim that the world has set temperature records in recent years. If
world temperatures may have been just as hot in the past as they
have
been recently, and if the the next two decades could be cooler
than they
have been recently, the theory of climate change becomes an even
tougher
case to make.
The IPCC is now on wobbly legs at all four corners. Its models are
inadequate and need overhaul, data integrity is at issue, the
climate is
not quite following the script, and the communication program
for the
whole campaign is a growing struggle.
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(4) OPINION: COOLING DOWN THE CASSANDRAS
The Washington Post, 1 October 2009

<[39]<http://www.washingtonpost.com/wp-dyn/content/article/2009/09/30/AR20090-93003569.html>>

By George F. Will

"Plateau in Temperatures Adds Difficulty to Task Of Reaching a

Solution"

--New York Times, Sept. 23

In this headline on a New York Times story about the difficulties confronting people alarmed about global warming, note the word "plateau." It dismisses the unpleasant -- to some people -- fact that

global warming is maddeningly (to the same people) slow to vindicate their apocalyptic warnings about it.

The "difficulty" -- the "intricate challenge," the Times says -- is "building momentum" for carbon reduction "when global temperatures have been relatively stable for a decade and may even drop in the next few

years." That was in the Times's first paragraph.

In the fifth paragraph, a "few years" became "the next decade or so,"

according to Mojib Latif, a German "prize-winning climate and ocean scientist" who campaigns constantly to promote policies combating global warming. Actually, Latif has said he anticipates "maybe even two" decades in which temperatures cool. But stay with the Times's "decade or so."

By asserting that the absence of significant warming since 1998 is a mere "plateau," not warming's apogee, the Times assures readers who are

alarmed about climate change that the paper knows the future and that

warming will continue: Do not despair, bad news will resume.

The Times reported that "scientists" -- all of them? -- say the 11 years

of temperature stability has "no bearing," none, on long-term warming.

Some scientists say "cool stretches are inevitable." Others say there

may be growth of Arctic sea ice, but the growth will be "temporary."

According to the Times, however, "scientists" say that "trying to communicate such scientific nuances to the public -- and to policymakers

-- can be frustrating."

The Times says "a short-term trend gives ammunition to skeptics of climate change." Actually, what makes skeptics skeptical is the accumulating evidence that theories predicting catastrophe from man-made

climate change are impervious to evidence. The theories are unfalsifiable, at least in the "short run." And the "short run" is defined as however many decades must pass until the evidence begins to

fit the hypotheses.

The Post recently reported the theory of a University of Virginia professor emeritus who thinks that, many millennia ago, primitive agriculture -- burning forests, creating methane-emitting rice paddies,

etc. -- produced enough greenhouse gases to warm the planet at least a degree. The theory is interesting. Even more interesting is the reaction to it by people such as the Columbia University professor who says it makes him "really upset" because it might encourage opponents of legislation combating global warming. Warnings about cataclysmic warming increase in stridency as evidence of warming becomes more elusive. A recent report from the United Nations Environment Program predicts an enormous 6.3 degrees Fahrenheit increase by the end of the century even if nations fulfill their most ambitious pledges concerning reduction of carbon emissions. The U.S. goal is an 80 percent reduction by 2050. But Steven Hayward of the American Enterprise Institute says that would require reducing greenhouse gas emissions to the 1910 level. On a per capita basis, it would mean emissions approximately equal to those in 1875. That will not happen. So, we are doomed. So, why try? America needs a national commission appointed to assess the evidence about climate change. Alarmists will fight this because the first casualty would be the carefully cultivated and media-reinforced myth of consensus -- the bald assertion that no reputable scientist doubts the gravity of the crisis, doubts being conclusive evidence of disreputable motives or intellectual qualifications. The president, however, could support such a commission because he is sure "there's finally widespread recognition of the urgency of the challenge before us." So he announced last week at the U.N. climate change summit, where he said the threat is so "serious" and "urgent" that unless all nations act "boldly, swiftly and together" -- "time . . . is running out" -- we risk "irreversible catastrophe." Prince Charles agrees. In March, seven months ago, he said humanity had 100 months -- until July 2017 -- to prevent "catastrophic climate change and the unimaginable horrors that this would bring." Evidently humanity will prevent this. Charles Moore of the Spectator notes that in July, the prince said that

by 2050 the planet will be imperiled by the existence of 9 billion people, a large portion of them consuming as much as Western people now do. Environmental Cassandras must be careful with their predictions lest they commit what climate alarmists consider the unpardonable faux pas of denying that the world is coming to an end.

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(5) U.S. THROWS SPANNER INTO CLIMATE TALKS

Times of India, 2 October 2009

<[40]http://timesofindia.indiatimes.com/news/environment/global-warming/US-trows-spanner-into-climate-talks/articleshow/5079332.cms>

Nitin Sethi, TNN

NEW DELHI: The promise of a deal at Copenhagen seem to be turning into a

pipedream as the US has refused to put down hard numbers for mitigation

under the second phase of Kyoto Protocol at the ongoing climate negotiations at Bangkok. EU too seems to be taking a deal-breaking condition saying, "environmental integrity" was central to the UN treaty

and "equity" of different countries' rights was just one element.

The negotiations at various levels seem to be grinding into a logjam with US determined not to sign on to the Kyoto Protocol. The US negotiators fought hard at different forums within the UN talks to block

any progress on industrialized countries' commitments to reduce emissions in the mid-term under the second phase of Kyoto Protocol.

India stood steadfast in demanding that the rich countries put up their

offers in terms of hard numbers for emission reductions over 2012-2020

under the existing protocol. But, US and many other developed countries

seemed determined to do away with the Kyoto Protocol entirely.

This is not the first time that US has voiced its opposition to the Kyoto Protocol which demands quantified targets from rich countries. US

had not signed on to Kyoto earlier and it continues to oppose the only

tool the global treaty has for making measurable and comparable reductions in the dangerous greenhouse gases.

The protocol is also seen by a select band of industrialized countries

such as US and Japan as a wall of differentiation constructed in the convention. The parent treaty -- UN Framework Convention on Climate Change -- lays most of the burden of mitigation on the industrialized

countries that caused it in the first place. The Kyoto Protocol activates this principle of burden sharing into hard actions and

targets. The protocol in its first phase sets fixed percentages by which countries reduce their emissions by 2012 below 1990 levels. Many of the industrialized countries have not moved on a trajectory to achieve the targets for 2012. Part of the discussions in the UN talks have been to set a higher level targets for the second phase of Kyoto Protocol between 2012-2020. But the US, not keen to take on any commitments in the mid-term, has always shown interest in disbanding with Kyoto Protocol and instead taking on a series of actions that are decided by countries on their own -- say energy efficiency targets -- and merely presented to the UN forum. India and developing countries have pointed out that would make the targets incomparable and render it impossible to figure out if any significant reductions have been made in emissions to prevent a climate calamity. Other industrialized countries too have so far shown little interest in offering credible and robust targets for the second phase of the protocol. The offers so far on the table from the industrialized countries, if implemented, would only bring in reductions in the range of 11-18% by 2020 below 1990 levels. India and other developing countries have demanded that the industrialized countries follow the recommendations of the UN climate science panel -- IPCC -- and take cuts in the range of 25-40% below 1990 levels by 2020 which would put the world on a trajectory to avoid temperatures reaching dangerous levels in the decades to come.

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(6) CAP AND TRADE MAY SINK OPPOSITION LEADER DOWN UNDER
The Australian, 2 October 2009

<[41]http://www.theaustralian.news.com.au/story/0,25197,26153820-2702,00.htm> l>

Lenore Taylor, National correspondent | October 02, 2009
MALCOLM Turnbull is on a collision course with his own back bench after staking his leadership on a demand that they back his climate change strategy. Several MPs immediately refused to do so. If the partyroom refused to back his strategy of negotiating amendments to the government's emissions trading scheme, Mr Turnbull said yesterday, the Coalition would "literally be a party with nothing to say ... a party with no ideas", and that was "not the party I am

prepared to
lead".

Throwing down the gauntlet to his internal critics, Mr Turnbull
said: "I

am asserting my authority as the leader of the Liberal Party and the
Leader of the Opposition."

"If the partyroom were to reject my recommendation to them, that
would

obviously be a leadership issue. That's perfectly plain, perfectly
clear," he told ABC Radio in Adelaide.

"I could not possibly lead a party that was on a
do-nothing-on-climate-change platform."

His critics were not cowed, despite the fact that both mooted
leadership

alternatives -- Joe Hockey and Tony Abbott -- support Mr Turnbull's
stance.

West Australian backbencher Wilson Tuckey said: "Mr Turnbull has
made

the ETS a leadership issue and we will now treat it as such." His
leader's ultimatum did not alter his "total opposition to an ETS
and to

the suggestion that we might amend it".

Victorian Liberal senator Julian McGauran said he stood by his
vow to

vote against the ETS in November, no matter what amendments were
negotiated.

Nationals senators also remain implacably opposed to the scheme. "He
hasn't got the partyroom with him on this one ... we are going
to stand

up for what we believe in," said senator Ron Boswell.

"This is not just another issue. This is not one we can let go
through

to the keeper," said senator Barnaby Joyce.

Mr Tuckey appeared to suggest Mr Turnbull's deputy, Julie
Bishop, as an

alternative leader, saying there were "many good potential
leaders in

the Liberal Party ... and perhaps some people who have had their
reputations tarnished by backgrounding from our side now deserve
reconsideration for the top job".

FULL STORY at

<[42]http://www.theaustralian.news.com.au/story/0,25197,26153820-2702,00.htm> 1>

===== e-mails to the editor =====

(7) THE MET OFFICE AND CRU'S YAMAL SCANDAL: EXPLAIN OR RESIGN

Jennifer Marohasy <jennifermarohasy@jennifermarohasy.com

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Leading UK Climate Scientists Must Explain or Resign, Jennifer
Marohasy

<[46]http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists->

must-explain-or-resign/>

MOST scientific sceptics have been dismissive of the various reconstructions of temperature which suggest 1998 is the warmest year of the past millennium. Our case has been significantly bolstered over the last week with statistician Steve McIntyre finally getting access to data used by Keith Briffa, Tim Osborn and Phil Jones to support the idea that there has been an unprecedented upswing in temperatures over the last hundred years - the infamous hockey stick graph. Mr McIntyre's analysis of the data - which he had been asking for since 2003 - suggests that scientists at the Climate Research Unit of the United Kingdom's Bureau of Meteorology have been using only a small subset of the available data to make their claims that recent years have been the hottest of the last millennium. When the entire data set is used, Mr McIntyre claims that the hockey stick shape disappears completely. [1]

Mr McIntyre has previously showed problems with the mathematics behind the 'hockey stick'. But scientists at the Climate Research Centre, in particular Dr Briffa, have continuously republished claiming the upswing in temperatures over the last 100 years is real and not an artifact of the methodology used - as claimed by Mr McIntyre. However, these same scientists have denied Mr McIntyre access to all the data. Recently they were forced to make more data available to Mr McIntyre after they published in the Philosophical Transactions of the Royal Society - a journal which unlike Nature and Science has strict policies on data archiving which it enforces. This week's claims by Steve McIntyre that scientists associated with the UK Meteorology Bureau have been less than diligent are serious and suggest some of the most defended building blocks of the case for anthropogenic global warming are based on the indefensible when the methodology is laid bare.

This sorry saga also raises issues associated with how data is archived at the UK Meteorological Bureau with in complete data sets that spuriously support the case for global warming being promoted while complete data sets are kept hidden from the public - including from scientific sceptics like Steve McIntyre.

It is indeed time leading scientists at the Climate Research Centre associated with the UK Meteorological Bureau explain how Mr McIntyre is

in error or resign.

[1] Yamal: A "Divergence" Problem, by Steve McIntyre, 27
September 2009

[47]<http://www.climateaudit.org/?p=7168>

Jennifer Marohasy BSc PhD

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(8) COOLING?

Rodney Chilton <maberrd@hotmail.com <mailto:maberrd@hotmail.com>

<[48]mailto:maberrd@hotmail.com <mailto:maberrd@hotmail.com>>>

Dear Benny:

Recently, there has been considerable discussion concerning the
slight

cooling of the earth's overall climate since about 2005. The
result of

the cooling has brought some scientists into the forefront to be
openly

critical of the still prominent view that climate changes over the
century or so have predominately been man caused. The proponents of
human initiated climate changes are of the opinion that the recent
cooling is but a temporary interruption in what soon again will be a
rapid climate warming.

I think one of the keys to alleviate some of this discussion is to
attempt to determine the triggers for two other climate shifts in
earlier times. The first of these, the "Little Ice Age" is generally
regarded by most scientists as resulting from a reduced output
of energy

from the sun. Coinciding as it did with an interval of very
little to

almost no sunspot activity, a time known as the "Maunder
Minimum", many

solar scientists suggest that as little as 0.25% decrease in solar
output initiated this cold climate period. Similarly, during
the mid

20th Century during the years from the end of the 1940's to
about the

mid 1970's, the sun was in one of its quiet modes (very few
sunspots).

The cause for what was a slightly cooler interval could logically be
linked to decreased energy from the sun. However, the quite recent
thirty year period is more commonly linked to increased dust in the
earth's atmosphere. Consistent with this view is the idea that
perhaps

the Little Ice Age too, was forced not by a decrease in the sun's
output, but by an increase in dust, not that produced by man, but by
extraterrestrial dust from a comet encounter. More details of this
particular scenario can be seen at the following website:

<[49]<http://www.bcclimate.com> <[50]<http://www.bcclimate.com/>>

<[51]<http://www.bcclimate.com/>>>

All of this raises the questions, what drove both the Little Ice
Age and

the thirty year interval in the middle of the last century? It is
possible that they were driven by the two different causes

outlined. It

is vital I think that the reason(s) for the two climate shifts be determined. This would go along way to settle the recent debate as to the importance of solar minima in initiating climate changes over more than just a few years. Further to this, the picture of the future will be clarified. If for example, decreases in solar output is proven to be of less importance during the past, then surely the present climate downturn will be likely only a temporary respite from the inexorable upward trend in temperatures worldwide. If on the other hand the solar cycles accompanied by low sun activity over decades and even longer can be proven as significant, then I believe we must re-examine the increased carbon dioxide scenario.

Rodney Chilton

=====

(9) RESOURCES DEPLETION WORRIES

Steven Zoraster <szoraster@szoraster.com

<[52]mailto:szoraster@szoraster.com> <[53]mailto:szoraster@szoraster.com>

<[54]mailto:szoraster@szoraster.com>>>

Benny,

Certainly someone with access to the hard numbers and more knowledge than I can do better proving or disproving the following argument about the ERoEI of nuclear power in the United States:

Today, 104 nuclear reactors supply 20% of the electricity used each year

in the United States. [1]They have been doing this for approximately 25

years. [2] Many existing reactors have now been approved to operate for

60 years. While the initial costs measured in energy use 25 years ago

were high and construction often took 5 years, I doubt that the construction process for all 104 reactors, required greater energy than

the equivalent of 20% of annual electricity used 25 years ago over a 5

year period. (I include the cost of design, obtaining permits, fighting

environmental lawsuits, manufacturing parts, and actual construction,

etc., in the total energy cost.)

Today the annual operating costs of maintaining, fuelling, and repairing

existing reactors are low compared to alternate sources of electricity

except hydroelectric. The nuclear waste from these reactors has been safely stored at the reactor sites without causing a single human death.

Conclusions: Assuming the generation of electric energy in the US since about 1985 has been and will be constant, the EROEI of nuclear power using 25 year old technology is greater than 12. (Twenty percent of all electric energy generated over 60 years divided by 20% of the same amount of pre-atomic electricity generated over 5 years.) Given that total electricity use in the US has almost doubled in the last 25 years [3], the EROEI may be greater than 24. More modern proposed reactor designs, with greater standardization, simpler fuel cycles, fail safe features, and increased automation, can be expected to have higher EROEI.

(I have not included the cost of decommissioning reactors. Numbers I found online are often estimates and seldom given in terms of energy.

Because fuel costs today and to be expected in the future are low, ignoring the option of recycling used fuel is not a significant factor in my calculations.)

Steven Zoraster

[1]

[55]http://www.nei.org/resourcesandstats/nuclear_statistics/usnuclearpowerplants/

[2]

[56]<http://www.eia.doe.gov/cneaf/nuclear/page/analysis/nuclearpower.html>

Reactors were being completed between 1957 and 1996. The first large commercial reactors date to 1968. The longest "build time" is 24 years.

Some reactors have been closed after being built and have been ignored

in my argument. My use of 25 years in these calculations is certainly a

suspect approximation or average.

[3] [57]<http://www.eia.doe.gov/emeu/aer/eh/frame.html> (Then click on "Electricity" on the left side of the page.)

=====

(10) COPENHAGEN SUMMIT: DO SCIENCE AND ECONOMICS SUPPORT GOVERNMENT ACTION ON GLOBAL WARMING?

Peter Kidson <peterdkidson@googlemail.com

<[58]mailto:peterdkidson@googlemail.com>

<[59]mailto:peterdkidson@googlemail.com>

<[60]mailto:peterdkidson@googlemail.com>>]

Hi Benny

You might perhaps want to publicise this public debate

<[61]<http://www.iea.org.uk/record.jsp?type=event&ID=217>>

<[62]<http://www.iea.org.uk/record.jsp?type=event&ID=217>>

<[63]<http://www.iea.org.uk/record.jsp?type=event&ID=217>>

<[64]<http://www.iea.org.uk/record.jsp?type=event&ID=217>>>>

Note that you need to reserve seats.

Regards

-Peter

=====

(11) A DEATH SPIRAL FOR CLIMATE ALARMISM?

Robert Bradley <rbradley@iertx.org <[65]mailto:rbradley@iertx.org>

<[66]mailto:rbradley@iertx.org <mailto:rbradley@iertx.org>>>

Ken Green's post at MasterResource today should be of interest.

<[67]http://masterresource.org/?p=5036>

Things are getting very shrill from the Climate Industry, but there is a

rethink going on starting with the physical science.

Robert L. Bradley Jr.

CEO & Founder, Institute for Energy Research

Houston, Texas 77057-3527

IER Website: [68]www.energyrealism.org

<[69]http://www.energyrealism.org/> <[70]http://www.energyrealism.org/>

Political Capitalism website: [71]www.politicalcapitalism.org

<[72]http://www.politicalcapitalism.org/>

<[73]http://www.politicalcapitalism.org/>

Energy Blog: [74]www.MasterResource.org

<[75]http://www.masterresource.org/> <[76]http://www.masterresource.org/>

=====

(12) AND FINALLY: 'PLANNED RECESSION' COULD AVOID CATASTROPHIC CLIMATE CHANGE

The Daily Telegraph, 30 September 2009

<[77]http://www.telegraph.co.uk/earth/earthnews/6248257/Planned-recession-could-avoid-catastrophic-climate-change.html>

By Louise Gray, Environment Correspondent

Britain will have to stop building airports, switch to electric cars and

shut down coal-fired power stations as part of a 'planned recession' to

avoid dangerous climate change.

At the moment the UK is committed to cutting greenhouse gases by a third by 2020.

However a new report from the Tyndall Centre for Climate Change Research

said these targets are inadequate to keep global warming below two degrees C above pre-industrial levels.

The report says the only way to avoid going beyond the dangerous tipping

point is to double the target to 70 per cent by 2020.

This would mean reducing the size of the economy through a "planned recession".

Kevin Anderson, director of the research body, said the building of new airports, petrol cars and dirty coal-fired power stations will have to

be halted in the UK until new technology provides an alternative to burning fossil fuels.

"To meet [Government] targets of not exceeding two degrees C, there would have to be a moratorium on airport expansion, stringent measures

on the type of vehicle being used and a rapid transition to low carbon technology," he said.

Prof Anderson also said individuals will have to consume less. "For most of the population it would mean fairly modest changes to how they live, maybe they will drive less, share a car to work or take more holidays in Britain."

More than 190 countries are due to meet in Copenhagen in December to decide a new international deal on climate change.

Speaking at an Oxford University conference on the threat of climate change, Prof. Anderson said rich countries will have to make much more ambitious cuts to have any chance of keeping temperature rise below four degrees C.

"If we do everything we can do then we might have a chance," he said.

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not necessarily reflect the opinions, beliefs and viewpoints of the editor. <[84]http://www.staff.livjm.ac.uk/spsbpeis/>

-- David W. Schnare

Center for Environmental Stewardship

-- David W. Schnare

Center for Environmental Stewardship

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For more options, visit this group at
[85]<http://groups.google.com/group/geoengineering?hl=en>

Prof. Phil Jones
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NR4 7TJ
UK

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5. <mailto:geoengineering@googlegroups.com>
6. <mailto:geoengineering@googlegroups.com>
7. <mailto:geoengineering@googlegroups.com>
8. <mailto:euggordon@comcast.net>
9. <mailto:euggordon@comcast.net%20%3Cmailto:euggordon@comcast.net>
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17. <mailto:jennifermarohasy@jennifermarohasy.com>
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28. <mailto:peterdkidson@googlemail.com>
29. <mailto:rbradley@iertx.org>
30. <mailto:rbradley@iertx.org>
31. <mailto:rbradley@iertx.org>
32. http://www.theregister.co.uk/2009/09/29/yamal_scandal/
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42. <http://www.theaustralian.news.com.au/story/0,25197,26153820-2702,00.htm>
43. <mailto:jennifermarohasy@jennifermarohasy.com>
44. <mailto:jennifermarohasy@jennifermarohasy.com>
45. <mailto:jennifermarohasy@jennifermarohasy.com>
46. <http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists-%3E%A0%A0%A0%A0%A0%A0%A0%A0>
47. <http://www.climateaudit.org/?p=7168>
48. <mailto:maberrd@hotmail.com%20%3Cmailto:maberrd@hotmail.com>
49. <http://www.bcclimate.com/>
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52. <mailto:szoraster@szoraster.com>
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55. http://www.nei.org/resourcesandstats/nuclear_statistics/usnuclearpowerpl
56. <http://www.eia.doe.gov/cneaf/nuclear/page/analysis/nuclearpower.html>
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60. <mailto:peterdkidson@googlemail.com>
61. <http://www.iea.org.uk/record.jsp?type=event&ID=217>%A0%A0%A0%A0%A0%A0%A0%A0>
62. <http://www.iea.org.uk/record.jsp?type=event&ID=217>
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65. <mailto:rbradley@iertx.org>
66. <mailto:rbradley@iertx.org%20%3Cmailto:rbradley@iertx.org>
67. <http://masterresource.org/?p=5036>
68. <http://www.energyrealism.org/>
69. <http://www.energyrealism.org/>
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71. <http://www.politicalcapitalism.org/>
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76. <http://www.masterresource.org/>
77. <http://www.telegraph.co.uk/earth/earthnews/6248257/Planned-recession-co%3E%A0%A0%A0%A0%A0%A0%A0%A0>
78. <mailto:listserv@ljmu.ac.uk>
79. <mailto:listserv@ljmu.ac.uk%3E%A0%A0%A0%A0%A0%A0%A0%A0>
80. <mailto:listserv@ljmu.ac.uk>
81. <mailto:listserv@ljmu.ac.uk>
82. <mailto:listserv@ljmu.ac.uk>
83. <mailto:listserv@ljmu.ac.uk>
84. <http://www.staff.livjm.ac.uk/spsbpeis/%3E%A0%A0%A0>
85. <http://groups.google.com/group/geoengineering?hl=en>

From: Phil Jones <p.jones@uea.ac.uk>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: Re: thanks and one question
Date: Mon Oct 5 10:55:36 2009
Cc: Michael Mann <mann@meteo.psu.edu>

Gavin, Mike,

Thanks for this!

I assume you are both aware of this prat - Neil Craig, see below. Keith won't be responding.

Checking facts doesn't seem important these days. As CA threads aren't publications this is difficult for non scientists.

I am going further over one email I got at the weekend - see also below. Typical of Sonia - although she now seems to only be an emeritus reader!

Cheers

Phil

Return-path: <CrgN143@aol.com>

From: CrgN143@aol.com

Full-name: CrgN143

Message-ID: <d03.64b01875.37f87aa4@aol.com>

Date: Sat, 3 Oct 2009 06:00:04 EDT

Subject: Tree rings - accusation that you were solely responsible.

To: k.briffa@uea.ac.uk

MIME-Version: 1.0

Content-Type: multipart/alternative; boundary="-----1254564004"

X-Mailer: 9.0 SE for Windows sub 5045

Professor Briffa,

I have written a couple of blogs on the current report by Steve McIntyre that the data used by Mann to "prove" the hockey Stick was fabricated. This & the following day's
[1]<http://a-place-to-stand.blogspot.com/2009/10/global-warming-proven-deliberate-fraud.html>

As a result I have received this email from somebody I am not acquainted with throwing the entire blame on you. This seems improbable to me & possibly an alarmist damage limitation exercise. If you wish to comment I would be happy for you to do so.

"Please note: Steve McIntyre's post concerns work by climate scientist Keith Briffa and not Michael Mann. You will probably wish to correct your post.

Cheers

Avisame"

I have posted this as an update with my reply:

"My understanding is that while Briffa did the tree ring measurement, Mann, in his paper, chose to choose 12 atypical tree rings out of at least 34 to fabricate the global warming trend. My assumption is that Mann is responsible for fabrications in his own paper & that this is a damage limitation exercise. I am open to correction on this & indeed have emailed Mr Briffa to see. "

Neil Craig

You may be interested in my political blog

[2]<http://a-place-to-stand.blogspot.com/>

We received this through our enquiries desk. I assume that you are aware of this person, including those copied on the message.

If we are to respond, it would be to indicate that there are multiple sources of supporting evidence and that we continue to place our confidence in the international scientific assessment process. This confidence has proven to be well placed.

Roger

From: Sonja A Boehmer-Christiansen <Sonja.B-C@hull.ac.uk>
Date: 2 October 2009 18:09:39 GMT+01:00
To: Stephanie Ferguson <stephanie.ferguson@ukcip.org.uk>
Cc: "Peiser, Benny" <B.J.Peiser@ljmu.ac.uk>, Patrick David Henderson <pdhenderson18@googlemail.com>, Christopher Monckton <monckton@mail.com>
Subject: RE: Please take note of potetially serious allegations of scientific 'fraud' by CRU and Met Office

Dear Stephanie

I expect that a great deal of UKCIP work is based on the data provided by CRU (as does the work of the IPCC and of course UK climate policy). Some of this, very fundamentally, would now seem to be open to scientific challenge, and may even face future legal enquiries. It may be in the interest of UKCIP to inform itself in good time and become a little more 'uncertain' about its policy advice.

Perhaps you can comment on the following and pass the allegations made on to the relevant people.

It is beyond my expertise to assess the claims made, but they would fit into my perception of the whole 'man-made global warming' cum energy policy debate. I know several of the people involved personally and have no reason to doubt their sincerity and honour as scientists, though I am also aware of their highly critical (of IPCC science) policy

positions.

I could also let you have statements by Steve McIntyre and Ross McKitrick. Ross McKitrick currently teaches at Westminster Business School and who is fully informed about the relevant issues. He recently addressed a meeting of about 50 people in London.

Best wishes

Sonja B-C

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Reader Emeritus, Department of Geography
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HULL HU6 7RX
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Fax: (0044) 1482 466340

TWO copied pieces follow, both relate to CRU and UK climate policy

a. THE MET OFFICE AND CRU'S YAMAL SCANDAL: EXPLAIN OR RESIGN

" Jennifer Marohasy <jennifermarohasy@jennifermarohasy.com>

Leading UK Climate Scientists Must Explain or Resign, Jennifer Marohasy
<<[4]http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists->
[5]http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists-
must-explain-or-resign/>

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2. <http://a-place-to-stand.blogspot.com/>
3. <http://www.multi-science.co.uk/>
4. <http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists>
5. <http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists>

From: Phil Jones <p.jones@uea.ac.uk>
To: k.briffa@uea.ac.uk
Subject: Message from Tom Wigley
Date: Mon Oct 5 11:35:44 2009

Keith,

Here's a message from Tom. It might be worth sending anything you've got to him to have a look through. Shorter responses are probably better. Detail can go in a poster.

Pointing out how often or not Yamal is used is useful. I don't think they have done this. I think many people confuse this with the polar urals chronology. That is different and it is based on density.

M&M rely on people not checking.

Cheers

Phil

Date: Mon, 05 Oct 2009 03:57:57 -0600
From: Tom Wigley <wigley@ucar.edu>
User-Agent: Thunderbird 2.0.0.14 (Windows/20080421)
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: [geo] Re: CCNet: A Scientific Scandal Unfolds
X-Canit-CHI2: 0.00
X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)
X-Spam-Score: 0.30 () [Hold at 5.00] PORN_RP_NASTY,SPF(none,0)
X-CanItPRO-Stream: UEA:f028 (inherits from UEA:default,base:default)
X-Canit-Stats-ID: 32219749 - e7f62debf1d6
X-Antispam-Training-Forget:
[1]<https://canit.uea.ac.uk/b.php?i=32219749&m=e7f62debf1d6&c=f>
X-Antispam-Training-Nonspam:
[2]<https://canit.uea.ac.uk/b.php?i=32219749&m=e7f62debf1d6&c=n>
X-Antispam-Training-Spam: [3]<https://canit.uea.ac.uk/b.php?i=32219749&m=e7f62debf1d6&c=s>
X-Scanned-By: CanIt (www . roaringpenguin . com) on 139.222.131.184

Phil,

It is distressing to read that American Stinker item. But Keith does seem to have got himself into a mess. As I pointed out in emails, Yamal is insignificant. And you say that (contrary to what M&M say) Yamal is *not* used in MBH, etc. So these facts alone are enough to shoot down M&M in a few sentences (which surely is the only way to go -- complex and wordy responses will be counter productive).

But, more generally, (even if it *is* irrelevant) how does Keith explain the McIntyre plot that compares Yamal-12 with Yamal-all? And how does he explain the apparent "selection" of the less well-replicated

chronology rather than the later (better replicated) chronology?

Of course, I don't know how often Yamal-12 has really been used in recent, post-1995, work. I suspect from what you say it is much less often than M&M say -- but where did they get their information? I presume they went through papers to see if Yamal was cited, a pretty foolproof method if you ask me. Perhaps these things can be explained clearly and concisely -- but I am not sure Keith is able to do this

as he is too close to the issue and probably quite pissed off.

And the issue of with-holding data is still a hot potato, one that affects both you and Keith (and Mann). Yes, there are reasons -- but many *good* scientists appear to be unsympathetic to these. The trouble here is that with-holding data looks like hiding something, and hiding means (in some eyes) that it is bogus science that is being hidden.

I think Keith needs to be very, very careful in how he handles this.

I'd be willing to check over anything he puts together.

Tom.

Prof. Phil Jones

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School of Environmental Sciences Fax +44 (0) 1603 507784

University of East Anglia

Norwich Email p.jones@uea.ac.uk

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References

1. <https://canit.uea.ac.uk/b.php?i=32219749&m=e7f62debf1d6&c=f>
2. <https://canit.uea.ac.uk/b.php?i=32219749&m=e7f62debf1d6&c=n>
3. <https://canit.uea.ac.uk/b.php?i=32219749&m=e7f62debf1d6&c=s>

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: NCDC data
Date: Mon Oct 5 12:35:37 2009
Cc: Ben Santer <santer1@llnl.gov>

Tom,

I can't see why the data become ERSSTv3b. b seems all that you can download.

I reviewed the 2008 paper. The version that I reviewed had something in for the problem of SST data now re drifters and ships, but they pulled that section. I recall saying it needed to be watertight and they needed to explain the spatial pattern to the ship minus drifter field. Maybe that version was a?

I was never that keen on their infilling. It biases the values before the 1920s when you infill with anomalies that are nearer to zero. You can see this in their Fig6. This version is better than their previous one.

I always assumed they still had gaps - as it would be impossible to infill the Antarctic and some parts of the Southern Oceans. Have you tried looking at their Antarctic average - 65-90S for example?

Their globe should be one domain, so not $(NH+SH)/2$ but for an infilled dataset this shouldn't make any difference.

I wonder if they downweight the infilled values in some way? They have their error field?

The 2008 paper doesn't say how they compute Global and NH and SH. Are NH and SH the same as you get?

Cheers

Phil

At 06:56 05/10/2009, Tom Wigley wrote:

Phil, Ben,

Have you looked at the latest NCDC global data? It seems odd.

The data on their site is ERSSTv3 (Smith et al. 2008). As far as I know, this is an infilled data set with no gaps. As such, $(NH+SH)/2$ should be the same as their global mean. For monthly data, this is not the case. There are actually some big differences, even recently.

Any idea why?

Tom.

Prof. Phil Jones

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NR4 7TJ
UK

Email p.jones@uea.ac.uk

From: Phil Jones <p.jones@uea.ac.uk>
To: Andrew Manning <a.manning@uea.ac.uk>
Subject: Re: Fwd: Co2 Data
Date: Tue Oct 6 08:38:04 2009

Andrew,

Getting a bit fed up with these baseless allegations.

You could point out several things to Martin.

1. Projections aren't made with observed data - instrumental or paleo. They are made with climate models.

2. The initial seed for all these allegations is made on Climate Audit. Here they are quite clever and don't go over the top. They leave it to others like the National Review, the American Thinker to make the ridiculous ones.

Here is what Stephen McIntyre says on Climate Audit.

"While there is much to criticise in the handling of this data by the authors and the journals, the results do not in any way show that 'AGW is a fraud' nor that this particular study was a 'fraud'.

McIntyre has no interest in publishing his results in the peer-review literature. IPCC won't be able to assess any of it unless he does.

You dad and Susan Solomon have had runs in with him and others

3. You might like to send him this pdf and its Figure 2. Three different groups get much the same result.

Here are the two web pages we have put up so far. Keith is working on the tree one and put much more later in the week.

[1]<http://www.cru.uea.ac.uk/cru/data/availability/>

So other groups around the world have also entered into agreements. I know this doesn't make it right, but it is the way of the world with both instrumental and paleo data. I frequently try and get data from other people without success, sometimes from people who send me the pdf of their paper then tell me they can't send me the series in their plots.

[2]<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2000/>

It is the right wing web sites doing all this, presumably in the build up to Copenhagen.

At 00:13 06/10/2009, Andrew Manning wrote:

Hi Phil,

is this another witch hunt (like Mann et al.)? How should I respond to the below? (I'm in the process of trying to persuade Siemens Corp. (a company with half a million employees in 190 countries!) to donate me a little cash to do some CO2 measurements here in the UK - looking promising, so the last thing I need is news articles calling into question (again) observed temperature increases - I thought we'd moved the debate beyond this, but seems that these sceptics are real die-hards!!).

Kind regards,

Andrew

Date: Mon, 5 Oct 2009 15:50:38 +0100

Subject: Co2 Data

From: Martin Lutyens <martinlutyens@googlemail.com>

To: Andrew Manning <a.manning@uea.ac.uk>

Dear Andrew,

I just came across an article in The Week, called "The case of the vanishing data". It writes in a rather wry and sceptical way about your UEA colleagues Phil Jones and Tom Wigley, saying that only their "homogenised" or "adjusted" historical data is available, and the original, raw data has gone missing. Apparently some other environmental gurus now want to look at the original data and were "fobbed off". According to the article, the adjusted data forms the basis for much of the climate change debate and, because others now want to look at the source data, it is "at the centre of an academic spat that could have major implications for the climate change debate". The author of the original article is Patrick Michaels in The National Review, who may just be stirring it.

The article concludes "In short, the data invoked to verify the most significant forecasts about the world's future, have simply vanished." Could you comment on this please, as someone (eg Siemens Corp.) may pick this up and I think we should all be forearmed by knowing what really happened and what to say if asked.

Many thanks, Martin

--

Martin Lutyens

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UK

References

1. <http://www.cru.uea.ac.uk/cru/data/availability/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2000/>

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>, Ben Santer <santer1@llnl.gov>
Subject: Re: help please
Date: Tue Oct 6 13:35:34 2009

Tom,

Agreed that NCDC must have some data gaps - but this isn't very clear from the web site.

GISS is inferior - not just because it doesn't use back data. They also impose some urbanization adjustment which is based on population/night lights which I don't think is very good. Their gridding also smooths things out. Plotting all three together for land only though they look similar at decadal timescales. GISS does have less year-to-year variability - when I last looked.

I assume NCDC should add the back data in - although there isn't the need if infilling is going on OK.

I've never looked to see if NCDC changes from year to year.

I think you can say that GISS is inferior to CRUTEM3. In Ch 3 of AR4 I put the station number counts in.

GISS and NCDC have more, but almost all of this is more data in the US. Their non-use of a base period (GISS using something very odd and NCDC first differences) means they can use very short series that we can't (as they don't have base periods) but with short series it is impossible to assess for homogeneity. So some of their extra series may be very short ones as well. As you know the more important thing is where the stations are (and in time).

The paper I sent you by Adrian Simmons shows great agreement with CRUTEM3 when subsampled according to CRU grid boxes. Also shows that ERA-INTERIM is very good. ERA-INTERIM's absolute is also within 0.2 deg C of the CRU 14 deg C value. It would give about 13.8 for 1961-90. Sometime I should write this up as more and more people seem to be using 15 deg C.

Away from tomorrow till next Tuesday.

Cheers

Phil

At 23:23 05/10/2009, Tom Wigley wrote:

Phil,

Thanks again.

Re ENSO/volcs, it was me who did this first ...

Wigley, T.M.L., 2000: ENSO, volcanoes and record breaking temperatures.

Geophysical Research Letters 27, 41014104.

Then in a paper with Ben (with you as a co-author) ...

Santer, B.D., Wigley, T.M.L., Doutriaux, C., Boyle, J.S., Hansen, J.E.,

Jones, P.D., Meehl, G.A., Roeckner, E., Sengupta, S. and Taylor K.E., 2001:

Accounting for the effects of volcanoes and ENSO in comparisons of modeled

and observed temperature trends. Journal of Geophysical Research 106, 2803328059.

I think my iterative method is better than Thompson's method. He has some weird volcano results. Removing the dynamic bit is not much use in my view.

So I have all these series with volc and ENSO removed (or just ENSO removed, but accounting for volcano obfuscation). I also use running approx. 20-year regressions usually -- as you know, the ENSO-globalT link breaks down in the 1930s, so using a relationship that comes from

a (e.g.) 100-year regression would impose a spurious anti-ENSO signal on the data in the 1930s. I think this is important -- ignored by Thompson. The reason for this breakdown is obscure, but I think it is because, for some reason, the N34/SOI link (i.e., really the SST/Walker circulation link) weakens in the 1930s. We need to look at this more fully in models.

I also have these series for different regions of the globe. I need to revise and update these. It is tricky to get the regional volc signal because of SNR problems at the smaller spatial scale.

I wrote all this up more than 10 years ago, but have not got around to finalizing it to submit for publication. (I have a number of other papers like this. Once I get done with an issue to a certain level I get sidetracked on other issues.)

The amplification *does* work for warming and cooling. Theory says about +30% for TLT/surface. This works for overall variability, and for RSS trend. But oddly the ENSO and volc amplification seems to be greater than this. I've asked Ben for his thoughts on why.

Re NCDC, it seems that there *must* be data gaps. This is the only way that global can differ from $(N+S)/2$.

It also seems that the NCDC data must be ERSST3b. But their web site is not clear on this. perhaps Ben knows.

Thanks for the GISS info. So this means that their series does not change from year to year, whereas HadCRU does (albiet by only small amounts). Does NCDC change each year? The GISS thing means that it must be inferior to HadCRU and NCDC. Should I say this in my report to EPRI?

Tom.

+++++

Phil Jones wrote:

Tom,

I don't think AR4 (Ch 3) went into the TLT/surface amplification issue. You can get the pdf of the chapter from here [1]<http://ipcc-wg1.ucar.edu/wg1/wg1-report.html> . This amplification issue is only addressed in some recent papers - mainly Ben's.

The timescale argument is quite convincing. It is a pity that there is only Pinatubo

that you can test it on. El Chichon ought to work but it is confused by ENSO. Does the amplification work well for the 1997/98 El Nino?

Did you pick up that Thompson et al paper due out in J. Climate soon? Factoring out ENSO and volcanoes might help in isolating this.

[2]<http://www.atmos.colostate.edu/faculty/thompson.php>

where there is a link to the paper and also the data

[3]<http://www.atmos.colostate.edu/~davet/ThompsonWallaceJonesKennedy/>

It seems as though you can get all the extraction parts. No need for the dynamic bit.

Anyway my thought is as Pinatubo gives the amplification then ENSO ought to as well.

A thought might be to take Dave Thompson's ENSO and volcanic subtraction series, then scale them by thermodynamic theory value then subtract these from RSS and UAH. Small issue of base periods to sort out

and assume there is no lag.

Need to do this with NCDC surface as well - have to use Dave T's numbers here. This can't do the 20N-20S - just the globe.

It would of course, at this and any other time, be very nice to show that UAH is wrong.

A couple of minor things in the paper

- the amplification should work for a cooling as well - not just warming trends?

In Fig 5 in your legend LOUAH should be UAHLO. This is in Fig 4 as well.

By the way - meant to add this to the earlier email.

NCDC ERSST3 side does talk about missing data, so any of this would mean the (NH+SH)/2 won't equal the global average that NCDC calculate.

I recall you asking about GISS. One thing I have learned about GISS is that they have a cut off date of the 8th of each month. After this date nothing is changed for the previous month and nothing earlier either. This means they never incorporate any back data and they don't get the second tranche of CLIMAT data which comes about the 16th of the following month. Countries like Paraguay and Bolivia mostly come in this way, plus some in Africa.

I'll see Tom Peterson later in the week. I'll ask him about their cut offs. I think they don't change a month later. This won't lose you much data though. It was Tom who told me about the data they can't use.

Cheers

Phil

At 05:25 04/10/2009, Tom Wigley wrote:

Hi Phil,

I'm writing a report for EPRI where I have to discuss the instrumental temperature record. What they are particularly concerned with is/are the criticisms that have been leveled at the surface record, especially differences from MSU data.

I think CCSP 1.1 does a good job on this -- not sure about AR4 (which I need to re-check). But things have changed since CCSP 1.1 and AR4, and I think I can make a better case against

UAH than either of these reports.

Could you please look at the attached and give me your opinion and comments (tracked if that makes it easier)? In my view, the evidence that the UAH data are flawed is overwhelming -- but I want to make the case in a logical and balanced way. Have I succeeded? The audience level for this is IPCC report level, perhaps a bit lower. So I need to be relatively simple, but authoritative. The MSU issue also comes up later in my report where I discuss the IJOC Santer et al. paper -- which is only mentioned briefly in the attached extract.

One thing I thought I might add is more about the other two surface data sets. A key point may be that 1998 is not the warmest year in the GISS record -- do you trust GISS? I've not looked at NOAA. Perhaps this still has 1998 as warmest?

Thanks for your help.

By the way, this report was due to EPRI last week. I'm hoping to get it to them by Friday (9 Oct.)

Best wishes,

Tom

Prof. Phil Jones

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References

1. <http://ipcc-wg1.ucar.edu/wg1/wg1-report.html%A0>
2. <http://www.atmos.colostate.edu/faculty/thompson.php>
3. <http://www.atmos.colostate.edu/~davet/ThompsonWallaceJonesKennedy/>

From: Viva Banzon <Viva.Banzon@noaa.gov>

To: Thomas.C.Peterson@noaa.gov

Subject: Re: ERSST

Date: Thu, 08 Oct 2009 14:48:11 -0400

Cc: Tom Wigley <wigley@ucar.edu>, Tom Karl <Thomas.R.Karl@noaa.gov>, Phil Jones <p.jones@uea.ac.uk>, Ben Santer <santer1@llnl.gov>, Richard.W.Reynolds@noaa.gov, Derek.Arndt@noaa.gov

Hello, everyone,

Additional info provided below.-Viva

ERSST refers only to the ocean temperature fields. Smith et al. (2008) described the updates to create ERSST version 3. This included the use of in situ and satellite data. The paper also presented updates to the Land Surface Temperature (LST) product and culminated in the computation of the Merged Land-Ocean Surface Temperature product. However, since ca. Nov 2008, satellite data was removed from the analysis, and was called v3b, but the methodology is essentially the same as in the paper. The reason was that there was a residual cold bias in the satellite data. This caused problems for users concerned with rankings. We do not handle the page for the LST and Merged ST product, and perhaps there should be more coordination among these webpages. We have noticed the confusion about the ERSST v3 and v3b in several articles, are in the process of updating the webpage.

The in situ data used for the ERSSTv3b is ICOADS. The current v3b was computed using ICOADS release 2.4 (1784-2007). In July 2009, a new release was made with additional data pre-1900's and during the war years, but we have no plans yet to reprocess. It is during such a reprocessing that we will include any missed data. Operationally, we run the code on the 3rd of each month using the available GTS data.

The baseline for the ERSST anomalies is 1971-2000. For the LST, the GHCN box averages are provided to us as anomalies already, so I am not sure what the baseline is (I just started 3 months ago so I have not worked a lot on the Merged product codes yet). In the programs, there is an adjustment of the LST anomaly to a 1971-2000 base. So the final merged ST anomaly has a 1971-2000 base period. The best practice would be to reconstruct the original ST by adding the 1971-2000 base. Then compare or adjust or change baselines as you please.

BTW, my last name is BANZON, no R. Alas I am not related to the 261st richest person.

--

[NOTE: The opinions expressed in this email are those of the author alone, and do not necessarily reflect official NOAA, Department of Commerce, or US government policy.]

Patria Viva F. Banzon

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[1]Viva.Banzon@noaa.gov

[2]Thomas.C.Peterson@noaa.gov wrote:

Dear Tom,

Phil Jones, who is sitting next to me here in southern Spain and also checking email, explained what you are working on and it sounds like a potentially very insightful analysis. I wish you luck.

Viva Branson (cc'd) is our new/improved keeper of ERSST. We sometimes refer to her as Dick Reynolds version b (Dick is cc'd as well). She will be able to answer your questions more accurately than I. But if I recall correctly from talking to them Monday, to avoid confusion they are trying to only make the latest (and therefore best) version of ERSST available. So the version you downloaded should be 3b. But Viva can verify this for you. I don't know which reference is currently the recommended one to use.

A decade ago, NCDC did a global land analysis and a global ocean analysis and then combined them with a weighting of 30/70. This could also arguably be the most accurate way to combine spatially incomplete data so that the world is not inappropriately weighted more towards the ocean than land (which tends to have larger gaps). Once we used Tom Smith's more spatially complete analysis, we went with a simple global average. While the data are more spatially complete, they are not complete. Data are set to missing over sea ice, much of the world north of 75N and Antarctica (Viva and I are currently reevaluating options for those last two).

ERSST is updated monthly. The SST portion is already updated for September and the land portion will wait another week or so for more data to come in. (I realize I've been assuming you are using ERSST as shorthand for NCDC's merged land/ocean data set, equivalent to HadCRU - if you're only asking about SSTs, Viva and Dick are the people to ask.)

The base period used for calculation of anomalies from the grid box mean of ERSST is, I believe, the 30 years 1961-90 (as that had the most data). So if you are using a gridded field, that is the relevant number - though Viva can verify my memory on the dates). But when we make global averaged temperature time series, we adjust the time series up or down so that the zero line is the mean of 1900-1999.

Viva, Dick, do you have anything to add (or correct)?

Tom, I've also cc'd Deke Arndt, the head of our Climate Monitoring Branch because if you find this confusing, he will probably want to make sure the web pages you read are made clearer.

Regards,
Tom P.

----- Original Message -----

From: Tom Wigley [3]<wigley@ucar.edu>

Date: Thursday, October 8, 2009 2:16 am

Subject: ERSST

Dear Toms,

Could you please clarify a few things for me ...

(1) Is the currently downloadable ERSST data version 3, or 3b?

It seems to be 3b -- but the web page is not entirely clear.

In one place it says that v.3 will be used from July, but elsewhere it says 3b will be used from July.

If it is v.3b, then does this mean that the Smith et al. reference is not (quite) appropriate?

(2) Is ERSST spatially complete? I think not. If it were, then $(NH+SH)/2$ should equal GL, but this is not the case. I'm sure you know that HadCRU uses $(NH+SH)/2$ for the global mean (arguably superior to a straight global area average). It seems odd that this issue has been glossed over.

(3) How often will ERSST be updated? I presume you are aware that HadCRU updates annually to get the late data in. It seems that ERSST only updates with new numbered versions -- so it misses late data. (GISS is worse.)

(4) What is the reference period? I think I saw somewhere on the web page that it is 1900-99? But methodologically perhaps it is difficult to define a reference period?

Thanks,
Tom

References

1. <mailto:Viva.Banzon@noaa.gov>
2. <mailto:Thomas.C.Peterson@noaa.gov>
3. <mailto:wigley@ucar.edu>

From: Ben Santer <santer1@llnl.gov>
To: Stephen H Schneider <shs@stanford.edu>
Subject: [Fwd: Re: CEI formal petition to derail EPA GHG endangerment finding with charge that destruction of CRU raw data undermines integrity of global temperature record]
Date: Fri, 09 Oct 2009 09:32:52 -0700
Reply-to: santer1@llnl.gov
Cc: "Kevin E. Trenberth" <trenbert@ucar.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, mann <mann@psu.edu>, Stefan Rahmstorf <rahmstorf@ozean-klima.de>, Tom Wigley <wigley@cgd.ucar.edu>, "Philip D. Jones" <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>

<x-flowed>
Dear Steve,

I was made aware of this yesterday (see forwarded email).

Best regards,

Ben

Benjamin D. Santer
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</x-flowed>

X-Account-Key: account1
X-Mozilla-Keys:
Return-Path: <santer1@llnl.gov>
Received: from mail-2.llnl.gov (unix socket)
by mail-2.llnl.gov (Cyrus v2.2.12) with LMTPA;
Thu, 08 Oct 2009 18:28:44 -0700
Received: from nspiron-1.llnl.gov (nspiron-1.llnl.gov [128.115.41.81])
by mail-2.llnl.gov (8.13.1/8.12.3/LLNL evision: 1.7 \$) with ESMTP id n991Sh62016185;
Thu, 8 Oct 2009 18:28:43 -0700
X-Attachments: None
Received: from dione.llnl.gov ([128.115.57.29])
by nspiron-1.llnl.gov with ESMTP; 08 Oct 2009 18:28:44 -0700
Message-ID: <4ACE91CA.7000006@llnl.gov>
Date: Thu, 08 Oct 2009 18:28:42 -0700
From: Ben Santer <santer1@llnl.gov>
Reply-To: santer1@llnl.gov
Organization: LLNL
User-Agent: Thunderbird 2.0.0.22 (X11/20090605)
MIME-Version: 1.0
To: Rick Piltz <piltz@comcast.net>
CC: Tom Wigley <wigley@ucar.edu>, Tom Karl <Thomas.R.Karl@noaa.gov>,
Jim Hansen <jeh1@columbia.edu>,
Bob Watson <robert.watson@defra.gsi.gov.uk>,
Mike MacCracken <mmaccrac@comcast.net>,
"John F. B. Mitchell" <john.f.mitchell@metoffice.gov.uk>
Subject: Re: CEI formal petition to derail EPA GHG endangerment finding with
charge that destruction of CRU raw data undermines integrity of global temperature
record
References: <80955b\$27nkli@smtp.llnl.gov>
In-Reply-To: <80955b\$27nkli@smtp.llnl.gov>
Content-Type: text/plain; charset=ISO-8859-1; format=flowed
Content-Transfer-Encoding: 7bit

<x-flowed>
Dear Rick,

I am prepared to help in any way that I can.

As I see it, there are two key issues here.

First, the CEI and Pat Michaels are arguing that Phil Jones and colleagues at the Climatic Research Unit (CRU) willfully and intentionally "destroyed" some of the raw surface temperature data used in the construction of the gridded surface temperature datasets.

Second, the CEI and Pat Michaels contend that the CRU surface temperature datasets provided the sole basis for IPCC "discernible human influence" conclusions.

Both of these arguments are factually incorrect. First, there was no intentional destruction of the primary source data. I am sure that, over 20 years ago, Phil could not have foreseen that the raw station data might be the subject of legal proceedings by the CEI and Pat Michaels. Raw data were NOT secretly destroyed to avoid efforts by other scientists to replicate the CRU and Hadley Centre-based estimates of global-scale changes in near-surface temperature. In fact, a key point here is that other groups (primarily at NCDC and at GISS, but also in Russia) WERE able to replicate the major findings of the CRU and Hadley Centre groups. The NCDC and GISS groups performed this replication completely independently. They made different choices in the complex process of choosing input data, adjusting raw station data for known inhomogeneities (such as urbanization effects, changes in instrumentation, site location, and observation time), and gridding procedures. NCDC and GISS-based estimates of global surface temperature changes are in good accord with the HadCRUT results.

I'm sure that Pat Michaels does not have the primary source data used in his Ph.D. thesis. Perhaps one of us should request the datasets used in Michaels' Ph.D. work, and then ask the University of Wisconsin to withdraw Michaels' Ph.D. if he fails to produce every dataset and computer program used in the course of his thesis research.

I'm equally sure that John Christy and Roy Spencer have not preserved every single version of their MSU-based estimates of tropospheric temperature change. Nor is it likely that Christy and Spencer have preserved for posterity each and every computer program they used to generate UAH tropospheric temperature datasets.

[One irony here is that the Christy/Spencer claim that the troposphere had cooled over the satellite era did not stand up to rigorous scientific scrutiny. Christy and Spencer have made a scientific career out of being wrong. In contrast, CRU's claim of a pronounced increase in global-mean surface temperature over the 20th century HAS withstood the test of time.]

The CEI and Michaels are applying impossible legal standards to science. They are essentially claiming that if we do not retain - and make available to self-appointed auditors - every piece of information about every scientific paper we have ever published, we are perpetrating some vast deception on the American public. I think most ordinary citizens understand that few among us have preserved every bank statement and every utility bill we've received in the last 20 years.

The second argument - that "discernible human influence" findings are like a house of cards, resting solely on one observational dataset - is also invalid. The IPCC Third Assessment Report (TAR) considers MULTIPLE observational estimates of global-scale near-surface temperature changes. It does not rely on HadCRUT data alone - as is immediately obvious from Figure 2.1b of the TAR, which shows CRU, NCDC, and GISS global-mean temperature changes.

As pointed out in numerous scientific assessments (e.g., the IPCC TAR and Fourth Assessment Reports, the U.S. Climate Change Science Program Synthesis and Assessment Report 1.1, and the CCSP "State of Knowledge" Report), rigorous statistical fingerprint studies have now been performed with a whole range of climate variables - and not with surface temperature only. Examples include variables like ocean heat content, atmospheric water vapor, surface specific humidity, continental river runoff, sea-level pressure patterns, stratospheric and tropospheric temperature, tropopause height, zonal-mean precipitation over land, and Arctic sea-ice extent. The bottom-line message from this body of work is that natural causes alone CANNOT plausibly explain the climate changes we have actually observed. The climate system is telling us an internally- and physically-consistent story. The integrity and reliability of this story does NOT rest on a single observational dataset, as Michaels and the CEI incorrectly claim.

Michaels should and does know better. I can only conclude from his behavior - and from his participation in this legal action - that he is

being intentionally dishonest. His intervention seems to be timed to influence opinion in the run-up to the Copenhagen meeting, and to garner publicity for himself. In my personal opinion, Michaels should be kicked out of the AMS, the University of Virginia, and the scientific community as a whole. He cannot on the one hand engage in vicious public attacks on the reputations of individual scientists (in the past he has attacked Tom Karl, Tom Wigley, Jim Hansen, Mike Mann, myself, and numerous others), and on the other hand expect to be treated as a valued member of our professional societies.

The sad thing here is that Phil Jones is one of the true gentlemen of our field. I have known Phil for most of my scientific career. He is the antithesis of the secretive, "data destroying" character the CEI and Michaels are trying to portray to the outside world. Phil and Tom Wigley have devoted significant portions of their scientific careers to the construction of the land surface temperature component of the HadCRUT dataset. They have conducted this research in a very open and transparent manner - examining sensitivities to different gridding algorithms, different ways of adjusting for urbanization effects, use of various subsets of data, different ways of dealing with changes in spatial coverage over time, etc. They have thoroughly and comprehensively documented all of their dataset construction choices. They have done a tremendous service to the scientific community - and to the planet - by making gridded surface temperature datasets available for scientific research. They deserve medals as big as soup plates - not the kind of crap they are receiving from Pat Michaels and the CEI.

The bottom line, Rick, is that I am incensed at the "data destruction" allegations that are being unfairly and incorrectly leveled against Phil and Tom by the CEI and Pat Michaels. Please let me know how you think I can be most effective in rebutting such allegations. Whatever you need from me - you've got it.

I hope you don't mind, but I'm also copying my email to John Mitchell at the Hadley Centre. I know that John also feels very strongly about these issues.

With best regards,

Ben

Rick Piltz wrote:

> Gentlemen--

>

> I expect that you have already been made aware of the petition to EPA
> from the Competitive Enterprise Institute (and Pat Michaels) calling for
> a re-opening of public comment on EPA's prospective "endangerment"
> finding on greenhouse gases. CEI is charging that the CRU at East Anglia
> has destroyed the raw data for a portion of the global temperature
> record, thus destroying the integrity of the IPCC assessments and any
> other work that treats the UK Jones-Wigley global temperature data
> record as scientifically legitimate. I have attached the petition in
> PDF, with a statements by CEI and Michaels.

>

> The story was reported in Environment & Energy Daily yesterday (below).
> They called me for it, presumably because I am on their call list as
> someone who gets in the face of the global warming disinformation
> campaign, among other things. I hit CEI, but I don't have a technical
> response to their allegations.

>

> Who is responding to this charge on behalf of the science community?
> Surely someone will have to, if only because EPA will need to know
> exactly what to say. And really I believe all of you, as the
> authoritative experts, should be prepared to do that in a way that has
> some collective coherence.

>

> I am going to be writing about this on my Climate Science Watch Website
> as soon as I think I can do so appropriately. I am most interested in
> what you have to say to set the record straight and put things in
> perspective -- either on or off the record, whichever you wish. Will
> someone please explain this to me?

>

> Best regards,

> Rick

>
>
> *1. CLIMATE: Free-market group attacks data behind EPA
> 'endangerment' proposal (E&E News PM, 10/07/2009)
>
> *
>
>
> *Robin Bravender, E&E reporter*
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> The Competitive Enterprise Institute -- a vocal foe of EPA's efforts to
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> been destroyed and that the available data are therefore unreliable.
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> University of East Anglia in Norwich, England, that includes surface
> temperature averages from weather stations around the world. *According
> to CEI, the data provided a foundation for the 1996 second assessment
> report by the Intergovernmental Panel on Climate Change, which EPA used
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> multiple sources for some sites, only the station series after
> adjustment for homogeneity issues. We, therefore, do not hold the
> original raw data but only the value-added (i.e. quality controlled and
> homogenized) data."
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> CEI general counsel Sam Kazman said this lack of raw data calls the
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> the most important regulatory issue we face."
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> *In a statement filed with CEI's petition, Cato Institute senior fellow
> Patrick Michaels called the development a "totally new element" in the
> endangerment debate. "It violates basic scientific principles and throws
> even more doubt onto the contention that anthropogenic greenhouse gas
> emissions endanger human welfare," he wrote.
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> *Michaels is a University of Virginia professor and author of the book,
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> down from his post as Virginia's state climatologist in 2007 after he
> came under fire for publicly doubting global warming while taking money
> from the utility industry (/ Greenwire/
> <<http://www.eenews.net/Greenwire/2007/09/27/archive/9>>, Sept. 27, 2007).
>
> Representatives of East Anglia University's Climatic Research Unit were
> not available to comment on the CEI petition.
>
> EPA spokeswoman Adora Andy said the agency will evaluate the petition.
> "But after initial review of the statement their position rests upon,"
> Andy added, "it certainly does not appear to justify upheaval."
>
> The petition is the latest in a string of CEI challenges to the
> proceedings surrounding the endangerment finding and other Obama
> administration climate policies. Last week, the group threatened to sue
> the administration over documents related to the costs of a federal
> cap-and-trade program to curb greenhouse gas emissions. And in June, the
> group accused EPA officials of suppressing dissenting views from an EPA
> environmental economist during the run-up to the release of the
> endangerment proposal.
>
> Rick Piltz, director of the watchdog group Climate Science Watch and a
> former official at the U.S. Climate Change Science Program, said that

> although the research unit's data are among key data sets used by the
> IPCC, "it's not the only data set that they use." He also said EPA drew
> on "multifaceted, robust" data in the technical support document
> underlying the finding.
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> EPA's endangerment finding relies most heavily on IPCC's 2007 fourth
> assessment; synthesis and assessment products of the U.S. Climate Change
> Science Program; National Research Council reports under the U.S.
> National Academy of Sciences; the EPA annual report on U.S. greenhouse
> gas emission inventories; and the EPA assessment of the effects of
> global change on regional U.S. air quality, according to the agency's
> technical support document.
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> "You do not need to reopen the IPCC reports and the technical support
> document on the EPA endangerment finding because of something having to
> do with the raw data from the temperature record from East Anglia
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> they use science, they use it tactically, and they will go to war with
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> public comment period on the endangerment finding to investigate the
> scientific merit of the research data.
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> its work, refuses to be transparent with the public about the most
> consequential rulemaking of our time," said Sen. James Inhofe (R-Okla.),
> ranking member of the Environment and Public Works Committee. Inhofe
> sent a joint press release with Sen. John Barrasso (R-Wyo.) accusing EPA
> of relying upon flawed data.
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> EPA's assertions can't engage in basic scientific work, such as assuring
> reproducibility and objectivity, because the data they seek have been
> destroyed," Inhofe said. "In order to conform to federal law and basic
> standards of scientific integrity, EPA must reopen the record so the
> public can judge whether EPA's claims are based on the best available
> scientific information."
>
> Rick Piltz
> Director, Climate Science Watch
> 301-807-2472
> www.*climatesciencewatch.org
>
> <http://www.*climatesciencewatch.org/>Climate Science Watch is a
> sponsored project of the Government Accountability Project, Washington,
> DC, dedicated to holding public officials accountable for using climate
> science and related research effectively and with integrity in
> responding to the challenges posed by global climate disruption.
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> The right to search for truth implies also a duty; one must not conceal
> any part of what one has recognized to be true.
> --Albert Einstein
>

--

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FAX: (925) 422-7675
email: santer1@llnl.gov

</x-flowed>

From: Ben Santer <santer1@llnl.gov>

To: P.Jones@uea.ac.uk

Subject: Re: CEI formal petition to derail EPA GHG endangerment finding with charge that destruction of CRU raw data undermines integrity of global temperature record

Date: Fri, 09 Oct 2009 11:07:56 -0700

Reply-to: santer1@llnl.gov

<x-flowed>

Dear Phil,

I've known Rick Piltz for many years. He's a good guy. I believe he used to work with Mike MacCracken at the U.S. Global Change Research Program.

I'm really sorry that you have to go through all this stuff, Phil. Next time I see Pat Michaels at a scientific meeting, I'll be tempted to beat the crap out of him. Very tempted.

I'll help you to deal with Michaels and the CEI in any way that I can. The only reason these guys are going after you is because your work is of crucial importance - it changed the way the world thinks about human effects on climate. Your work mattered in the 1980s, and it matters now.

With best wishes,

Ben

P.Jones@uea.ac.uk wrote:

> Ben,
> Thanks for backing me up with whoever Rick is. I forwarded the message
> to Rick. So if you want to add anything else feel free to do so.
> We have more stations going into the latest CRU data than we did in the
> 1980s.
>
> In Lecce next week for 2 days at a GKSS summer school led by Hans VS!
>
> Cheers
> Phil
>
>> Dear Rick,
>>
>> I am prepared to help in any way that I can.
>>
>> As I see it, there are two key issues here.
>>
>> First, the CEI and Pat Michaels are arguing that Phil Jones and

>> colleagues at the Climatic Research Unit (CRU) willfully and
>> intentionally "destroyed" some of the raw surface temperature data used
>> in the construction of the gridded surface temperature datasets.
>>
>> Second, the CEI and Pat Michaels contend that the CRU surface
>> temperature datasets provided the sole basis for IPCC "discernible human
>> influence" conclusions.
>>
>> Both of these arguments are factually incorrect. First, there was no
>> intentional destruction of the primary source data. I am sure that, over
>> 20 years ago, Phil could not have foreseen that the raw station data
>> might be the subject of legal proceedings by the CEI and Pat Michaels.
>> Raw data were NOT secretly destroyed to avoid efforts by other
>> scientists to replicate the CRU and Hadley Centre-based estimates of
>> global-scale changes in near-surface temperature. In fact, a key point
>> here is that other groups (primarily at NCDC and at GISS, but also in
>> Russia) WERE able to replicate the major findings of the CRU and Hadley
>> Centre groups. The NCDC and GISS groups performed this replication
>> completely independently. They made different choices in the complex
>> process of choosing input data, adjusting raw station data for known
>> inhomogeneities (such as urbanization effects, changes in
>> instrumentation, site location, and observation time), and gridding
>> procedures. NCDC and GISS-based estimates of global surface temperature
>> changes are in good accord with the HadCRUT results.
>>
>> I'm sure that Pat Michaels does not have the primary source data used in
>> his Ph.D. thesis. Perhaps one of us should request the datasets used in
>> Michaels' Ph.D. work, and then ask the University of Wisconsin to
>> withdraw Michaels' Ph.D. if he fails to produce every dataset and
>> computer program used in the course of his thesis research.
>>
>> I'm equally sure that John Christy and Roy Spencer have not preserved
>> every single version of their MSU-based estimates of tropospheric
>> temperature change. Nor is it likely that Christy and Spencer have
>> preserved for posterity each and every computer program they used to
>> generate UAH tropospheric temperature datasets.
>>
>> [One irony here is that the Christy/Spencer claim that the troposphere
>> had cooled over the satellite era did not stand up to rigorous
>> scientific scrutiny. Christy and Spencer have made a scientific career
>> out of being wrong. In contrast, CRU's claim of a pronounced increase in
>> global-mean surface temperature over the 20th century HAS withstood the
>> test of time.]
>>

>> The CEI and Michaels are applying impossible legal standards to science.
>> They are essentially claiming that if we do not retain - and make
>> available to self-appointed auditors - every piece of information about
>> every scientific paper we have ever published, we are perpetrating some
>> vast deception on the American public. I think most ordinary citizens
>> understand that few among us have preserved every bank statement and
>> every utility bill we've received in the last 20 years.

>>
>> The second argument - that "discernible human influence" findings are
>> like a house of cards, resting solely on one observational dataset - is
>> also invalid. The IPCC Third Assessment Report (TAR) considers MULTIPLE
>> observational estimates of global-scale near-surface temperature
>> changes. It does not rely on HadCRUT data alone - as is immediately
>> obvious from Figure 2.1b of the TAR, which shows CRU, NCDC, and GISS
>> global-mean temperature changes.

>>
>> As pointed out in numerous scientific assessments (e.g., the IPCC TAR
>> and Fourth Assessment Reports, the U.S. Climate Change Science Program
>> Synthesis and Assessment Report 1.1, and the CCSP "State of Knowledge"
>> Report), rigorous statistical fingerprint studies have now been
>> performed with a whole range of climate variables - and not with surface
>> temperature only. Examples include variables like ocean heat content,
>> atmospheric water vapor, surface specific humidity, continental river
>> runoff, sea-level pressure patterns, stratospheric and tropospheric
>> temperature, tropopause height, zonal-mean precipitation over land, and
>> Arctic sea-ice extent. The bottom-line message from this body of work is
>> that natural causes alone CANNOT plausibly explain the climate changes
>> we have actually observed. The climate system is telling us an
>> internally- and physically-consistent story. The integrity and
>> reliability of this story does NOT rest on a single observational
>> dataset, as Michaels and the CEI incorrectly claim.

>>
>> Michaels should and does know better. I can only conclude from his
>> behavior - and from his participation in this legal action - that he is
>> being intentionally dishonest. His intervention seems to be timed to
>> influence opinion in the run-up to the Copenhagen meeting, and to garner
>> publicity for himself. In my personal opinion, Michaels should be kicked
>> out of the AMS, the University of Virginia, and the scientific community
>> as a whole. He cannot on the one hand engage in vicious public attacks
>> on the reputations of individual scientists (in the past he has attacked
>> Tom Karl, Tom Wigley, Jim Hansen, Mike Mann, myself, and numerous
>> others), and on the other hand expect to be treated as a valued member
>> of our professional societies.

>>

>> The sad thing here is that Phil Jones is one of the true gentlemen of
>> our field. I have known Phil for most of my scientific career. He is the
>> antithesis of the secretive, "data destroying" character the CEI and
>> Michaels are trying to portray to the outside world. Phil and Tom Wigley
>> have devoted significant portions of their scientific careers to the
>> construction of the land surface temperature component of the HadCRUT
>> dataset. They have conducted this research in a very open and
>> transparent manner - examining sensitivities to different gridding
>> algorithms, different ways of adjusting for urbanization effects, use of
>> various subsets of data, different ways of dealing with changes in
>> spatial coverage over time, etc. They have thoroughly and
>> comprehensively documented all of their dataset construction choices.
>> They have done a tremendous service to the scientific community - and to
>> the planet - by making gridded surface temperature datasets available
>> for scientific research. They deserve medals as big as soup plates - not
>> the kind of crap they are receiving from Pat Michaels and the CEI.

>>
>> The bottom line, Rick, is that I am incensed at the "data destruction"
>> allegations that are being unfairly and incorrectly leveled against Phil
>> and Tom by the CEI and Pat Michaels. Please let me know how you think I
>> can be most effective in rebutting such allegations. Whatever you need
>> from me - you've got it.

>>
>> I hope you don't mind, but I'm also copying my email to John Mitchell at
>> the Hadley Centre. I know that John also feels very strongly about these
>> issues.

>>
>> With best regards,

>>
>> Ben

>>
>> Rick Piltz wrote:

>>> Gentlemen--

>>>
>>> I expect that you have already been made aware of the petition to EPA
>>> from the Competitive Enterprise Institute (and Pat Michaels) calling for
>>> a re-opening of public comment on EPA's prospective "endangerment"
>>> finding on greenhouse gases. CEI is charging that the CRU at East Anglia
>>> has destroyed the raw data for a portion of the global temperature
>>> record, thus destroying the integrity of the IPCC assessments and any
>>> other work that treats the UK Jones-Wigley global temperature data
>>> record as scientifically legitimate. I have attached the petition in
>>> PDF, with a statements by CEI and Michaels.

>>>

>>> The story was reported in Environment & Energy Daily yesterday (below).
>>> They called me for it, presumably because I am on their call list as
>>> someone who gets in the face of the global warming disinformation
>>> campaign, among other things. I hit CEI, but I don't have a technical
>>> response to their allegations.

>>>
>>> Who is responding to this charge on behalf of the science community?
>>> Surely someone will have to, if only because EPA will need to know
>>> exactly what to say. And really I believe all of you, as the
>>> authoritative experts, should be prepared to do that in a way that has
>>> some collective coherence.

>>>
>>> I am going to be writing about this on my Climate Science Watch Website
>>> as soon as I think I can do so appropriately. I am most interested in
>>> what you have to say to set the record straight and put things in
>>> perspective -- either on or off the record, whichever you wish. Will
>>> someone please explain this to me?

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>>> Best regards,
>>> Rick

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>>> *1. CLIMATE: Free-market group attacks data behind EPA
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>>> *Robin Bravender, E&E reporter*

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>>> behind U.S. EPA's proposed finding that greenhouse gases endanger human
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>>> University of East Anglia in Norwich, England, that includes surface
>>> temperature averages from weather stations around the world. *According

>>> to CEI, the data provided a foundation for the 1996 second assessment
>>> report by the Intergovernmental Panel on Climate Change, which EPA used
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>>> availability in the 1980s meant that we were not able to keep the
>>> multiple sources for some sites, only the station series after
>>> adjustment for homogeneity issues. We, therefore, do not hold the
>>> original raw data but only the value-added (i.e. quality controlled and
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>>> endangerment finding into question. *"EPA is resting its case on
>>> international studies that in turn relied on CRU data. But CRU's
>>> suspicious destruction of its original data, disclosed at this late
>>> date, makes that information totally unreliable," he said.* "If EPA
>>> doesn't re-examine the implications of this, it's stumbling blindly into
>>> the most important regulatory issue we face."

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>>> Patrick Michaels called the development a "totally new element" in the
>>> endangerment debate. "It violates basic scientific principles and throws
>>> even more doubt onto the contention that anthropogenic greenhouse gas
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>>> not available to comment on the CEI petition.

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>>> EPA spokeswoman Adora Andy said the agency will evaluate the petition.
>>> "But after initial review of the statement their position rests upon,"
>>> Andy added, "it certainly does not appear to justify upheaval."

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>>> The petition is the latest in a string of CEI challenges to the
>>> proceedings surrounding the endangerment finding and other Obama
>>> administration climate policies. Last week, the group threatened to sue
>>> the administration over documents related to the costs of a federal
>>> cap-and-trade program to curb greenhouse gas emissions. And in June, the

>>> group accused EPA officials of suppressing dissenting views from an EPA
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>>> technical support document.

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>>> "You do not need to reopen the IPCC reports and the technical support
>>> document on the EPA endangerment finding because of something having to
>>> do with the raw data from the temperature record from East Anglia
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>>> vets its data.

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>>> finalize the endangerment finding and is "grasping at straws" by
>>> challenging the IPCC data.

>>>

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>>> they use science, they use it tactically, and they will go to war with
>>> the mainstream science community."

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>>> public comment period on the endangerment finding to investigate the
>>> scientific merit of the research data.

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>>> consequential rulemaking of our time," said Sen. James Inhofe (R-Okla.),
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>>> of relying upon flawed data.

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>>> "Now the evidence shows that scientists interested in testing some of
>>> EPA's assertions can't engage in basic scientific work, such as assuring
>>> reproducibility and objectivity, because the data they seek have been
>>> destroyed," Inhofe said. "In order to conform to federal law and basic
>>> standards of scientific integrity, EPA must reopen the record so the
>>> public can judge whether EPA's claims are based on the best available
>>> scientific information."

>>>

>>> Rick Piltz

>>> Director, Climate Science Watch

>>> 301-807-2472

>>> www.**climatesciencewatch.org

>>>

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>>> DC, dedicated to holding public officials accountable for using climate
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>>> any part of what one has recognized to be true.

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>>>

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>> --

>> -----

>> Benjamin D. Santer

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>> Livermore, CA 94550, U.S.A.

>> Tel: (925) 422-3840

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>> email: santer1@llnl.gov

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From: P.Jones@uea.ac.uk
To: "Rick Piltz" <piltz@comcast.net>
Subject: Re: Your comments on the latest CEI/Michaels gambit
Date: Sun, 11 Oct 2009 18:03:13 +0100 (BST)
Cc: "Phil Jones" <p.jones@uea.ac.uk>, "Ben Santer" <santer1@llnl.gov>

Rick,

What you've put together seems fine from a quick read. I'm in Lecce in the heal of Italy till Tuesday. I should be back in the UK by Wednesday.

The original raw data are not lost either. I could reconstruct what we had from some DoE reports we published in the mid-1980s. I would start with the GHCN data. I know that the effort would be a complete wate of time though. I may get around to it some time. As you've said, the documentation of what we've done is all in the literature.

I think if it hadn't been this issue, the CEI would have dreamt up something else!

Cheers
Phil

> Phil and Ben--
>
> Thanks for writing. I appreciate very much what you're saying.
>
> I'm going to be posting some entries on this matter on the Climate
> Science Watch Web site. I'm sure others will weigh in on it in
> various venues (Steve Schneider has supplied me with an on-the-record
> quote), and I suppose that a more formal response by the relevant
> scientists is likely eventually to become part of the EPA docket as
> part of their rejection of the CEI petition. But that will drag on,
> and meanwhile CEI and Michaels will demagogue their allegations, as
> they do with everything. No way to prevent that. But I would like to
> expedite documenting some immediate pushback, helping to set the
> record straight and put what CEI and Michaels are up to in perspective.
>
> I have taken the liberty of editing what you wrote just a bit (and
> adding some possible URL links and writing-out of acronyms), in the
> hope that, with your permission and with any revisions or additions
> you might care to make, we could post your comments. This requires

> no clearance other than you and me. I would draft appropriate text to
> provide context. Please take a look at this and RSVP:
>
> Ben's comment:
>
> As I see it, there are two key issues here.
>
> First, the Competitive Enterprise Institute (CEI) and Pat Michaels
> are arguing that Phil Jones and colleagues at the CRU [Climatic
> Research Unit at the University of East Anglia, UK] willfully,
> intentionally, and suspiciously "destroyed" some of the raw surface
> temperature data used in the construction of the gridded surface
> temperature datasets.
>
> Second, the CEI and Pat Michaels contend that the CRU surface
> temperature datasets provided the sole basis for IPCC "discernible
> human influence" conclusions.
>
> Both of these arguments are incorrect. First, there was no
> intentional destruction of the primary source data. I am sure that,
> over 20 years ago, the CRU could not have foreseen that the raw
> station data might be the subject of legal proceedings by the CEI and
> Pat Michaels. Raw data were NOT secretly destroyed to avoid efforts
> by other scientists to replicate the CRU and Hadley Centre-based
> estimates of global-scale changes in near-surface temperature. In
> fact, a key point here is that other groups -- primarily at the NCDC
> [NOAA National Climatic Data Center] and at GISS [NASA Goddard
> Institute for Space Studies], but also in Russia -- WERE able to
> replicate the major findings of the CRU and UK Hadley Centre groups.
> The NCDC and GISS groups performed this replication completely
> independently. They made different choices in the complex process of
> choosing input data, adjusting raw station data for known
> inhomogeneities (such as urbanization effects, changes in
> instrumentation, site location, and observation time), and gridding
> procedures. NCDC and GISS-based estimates of global surface
> temperature changes are in good accord with the HadCRUT data results.
>
> The second argument -- that "discernible human influence" findings
> are like a house of cards, resting solely on one observational
> dataset -- is also invalid. The IPCC Third Assessment Report (TAR)
> considers MULTIPLE observational estimates of global-scale
> near-surface temperature changes. It does not rely on HadCRUT data
> alone - as is immediately obvious from Figure 2.1b of the TAR, which

> shows CRU, NCDC, and GISS global-mean temperature changes.
>
> As pointed out in numerous scientific assessments (e.g., the IPCC TAR
> and Fourth Assessment Reports, the U.S. Climate Change Science
> Program Synthesis and Assessment Report 1.1 (Temperature trends in
> the lower atmosphere: steps for understanding and reconciling
> differences), and the state of knowledge report, Global Climate
> Change Impacts on the United States, rigorous statistical fingerprint
> studies have now been performed with a whole range of climate
> variables -- and not with surface temperature only. Examples include
> variables like ocean heat content, atmospheric water vapor, surface
> specific humidity, continental river runoff, sea-level pressure
> patterns, stratospheric and tropospheric temperature, tropopause
> height, zonal-mean precipitation over land, and Arctic sea-ice
> extent. The bottom-line message from this body of work is that
> natural causes alone CANNOT plausibly explain the climate changes we
> have actually observed. The climate system is telling us an
> internally- and physically-consistent story. The integrity and
> reliability of this story does NOT rest on a single observational
> dataset, as Michaels and the CEI incorrectly claim.
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> antithesis of the secretive, "data destroying" character the CEI and
> Michaels are trying to portray to the outside world. Phil and Tom
> Wigley have devoted significant portions of their scientific careers
> to the construction of the land surface temperature component of the
> HadCRUT dataset. They have conducted this research in a very open and
> transparent manner -- examining sensitivities to different gridding
> algorithms, different ways of adjusting for urbanization effects, use
> of various subsets of data, different ways of dealing with changes in
> spatial coverage over time, etc. They have thoroughly and
> comprehensively documented all of their dataset construction choices.
> They have done a tremendous service to the scientific community --
> and to the planet -- by making gridded surface temperature datasets
> available for scientific research. They deserve medals -- not the
> kind of deliberately misleading treatment they are receiving from Pat
> Michaels and the CEI.
>
>
> Phil's comment:
>
> No one, it seems, cares to read what we put up on the CRU web page.
> These people just make up motives for what we might or might not have

> done.
> <<http://www.cru.uea.ac.uk/cru/data/temperature/>><http://www.cru.uea.ac.uk/cru/data/temperature/>
>
> Almost all the data we have in the CRU archive is exactly the same as
> in the GHCN archive [Global Historical Climatology Network, used by
> the NOAA National Climate Data Center].
> <http://www.ncdc.noaa.gov/oa/climate/ghcn-monthly/index.php>
> <http://www.ncdc.noaa.gov/oa/climate/research/ghcn/ghcngrid.html>
>
> If we have lost any data it is the following:
>
> 1. Station series for sites that in the 1980s we deemed then to be
> affected by either urban biases or by numerous site moves, that were
> either not correctable or not worth doing as there were other series
> in the region.
>
> 2. The original data for sites that we adjusted the temperature data
> [Phil: for known inhomogeneities, or what?] in the 1980s. We still
> have our adjusted data, of course, and these along with all other
> sites that didn't need adjusting.
>
> 3. Since the 1980s as colleagues and NMSs [National Meteorological
> Services] have produced adjusted series for regions and or countries,
> then we replaced the data we had with the better series.
> http://www.wmo.int/pages/members/index_en.html
>
> In the papers, I've always said that homogeneity adjustments are best
> produced by NMSs. A good example of this is the work by Lucie Vincent
> in Canada. Here we just replaced what data we had for the 200+ sites
> she sorted out.
>
> The CRUTEM3 data for land look much like the GHCN and GISS [NASA
> Goddard Institute for Space Studies] data for the same domains.
> <http://data.giss.nasa.gov/gistemp/>
>
> Apart from a figure in the IPCC AR4 [Fourth Assessment Report, 2007]
> showing this, there is also this paper from Geophysical Research
> Letters in 2005 by Russ Vose et al. Figure 2 is similar to the AR4 plot.
> [Vose et al paper]
>
> All best,
> Rick
>

>
> Rick Piltz
> Director, Climate Science Watch
> 301-807-2472
> www.climate-science-watch.org

>
> Climate Science Watch is a sponsored project of the Government
> Accountability Project, Washington, DC, dedicated to holding public
> officials accountable for using climate science and related research
> effectively and with integrity in responding to the challenges posed
> by global climate disruption.

>
> The right to search for truth implies also a duty; one must not
> conceal any part of what one has recognized to be true.
> --Albert Einstein

>

From: Stephen H Schneider <shs@stanford.edu>
To: Myles Allen <allen@atm.ox.ac.uk>, peter stott
<peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>,
Benjamin Santer <santer1@llnl.gov>, Tom Wigley <wigley@ucar.edu>, Thomas
R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>,
James Hansen <jhansen@giss.nasa.gov>, trenbert <trenbert@ucar.edu>,
Michael Mann <mann@meteo.psu.edu>, Michael Oppenheimer
<omichael@Princeton.EDU>
Subject: Fwd: BBC U-turn on climate
Date: Sun, 11 Oct 2009 23:32:11 -0700 (PDT)

Hi all. Any of you want to explain decadal natural variability and
signal to noise and
sampling errors to this new "IPCC Lead Author" from the BBC? As we
enter an El Nino year
and as soon, as the sunspots get over their temporary--presumed--
vacation worth a few
tenths of a Watt per meter squared reduced forcing, there will likely
be another dramatic
upward spike like 1992-2000. I heard someone--Mike Schlesinger
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alot of money on it happening in next 5 years?? Meanwhile the past 10
years of global mean
temperature trend stasis still saw what, 9 of the warmest in
reconstructed 1000 year record
and Greenland and the sea ice of the North in big retreat?? Some of
you observational folks
probably do need to straighten this out as my student suggests below.
Such "fun", Cheers,

Steve
Stephen H. Schneider
Melvin and Joan Lane Professor for Interdisciplinary Environmental
Studies,
Professor, Department of Biology and
Senior Fellow, Woods Institute for the Environment
Mailing address:
Yang & Yamazaki Environment & Energy Building - MC 4205
473 Via Ortega
Ph: 650 725 9978
F: 650 725 4387
Websites: climatechange.net
patientfromhell.org

----- Forwarded Message -----
From: "Narasimha D. Rao" <ndrao@stanford.edu>
To: "Stephen H Schneider" <shs@stanford.edu>
Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00 US/Canada
Pacific
Subject: BBC U-turn on climate

Steve,

You may be aware of this already. Paul Hudson, BBCTs reporter on
climate change, on Friday

wrote that there's been no warming since 1998, and that pacific oscillations will force cooling for the next 20-30 years. It is not outrageously biased in presentation as are other skeptics' views.

<http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>

<http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>

BBC has significant influence on public opinion outside the US.

Do you think this merits an op-ed response in the BBC from a scientist?

Narasimha

PhD Candidate,

Emmett Interdisciplinary Program in Environment and Resources (E-IPER)
Stanford University

Tel: 415-812-7560

From: Kevin Trenberth <trenbert@ucar.edu>
To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: BBC U-turn on climate
Date: Mon, 12 Oct 2009 08:57:37 -0600
Cc: Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Tom Wigley <wigley@ucar.edu>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <comichael@Princeton.EDU>

Hi all
Well I have my own article on where the heck is global warming? We are asking that here in Boulder where we have broken records the past two days for the coldest days on record. We had 4 inches of snow. The high the last 2 days was below 30F and the normal is 69F, and it smashed the previous records for these days by 10F. The low was about 18F and also a record low, well below the previous record low. This is January weather (see the Rockies baseball playoff game was canceled on Saturday and then played last night in below freezing weather).

Trenberth, K. E. 2009. An imperative for climate change planning: tracking Earth's global energy. Current Opinion in Environmental Sustainability, 1, 19-27.
doi:10.1016/j.coust.2009.06.001. [1][PDF] (A PDF of the published version can be obtained from the author.)

The fact is that we can't account for the lack of warming at the moment and it is a travesty that we can't. The CERES data published in the August BAMS 09 supplement on 2008 shows there should be even more warming; but the data are surely wrong. Our observing system is inadequate.

That said there is a LOT of nonsense about the PDO. People like CPC are tracking PDO on a monthly basis but it is highly correlated with ENSO. Most of what they are seeing is the change in ENSO not real PDO. It surely isn't decadal. The PDO is already reversing with the switch to El Nino. The PDO index became positive in September for first time since Sept 2007. see

[2]http://www.cpc.ncep.noaa.gov/products/GODAS/ocean_briefing_gif/global_ocean_monitoring_current.ppt

Kevin

Michael Mann wrote:

extremely disappointing to see something like this appear on BBC. its particularly odd, since climate is usually Richard Black's beat at BBC (and he does a great job), from what I can tell, this guy was formerly a weather person at the Met Office.

We may do something about this on RealClimate, but meanwhile it might be appropriate for the Met Office to have a say about this, I might ask Richard Black what's up here?

mike

On Oct 12, 2009, at 2:32 AM, Stephen H Schneider wrote:

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Steve

Stephen H. Schneider

Melvin and Joan Lane Professor for Interdisciplinary Environmental Studies,

Professor, Department of Biology and

Senior Fellow, Woods Institute for the Environment

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Yang & Yamazaki Environment & Energy Building - MC 4205

473 Via Ortega

Ph: 650 725 9978

F: 650 725 4387

Websites: climatechange.net

pattenfromhell.org

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From: "Narasimha D. Rao" <[3]ndrao@stanford.edu>

To: "Stephen H Schneider" <[4]shs@stanford.edu>

Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00 US/Canada Pacific

Subject: BBC U-turn on climate

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[5]<http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>

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--

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Professor

Director, Earth System Science Center (ESSC)

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503 Walker Building FAX: (814) 865-3663

The Pennsylvania State University email: [7]mann@psu.edu

University Park, PA 16802-5013

website: [8]<http://www.meteo.psu.edu/~mann/Mann/index.html>
"Dire Predictions" book site:
[9]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Kevin E. Trenberth e-mail: [10]trenbert@ucar.edu
Climate Analysis Section, [11]www.egd.ucar.edu/cas/trenbert.html
NCAR
P. O. Box 3000, (303) 497 1318
Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

References

1. <http://www.egd.ucar.edu/cas/Trenberth/trenberth.papers/EnergyDiagnostics09final.pdf>
2. http://www.cpc.ncep.noaa.gov/products/GODAS/ocean_briefing_gif/global_ocean_monitoring_current.ppt
3. <mailto:ndrao@stanford.edu>
4. <mailto:shs@stanford.edu>
5. <http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>
6. <http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>
7. <mailto:mann@psu.edu>
8. <http://www.meteo.psu.edu/%7Emann/Mann/index.html>
9. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
10. <mailto:trenbert@ucar.edu>
11. <http://www.egd.ucar.edu/cas/trenbert.html>

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Cc: Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <pj.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Tom Wigley <wigley@ucar.edu>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, trenbert <trenbert@ucar.edu>, Michael Oppenheimer <omichael@Princeton.EDU>

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References

- Visible links
1. <mailto:ndrao@stanford.edu>
 2. <mailto:shs@stanford.edu>
 3. <http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>
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 6. <http://www.meteo.psu.edu/~mann/Mann/index.html>
 7. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

8. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Tom Wigley <wigley@ucar.edu>
To: Rick Piltz <piltz@comcast.net>
Subject: Re: FYI-"Phil Jones and Ben Santer respond to CEI and Pat Michaels attack on temperature data record"
Date: Tue, 13 Oct 2009 19:45:45 -0600
Cc: Thomas.R.Karl@noaa.gov, Jim Hansen <jhansen@giss.nasa.gov>, Steve Schneider <sks@stanford.edu>, Gavin Schmidt <gschmidt@giss.nasa.gov>, Kevin Trenberth <trenbert@ucar.edu>, Michael Mann <mann@meteo.psu.edu>, Stefan Rahmstorf <rahmstorf@ozean.klima.de>, Phil Jones <P.Jones@uea.ac.uk>, Ben Santer <santer1@llnl.gov>

<x-flowed>
Dear folks,

You may be interesting in this snippet of information about Pat Michaels. Perhaps the University of Wisconsin ought to open up a public comment period to decide whether Pat Michaels, PhD needs re-assessing?

Michaels' PhD was, I believe, supervised by Reid Bryson. It dealt with statistical (regression-based) modeling of crop-climate relationships. In his thesis, Michaels claims that his statistical model showed that weather/climate variations could explain 95% of the inter-annual variability in crop yields. Had this been correct, it would have been a remarkable results. Certainly, it was at odds with all previous studies of crop-climate relationships, which generally showed that weather/climate could only explain about 50% of inter-annual yield variability.

How did result come about? The answer is simple. In Michaels' regressions he included a trend term. This was at the time a common way to account for the effects of changing technology on yield. It turns out that the trend term accounts for 90% of the variability, so that, in Michaels' regressions, weather/climate explains just 5 of the remaining 10%. In other words, Michaels' claim that weather/climate explains 95% of the variability is completely bogus.

Apparently, none of Michaels' thesis examiners noticed this. We are left with wondering whether this was deliberate misrepresentation by Michaels, or whether it was simply ignorance.

As an historical note, I discovered this many years ago when working with Dick Warrick and Tu Qipu on crop-climate modeling. We used a spatial regression method, which we developed for the wheat belt of southwestern Western Australia. We carried out similar analyses for winter wheat in the USA, but never published the results.

Wigley, T.M.L. and Tu Qipu, 1983: Crop-climate modelling using spatial

patterns of yield and climate: Part 1, Background and an example from

Australia. Journal of Climate and Applied Meteorology 22, 1831-1841.

There never was a "Part 2".

Tom

+++++

Rick Piltz wrote:

> Just posted on Climate Science Watch Website.
> --RP
>
> <http://www.climate-science-watch.org/index.php/csw/details/phil-jones-and-ben-santer-comment-on-cei/>
>
>
> *Phil Jones and Ben Santer respond to CEI and Pat Michaels attack on
> temperature data record*
>
> /Posted on Tuesday, October 13, 2009
>
> /Prof. Phil Jones, Director of the Climatic Research Unit at the
> University of East Anglia in the UK and Ben Santer at Lawrence Livermore
> National Laboratory comment in response to a petition to EPA by the
> Competitive Enterprise Institute and Pat Michaels, which misleadingly
> seeks to obstruct EPA's process in making an "endangerment" finding on
> greenhouse gases. This new CEI tactic is to call into question the
> integrity of the global temperature data record and, by implication, the
> integrity of leading climate scientists.
>
> /E&E News PM/ reported on October 7 ("CLIMATE: Free-market group attacks
> data behind EPA 'endangerment' proposal"):
>
> The Competitive Enterprise Institute-a vocal foe of EPA's efforts to
> finalize its "endangerment finding"-petitioned the agency this week
> to reopen the public comment period on the proposal, arguing that
> critical data used to formulate the plan have been destroyed and
> that the available data are therefore unreliable.
> At issue is a set of raw data from the Climatic Research Unit at the
> University of East Anglia in Norwich, England, that includes surface

> temperature averages from weather stations around the world....
> Republican senators also weighed in yesterday, urging EPA to reopen
> the public comment period on the endangerment finding to investigate
> the scientific merit of the research data....
>
> We talked with E&E News on this latest maneuver by the ideologues at CEI
> and contrarian scientist Pat Michaels and posted on October 8
> <<http://www.climate-science-watch.org/index.php/csw/details/cei-epa-endangerment-petition-oct09/>>:
> "CEI global warming denialists try another gambit seeking to derail EPA
> 'endangerment' finding"
>
> The process initiated by the CEI petition will, we suppose, produce an
> appropriate response for the record from EPA and relevant members of the
> science community. And while that process drags on, CEI and Michaels no
> doubt will use their petition as a basis for attempting to muddy the
> waters of scientific discourse, while sliming leaders of the
> international climate science community and questioning their motives.
>
> A few of those leaders have begun to comment on this attempt. We post
> below comments Climate Science Watch has received from Ben Santer at
> Lawrence Livermore National Laboratory and Prof. Phil Jones, Director of
> the Climatic Research Unit at the University of East Anglia in the UK:
>
> Comment by Benjamin D. Santer
> <<http://www-pcmdi.llnl.gov/about/staff/Santer/index.php>>, Program for
> Climate Model Diagnosis and Intercomparison, Lawrence Livermore National
> Laboratory:
>
> As I see it, there are two key issues here.
> First, the Competitive Enterprise Institute (CEI) and Pat Michaels
> are arguing that Phil Jones and colleagues at the Climatic Research
> Unit at the University of East Anglia (CRU) willfully,
> intentionally, and suspiciously "destroyed" some of the raw surface
> temperature data used in the construction of the gridded surface
> temperature datasets.
> Second, the CEI and Pat Michaels contend that the CRU surface
> temperature datasets provided the sole basis for IPCC "discernible
> human influence" conclusions.
> Both of these arguments are incorrect. First, there was no
> intentional destruction of the primary source data. I am sure that,
> over 20 years ago, the CRU could not have foreseen that the raw
> station data might be the subject of legal proceedings by the CEI
> and Pat Michaels. Raw data were NOT secretly destroyed to avoid
> efforts by other scientists to replicate the CRU and Hadley
> Centre-based estimates of global-scale changes in near-surface
> temperature. In fact, a key point here is that other
> groups-primarily at the NOAA National Climatic Data Center (NCDC)
> and at the NASA Goddard Institute for Space Studies (GISS), but also
> in Russia-WERE able to replicate the major findings of the CRU and
> UK Hadley Centre groups. The NCDC and GISS groups performed this
> replication completely independently. They made different choices in
> the complex process of choosing input data, adjusting raw station
> data for known inhomogeneities (such as urbanization effects,
> changes in instrumentation, site location, and observation time),
> and gridding procedures. NCDC and GISS-based estimates of global
> surface temperature changes are in good accord with the HadCRUT data
> results.
>
> The second argument-that "discernible human influence" findings are
> like a house of cards, resting solely on one observational
> dataset-is also invalid. The IPCC Third Assessment Report (TAR)
> considers MULTIPLE observational estimates of global-scale
> near-surface temperature changes. It does not rely on HadCRUT data
> alone-as is immediately obvious from Figure 2.1b of the TAR, which
> shows CRU, NCDC, and GISS global-mean temperature changes.
> As pointed out in numerous scientific assessments (e.g., the IPCC
> TAR and Fourth Assessment Reports, the U.S. Climate Change Science
> Program Synthesis and Assessment Report 1.1 (Temperature trends in
> the lower atmosphere: Steps for understanding and reconciling
> differences), and the state of knowledge report, Global Climate
> Change Impacts on the United States, rigorous statistical
> fingerprint studies have now been performed with a whole range of
> climate variables-and not with surface temperature only. Examples
> include variables like ocean heat content, atmospheric water vapor,
> surface specific humidity, continental river runoff, sea-level
> pressure patterns, stratospheric and tropospheric temperature,
> tropopause height, zonal-mean precipitation over land, and Arctic
> sea-ice extent. The bottom-line message from this body of work is
> that natural causes alone CANNOT plausibly explain the climate
> changes we have actually observed. The climate system is telling us
> an internally- and physically-consistent story. The integrity and
> reliability of this story does NOT rest on a single observational
> dataset, as Michaels and the CEI incorrectly claim.
> I have known Phil for most of my scientific career. He is the
> antithesis of the secretive, "data destroying" character the CEI and
> Michaels are trying to portray to the outside world. Phil and Tom
> Wigley have devoted significant portions of their scientific careers
> to the construction of the land surface temperature component of the

> HadCRUT dataset. They have conducted this research in a very open
> and transparent manner-examining sensitivities to different gridding
> algorithms, different ways of adjusting for urbanization effects,
> use of various subsets of data, different ways of dealing with
> changes in spatial coverage over time, etc. They have thoroughly and
> comprehensively documented all of their dataset construction
> choices. They have done a tremendous service to the scientific
> community-and to the planet-by making gridded surface temperature
> datasets available for scientific research. They deserve medals-not
> the kind of deliberately misleading treatment they are receiving
> from Pat Michaels and the CEI.
>
> (Santer has received several honors, awards and fellowships including
> the Department of Energy Distinguished Scientist Fellowship
> <https://publicaffairs.llnl.gov/news/news_releases/2005/NR-05-10-01.html>,
> the E.O. Lawrence Award, and the "Genius Award" by the MacArthur
> Foundation.)
>
> Comment by Prof. Phil Jones
> <<http://www.cru.uea.ac.uk/cru/people/pjones/>>, Director, Climatic
> Research Unit (CRU), and Professor, School of Environmental Sciences,
> University of East Anglia, Norwich, UK:
>
> No one, it seems, cares to read what we put up
> <<http://www.cru.uea.ac.uk/cru/data/temperature/>> on the CRU web
> page. These people just make up motives for what we might or might
> not have done.
> Almost all the data we have in the CRU archive is exactly the same
> as in the Global Historical Climatology Network (GHCN) archive used
> by the NOAA National Climatic Data Center [see here
> <<http://www.ncdc.noaa.gov/oa/climate/ghcn-monthly/index.php>> and
> here <<http://www.ncdc.noaa.gov/oa/climate/research/ghcn/ghcngrid.html>>].
> The original raw data are not "lost." I could reconstruct what we
> had from U.S. Department of Energy reports we published in the
> mid-1980s. I would start with the GHCN data. I know that the effort
> would be a complete waste of time, though. I may get around to it
> some time. The documentation of what we've done is all in the
> literature.
> If we have "lost" any data it is the following:
> 1. Station series for sites that in the 1980s we deemed then to be
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> either not correctable or not worth doing as there were other series
> in the region.
> 2. The original data for sites for which we made appropriate
> adjustments in the temperature data in the 1980s. We still have our
> adjusted data, of course, and these along with all other sites that
> didn't need adjusting.
> 3. Since the 1980s as colleagues and National Meteorological
> Services <http://www.wmo.int/pages/members/index_en.html> (NMSs)
> have produced adjusted series for regions and/or countries, then we
> replaced the data we had with the better series.
> In the papers, I've always said that homogeneity adjustments are
> best produced by NMSs. A good example of this is the work by Lucie
> Vincent in Canada. Here we just replaced what data we had for the
> 200+ sites she sorted out.
> The CRUTEM3 data for land look much like the GHCN and NASA Goddard
> Institute for Space Studies data
> <<http://data.giss.nasa.gov/gistemp/>> for the same domains.
> Apart from a figure in the IPCC Fourth Assessment Report (AR4)
> showing this, there is also this paper from Geophysical Research
> Letters in 2005 by Russ Vose et al.
> <<http://www.climate-science-watch.org/file-uploads/Vose-et-al-TempTrends-GRL2005.pdf>>
> Figure 2 is similar to the AR4 plot.
>
> I think if it hadn't been this issue, the Competitive Enterprise
> Institute would have dreamt up something else!
>

</x-flowed>

From: Tom Wigley <wigley@ucar.edu>
To: Kevin Trenberth <trenberth@ucar.edu>
Subject: Re: BBC U-turn on climate
Date: Wed, 14 Oct 2009 01:01:24 -0600
Cc: Michael Mann <mam@meteo.psu.edu>, Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <omichael@Princeton.EDU>

Content-Type: text/plain; charset=windows-1252; format=flowed
X-MIME-Autoconverted: from 8bit to quoted-printable by ueamailgate01.uea.ac.uk id n9E71p4015864

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>>> Stephen H. Schneider
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>>> Senior Fellow, Woods Institute for the Environment
>>> Mailing address:
>>> Yang & Yamazaki Environment & Energy Building - MC 4205
>>> 473 Via Ortega
>>> Ph: 650 725 9978
>>> F: 650 725 4387
>>> Websites: climatechange.net
>>> patientfromhell.org
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Mike
Here are some of the issues as I see them:
Saying it is natural variability is not an explanation. What are the physical processes?
Where did the heat go? We know there is a build up of ocean heat prior to El Nino, and a discharge (and sfc T warming) during late stages of El Nino, but is the observing system sufficient to track it? Quite aside from the changes in the ocean, we know there are major changes in the storm tracks and teleconnections with ENSO, and there is a LOT more rain on land during La Nina (more drought in El Nino), so how does the albedo change overall (changes in cloud)? At the very least the extra rain on land means a lot more heat goes into evaporation rather than raising temperatures, and so that keeps land temps down; and should generate cloud. But the resulting evaporative cooling means the heat goes into atmosphere and should be radiated to space: so we should be able to track it with CERES data. The CERES data are unfortunately wanting and so too are the cloud data. The ocean data are also lacking although some of that may be related to the ocean current changes and burying heat at depth where it is not picked up. If it is sequestered at depth then it comes back to haunt us later and so we should know about it.

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----- Forwarded Message -----

From: "Narasimha D. Rao" <[3]ndrao@stanford.edu <[4]mailto:ndrao@stanford.edu>>

To: "Stephen H Schneider" <[5]shs@stanford.edu <[6]mailto:shs@stanford.edu>>

Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00 US/Canada Pacific

Subject: BBC U-turn on climate

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[7]<http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>

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BBC has significant influence on public opinion outside the US.

Do you think this merits an op-ed response in the BBC from a scientist?

Narasimha

PhD Candidate,

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Kevin E. Trenberth e-mail: [14]trenbert@ucar.edu

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References

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To: Kevin Trenberth <trenbert@ucar.edu>
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Date: Wed, 14 Oct 2009 10:25:25 -0400
Cc: Tom Wigley <wigley@ucar.edu>, Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <comichael@Princeton.EDU>

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References

Visible links

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Hidden links:

17. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Michael Mann <mann@meteo.psu.edu>
To: Kevin Trenberth <trenbert@ucar.edu>
Subject: Re: BBC U-turn on climate
Date: Wed, 14 Oct 2009 10:53:52 -0400
Cc: Tom Wigley <wigley@ucar.edu>, Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <comichael@Princeton.EDU>

thanks Kevin, yes, it's a matter of what question one is asking. to argue that the observed global mean temperature anomalies of the past decade falsifies the model projections of global mean temperature change, as contrarians have been fond of claiming, is clearly wrong, but that doesn't mean we can explain exactly what's going on, there is always the danger of falling a bit into the "we don't know everything, so we know nothing" fallacy. hence, I wanted to try to clarify where we all agree, and where there may be disagreement.

mike

On Oct 14, 2009, at 10:36 AM, Kevin Trenberth wrote:

Mike
Here are some of the issues as I see them:
Saying it is natural variability is not an explanation. What are the physical processes? Where did the heat go? We know there is a build up of ocean heat prior to El Nino, and a discharge (and sfc T warming) during late stages of El Nino, but is the observing system sufficient to track it? Quite aside from the changes in the ocean, we know there are major changes in the storm tracks and teleconnections with ENSO, and there is a LOT more rain on land during La Nina (more drought in El Nino), so how does the albedo change overall (changes in cloud)? At the very least the extra rain on land means a lot more heat goes into evaporation rather than raising temperatures, and so that keeps land temps down; and should generate cloud. But the resulting evaporative cooling means the heat goes into atmosphere and should be radiated to space: so we should be able to track it with CERES data. The CERES data are unfortunately wanting and so too are the cloud data. The ocean data are also lacking although some of that may be related to the ocean current changes and burying heat at depth where it is not picked up. If it is sequestered at depth then it comes back to haunt us later and so we should know about it.
Kevin

Michael Mann wrote:

Kevin, that's an interesting point. As the plot from Gavin I sent shows, we can easily account for the observed surface cooling in terms of the natural variability seen in the CMIP3 ensemble (i.e. the observed cold dip falls well within it). So in that sense, we can "explain" it. But this raises the interesting question, is there something going on here w/ the energy & radiation budget which is inconsistent with the modes of internal variability that leads to similar temporary cooling periods within the models. I'm not sure that this has been addressed--has it?

m

On Oct 14, 2009, at 10:17 AM, Kevin Trenberth wrote:

Hi Tom
How come you do not agree with a statement that says we are no where close to knowing where energy is going or whether clouds are changing to make the planet brighter. We are not close to balancing the energy budget. The fact that we can not account for what is happening in the climate system makes any consideration of geoengineering quite hopeless as we will never be able to tell if it is successful or not! It is a travesty!
Kevin
Tom Wigley wrote:

Dear all,

At the risk of overload, here are some notes of mine on the recent

lack of warming. I look at this in two ways. The first is to look at

the difference between the observed and expected anthropogenic trend relative to the pdf for unforced variability. The second is to remove ENSO, volcanoes and TSI variations from the observed data.

Both methods show that what we are seeing is not unusual. The second

method leaves a significant warming over the past decade.

These sums complement Kevin's energy work.

Kevin says ... "The fact is that we can't account for the lack of warming at the moment and it is a travesty that we can't". I do not

agree with this.

Tom.

+++++

Kevin Trenberth wrote:

Hi all

Well I have my own article on where the heck is global warming? We are asking that here in Boulder where we have broken records the past two days for the coldest days on record. We had 4 inches of snow. The high the last 2 days was below 30F and the normal is 69F, and it smashed the previous records for these days by 10F. The low was about 18F and also a record low, well below the previous record low. This is January weather (see the Rockies baseball playoff game was canceled on saturday and then played last night in below freezing weather).

Trenberth, K. E., 2009: An imperative for climate change planning: tracking Earth's global energy. Current Opinion in Environmental Sustainability, 1, 19-27. doi:10.1016/j.cosust.2009.06.001. [PDF]
<[http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/EnergyDiagnostics09final.pdf]>
(A PDF of the published version can be obtained from the author.)

The fact is that we can't account for the lack of warming at the moment and it is a travesty that we can't. The CERES data published in the August BAMS 09 supplement on 2008 shows there should be even more warming; but the data are surely wrong. Our observing system is inadequate.

That said there is a LOT of nonsense about the PDO. People like CPC are tracking PDO on a monthly basis but it is highly correlated with ENSO. Most of what they are seeing is the change in ENSO not real PDO. It surely isn't decadal. The PDO is already reversing with the switch to El Nino. The PDO index became positive in September for first time since Sept 2007. see
[2]http://www.cpc.ncep.noaa.gov/products/GODAS/ocean_briefing_gif/global_ocean_monitoring_current.ppt

Kevin

Michael Mann wrote:

extremely disappointing to see something like this appear on BBC. its particularly odd, since climate is usually Richard Black's beat at BBC (and he does a great job), from what I can tell, this guy was formerly a weather person at the Met Office.

We may do something about this on RealClimate, but meanwhile it might be appropriate for the Met Office to have a say about this, I might ask Richard Black what's up here?

mike

On Oct 12, 2009, at 2:32 AM, Stephen H Schneider wrote:

Hi all. Any of you want to explain decadal natural variability and signal to noise and sampling errors to this new "IPCC Lead Author" from the BBC? As we enter an El Nino year and as soon, as the sunspots get over their temporary--presumed--vacation worth a few tenths of a Watt per meter squared reduced forcing, there will likely be another dramatic upward spike like 1992-2000. I heard someone--Mike Schlessinger maybe?--was willing to bet alot of money on it happening in next 5 years?? Meanwhile the past 10 years of global mean temperature trend stasis still saw what, 9 of the warmest in reconstructed 1000 year record and Greenland and the sea ice of the North in big retreat?? Some of you observational folks probably do need to straighten this out as my student suggests below. Such "fun", Cheers, Steve

Stephen H. Schneider

Melvin and Joan Lane Professor for Interdisciplinary Environmental Studies,

Professor, Department of Biology and

Senior Fellow, Woods Institute for the Environment

Mailing address:

Yang & Yamazaki Environment & Energy Building - MC 4205

473 Via Ortega

Ph: 650 725 9978

F: 650 725 4387

Websites: climatechange.net

patientfromhell.org

----- Forwarded Message -----

From: "Narasimha D. Rao" <3jndrao@stanford.edu> <4jmailto:ndrao@stanford.edu>>

To: "Stephen H Schneider" <5jshs@stanford.edu> <6jmailto:shs@stanford.edu>>

Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00 US/Canada Pacific

Subject: BBC U-turn on climate

Steve,

You may be aware of this already. Paul Hudson, BBC's reporter on climate change, on Friday wrote that theres been no warming since 1998, and that pacific oscillations will force cooling for the next 20-30 years. It is not outrageously biased in presentation as are other skeptics views.

[7]<http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>

[8]<http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>

BBC has significant influence on public opinion outside the US.

Do you think this merits an op-ed response in the BBC from a scientist?

Narasimha

PhD Candidate,

Emmett Interdisciplinary Program in Environment and Resources (E-IPER)

Stanford University

Tel: 415-812-7560

--

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<[12]http://www.meteo.psu.edu/%7Emann/Mann/index.html>

"Dire Predictions" book site:
[13]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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"Dire Predictions" book site:

[20]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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References

Visible links

1. <http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/EnergyDiagnostics09final.pdf>
2. http://www.cpc.ncep.noaa.gov/products/GODAS/ocean_briefing_gif/global_ocean_monitoring_current.ppt
3. <mailto:ndrao@stanford.edu>
4. <mailto:ndrao@stanford.edu>
5. <mailto:shs@stanford.edu>
6. <mailto:shs@stanford.edu>
7. <http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>
8. <http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>
9. <mailto:menn@psu.edu>
10. <mailto:menn@psu.edu>
11. <http://www.meteo.psu.edu/~mann/Mann/index.html>
12. <http://www.meteo.psu.edu/%7Emann/Mann/index.html>
13. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
14. <mailto:trenbert@ucar.edu>
15. <http://www.cgd.ucar.edu/cas/trenbert.html>
16. <mailto:trenbert@ucar.edu>
17. <http://www.cgd.ucar.edu/cas/trenbert.html>
18. <mailto:menn@psu.edu>
19. <http://www.meteo.psu.edu/%7Emann/Mann/index.html>
20. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
21. <mailto:trenbert@ucar.edu>
22. <http://www.cgd.ucar.edu/cas/trenbert.html>
23. <mailto:menn@psu.edu>
24. <http://www.meteo.psu.edu/~mann/Mann/index.html>
25. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

Hidden links:

26. <http://www.met.psu.edu/dept/faculty/mann.htm>

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@ucar.edu>
Subject: Re: FYI--"Phil Jones and Ben Santer respond to CEI and Pat Michaels attack on temperature data record"
Date: Wed Oct 14 12:41:21 2009
Cc: Ben Santer <santer1@llnl.gov>

Tom,

What you'd need to point this out is a pdf of his thesis! Or is there a paper where the thesis is referred to?

I recall Pat wasn't very good at writing stuff up. There was one paper about warming in Alaska that I recall either you or me reviewing. It related to surface warming in Alaska and the borehole from Lachenbruch/Marshall (?) from about 1986.

With the pdf you wouldn't need to say that much, as it is as you say stupid to leave the Trend in with the rest of the variance.

Did the NCDC info help you sort out that data. Tom P told me that they don't infill certain areas in early decades, so there is missing data. Tom P isn't that keen on the method. He rightly thinks that it discourages them from looking for early data or including any new stuff they get - as they have infilled it, so it won't make a difference. It won't make a difference, but that isn't the point.

Cheers

Phil

At 02:45 14/10/2009, Tom Wigley wrote:

Dear folks,

You may be interesting in this snippet of information about Pat Michaels. Perhaps the University of Wisconsin ought to open up a public comment period to decide whether Pat Michaels, PhD needs re-assessing?

Michaels' PhD was, I believe, supervised by Reid Bryson. It dealt with statistical (regression-based) modeling of crop-climate relationships. In his thesis, Michaels claims that his statistical model showed that weather/climate variations could explain 95% of the inter-annual variability in crop yields. Had this been correct, it would have been a remarkable results. Certainly, it was at odds with all previous studies of crop-climate relationships, which generally showed that weather/climate could only explain about 50% of inter-annual yield variability.

How did result come about? The answer is simple. In Michaels' regressions he included a trend term. This was at the time a common way to account for the effects of changing technology on yield. It turns out that the trend term accounts for 90% of the variability, so that, in Michaels' regressions, weather/climate explains just 5 of the remaining 10%. In other words, Michaels' claim that weather/climate explains 95% of the variability is completely bogus.

Apparently, none of Michaels' thesis examiners noticed this. We are left with wondering whether this was deliberate misrepresentation

by Michaels, or whether it was simply ignorance.

As an historical note, I discovered this many years ago when working with Dick Warrick and Tu Qipu on crop-climate modeling. We used a spatial regression method, which we developed for the wheat belt of southwestern Western Australia. We carried out similar analyses for winter wheat in the USA, but never published the results.

Wigley, T.M.L. and Tu Qipu, 1983: Crop-climate modelling using spatial patterns of yield and climate: Part 1, Background and an example from Australia. Journal of Climate and Applied Meteorology 22, 1831-1841.

There never was a "Part 2".

Tom

+++++

Rick Piltz wrote:

Just posted on Climate Science Watch Website.

--RP

[1]<http://www.climatesciencewatch.org/index.php/csw/details/phil-jones-and-ben-santer-comment-on-cei/>

Phil Jones and Ben Santer respond to CEI and Pat Michaels attack on temperature data record

/Posted on Tuesday, October 13, 2009

/Prof. Phil Jones, Director of the Climatic Research Unit at the University of East Anglia in the UK and Ben Santer at Lawrence Livermore National Laboratory comment in response to a petition to EPA by the Competitive Enterprise Institute and Pat Michaels, which misleadingly seeks to obstruct EPAs process in making an endangerment finding on greenhouse gases. This new CEI tactic is to call into question the integrity of the global temperature data record and, by implication, the integrity of leading climate scientists.

/E&E News PM/ reported on October 7 (CLIMATE: Free-market group attacks data behind EPA endangerment proposal):

The Competitive Enterprise Institute a vocal foe of EPAs efforts to finalize its endangerment finding petitioned the agency this week to reopen the public comment period on the proposal, arguing that critical data used to formulate the plan have been destroyed and that the available data are therefore unreliable.

At issue is a set of raw data from the Climatic Research Unit at the University of East Anglia in Norwich, England, that includes surface temperature averages from weather stations around the world.

Republican senators also weighed in yesterday, urging EPA to reopen the public comment period on the endangerment finding to investigate the scientific merit of the research data.

We talked with E&E News on this latest maneuver by the ideologues at CEI and contrarian scientist Pat Michaels and posted on October 8

<[2]<http://www.climatesciencewatch.org/index.php/csw/details/cei-epa-endangerment-petition-oct09/>>: CEI global warming denialists try another gambit seeking to derail EPA endangerment finding

The process initiated by the CEI petition will, we suppose, produce an appropriate response for the record from EPA and relevant members of the science community. And

while that process drags on, CEI and Michaels no doubt will use their petition as a basis for attempting to muddy the waters of scientific discourse, while slinging leaders of the international climate science community and questioning their motives. A few of those leaders have begun to comment on this attempt. We post below comments Climate Science Watch has received from Ben Santer at Lawrence Livermore National Laboratory and Prof. Phil Jones, Director of the Climatic Research Unit at the University of East Anglia in the UK:

Comment by Benjamin D. Santer

<[3]<http://www-pcmdi.llnl.gov/about/staff/Santer/index.php>>, Program for Climate Model Diagnosis and Intercomparison, Lawrence Livermore National Laboratory:

As I see it, there are two key issues here.

First, the Competitive Enterprise Institute (CEI) and Pat Michaels are arguing that Phil Jones and colleagues at the Climatic Research Unit at the University of East Anglia (CRU) willfully, intentionally, and suspiciously destroyed some of the raw surface temperature data used in the construction of the gridded surface temperature datasets.

Second, the CEI and Pat Michaels contend that the CRU surface temperature datasets provided the sole basis for IPCC discernible human influence conclusions.

Both of these arguments are incorrect. First, there was no intentional destruction of the primary source data. I am sure that, over 20 years ago, the CRU could not have foreseen that the raw station data might be the subject of legal proceedings by the CEI and Pat Michaels. Raw data were NOT secretly destroyed to avoid efforts by other scientists to replicate the CRU and Hadley Centre-based estimates of global-scale changes in near-surface temperature. In fact, a key point here is that other groups primarily at the NOAA National Climatic Data Center (NCDC) and at the NASA Goddard Institute for Space Studies (GISS), but also in Russia WERE able to replicate the major findings of the CRU and UK Hadley Centre groups. The NCDC and GISS groups performed this replication completely independently. They made different choices in the complex process of choosing input data, adjusting raw station data for known inhomogeneities (such as urbanization effects, changes in instrumentation, site location, and observation time), and gridding procedures. NCDC and GISS-based estimates of global surface temperature changes are in good accord with the HadCRUT data results.

The second argument that discernible human influence findings are like a house of cards, resting solely on one observational dataset is also invalid. The IPCC Third Assessment Report (TAR) considers MULTIPLE observational estimates of global-scale near-surface temperature changes. It does not rely on HadCRUT data alone as is immediately obvious from Figure 2.1b of the TAR, which shows CRU, NCDC, and GISS global-mean temperature changes.

As pointed out in numerous scientific assessments (e.g., the IPCC TAR and Fourth Assessment Reports, the U.S. Climate Change Science

Program Synthesis and Assessment Report 1.1 (Temperature trends in the lower atmosphere: Steps for understanding and reconciling differences), and the state of knowledge report, Global Climate Change Impacts on the United States, rigorous statistical fingerprint studies have now been performed with a whole range of climate variables and not with surface temperature only. Examples include variables like ocean heat content, atmospheric water vapor, surface specific humidity, continental river runoff, sea-level pressure patterns, stratospheric and tropospheric temperature, tropopause height, zonal-mean precipitation over land, and Arctic sea-ice extent. The bottom-line message from this body of work is that natural causes alone CANNOT plausibly explain the climate changes we have actually observed. The climate system is telling us an internally- and physically-consistent story. The integrity and reliability of this story does NOT rest on a single observational dataset, as Michaels and the CEI incorrectly claim.

I have known Phil for most of my scientific career. He is the antithesis of the secretive, data destroying character the CEI and Michaels are trying to portray to the outside world. Phil and Tom Wigley have devoted significant portions of their scientific careers to the construction of the land surface temperature component of the HadCRUT dataset. They have conducted this research in a very open and transparent manner examining sensitivities to different gridding algorithms, different ways of adjusting for urbanization effects, use of various subsets of data, different ways of dealing with changes in spatial coverage over time, etc. They have thoroughly and comprehensively documented all of their dataset construction choices. They have done a tremendous service to the scientific community and to the planet by making gridded surface temperature datasets available for scientific research. They deserve medals not the kind of deliberately misleading treatment they are receiving from Pat Michaels and the CEI.

(Santer has received several honors, awards and fellowships including the Department of Energy Distinguished Scientist Fellowship

<[4]https://publicaffairs.llnl.gov/news/news_releases/2005/NR-05-10-01.html>, the E.O. Lawrence Award, and the Genius Award by the MacArthur Foundation.)

Comment by Prof. Phil Jones <[5]<http://www.cru.uea.ac.uk/cru/people/pjones/>>, Director, Climatic Research Unit (CRU), and Professor, School of Environmental Sciences, University of East Anglia, Norwich, UK:

No one, it seems, cares to read what we put up <[6]<http://www.cru.uea.ac.uk/cru/data/temperature/>> on the CRU web page. These people just make up motives for what we might or might not have done.

Almost all the data we have in the CRU archive is exactly the same as in the Global Historical Climatology Network (GHCN) archive used by the NOAA National Climatic Data Center [see here

<[7]<http://www.ncdc.noaa.gov/oa/climate/ghcn-monthly/index.php>> and here <[8]<http://www.ncdc.noaa.gov/oa/climate/research/ghcn/ghcngrid.html>>].

The original raw data are not lost. I could reconstruct what we had from U.S. Department of Energy reports we published in the mid-1980s. I would start with the GHCN data. I know that the effort would be a complete waste of time, though. I may get around to it some time. The documentation of what weve done is all in the literature.

If we have lost any data it is the following:

1. Station series for sites that in the 1980s we deemed then to be affected by either urban biases or by numerous site moves, that were either not correctable or not worth doing as there were other series in the region.
2. The original data for sites for which we made appropriate adjustments in the temperature data in the 1980s. We still have our adjusted data, of course, and these along with all other sites that didnt need adjusting.
3. Since the 1980s as colleagues and National Meteorological Services <[9]http://www.wmo.int/pages/members/index_en.html> (NMSs) have produced adjusted series for regions and or countries, then we replaced the data we had with the better series.

In the papers, Ive always said that homogeneity adjustments are best produced by NMSs. A good example of this is the work by Lucie Vincent in Canada. Here we just replaced what data we had for the 200+ sites she sorted out.

The CRUTEM3 data for land look much like the GHCN and NASA Goddard Institute for Space Studies data

<[10]<http://data.giss.nasa.gov/gistemp/>> for the same domains. Apart from a figure in the IPCC Fourth Assessment Report (AR4) showing this, there is also this paper from Geophysical Research Letters in 2005 by Russ Vose et al.

<[11]<http://www.climate-science-watch.org/file-uploads/Vose-et-al-TempTrends-GRL2005.pdf>>
Figure 2 is similar to the AR4 plot.

I think if it hadnt been this issue, the Competitive Enterprise Institute would have dreamt up something else!

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
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References

1. <http://www.climate-science-watch.org/index.php/csw/details/phil-jones-and-ben-santer-comment-on-cei/>
2. <http://www.climate-science-watch.org/index.php/csw/details/cei-epa-endangerment-petition-oct09/>

3. <http://www-pcmdi.llnl.gov/about/staff/Santer/index.php>
4. https://publicaffairs.llnl.gov/news/news_releases/2005/NR-05-10-01.html
5. <http://www.cru.uea.ac.uk/cru/people/pjones/>
6. <http://www.cru.uea.ac.uk/cru/data/temperature/>
7. <http://www.ncdc.noaa.gov/oa/climate/ghcn-monthly/index.php>
8. <http://www.ncdc.noaa.gov/oa/climate/research/ghcn/ghcngrid.html>
9. http://www.wmo.int/pages/members/index_en.html
10. <http://data.giss.nasa.gov/gistemp/>
11. <http://www.climate-science-watch.org/file-uploads/Vose-et-al-TempTrends-GRL2005.pdf>

From: Tom Wigley <wigley@ucar.edu>
To: Kevin Trenberth <trenberth@ucar.edu>
Subject: Re: BBC U-turn on climate
Date: Wed, 14 Oct 2009 16:09:35 -0600
Cc: Michael Mann <mann@meteo.psu.edu>, Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Gavin Schmidt <gschmidt@giss.nasa.gov>, James Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <michael@Princeton.EDU>

<x-flowed>
Kevin,

I didn't mean to offend you. But what you said was "we can't account for the lack of warming at the moment". Now you say "we are no where close to knowing where energy is going". In my eyes these are two different things -- the second relates to our level of understanding, and I agree that this is still lacking.

Tom.

+++++

Kevin Trenberth wrote:
> Hi Tom
> How come you do not agree with a statement that says we are no where
> close to knowing where energy is going or whether clouds are changing to
> make the planet brighter. We are not close to balancing the energy
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>> Both methods show that what we are seeing is not unusual. The second
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>>

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>> warming at the moment and it is a travesty that we can't". I do not
>> agree with this.
>>

>> Tom.
>>
>> +++++

>>> Kevin Trenberth wrote:
>>> Hi all
>>> Well I have my own article on where the heck is global warming? We
>>> are asking that here in Boulder where we have broken records the past
>>> two days for the coldest days on record. We had 4 inches of snow.
>>> The high the last 2 days was below 30F and the normal is 69F, and it
>>> smashed the previous records for these days by 10F. The low was
>>> about 18F and also a record low, well below the previous record low.
>>> This is January weather (see the Rockies baseball playoff game was
>>> canceled on saturday and then played last night in below freezing
>>> weather).
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>>> Trenberth, K. E., 2009: An imperative for climate change planning:
>>> tracking Earth's global energy. /Current Opinion in Environmental
>>> Sustainability/, #1*, 19-27, doi:10.1016/j.coust.2009.06.001. [PDF]
>>> <http://www.cgd.ucar.edu/cas/Trenberth/trenberth.papers/EnergyDiagnostics09final.pdf>
>>> (A PDF of the published version can be obtained from the author.)
>>>

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>>>>>
>>>>>
>>>>> Stephen H. Schneider
>>>>> Melvin and Joan Lane Professor for Interdisciplinary Environmental
>>>>> Studies,
>>>>> Professor, Department of Biology and
>>>>> Senior Fellow, Woods Institute for the Environment
>>>>> Mailing address:
>>>>> Yang & Yamazaki Environment & Energy Building - MC 4205
>>>>> 473 Via Ortega
>>>>> Ph: 650 725 9978
>>>>> F: 650 725 4387
>>>>> Websites: climatechange.net
>>>>> patientfromhell.org
>>>>>

>>>>> ----- Forwarded Message -----
>>>>> From: "Narasimha D. Rao" <ndrao@stanford.edu>
>>>>> <mailto:ndrao@stanford.edu>
>>>>> To: "Stephen H Schneider" <shs@stanford.edu <mailto:shs@stanford.edu>>
>>>>> Sent: Sunday, October 11, 2009 10:25:53 AM GMT -08:00 US/Canada
>>>>> Pacific
>>>>> Subject: BBC U-turn on climate
>>>>>
>>>>> Steve,
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>>>>> <http://news.bbc.co.uk/2/hi/science/nature/8299079.stm>
>>>>> <http://blogs.telegraph.co.uk/news/damianthompson/100013173/the-bbcs-amazing-u-turn-on-climate-change/>
>>>>>

>>>>> BBC has significant influence on public opinion outside the US.
>>>>>

>>>>> Do you think this merits an op-ed response in the BBC from a
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>>>>>

>>>>> Narasimha
>>>>>

>>>>> -----
>>>>> PhD Candidate,
>>>>> Emmett Interdisciplinary Program in Environment and Resources (E-IPER)
>>>>> Stanford University
>>>>> Tel: 415-812-7560
>>>>>

>>>>> Michael E. Mann
>>>>> Professor
>>>>> Director, Earth System Science Center (ESSC)
>>>>>
>>>>> Department of Meteorology Phone: (814) 863-4075
>>>>> 503 Walker Building FAX: (814) 865-3663
>>>>> The Pennsylvania State University email: mann@psu.edu
>>>>> <mailto:mann@psu.edu>
>>>>> University Park, PA 16802-5013
>>>>>

>>>>> website: <http://www.meteo.psu.edu/~mann/Mann/index.html>
>>>>> <<http://www.meteo.psu.edu/%7Emann/Mann/index.html>>
>>>>> "Dire Predictions" book site:
>>>>> http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
>>>>>

>>>>> *****
>>>>> Kevin E. Trenberth e-mail: trenbert@ucar.edu
>>>>> Climate Analysis Section, www.esd.ucar.edu/cas/trenbert.html
>>>>> NCAR
>>>>> P. O. Box 3000, (303) 497 1318
>>>>> Boulder, CO 80307 (303) 497 1333 (fax)
>>>>>

>>>>> Street address: 1850 Table Mesa Drive, Boulder, CO 80305

>>>
>

</x-flowed>

From: Tom Wigley <wigley@ucar.edu>
To: Gavin Schmidt <gschmidt@giss.nasa.gov>
Subject: Re: BBC U-turn on climate
Date: Wed, 14 Oct 2009 16:43:54 -0600
Cc: Michael Mann <mann@meteo.psu.edu>, Kevin Trenberth <trenberth@ucar.edu>, Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@lnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Jim Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <michael@Princeton.EDU>

<x-flowed>
Gavin,

I just think that you need to be up front with uncertainties and the possibility of compensating errors.

Tom.

+++++

Gavin Schmidt wrote:
> Tom, with respect to the difference between the models and the data, the
> fundamental issue on short time scales is the magnitude of the internal
> variability. Using the full CMIP3 ensemble at least has multiple
> individual realisations of that internal variability and so is much more
> suited to a comparison with a short period of observations. MAGICC is
> great at the longer time scale, but its neglect of unforced variability
> does not make it useful for these kinds of comparison.

>
> The kind of things we are hearing "no model showed a cooling", the "data
> is outside the range of the models" need to be addressed directly.

>
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>
> On Wed, 2009-10-14 at 18:06, Michael Mann wrote:

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>> ensemble, so at least the plot is sampling the range of choices
>> regarding if and how indirect effects are represented, what the cloud
>> radiative feedback & sensitivity is, etc. across the modeling
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>> (after all, there is an interesting and perhaps somewhat disturbing
>> compensation between indirect aerosol forcing and sensitivity across
>> the CMIP3 models that defies the assumption of independence), but if
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>> what sort of comparison wouldn't be deceptive (your point re MAGICC
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To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: BBC U-turn on climate
Date: 14 Oct 2009 18:21:07 -0400
Cc: Tom Wigley <wigley@ucar.edu>, Kevin Trenberth <trenbert@ucar.edu>, Stephen H Schneider <shs@stanford.edu>, Myles Allen <allen@atm.ox.ac.uk>, peter stott <peter.stott@metoffice.gov.uk>, "Philip D. Jones" <p.jones@uea.ac.uk>, Benjamin Santer <santer1@llnl.gov>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Jim Hansen <jhansen@giss.nasa.gov>, Michael Oppenheimer <omichael@Princeton.EDU>

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>>>>>> patientfromhell.org
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>>>>>> From: "Narasimha D. Rao" <ndrao@stanford.edu>
>>>>>> <mailto:ndrao@stanford.edu>
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>>>>>> Michael E. Mann
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>>>>>> Kevin E. Trenberth e-mail: trenbert@ucar.edu
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>>>>>> P. O. Box 3000, (303) 497 1318
>>>>>> Boulder, CO 80307 (303) 497 1333 (fax)
>>>>>> Street address: 1850 Table Mesa Drive, Boulder, CO 80305
>>>>>> <Wigley-RecentTemps.doc>
>>>>>> --

From: "Davies Trevor Prof (ENV)" <T.D.Davies@uea.ac.uk>
To: "Ogden Annie Ms (MAC)" <k319@uea.ac.uk>, "Briffa Keith Prof (ENV)" <K.Briffa@uea.ac.uk>, "Jones Philip Prof (ENV)" <P.Jones@uea.ac.uk>
Subject: RE: Climate Research Centre crisis spreads
Date: Thu, 22 Oct 2009 08:33:16 +0100
Cc: "Summers Brian Mr (REG)" <B.Summers@uea.ac.uk>, "Preece Alan Mr (MAC)" <A.Preece@uea.ac.uk>

WE should make a statement along these lines. We should also stress that McIntyres analysis has not been peer-reviewed (& we need to explain what this means - for the man-in-the street).

Given the fact that this campaign is clearly not going to die down & we now have a silly attempt to escalate it locally (dragging Norfolk's reputation thro the mud), I have revised my view & feel we do need to pursue the spectator more vigorously. To me, it seems straightforward - Keith has been accused of fraud on an official Spectator website - that is (wherever the legal word is).

Trevor

>-----Original Message-----
>From: Ogden Annie Ms (MAC)
>Sent: Tuesday, October 20, 2009 3:16 PM
>To: Briffa Keith Prof (ENV); Jones Philip Prof (ENV)
>Cc: Davies Trevor Prof (ENV); Summers Brian Mr (REG); Preece
>Alan Mr (MAC)
>Subject: FW: Climate Research Centre crisis spreads
>
>Dear Phil and Keith,
>Marcus has just received this message below from the EDP
>environment correspondent. He is telling her he knows nothing
>about it (true, as he has just returned from China).
>
>I have just dropped a note to the solicitor asking if she sees
>any problem in our warning her to be very cautious in how
>anything is phrased and issuing a statement along the
>following lines. (I think the last line would have to come
>directly from you Keith)
>
>For info, still no response from the Spectator to the letter.
>I have rung three times (fist time PA told me message had been
>opened) and emailed. Solicitor is now looking closely at the
>piece in the Spectator to judge whether to send a solicitor's letter.
>Best, Annie
>
>
>Draft statement
>Any implication that Professor Keith Briffa deliberately
>selected tree-ring data in order to manufacture evidence of
>recent dramatic warming in the Yamal region of northern Russia
>is completely false. A full rebuttal is published on the
>Climatic Research Unit's website.
>
>This stems from a report on the Climate Audit blog site - a
>site for climate change sceptics. The blog's editor, Steve
>McIntyre, has produced an alternative history of tree-growth
>changes in the Yamal region by substituting some of the data
>used in Prof Briffa's published and peer-reviewed analysis,
>with recent data from a more localised origin than the data
>analysed by Prof Briffa. While McIntyre's selection produces
>a different result, it cannot be considered to be more authoritative.
>
>This appears to be an attempt to discredit the work of the
>Intergovernmental Panel of Climate Change in the run-up to the
>Copenhagen climate talks.
>
>

>-----
>Annie Ogden, Head of Communications,
>University of East Anglia,
>Norwich, NR4 7TJ,
>Tel:+44 (0)1603 592764
>www.uea.ac.uk/comm
>.....

>-----Original Message-----
>From: Armes Marcus Mr (VCO)
>Sent: Tuesday, October 20, 2009 2:40 PM
>To: Ogden Annie Ms (MAC)
>Subject: FW: Climate Research Centre crisis spreads
>

> Here it is Annie

>-----Original Message-----
>From: Greaves, Tara [mailto:Tara.Greaves@archant.co.uk]
>Sent: Tuesday, October 20, 2009 12:11 PM
>To: Armes Marcus Mr (VCO)
>Subject: FW: Climate Research Centre crisis spreads
>

>Also, do you know anything about this?

>-----Original Message-----
>From: David_Robinson [mailto:darobin@netcomuk.co.uk]
>Sent: 19 October 2009 22:45
>To: newsdesk@archant.co.uk
>Subject: Climate Research Centre crisis spreads
>

>Sir,
>I draw your attention to the growing international climate
>change scandal that is engulfing the CRU and dragging the

>reputation of it, and Norfolk, through the mud.
>
>After several weeks of open criticism of the use of a
>particular, allegedly flawed, CRU dataset there has been no
>attempted rebuttle by the CRU. Latest information suggests
>that dozens of 'peer reviewed' scientific papers that relied
>on the same dataset are now 'similarly flawed' and should be
>withdrawn. This, unfortunately, draws into question a
>fundamental part of the IPCC conclusion - namely, whether the
>recent global warming is in fact abnormal and hence
>attributable to man.
>
>I think the continued silence by the CRU on this subject
>profoundly worrying given the importance of the topic.
>
>Any light you can shed on this whole sorry story would be
>greatly in the public interest, especially given the
>Copenhagen summit fast approaching.
>
>David Robinson
>
><http://www.climateaudit.org/?p=7374#comments>
>---
>Sent via BlackBerry
>David Robinson MSc
>Blacklock and Bowers Limited
>
>This email and any attachments to it are confidential and
>intended solely for the individual or organisation to whom
>they are addressed.
>You must not copy or retransmit this e-mail or its attachments
>in whole or in part to anyone else without our permission. The
>views expressed in them are those of the individual author and
>do not necessarily represent the views of this Company.
>
>Whilst we would never knowingly transmit anything containing a
>virus we cannot guarantee that this e-mail is virus-free and
>you should take all steps that you can to protect your systems
>against viruses.
>
>Archant Regional Limited, is registered in England under
>Company Registration Number 19300, and the Registered Office
>is Prospect House, Rouen Road, Norwich NR1 1RE.
>
>

From: Kevin Trenberth <trenbert@ucar.edu>

To: James Annan <jdannan@jamstec.go.jp>

Subject: Re: FW: 2009JD012960 (Editor - Steve Ghan):Decision Letter

Date: Fri, 23 Oct 2009 08:55:24 -0600

Cc: Jim Salinger <j.salinger@auckland.ac.nz>, Grant Foster <tamino_9@hotmail.com>, Mike Mann <mann@meteo.psu.edu>, p.jones@uea.ac.uk, b.mullan@niwa.co.nz, Gavin Schmidt <gschmidt@giss.nasa.gov>, j.renwick@niwa.co.nz

<x-flowed>

Hi James

Thanks for doing this and let's keep it moving as fast as possible. Yes the formatting in places is disconcerting and the line numbering is a bit on and off.

I have suggestions for changing two words.

Line 13 "severely" to "greatly"

Line 79 "more dramatic" to "greater"

As they stand, words like those used carry a lot of extra subjective tone that implies "bad" or has a commentary that is not desirable as per Rev 3. I wonder if you should not be a bit more specific in responding to Rev 3 and say what other words were changed in the abstract at least? If it were "word" I would send in a version of the abstract with tracking on. It might make the difference between having the editor approve it and sending it back to Rev 3.

Best regards

Kevin

James Annan wrote:

> Dear All,

>

> I had a reply from Grant, and have made some changes to the paper -

> very little of substance, but I've lightly edited the wording

> throughout. I also added refs to Newell and Weare, and Angell (not

> A+Korshover), which seem relevant. Despite this, I've managed to cut a

> few lines off in total. I have also drafted replies to the reviewers

> (with their comments appended for reference).

>

> We do have a 2 week extension agreed, to 11 Nov. However it doesn't

> really seem like there is much more that needs doing. More suggestions

> are welcome, however, and before resubmitting, *I need an explicit OK

> from each author*.

>

> James

--

Kevin E. Trenberth e-mail: trenbert@ucar.edu

Climate Analysis Section, www.cgd.ucar.edu/cas/trenbert.html

NCAR

P. O. Box 3000, (303) 497 1318

Boulder, CO 80307 (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80305

</x-flowed>

From: Mike Salmon <m.salmon@uea.ac.uk>
To: Mike Salmon <m.salmon@uea.ac.uk>
Subject: Re: Yamal 2009
Date: Fri, 23 Oct 2009 22:58:44 +0100
Cc: Keith Briffa <k.briffa@uea.ac.uk>, Tom Melvin <t.m.melvin@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>

<x-flowed>

I'm not thinking straight. It makes far more sense to have password-protection rather than IP-address protection. So, to access those pages

Username: steve
Password: tosser

Have a good weekend!

Mike

Mike Salmon wrote:

> Figure E added; figure F updated. I still need "ALT" tags for each
> figure. Data page needs a lot of work.
>
> Tim: I understand you're providing a whole new page?
>
> Tom: I definitely don't have the list of references for sensit.htm.
> Please send me the Word file or tell me where to look on your PC.
>
> Briffa et al 1996 added to <http://www.cru.uea.ac.uk/cru/people/briffa/>
>
> Access to the Yamal 2009 pages is currently restricted by IP address.
> Try to access them from home, then tell me the time at which you tried.
> I'll pick your IP address out of the logs and add it to the "permitted"
> list.
>
> <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009-temporary/main.htm>
> <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009-temporary/sensit.htm>
> <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009-temporary/data/>
>
> Mike
>
>

</x-flowed>

From: Phil Jones <p.jones@uea.ac.uk>
To: Michael Mann <mann@meteo.psu.edu>
Subject: Re: The web page is up about the Yamal tree-ring chronology
Date: Wed Oct 28 09:04:27 2009
Cc: Gavin Schmidt <gschmidt@giss.nasa.gov>

Mike,

Sept 2009 isn't up yet. I expect it in the next day or so. I'll check again tomorrow. Away Friday and Mon/Tues next week. Our web site will update on Sunday if the HC have updated theirs.

Seems nothing yet on Keith's Yamal.

One of the Russians has a reason why Khad hasn't grown so much. All the sites in the region have permafrost at depth. Those nearer the rivers have the permafrost at a greater depth, partly due to the rivers. Warmth in the 20th century has meant greater depths for the roots. Khad is a walk from the river and slightly higher, so possibly has less available soil depth above the permafrost. All the sites are sampled through river transport. When the coring was done in the 1980s and early 1990s the fieldwork teams ate a lot of fish!

Permafrost idea is impossible to prove without going back to the sites and drilling down. The Russians plan to do this when they revisit the area, but that depends on resources.

Cheers

Phil

Cheers

Phil

At 17:07 27/10/2009, Michael Mann wrote:

Hi Phil,

Thanks--we know that. The point is simply that if we want to talk about about a meaningful "2009" anomaly, every additional month that is available from which to calculate an annual mean makes the number more credible. We already have this for GISTEMP, but have been awaiting HadCRU to be able to do a more decisive update of the status of the disingenuous "globe is cooling" contrarian talking point,

mike

p.s. be a bit careful about what information you send to Andy and what emails you copy him in on. He's not as predictable as we'd like

On Oct 27, 2009, at 1:04 PM, Phil Jones wrote:

Mike,

Yes a link will be fine.

I'll look into Sept numbers, but you shouldn't be looking at individual months.

Cheers

Phil

At 16:54 27/10/2009, Michael Mann wrote:

thanks Phil,

Perhaps we'll do a simple update to the Yamal post, e.g. linking Keith/s new page--Gavin t?

As to the issues of robustness, particularly w.r.t. inclusion of the Yamal series, we actually emphasized that (including the Osborn and Briffa '06 sensitivity test) in our original post! As we all know, this isn't about truth at all, its about plausibly deniable accusations,

m

p.s. any word on HadCRU Sep numbers yet???

On Oct 27, 2009, at 12:37 PM, Phil Jones wrote:

Gavin, Mike, Andy,

It has taken Keith longer than he would have liked, but it is up. There is a lot to read and understand. It is structured for different levels. The link goes to the top level. There is more detail below this and then there are the data below that.

You can either go to our main page

[1]<http://www.cru.uea.ac.uk/> then click on the link

or directly here

[2]<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>

I'll let you make up you own minds! It seems to me as though McIntyre cherry picked for effect.

There is an additional part that shows how many series from Ch 6 of AR4 used Yamal - most didn't! Also there is a sensitivity test of omitting it - which comes from the Supplementary Info with Osborn and Briffa (2006). As expected omitting it makes very little difference. To get to this follow the links from the above link.

McIntyre knows that the millennial temperature record is pretty robust, otherwise he would produce his own series. Similarly the instrumental temperature is even more robust, which he also knows.

Cheers

Phil

Prof. Phil Jones

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Norwich

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--

Michael E. Mann

Professor

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503 Walker Building FAX: (814) 865-3663

The Pennsylvania State University email: [4]mann@psu.edu

University Park, PA 16802-5013

website: [5]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[6]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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Professor

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University Park, PA 16802-5013

website: [9]<http://www.meteo.psu.edu/~mann/Mann/index.html>

"Dire Predictions" book site:

[10]http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

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References

1. <http://www.cru.uea.ac.uk/>
2. <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>
3. <mailto:p.jones@uea.ac.uk>
4. <mailto:mann@psu.edu>
5. <http://www.meteo.psu.edu/~mann/Mann/index.html>
6. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html
7. <mailto:p.jones@uea.ac.uk>
8. <mailto:mann@psu.edu>
9. <http://www.meteo.psu.edu/~mann/Mann/index.html>
10. http://www.essc.psu.edu/essc_web/news/DirePredictions/index.html

From: Phil Jones <p.jones@uea.ac.uk>

To: "Mitchell, John FB (Director of Climate Science)" <john.f.mitchell@metoffice.gov.uk>

Subject: Yamal response from Keith

Date: Wed Oct 28 12:26:39 2009

John,

[1]<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>

This went up last night about 5pm. There is a lot to read at various levels. If you get time just the top level is necessary. There is also a bit from Tim Osborn showing that Yamal was used in 3 of the 12 millennial reconstructions used in Ch 6.

Also McIntyre had the Yamal data in Feb 2004 - although he seems to have forgotten this. Keith succeeding in being very restrained in his response. McIntyre knew what he was doing when he replaced some of the trees with those from another site.

Cheers

Phil

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References

1. <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>

From: Phil Jones <p.jones@uea.ac.uk>
To: k.briffa@uea.ac.uk
Subject: FW: Yamal and paleoclimatology
Date: Wed Oct 28 16:04:00 2009

Keith,

There is a lot more there on CA now. I would be very wary about responding to this person now having seen what McIntyre has put up.

You and Tim talked about Yamal. Why have the bristlecones come in now.

[1]<http://www.climateaudit.org/?p=7588#comments>

This is what happens - they just keep moving the goalposts.

Maybe get Tim to redo OB2006 without a few more series.

Cheers

Phil

X-Authentication-Warning: ueamailgate02.uea.ac.uk: defang set sender to

<Don.Keiller@anglia.ac.uk> using -f

Subject: FW: Yamal and paleoclimatology

Date: Wed, 28 Oct 2009 15:39:48 -0000

X-MS-Has-Attach:

X-MS-TNEF-Correlator:

Thread-Topic: Yamal and paleoclimatology

Thread-Index: AcpDQ2sqWC+z2djuSqC1Ax4HdHoH1wUn1Ocw

From: "Keiller, Donald" <Don.Keiller@anglia.ac.uk>

To: <k.briffa@uea.ac.uk>

Cc: <p.jones@uea.ac.uk>

X-ARU-HELO: CAMEXCH.ANGLIA.LOCAL

X-ARU-sender-host: cambe01.ad.anglia.ac.uk (CAMEXCH.ANGLIA.LOCAL) [193.63.55.171]:25427

X-ARU-Mailhub: yes

X-ARU-Exchange: yes

X-ARU-MailFilter: message scanned

X-Spam-Status: no

Reply-to: Don.Keiller@anglia.ac.uk

X-Canit-CHI2: 0.00

X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)

X-Spam-Score: 0.00 () [Hold at 5.00] SPF(none,0)

X-CanItPRO-Stream: UEA:f028 (inherits from UEA:default,base:default)

X-Canit-Stats-ID: 34330416 - 89bde843c4e5 (trained as not-spam)

X-Antispam-Training-Forget:

[2]<https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=f>

X-Antispam-Training-Nonspam:

[3]<https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=n>

X-Antispam-Training-Spam: [4]<https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=s>

X-Scanned-By: CanIt (www . roaringpenguin . com) on 127.0.0.1

Dear Professor Briffa, I am pleased to hear that you appear to have recovered from your recent illness sufficiently to post a response to the controversy surrounding the use of the Yamal chronology;

([5]<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/cautious/cautious.htm>) and the chronology itself;

([6]<http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>)

Unfortunately I find your explanations lacking in scientific rigour and I am more inclined to believe the analysis of McIntyre

([7]<http://www.climateaudit.org/?p=7588>)

Can I have a straightforward answer to the following questions

- 1) Are the reconstructions sensitive to the removal of either the Yamal data and Strip pine bristlecones, either when present singly or in combination?
- 2) Why these series, when incorporated with white noise as a background, can still produce a Hockey-Stick shaped graph if they have, as you suggest, a low individual weighting?

And once you have done this, please do me the courtesy of answering my initial email.

Dr. D.R. Keiller

-----Original Message-----

From: Keiller, Donald

Sent: 02 October 2009 10:34

To: 'k.briffa@uea.ac.uk'

Cc: 'p.jones@uea.ac.uk'

Subject: Yamal and paleoclimatology

Dear Professor Briffa, my apologies for contacting you directly, particularly since I hear that you are unwell.

However the recent release of tree ring data by CRU has prompted much discussion and indeed disquiet about the methodology and conclusions of a number of key papers by you and co-workers.

As an environmental plant physiologist, I have followed the long debate starting with Mann et al (1998) and through to Kaufman et al (2009).

As time has progressed I have found myself more concerned with the whole scientific basis of dendroclimatology. In particular;

- 1) The appropriateness of the statistical analyses employed
- 2) The reliance on the same small datasets in these multiple studies
- 3) The concept of "teleconnection" by which certain trees respond to the "Global Temperature Field", rather than local climate
- 4) The assumption that tree ring width and density are related to temperature in a linear manner.

Whilst I would not describe myself as an expert statistician, I do use inferential statistics routinely for both research and teaching and find difficulty in understanding the statistical rationale in these papers.

As a plant physiologist I can say without hesitation that points 3 and 4 do not agree with the accepted science.

There is a saying that "extraordinary claims require extraordinary proof". Given the scientific, political and economic importance of these papers, further detailed explanation is urgently required.

Yours sincerely,
Dr. Don Keiller.

--

EMERGING EXCELLENCE: In the Research Assessment Exercise (RAE) 2008, more than 30% of our submissions were rated as 'Internationally Excellent' or 'World-leading'. Among the academic disciplines now rated 'World-leading' are Allied Health Professions & Studies; Art & Design; English Language & Literature; Geography & Environmental Studies; History; Music; Psychology; and Social Work & Social Policy & Administration. Visit [8]www.anglia.ac.uk/rae for more information.

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Email has been scanned for viruses by Altman Technologies' email management service - [9]www.altman.co.uk/emailsystems

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References

1. <http://www.climateaudit.org/?p=7588#comments>
2. <https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=f>
3. <https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=n>
4. <https://canit.uea.ac.uk/b.php?i=34330416&m=89bde843c4e5&c=s>
5. <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/cautious/cautious.htm>
6. <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>
7. <http://www.climateaudit.org/?p=7588>
8. <http://www.anglia.ac.uk/rae>
9. <http://www.altman.co.uk/emailsystems>

From: "Graham F Haughton" <G.F.Haughton@hull.ac.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: RE: Dr Sonja BOEHMER-CHRISTIANSEN
Date: Wed, 28 Oct 2009 17:32:24 -0000

Content-class: urn:content-classes:message
Content-Type: text/plain;
charset="iso-8859-1"

I know, I feel for you being in that position. If its any consolation we've had it here for years, very pointed commentary at all external seminars and elsewhere, always coming back to the same theme. Since Sonja retired I am a lot more free to push my environmental interests without ongoing critique of my motives and supposed misguidedness - I've signed my department up to 10:10 campaign and have a taskforce of staff and students involved in it.... Every now and then people say to me sotto voce with some bemusement, 'and when Sonja finds out, how will you explain it to her...!'

Graham

-----Original Message-----

From: Phil Jones [mailto:p.jones@uea.ac.uk]
Sent: 28 October 2009 16:39
To: Graham F Haughton
Subject: RE: Dr Sonja BOEHMER-CHRISTIANSEN

Dear Graham,

Thanks for the speedy reply. Just like you are, we are trying here to do bits of research mostly related to the current set of contracts we have. Trying to respond to blogs is just not part of the deadlines we have entered into with the Research Councils, the EU and DEFRA.

You are probably aware of this, but the journal Sonja edits is at the very bottom of almost all climate scientists lists of journals to read. It is the journal of choice of climate change skeptics and even here they don't seem to be bothering with journals at all recently.

I don't think there is anything more you can do. I have vented my frustration and have had a considered reply from you.

Cheers
Phil

At 18:45 27/10/2009, you wrote:
>Content-class: urn:content-classes:message
>Content-Type: text/plain;
> charset="iso-8859-1"
>

>Dear Phil, sorry to hear this. I don't see much
>of her these days, but when I do see Sonja next
>I'll try and have a quiet word with her about
>the way the affiliation to us is used, but at
>the moment in fairness she is entitled to use it
>in the way she does. Fortunately I don't get to
>see many of these email exchanges but I do
>occasionally hear about them or see them and
>frankly am rarely convinced by what I read. But
>as with all academics, I'd want to protect
>another academic's freedom to be contrary and
>critical, even if I personally believe she is
>probably wrong. I agree with you that it'd be
>better for these exchanges to be conducted
>through the peer review process but these forms
>of e-communication are now part of the public
>debate and its difficult to do much about it
>other than to defend your position in this and
>other fora, or just ignore it as being, in your words, malicious.

>
>I can understand your frustration and I am
>pretty sure I'd be feeling exactly the same in
>your shoes, but I am not sure at the moment that
>I can do much more. If you think I can and
>should do more then feel free to ring and I am happy to discuss the
matter.

>
>Graham

>
>
>-----Original Message-----
>From: Phil Jones [mailto:p.jones@uea.ac.uk]
>Sent: 27 October 2009 17:05
>To: Graham F Haughton
>Subject: Dr Sonja BOEHMER-CHRISTIANSEN

>
>
> Dear Professor Haughton,
> The email below was brought to my attention
>by the help desk of UKCP09 - the new set of UK
>climate scenarios developed for DEFRA. It was
>sent by the person named in the header of this
>email. I regard this email as very malicious. Dr
>Boehmer-Christiansen states that it is beyond her
>expertise to assess the claims made. If this is
>the case then she shouldn't be sending malicious
>emails like this. The two Canadians she refers
>to have never developed a tree-ring chronology in
>their lives and McIntyre has stated several times
>on his blog site that he has no aim to write up
>his results for publication in the peer-review literature.

> I'm sure you will be of the same opinion as
>me that science should be undertaken through the
>peer-review literature as it has been for over

>300 years. The peer-review system is the
>safeguard science has developed to stop bad science being published.

>
> In case you want to read more about the
>subject my colleague Keith Briffa has just put this up on his web site.

>
> <http://www.cru.uea.ac.uk/cru/people/briffa/yamal2009/>

>
> It has taken him some time, partly as he has
>been off after a serious operation in June. He
>has had to return early to respond to this. He
>has also had some difficulty contacting our Russian colleagues.

>
> The claims on the Climate Audit site are
>exaggerated, but get taken completely out of
>context by the other blog sites that get referred
>to in Dr Boehmer-Christiansen's email. I will
>draw your attention to two things

>
> 1. The Yamal chronology is only used in 3 of
>the 12 millennial temperature reconstructions in Ch 6 of the 2007 IPCC
Report.

>
> 2. McIntyre was sent the data for Yamal by our
>Russian colleagues on Feb 2, 2004.

>
> I realize Dr Boehmer-Christensen no longer
>works for you, but she is still using your affiliation.

>
> Best Regards
> Phil Jones

>
>
> From: Sonja A Boehmer-Christiansen <Sonja.B-C@hull.ac.uk>
> Date: 2 October 2009 18:09:39 GMT+01:00
> To: Stephanie Ferguson <stephanie.ferguson@ukcip.org.uk>
> Cc: "Peiser, Benny"
><B.J.Peiser@ljmu.ac.uk>, Patrick David Henderson
><pdhenderson18@googlemail.com>, Christopher Monckton <monckton@mail.com>
> Subject: RE: Please take note of
>potentially serious allegations of scientific 'fraud' by CRU and Met
Office

>
>
>
>
> Dear Stephanie

>
> I expect that a great deal of UKCIP work
>is based on the data provided by CRU (as does the
>work of the IPCC and of course UK climate
>policy). Some of this, very fundamentally, would
>now seem to be open to scientific challenge, and
>may even face future legal enquiries. It may be

>in the interest of UKCIP to inform itself in good
>time and become a little more 'uncertain' about its policy advice.

>
> Perhaps you can comment on the following
>and pass the allegations made on to the relevant people.

>
> It is beyond my expertise to assess the
>claims made, but they would fit into my
>perception of the whole 'man-made global warming'
>cum energy policy debate. I know several of
>the people involved personally and have no
>reason to doubt their sincerity and honour as
>scientists, though I am also aware of their
>highly critical (of IPCC science) policy positions.

>
> I could also let you have statements by
>Steve McIntyre and Ross McKittrick. Ross McKittrick
>currently teaches at Westminster Business School
>and who is fully informed about the relevant
>issues. He recently addressed a meeting of about 50 people in London.

>
> Best wishes

>
> Sonja B-C

>
> Dr.Sonja A.Boehmer-Christiansen
> Reader Emeritus, Department of Geography
> Hull University
> Editor, Energy&Environment
> Multi-Science (www.multi-science.co.uk)
> HULL HU6 7RX
> Phone:(0044)1482 465369/465385
> Fax: (0044) 1482 466340

>
> TWO copied pieces follow, both relate to CRU and UK climate
policy

>
> a. THE MET OFFICE AND CRU'S YAMAL SCANDAL: EXPLAIN OR RESIGN

>
> " Jennifer Marohasy <jennifermarohasy@jennifermarohasy.com>

>
> Leading UK Climate Scientists Must
>Explain or Resign, Jennifer Marohasy

>
> <
><<http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists->
>
>[http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists-](http://jennifermarohasy.com/blog/2009/09/leading-uk-climate-scientists-must-explain-or-resign/)
> must-explain-or-resign/>

>
> MOST scientific sceptics have been
>dismissive of the various reconstructions of
>temperature which suggest 1998 is the warmest
>year of the past millennium. Our case has been

>significantly bolstered over the last week with
>statistician Steve McIntyre finally getting
>access to data used by Keith Briffa, Tim Osborn
>and Phil Jones to support the idea that there has
>been an unprecedented upswing in temperatures
>over the last hundred years - the infamous hockey stick graph.

>
> Mr McIntyre's analysis of the data -
>which he had been asking for since
> 2003 - suggests that scientists at the
>Climate Research Unit of the United Kingdom's
>Bureau of Meteorology have been using only a
>small subset of the available data to make their
>claims that recent years have been the hottest of
>the last millennium. When the entire data set is
>used, Mr McIntyre claims that the hockey stick shape disappears
> completely. [1]

>
> Mr McIntyre has previously showed
>problems with the mathematics behind the 'hockey
>stick'. But scientists at the Climate Research
>Centre, in particular Dr Briffa, have
>continuously republished claiming the upswing in
>temperatures over the last 100 years is real and
>not an artifact of the methodology used - as
>claimed by Mr McIntyre. However, these same
>scientists have denied Mr McIntyre access to all
>the data. Recently they were forced to make more
>data available to Mr McIntyre after they
>published in the Philosophical Transactions of
>the Royal Society - a journal which unlike Nature
>and Science has strict policies on data archiving which it
> enforces.

>
> This week's claims by Steve McIntyre that
>scientists associated with the UK Meteorology
>Bureau have been less than diligent are serious
>and suggest some of the most defended building
>blocks of the case for anthropogenic global
>warming are based on the indefensible when the
> methodology is laid bare.

>
> This sorry saga also raises issues
>associated with how data is archived at the UK
>Meteorological Bureau with in complete data sets
>that spuriously support the case for global
>warming being promoted while complete data sets
>are kept hidden from the public - including from
>scientific sceptics like Steve McIntyre.

>
> It is indeed time leading scientists at
>the Climate Research Centre associated with the
>UK Meteorological Bureau explain how Mr McIntyre is in error or resign.

>

> [1] Yamal: A "Divergence" Problem, by
>Steve McIntyre, 27 September 2009
> <http://www.climateaudit.org/?p=7168>
>
> Jennifer Marohasy BSc PhD
>
>
>
> b. National Review Online, 23 September 2009
>
><<http://article.nationalreview.com/?q=ZTBiMTRlMDQxNzEyMmRhZjU3ZmYzODI5MGY4ZWl5OWM=>By>
>
>Patrick J. Michaels
>
>
> Imagine if there were no reliable
>records of global surface temperature. Raucous
>policy debates such as cap-and-trade would have
>no scientific basis, Al Gore would at this point
>be little more than a historical footnote, and
>President Obama would not be spending this U.N.
>session talking up a (likely unattainable)
>international climate deal in Copenhagen in
>December. Steel yourself for the new reality,
>because the data needed to verify the
>gloom-and-doom warming forecasts have disappeared.
>
> Or so it seems. Apparently, they were
>either lost or purged from some discarded
>computer. Only a very few people know what really
>happened, and they aren't talking much. And what
>little they are saying makes no sense.
> In the early 1980s, with funding from
>the U.S. Department of Energy, scientists at the
>United Kingdom's University of East Anglia
>established the Climate Research Unit (CRU) to
>produce the world's first comprehensive history
>of surface temperature. It's known in the trade
>as the "Jones and Wigley" record for its authors,
>Phil Jones and Tom Wigley, and it served as the
>primary reference standard for the U.N.
>Intergovernmental Panel on Climate Change (IPCC)
>until 2007. It was this record that prompted the
>IPCC to claim a "discernible human influence on global climate."
> Putting together such a record isn't at
>all easy. Weather stations weren't really
>designed to monitor global climate. Long-standing
>ones were usually established at points of
>commerce, which tend to grow into cities that
>induce spurious warming trends in their records.
>Trees grow up around thermometers and lower the
>afternoon temperature. Further, as documented by
>the University of Colorado's Roger Pielke Sr.,

>many of the stations themselves are placed in
>locations, such as in parking lots or near heat
>vents, where artificially high temperatures are bound to be recorded.

> So the weather data that go into the
>historical climate records that are required to
>verify models of global warming aren't the
>original records at all. Jones and Wigley,
>however, weren't specific about what was done to
>which station in order to produce their record,
>which, according to the IPCC, showed a warming of
>0.6T +/- 0.2T°C in the 20th century.

>
> Now begins the fun. Warwick Hughes, an
>Australian scientist, wondered where that "+/-"
>came from, so he politely wrote Phil Jones in
>early 2005, asking for the original data. Jones's
>response to a fellow scientist attempting to
>replicate his work was, "We have 25 years or so
>invested in the work. Why should I make the data
>available to you, when your aim is to try and find something wrong with
it?"

> Reread that statement, for it is
>breathtaking in its anti-scientific thrust. In
>fact, the entire purpose of replication is to
>"try and find something wrong." The ultimate
>objective of science is to do things so well that, indeed, nothing is
wrong.

>
> Then the story changed. In June 2009,
>Georgia Tech's Peter Webster told Canadian
>researcher Stephen McIntyre that he had requested
>raw data, and Jones freely gave it to him. So
>McIntyre promptly filed a Freedom of Information
>Act request for the same data. Despite having
>been invited by the National Academy of Sciences
>to present his analyses of millennial
>temperatures, McIntyre was told that he couldn't
>have the data because he wasn't an "academic." So
>his colleague Ross McKittrick, an economist at the
>University of Guelph, asked for the data. He was turned down, too.

> Faced with a growing number of such
>requests, Jones refused them all, saying that
>there were "confidentiality" agreements regarding
>the data between CRU and nations that supplied
>the data. McIntyre's blog readers then requested
>those agreements, country by country, but only a
>handful turned out to exist, mainly from Third
>World countries and written in very vague language.

> It's worth noting that McKittrick and I
>had published papers demonstrating that the
>quality of land-based records is so poor that the
>warming trend estimated since 1979 (the first
>year for which we could compare those records to
>independent data from satellites) may have been

>overestimated by 50 percent. Webster, who
>received the CRU data, published studies linking
>changes in hurricane patterns to warming (while others have found
otherwise).

> Enter the dog that ate global warming.

>

> Roger Pielke Jr., an esteemed professor
>of environmental studies at the University of
>Colorado, then requested the raw data from Jones. Jones responded:

> Since the 1980s, we have merged the data
>we have received into existing series or begun
>new ones, so it is impossible to say if all
>stations within a particular country or if all of
>an individual record should be freely available.
>Data storage availability in the 1980s meant that
>we were not able to keep the multiple sources for
>some sites, only the station series after
>adjustment for homogeneity issues. We, therefore,
>do not hold the original raw data but only the
>value-added (i.e., quality controlled and homogenized) data.

> The statement about "data storage" is
>balderdash. They got the records from somewhere.
>The files went onto a computer. All of the
>original data could easily fit on the 9-inch tape
>drives common in the mid-1980s. I had all of the
>world's surface barometric pressure data on one such tape in 1979.

> If we are to believe Jones's note to the
>younger Pielke, CRU adjusted the original data
>and then lost or destroyed them over twenty years
>ago. The letter to Warwick Hughes may have been
>an outright lie. After all, Peter Webster
>received some of the data this year. So the
>question remains: What was destroyed or lost,
>when was it destroyed or lost, and why?

>

> All of this is much more than an
>academic spat. It now appears likely that the
>U.S. Senate will drop cap-and-trade climate
>legislation from its docket this fall - whereupon
>the Obama Environmental Protection Agency is
>going to step in and issue regulations on
>carbon-dioxide emissions. Unlike a law, which
>can't be challenged on a scientific basis, a
>regulation can. If there are no data, there's no
>science. U.S. taxpayers deserve to know the
>answer to the question posed above. (Patrick J.
>Michaels is a senior fellow in environmental
>studies at the Cato Institute and author of
>Climate of Extremes: Global Warming Science They Don't Want You to
Know.) "

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>*****

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>
>
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From: Tom Wigley <wigley@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Revised CC text
Date: Fri, 06 Nov 2009 13:40:57 -0700

<x-flowed>

Thanks, Phil.

A bunch of us are putting something together on the latest Lindzen and Choi crap (GRL). Not a comment, but a separate paper to avoid giving Lindzen the last word.

Tom.

+++++

Phil Jones wrote:

>
>> Tom,
>
> Got to this sooner than I thought. I've responded to your points by
> saying things in comments and also responding to some points at the end
> of the references.
>
> Over the weekend I'll get the references into the same format. Can
> you have another look through? I think we are there on almost everything.
>
> Keith should be replying about the trees - a possible reason why KHAD
> is anomalous relates to permafrost depth. Impossible to prove and it's
> likely much more complicated. Difficult to detail with MM when they
> won't publish anything. They also know the global temperature record is
> robust, the millennial records less so. Taking one or two records out
> makes no difference and they know that. They go on about issues that
> have no effect.
>
> The CC article explains why the global T record is robust, so
> something to refer to. I don't think it is going to help our H-Indexes
> though!
>
> Have a good weekend!
>
> Phil

From: Tom Wigley <wigley@ucar.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: LAND vs OCEAN
Date: Fri, 06 Nov 2009 17:36:15 -0700

<x-flowed>

We probably need to say more about this. Land warming since 1980 has been twice the ocean warming -- and skeptics might claim that this proves that urban warming is real and important.

See attached note.

Comments?

Tom

</x-flowed>

Attachment Converted: "c:\eudora\attach\LANDvsOCEAN.doc"

From: "IPCC WGI TSU" <wg1@ipcc.unibe.ch>

Subject: IPCC Draft Good Practice Guidance Paper on Detection and Attribution for Review

Date: Tue, 10 Nov 2009 04:59:07 +0100 (CET)

Reply-to: wg1@ipcc.unibe.ch

Cc: stocker@climate.unibe.ch, qdh@cma.gov.cn, barros@at.fcen.uba.ar, cfield@ciw.edu, plattner@ipcc.unibe.ch, krisebi@ipcc-wg2.gov, midgley@ipcc.unibe.ch, tignor@ipcc.unibe.ch, wg1@ipcc.unibe.ch, tsu@ipcc-wg2.gov

Content-Type: text/plain; charset="iso-8859-1"

X-MIME-Autoconverted: from 8bit to quoted-printable by ueamailgate02.uea.ac.uk id nAA3xK1S014515

Dear Participants of the IPCC Expert Meeting on Detection & Attribution,
dear Colleagues,

Please find attached the draft version of the Good Practice Guidance Paper (GPGP) which has been prepared by the Core Writing Team (CWT) following the IPCC joint WGI/II Expert Meeting on Detection and Attribution. Gabi, Ove, Camille, David, Gino, Marty, Peter, and Sari, have been working very hard to meet the TSU deadline and have managed to provide the Co-Chairs with the attached draft version right in time for presentation at the IPCC Plenary in Bali the last week of October. We all owe them our sincere thanks for the efforts put into the preparation of this document.

Logistics:

We would now like to invite all participants of the Geneva Expert Meeting to review the GPGP and to provide comments and suggestions on the attached draft within 2 weeks from today (i.e. by *November 24*). If you do plan to provide your inputs, please prepare your comments in a separate document (word or plain text) in order to facilitate the handling of the comments from potentially ~30 participants. Submission of the files will be by email to the WGI TSU at wg1@ipcc.unibe.ch. We will collect all the reviews, combine them into an easily manageable format and will then forward them to the CWT. The task of the CWT will then be to consider all your comments and revise the GPGP accordingly. We do not plan to send the Guidance Paper out for a second round of comments, but trust that the CWT will make every effort to take your suggestions into account as much as possible.

Changes to terminology discussed in Geneva:

Please note that the CWT, after intense discussions, had to make a few changes to the language used in the "approved" documents from the last day's final plenary. One of the changes is the change from "direct" to "single step" attribution. Given the level of discussion created within the CWT and also during the meeting, the CWT felt it was more constructive NOT to insinuate which methods are better or stronger and so strived for neutral language, particularly as the views about what constitutes a strong method differed between groups (not only IPCC WGs). Note that the word "direct" already had created discussion during the final plenary of the Geneva meeting and was flagged as unresolved in the material sent to the CWT by the WGI TSU. As a consequence, the CWT has then changed "sequential" to "multi-step" to keep language consistent. The CWT has highlighted in the text by brackets where language was changed in order to maintain maximum transparency.

Material to be included in the Expert Meeting Report:

The GPGP will be part of the full meeting report which we are currently preparing at the WGI TSU. The full meeting report will include all the materials from the conference documentation, i.e. abstracts, participants list, agenda, etc. In Geneva, we also discussed to include additional science background material going along with the Guidance Paper. In light of the substantial GPGP we currently have, it seems sufficient to add a few (2-3) practical examples of D&A to the report which would illustrate and clarify in concrete terms the different points raised in the GPGP. As such examples are of a different nature than the GPGP text, we propose to present them in separate boxes. Our proposal is that the CWT will work on these D&A examples while the participants are commenting on the GPGP, and while the WGI TSU works on preparing the full meeting report. A further science element to be included in the full meeting report would be a non-comprehensive bibliography of D&A literature added at the end of the report (see separate email following).

I hope this way forward is acceptable to you. Thank you very much for your continued efforts and contributions to this important IPCC activity. We are looking forward to your inputs,

Cheers, Kasper
IPCC WGI TSU

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Attachment Converted: "c:\eudora\attach\IPCC_Guidance_DA_v081109.pdf"

From: Phil Jones <p.jones@uea.ac.uk>
To: Gil Compo <compo@colorado.edu>
Subject: Re: Twentieth Century Reanalysis preliminary version 2 data - One other thing!
Date: Tue Nov 10 12:40:26 2009

Gil,

One other good plot to do is this. Plot land minus ocean. as a time series.

This should stay relatively close until the 1970s. Then the land should start moving away from the ocean.

This departure is part of AGW. The rest is in your Co2 increases.

Cheers

Phil

Gil,

These will do for my purpose. I won't pass them on. I am looking forward to the draft paper. As you're fully aware you're going to have to go some ways to figuring out what's causing the differences.

You will have to go down the sub-sampling, but I don't think it is going to make much difference. The agreement between CRU and GISS is amazing good, as already know. You ought to include the NCDC dataset as well.

[1]<http://www.ncdc.noaa.gov/oa/climate/research/anomalies/index.html> the ERSST3b dataset.

In the lower two plots there appear to be two types of differences, clearer in the NH20-70 land domain.

The first is when reanl20v2 differs for a single year (like a year in the last 1960s, 1967 or 1968) and then when it differs for about 10 years or so. It is good that it keeps coming back. For individual years there are a couple of years in the first decade of the 20th century (the 1900s).

The longer periods are those you've noticed - the 1920s and the 1890s. There is also something up with the period 1955-65 and the 1970s. The 1920s seems to get back then go off again from about 1935 to early 1940s. Best thing to try and isolate some of the reasons would be maps for decades or individual years. For the 1920s I'd expect the differences to be coming from Siberia as opposed to Canada. I think the 1890s might be just down to sparser coverage. The 1890s is the only period where the difference brings your pink line back towards the long-term zero. All the others have the pink line more extreme than the HadCRUT3/GISS average.

Rob Allan just called. I briefly mentioned this to him. He suggested maps of data input during these times. He also suggested looking at the spread of the ensembles. Your grey spread is sort of this, but this is a different sort of ensemble to what Rob implied you might have?

One final thing - don't worry too much about the 1940-60 period, as I think we'll be changing the SSTs there for 1945-60 and with more digitized data for 1940-45. There is also a tendency for the last 10 years (1996-2005) to drift slightly low - all 3 lines. This may be down to SST issues.

Once again thanks for these! Hoping you'll send me a Christmas Present of the draft!

Cheers

Phil

At 20:45 09/11/2009, you wrote:

Phil,

1. I didn't get the attached.

Both version1 and version2 use HadISST1.1 for SST and sea ice.

2. time-varying CO2, volcanic aerosols, and solar variability (11-year cycle until 1949, "observed" after that) are specified.

Attached is a research figure. Please do not share.

In it, I have plotted the annual average (top panel) 50S to 70N global average 2m temperature from 20CRv2, SST/2m temperature from HadCRU3, SST/2m temperature from GISTEMP 1200km, and the 90% range of 2m air temperature from 25 CMIP3 models that can be extended beyond their 20C3M runs with SRESA1B. The ensemble mean is the thick gray curve. Averages are July-June.

(middle panel) 50S to 70N land-only 2m temperature from 20CRv2, 2m temperature from CRUTEM3, 2m temperature from GISTEMP land-only 1200km. CMIP3 data is the same.

(bottom panel) same as middle panel but for Northern Hemisphere land-only (20N to 70N).

Anomalies are with respect to 1901-2000. period is July 1891 to June 2005. The CRU (HadCRU) curves are supposed to be black.

No data has been masked by another dataset's observational availability, but missing values are not included in that dataset's area-weighted average.

Your ERA-Interim finding about it being warmer seems to be the case in the late 19th century but not the early 1920's.

Note that the only thermometer data in the magenta curve (20CRv2) is the HadISST1.1 over oceans. The two landonly panels are independent of thermometers, aside from the specified SSTs.

There are some very interesting differences, particularly late-19th century, 1920s, and WWII.

Correlations (I told you this was research, right?). The second pair is for linearly detrended data.

GLOBE (70N-50S)

reanl20v2.70n50s.landocean.juljun
hadcru3.70n50s.landocean.juljun 0.94370

reanl20v2.70n50s.landocean.juljun
hadcru3.70n50s.landocean.juljun 0.82017

reanl20v2.70n50s.landocean.juljun
gistemp_combined1200.70n50s.landocean.juljun 0.95284

reanl20v2.70n50s.landocean.juljun
gistemp_combined1200.70n50s.landocean.juljun 0.85808

hadcru3.70n50s.landocean.juljun
gistemp_combined1200.70n50s.landocean.juljun 0.99088

hadcru3.70n50s.landocean.juljun
gistemp_combined1200.70n50s.landocean.juljun 0.97383
GLOBAL LAND (70N-50S)

reanl20v2.70n50s.landonly.juljun
cru3.70n50s.landonly.juljun 0.85167

reanl20v2.70n50s.landonly.juljun
cru3.70n50s.landonly.juljun 0.68755

reanl20v2.70n50s.landonly.juljun
gistemp_land1200.70n50s.landonly.juljun 0.81469

reanl20v2.70n50s.landonly.juljun
gistemp_land1200.70n50s.landonly.juljun 0.60152

cru3.70n50s.landonly.juljun
gistemp_land1200.70n50s.landonly.juljun 0.98050

cru3.70n50s.landonly.juljun
gistemp_land1200.70n50s.landonly.juljun 0.95316
NH Land (20N-70N)

reanl20v2.nh_nohigh.landonly.juljun
cru3.nh_nohigh.landonly.juljun 0.82956

reanl20v2.nh_nohigh.landonly.juljun
cru3.nh_nohigh.landonly.juljun 0.67989

reanl20v2.nh_nohigh.landonly.juljun
gistemp_land1200.nh_nohigh.landonly.juljun 0.79247

reanl20v2.nh_nohigh.landonly.juljun
gistemp_land1200.nh_nohigh.landonly.juljun 0.59900

cru3.nh_nohigh.landonly.juljun
gistemp_land1200.nh_nohigh.landonly.juljun 0.98001

cru3.nh_nohigh.landonly.juljun

gistemp_land1200.nh_nohigh.landonly.juljun 0.95880

I thought that correlations of 0.8 to 0.85 were high for an independent dataset this long. I think that these are higher than the proxies?

The global isn't that fair because we have the HadISST.

The correlations are about the same as for AMIP runs, though. See

Hoerling M., A. Kumar, J. Eischeid, B. Jha (2008), What is causing the variability in global mean land temperature?, Geophys. Res. Lett., 35, L23712, doi:10.1029/2008GL035984.

It will be interesting to see if the masked numbers change.

Let me know if you need anything else on this for your essay material.

best wishes,

gil

Phil Jones wrote on 11/9/09 2:55 AM:

Gil,

A couple of questions.

1. See the attached. Is this paper providing the SST input to 20CRv2?
2. Do you change greenhouse gases in the run?

Apologies if these are answered elsewhere.

Do you have any pre-draft plots without subsampling to get some idea of how good the agreement?

I'm asking these questions as I'm writing an essay for Climate Change. There are no diagrams in this, but showing the agreement with 20CRv2 will be a nice way to finish the paper.

Paper briefly documents the magnitude of all the problems in global temperature data - such as SST biases, exposure issues, urbanization and site changes (in order of importance). Site changes for global averages are the least important. Trying to point to a few home truths to skeptics who keep on going on about the land data.

Cheers

Phil

At 15:39 03/11/2009, Gil Compo wrote:

Phil,

Already calculated. We don't suffer from some of the issues that you and Adrian raised because we use only surface pressure.

In the Northern Hemisphere extratropics, the agreement with the various (yours, GISTEMP, NOAA) thermometer-based near surface T is high, but in the Tropics and Southern Hemisphere, there are discrepancies, particularly over Africa and South America. The 20CRv2 does not have the intensity of the Siberia warming.

There are also discrepancies in the WWII period. I have not subset the reanalysis to correspond to a particular dataset's missing mask as all 3 have different coverages.

I'll be making plots for the paper (with a draft coming) soon.

best wishes,

gil

[2]P.Jones@uea.ac.uk wrote on 11/3/09 3:37 AM:

Gil,

I'm sitting in a meeting in Bristol with Rob Allan. We've had a thought. When you finish v2 will you be quickly calculating the global T average for the 1891-2006 period? Do you expect this to look like the real global T, or do you expect it to not show the longer timescale change that NCEP from 1948 showed?

I can send a paper with Adrian Simmons from JGR in 2004 on this when I'm back in Norwich tomorrow.

Cheers

Phil

Dear Colleagues,

Courtesy of the NOAA Earth System Research Laboratory Physical Sciences Division and University of Colorado CIRES Climate Diagnostics Center, at

[3]ftp://ftp.cdc.noaa.gov/Datasets/20thC_Rean/provisionalV2/ , please find temporary netCDF files from the 20th Century Reanalysis version 2 (1891-2006). These yearly files are for the ensemble mean analysis (means) and ensemble standard deviation (spreads) of selected variables. Colleagues from organizations contributing to the 20th Century Reanalysis version 2 or the International Surface Pressure Databank version 2.2, the observational input dataset, are welcome to investigate these preliminary files. Colleagues on the Atmospheric Circulation Reconstructions over the Earth Working Group 3 Verification and Validation of reanalyses are also welcome to begin working with these files.

We are working with our distribution partners at the National Center for Atmospheric Research, the National Oceanic and Atmospheric Administration (NOAA) Earth System Research Laboratory and NOAAs National Climatic Data Center on wider availability and documentation.

A rough draft of important documentation is attached.

Also, please see our new homepage at

[4]http://www.esrl.noaa.gov/psd/data/20thC_Rean/ which includes access to images of 6-hourly sea level pressure and 500 geopotential maps generated from the version 2 data.

When production is complete, the 20CR version 2 will span 1871 to present.

The references for the dataset are

Compo, G.P., J.S. Whitaker, P.D. Sardeshmukh, N. Matsui, R.J. Allan, X. Yin, B.E. Gleason, R.S. Vose, G. Rutledge, P. Bessemoulin, S. Brönnimann, M. Brunet, R.I. Crouthamel, A.N. Grant, P.Y. Groisman, P.D. Jones, M. Kruk, A.C. Kruger, G.J. Marshall, M. Maugeri, H.Y. Mok, Ø. Nordli, T.F. Ross, R.M. Trigo, X.L. Wang, S.D. Woodruff, S.J. Worley, 2009: The Twentieth Century Reanalysis Project. Quarterly J. Roy. Met. Soc., in preparation.

Compo, G.P., J.S. Whitaker, P.D. Sardeshmukh, 2008: The 20th Century Reanalysis Project. Third WCRP International Conference on Reanalysis, 28 January 2008, Tokyo, Japan

<

[5]http://wcrp.ipsl.jussieu.fr/Workshops/Reanalysis2008/Documents/V5-511_ea.pdf

>

Compo, G.P., J.S. Whitaker, and P.D. Sardeshmukh, 2006: Feasibility of a 100 year reanalysis using only surface pressure data. Bull. Amer. Met. Soc., 87, 175-190.

Whitaker, J.S., G.P. Compo, X. Wei, and T.M. Hamill 2004: Reanalysis without radiosondes using ensemble data assimilation. Mon. Wea. Rev., 132, 1190-1200.

Please let us know of any questions about the dataset. And, thank you for your contributions to its development.

Best wishes,

Gil Compo

[6]<compo@colorado.edu>

Jeffrey S. Whitaker

[7]

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20th Century Reanalysis Project leads

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+++++

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"Stop and consider the wondrous works of God."
Job 37:34

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[14]<http://www.esrl.noaa.gov/psd/people/gilbert.p.compo>

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References

1. <http://www.ncdc.noaa.gov/oa/climate/research/anomalies/index.html%A0>
2. <mailto:P.Jones@uea.ac.uk>
3. ftp://ftp.cdc.noaa.gov/Datasets/20thC_Rean/provisionalV2/
4. http://www.esrl.noaa.gov/psd/data/20thC_Rean/
5. http://wcrp.ipsl.jussieu.fr/Workshops/Reanalysis2008/Documents/V5-511_ea.pdf
6. <mailto:compo@colorado.edu>
7. <mailto:Jeffrey.S.Whitaker@noaa.gov>
8. <mailto:compo@colorado.edu>
9. <http://www.esrl.noaa.gov/psd/people/gilbert.p.compo>

10. <mailto:compo@colorado.edu>
11. <http://www.esrl.noaa.gov/psd/people/gilbert.p.compo>
12. <mailto:p.jones@uea.ac.uk>
13. <mailto:compo@colorado.edu>
14. <http://www.esrl.noaa.gov/psd/people/gilbert.p.compo>

From: AGU Atmospheric Sciences Section <Section@AGU.ORG>
To: <AS-SECTION_D@listserv.agu.org>
Subject: Letter to Atmospheric Sciences members
Date: Tue, 10 Nov 2009 14:23:32 -0500
Reply-to: AGU Atmospheric Sciences Section <Section@AGU.ORG>

Dear Atmospheric Sciences Section members,

First, I would like to remind you of three very close deadlines:

Nov. 12, Discounted registration for AGU Fall Meeting. Register at [1]https://www.associationsciences.org/agu/meet_demog.jsp, and sign up for our Atmospheric Sciences banquet on Dec. 15.

Nov. 13, Vote yes on AGU governance changes, [2]<http://www.agu.org/governancevote/>

Nov. 13, Please respond to [3]stacyjackson@berkeley.edu if you are willing to volunteer your expertise to help answer questions during the Copenhagen Conference of the Parties of the Framework Convention on Climate Change (see below).

Second, I would like to give you some information about where your contributions to AGU go. Last year, members of the Atmospheric Sciences Section contributed \$43,410 to AGU's Voluntary Contribution Campaign. In 2008, due largely to member donations like these, AGU facilitated career development events attended by 600 students, hosted 75 K-12 teachers at Fall Meeting workshops, and sponsored 31 members' visits with U.S. policy makers. Additionally, voluntary contributions allowed AGU to provide travel grants to 135 deserving students to present their research for the first time at an AGU meeting. These programs are essential for AGU's relevance and vitality. I know Atmospheric Science members want AGU to do more. Please join me in supporting AGU's efforts to strengthen our scientific society by making a gift to the 2010 Voluntary Contribution Campaign. Unrestricted contributions are used to support AGU's greatest needs, but you can directly support students pursuing Atmospheric Sciences by making a gift to the David Hofmann Travel Grant, Holton-Kaufman Grant, or Namias Travel Grant. You can make your gift when you renew your AGU membership, or you can give today at:

[4]https://www.agu.org/givingtoagu/making_your_gift.php

Sincerely,

Alan Robock

President, Atmospheric Sciences Section, AGU [5]robock@envsci.rutgers.edu

AGU Climate Scientists,

We are writing to encourage hundreds of you to participate in a unique opportunity to improve the public's climate knowledge during the week before and the week of this year's AGU Fall Meeting.

As you know, the Copenhagen negotiations (Dec. 7-18) are attracting hundreds of journalists and will result in a proliferation of media articles about climate change. Recently, the American public's "belief" in climate change has waned (36% think humans are warming the earth according to the Pew Center's October poll), and December's media blitz provides an opportunity to reverse the trend.

Your participation is needed to ensure that climate science coverage across media channels is accurate, fact-based, and nuanced. Provided that enough AGU members sign up to participate, we will be offering the opportunity for journalists reporting during the Copenhagen conference to submit their questions on-line and receive a response from a climate expert before an article goes to press.

We are asking each of you to sign up for two hours over the course of those two weeks (12/7-18) to respond to questions from journalists. You will be able to choose which queries to answer based on your expertise, and there will be an option to double-team when questions span multiple areas of expertise. We will be setting up the appropriate logistics to enable both virtual participation and a central work area at the AGU meeting. If you have any questions, feel free to email Stacy Jackson at the email address below.

If you are willing to participate, please respond in the affirmative by Friday November 13th to [6]stacyjackson@berkeley.edu. Given the magnitude of the media coverage, we are seeking several hundred willing climate scientists. More details will be forthcoming.

Thanks in advance,

Alan Robock, President, AGU Atmospheric Sciences Section

Anne Thompson, President-Elect, AGU Atmospheric Sciences Section

References

1. https://www.associationsciences.org/agu/meet_demog.jsp
2. <http://www.agu.org/governancevote/>
3. <mailto:stacyjackson@berkeley.edu>
4. https://www.agu.org/givingtoagu/making_your_gift.php
5. <mailto:robock@envsci.rutgers.edu>
6. <mailto:stacyjackson@berkeley.edu>

From: Phil Jones <p.jones@uea.ac.uk>
To: c.harpham@uea.ac.uk
Subject: FW: Helpdesk query 1489: Hourly data have discontinuities at day joins
Date: Tue Nov 10 16:35:20 2009

Colin,
I thought that this didn't happen.
Cheers
Phil

From: C G Kilsby <c.g.kilsby@newcastle.ac.uk>
To: "p.jones@uea.ac.uk" <p.jones@uea.ac.uk>
Date: Tue, 10 Nov 2009 15:35:37 +0000
Subject: FW: Helpdesk query 1489: Hourly data have discontinuities at day joins
Thread-Topic: Helpdesk query 1489: Hourly data have discontinuities at day joins
Thread-Index: AcpiFAtfZVu2N5gLTBW4NaA+k/QJowAB1zVA
Accept-Language: en-GB
X-MS-Has-Attach:
X-MS-TNEF-Correlator:
acceptlanguage: en-GB
X-smtpf-Report: sid=1A9FZe094454569100; tid=1A9FZe0944545691XL;
client=lan,relay,white,ipv6; mail=; rcpt=; nrcpt=1:0; fails=0
X-Canit-CHI2: 0.00
X-Bayes-Prob: 0.0001 (Score 0, tokens from: @@RPTN, f028)
X-Spam-Score: 0.00 () [Hold at 5.00] HTML_MESSAGE,SPF(pass,0)
X-CanItPRO-Stream: UEA:f028 (inherits from UEA:default,base:default)
X-Canit-Stats-ID: 35355645 - b33bcd1c960c (trained as not-spam)
X-Antispam-Training-Forget:
[1]<https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=f>
X-Antispam-Training-Nospam:
[2]<https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=n>
X-Antispam-Training-Spam: [3]<https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=s>
X-Scanned-By: CanIt (www . roaringpenguin . com) on 139.222.131.184
Interesting one for you....

From: Lyndsey Middleton [[4]mailto:lyndsey.middleton@ukcip.org.uk]
Sent: 10 November 2009 2:43 PM
To: C G Kilsby
Subject: Helpdesk query 1489: Hourly data have discontinuities at day joins

Hi Chris,

Another Weather Generator query for you. It was raised by Richard Watkins of Manchester University (and COPSE project) following a visit from Roger yesterday.

Can you let me know your response please?

Cheers,
Lyndsey

Long Description=The hourly data from the Weather

Generator have discontinuities at each

midnight join. The e.g. temperature

jumps, may be as high as $9\frac{1}{2}^{\circ}\text{C}$. The

hourly data seem to have been generated

independently for each day, rather than

fitting a curve from the maximum of one

day to the minimum of the next. The

minimum to maximum curve, i.e. within

each day, is fine.

Could the Weather Generator be altered

to produce more realistic hourly data

by fitting from Tmax to Tmin the

following day, please? This would be

helpful particularly for any use of the

data for building simulation with plant

controls.

Thanks,

Richard Watkins

Lyndsey Middleton
Enquiries Officer

UK Climate Impacts Programme
School of Geography and Environment
OUCE
South Parks Road
Oxford OX1 3QY

[5]www.ukcip.org.uk

Tel: 01865 285 718 (direct) or 01865 285717 (switchboard)

My working days are: Tuesday and Wednesday 9am to 5pm and Friday 9 am to 12.30pm

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References

1. <https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=f>
2. <https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=n>

3. <https://canit.uea.ac.uk/b.php?i=35355645&m=b33bcd1c960c&c=s>

4. <mailto:lyndsey.middleton@ukcip.org.uk>

5. <http://www.ukcip.org.uk/>

From: Phil Jones <p.jones@uea.ac.uk>
To: Sandy Tudhope <sandy.tudhope@ed.ac.uk>
Subject: Latest draft of WPI
Date: Thu Nov 12 10:18:54 2009
Cc: "Wolff, Eric W" <ewwo@bas.ac.uk>, Rob Wilson <rjsw@st-andrews.ac.uk>, "Bass, Catherine" <C.J.Bass@exeter.ac.uk>, "Turney, Christian" <C.Turney@exeter.ac.uk>, Rob Allan <rob.allan@metoffice.gov.uk>, Keith Briffa <k.briffa@uea.ac.uk>, "t.osborn@uea.ac.uk" <t.osborn@uea.ac.uk>

Dear All (especially Chris/Catherine),

Here's the latest draft of WPI. All in the group have now commented and amended this. You should have the 3 supporting letters from Tree partners. Eric was contacting Eric Steig and Sandy (see below) is contacting 3 coral people.

There is an issue about a Map. Rob W put one in his PhD page. This shows the corals. If we were to add the tree-ring sites we would mainly get a splodge of points in South America and NZ. Ice cores would just be over the AP and in the low-lat Andes. Issue is one of space. We already have 3pp fo this WP. Refs will reduce to about 0.5pp once we go to et al for 3 or more authors. A map would be useful for presentation to NERC, but is it essential for the submission?

I'm away from tomorrow lunchtime for the weekend. Back in on Monday. Hope we'll be looking through more complete drafts next week!

Cheers

Phil

At 19:02 11/11/2009, Sandy Tudhope wrote:

Dear Phil et al,

Good to speak to you earlier Phil and Rob W..

Please find attached a slightly modified version for WPI ... I've just changed the coral section a bit. Briefly, I've identified the new coral coring sites (rather than get bogged down trying to describe how we will use analysis of model output to prioritise), plus I've added back in some references and details that I think help, but don't add too much length.

I've written to Janice Lough, Julie Cole and Kim Cobb re being Project Partners (I actually spoke to Kim and she is keen).

FIGURE: I still think it might be useful to have a map in the main proposal ... basically like the one Rob has in the PhD proposal ... we can simply have boxes around the tree ring and ice core regions. This map needn't be any larger than Rob already has it ... but it does help illustrate where we will get/have data. What do you all think?

Cheers,

Sandy

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From: "Thorne, Peter (Climate Research)" <peter.thorne@metoffice.gov.uk>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Letter draft
Date: Thu, 12 Nov 2009 14:17:44 -0000

Phil, attached is a draft letter. We were keen to keep it as short, sweet and uncomplicated as possible without skipping over important details. Shorter, simpler, requests are more likely to get read and acted upon was the specific advice from international relations.

--

Peter Thorne, Climate Research scientist
Met Office Hadley Centre, FitzRoy Road, Exeter, EX1 3PB.
tel. +44 1392 886552 fax. +44 1392 885681
<http://www.hadobs.org>

Attachment Converted: "c:\eudora\attach\Phil_letter_draft_091109.doc"